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Abstract
The authors estimate the effect of the 2004–6 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 who do not have a high school diploma. Using data drawn from the 2004 and 2006 Current Population Survey, they employ difference-in-difference estimates to show that the NYS minimum wage increase is associated with a 20.2% to 21.8% reduction in the employment of less-skilled, less-educated workers, with the largest effects on those aged 16 to 24. Their estimates imply a median employment elasticity with respect to the minimum wage of around –0.7, large relative to previous researchers' estimates. The authors' findings are robust to their choice of geographically proximate comparison states, the use of a more highly skilled within-state comparison group, and a synthetic control design approach. Moreover, their results provide plausible evidence that state minimum wage increases can have substantial adverse labor demand effects for low-skilled individuals that are outside previous elasticity estimates, ranging from –0.1 to –0.3.

Keywords
minimum wage, employment, difference-in-difference

Cover Page Footnote
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: 1285 University of Oregon, Eugene OR 97403, bchansen@uoregon.edu, and from Joe Sabia at: San Diego State University, Department of Economics, 5500 Campanile Drive, San Diego, CA 92182-4485, jsabia@mail.sdsu.edu. Acknowledgments: 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.

ARE THE EFFECTS OF MINIMUM WAGE INCREASES ALWAYS SMALL? NEW EVIDENCE FROM A CASE STUDY OF NEW YORK STATE

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The authors estimate the effect of the 2004–6 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 who do not have a high school diploma. Using data drawn from the 2004 and 2006 Current Population Survey, they employ difference-in-difference estimates to show that the NYS minimum wage increase is associated with a 20.2% to 21.8% reduction in the employment of less-skilled, less-educated workers, with the largest effects on those aged 16 to 24. Their estimates imply a median employment elasticity with respect to the minimum wage of around –0.7, large relative to previous researchers' estimates. The authors' findings are robust to their choice of geographically proximate comparison states, the use of a more highly skilled within-state comparison group, and a synthetic control design approach. Moreover, their results provide plausible evidence that state minimum wage increases can have substantial adverse labor demand effects for low-skilled individuals that are outside previous elasticity estimates, ranging from –0.1 to –0.3.

Although 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 ef-

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fects 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. 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. 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.

Studying New York State's large minimum wage increase has important advantages over earlier state case studies, which have been scrutinized over their choice of counterfactual comparison states. In our case study of New York, we not only examine pretreatment employment trends for both lower-skilled *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 2004–6 New York minimum wage hike. This feature allows us to explore whether low-skill 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, our study is the first in the minimum wage literature to employ a synthetic control design. In it we generate a synthetic comparison state that most closely resembles the treatment state based on pretreatment 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 more than half 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.

Literature

The iconoclastic work of Card and Krueger (1994; 1995) prompted 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, researchers have undertaken a substantial number of new studies of the effect of state and federal minimum wage laws that use more precise data and often use natural experiment techniques. Neumark and Wascher (2007; 2008) review more than 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 study finds that controlling for state-specific linear time trends reduces the estimated effect of minimum wages on employment. But, although 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 subgroups that are differentially affected (Neumark and Wascher 2008). Although Brown (1999: 2114–15) and Neumark and Wascher (2007: 61–62) provide a strategy for adjusting employment elasticities for heterogeneous treatment groups that uses 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. An important drawback of this approach, however, is that it measures only 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: 84)

Thompson (2009) took another approach to identifying those for whom minimum wage increases are binding. Using a repeated cross-section of counties drawn from census data, Thompson finds that minimum wages increases from 1996 to 2000 had a small, statistically insignificant effect on overall teenage employment. But when he focuses on more localized labor markets in which the minimum wage was binding—counties where the pretreatment 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 these large national panel studies, others 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. They use Pennsylvania as their control state and find no evidence of adverse employment effects, but they do find evidence of positive employment effects. The findings of this study, however, have been criticized for 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 from California's minimum wage increase on state retail employment growth. Similarly, 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 it finds little very limited evidence of adverse employment effects but no evidence of positive employment effects as found in Card and Krueger (1994).

One 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, and this raises concerns that Card's estimates were subject to omitted variable bias.[1] Hamermesh (1995) also questions this identification strategy and finds that beginning in 1988, employment trends in New Jersey began to diverge significantly from those in Pennsylvania, which casts doubt on the findings of Card and Krueger (1994). More generally, Hamermesh cautions that in these case studies, "any changes in the relative demand shocks will swamp the effect of a higher minimum wage." (1995: 837).

In summary, previous case studies of minimum wage hikes have tended to find small or no adverse employment effects, and critiques of these case studies 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, although previous case studies of the minimum wage have estimated industrywide 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 because we have been able not only to explore pretreatment trends in low-skilled employment in both treatment and comparison states before the minimum wage increase, but also to explore a period just after the hike when all states raised their minimum wages. This allows us to better explore whether any differential trends attributed to the minimum wage can be explained by pre-existing or subsequent employment trends. 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 resemble the treatment state based on pretreatment trends in observable economic conditions rather than geographic proximity to New York.

[1]Card and Krueger (1995), however, note that employment trends were similar in the period before 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 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.

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.

Although 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, because of their potential to be affected by minimum wage hikes, non-teenage less-educated individuals have increasingly drawn attention from researchers. 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, 2000b) examine less-educated individuals in their twenties. Second, as Neumark and Wascher 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. (2007: 61)

Although there are reasons relevant to policy decisions to include less-educated older individuals in our treatment group—in addition to the gains in statistical power from drawing on a larger sample—we also explore heterogeneity in the effects of minimum wage increases by age because we might expect the youngest, least experienced individuals to experience the largest minimum wage effects.

We will first show that the New York 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 on the share earning \$6.75 and then by comparing these trends with those in our control states, where the minimum wage remained at \$5.15 per hour during 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 to take a difference-in-difference approach similar to that used by Card (1992) and Card and Krueger (1994). We restrict the sample to individuals age 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 1 if the individual lives in New York in 2006 and 0 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.[2] The key parameter of interest in this model is β_1, the difference-in-difference (DD) estimator. The estimate of β_1 will be unbiased only if unmeasured employment trends are similar in the treatment and comparison states. Thus, our choice of comparison states is critical.

We begin by using low-skilled individuals in bordering or geographically proximate states to form a comparison group. In the first two columns of Table A.1 in the appendix, 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 before New York State's minimum wage increase (2002–4). We find that, in general, the characteristics of our selected geographically proximate comparison states (column 2) 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 that had a $5.15 minimum over the 2004–6 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.[3]

The key concern with a difference-in-difference approach is whether the choice of comparison group serves as an appropriate counterfactual. Although 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 we estimate a difference-in-difference-in-difference (DDD) model of the following form:

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

[2] 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 here.

[3] The unionization rate for prime-age males was also higher in New York State, but New York has the highest unionization rate (21.4%) in the nation, while the national average is 12.3%.

where A_{ist} is an indicator variable coded equal to 1 if the respondent is a 16-to–29-year-old without a high school degree and equal to 0 if the respondent is a member of the more highly skilled within-state comparison group. The key parameter of interest in equation (2), β_1, is the difference-in-difference-in-difference estimator.[4]

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 before and just after the 2004–06 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 2004–6 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 uses factors that are likely predicators of changes in employment rates and 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 pretreatment trends in the above factors, weights are chosen from a set of donor states—in our case, the 25 states with a $5.15 minimum wage from 2002 to 2006—to construct a synthetic control group whose labor market characteristics most closely resemble those of the treatment state (see Abadie, Diamond, and Hainmueller 2010).[5]

The synthetic control group is obtained by aggregating the microdata 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 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) and give all other states a weight of zero. In the latter, we allow

[4]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 and if low- and high-skilled workers are gross substitutes or complements, the demand for higher-skilled workers may be affected. 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.

[5]The donor states for our analysis are the 25 states that 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.

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 an additional comparison group with which to estimate the labor demand effects of New York State minimum wage, and 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 that in large part support our ex-ante chosen counterfactual group, this would add additional credibility to our identification strategy.

Results

All the estimates here are weighted by the relevant state population, and bootstrapped standard errors are corrected for clustering on the state (Bertrand, Duflo, and Mullainathan 2004).[6]

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. The wage rate for workers who report being paid hourly is directly reported from their current job. Wage rates for those who are not paid hourly 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 %) 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 and would be directly affected by

[6]Inference in the presence of serial correlation has posed a problem in hypothesis testing for some time. Athough 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, 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 on average than bootstraps that lack such higher-order properties. Although the Wild bootstrap was the method of choice in the Cameron, Gelbach, and Miller (2008) study, its power quickly falls to 0 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 $\hat{\sigma}_b^2 = \frac{\Sigma_{b=1}^{B}(\hat{\beta}_b - \bar{\beta})^2}{B}$ where $\bar{\beta} = \frac{\Sigma_{b=1}^{B}\hat{\beta}_b}{B}$, resampling within groups to replicate the inherent correlation present in the data. The square root of the bootstrap variance yields a standard error that 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.

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

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

Notes: Estimates are obtained using data from the 2004 and 2006 Current Population Survey Outgoing Rotation Groups from respondents age 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.
*Statistically significant at the .10% level; **at the .05 level; ***at the .01% level.

the minimum wage hike.[7] 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.[8] 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 who fell in each wage category. We find that the 2004–6 New York minimum wage increase is associated with a 6.6 percentage-point decline in the share of low-skilled workers who earned hourly wages between \$5.15 and \$5.99 and a 6.7 percentage-point decline in the share of workers who 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 who earned \$6.75 per hour. We find no evidence of spillover effects, whereby workers without high school degrees earning above the minimum wage (i.e., 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 age 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 % of New York's employed teenagers (age 16 to 19) without a high school degree earned between \$5.15 and \$6.74 per hour compared to 19.6% of 20- to 24-year-old dropouts, and 9.8 percent of 25- to 29-year-old dropouts.

[7]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. Our estimated wage effects, however, 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, 1994b) 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. But in New York, we find little evidence that the minimum wage affects the share of workers earning less than \$5.15 per hour, presumably in uncovered jobs.

[8]The share of workers earning between \$6.50 and \$6.74 per hour, however, remained fairly steady between 2004 and 2006. In fact, in 2006, just over 20% earned wages less than \$6.75, which could suggest (1) lagged enforcement effects, (2) a shift in employment toward the uncovered sector not covered by state minimum wages, or (3) reporting error in hourly wages. For example, it may be the 6.5 % of wage earners reporting wages between \$6.50 and \$6.74 are actually earning the minimum wage.

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

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.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

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 findings suggest 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 16- to 29-year-old low-skilled New Yorkers fell from 0.362 to 0.291, a decline of 7.1 percentage-points (19.6 %) 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 (col-

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

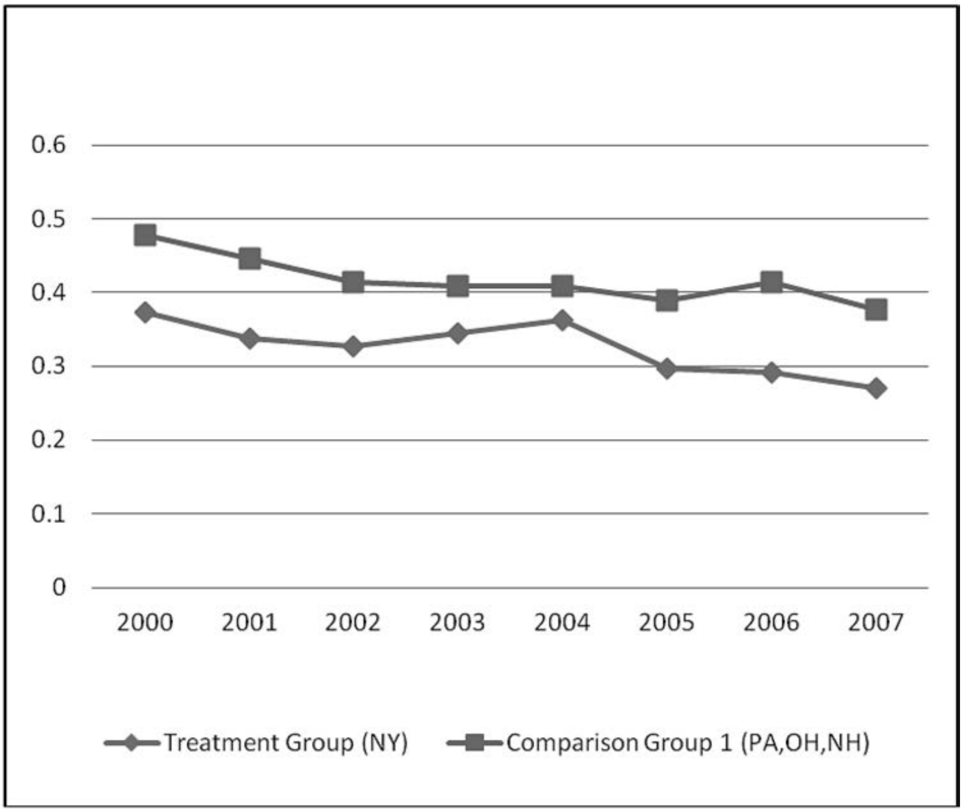
Notes: Estimates obtained using data from the 2004 and 2006 Current Population Survey Outgoing Rotation Groups. All estimates are weighted. Columns (1)-(4) present means with standard deviations in parentheses and sample sizes are in brackets. Column (5) shows difference-in-difference estimates with bootstrapped standard errors corrected for clustering on the state in parentheses.
*Statistically significant at the .10% level; **at the .05 level; ***at the .01 level.

umn 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 % minimum wage hike was associated with an 20.2 % 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).[9]

[9]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: 2114–16) and Neumark and Wascher (2007: 61–62) provide a method for adjusting these elasticities to obtain employment elasticities for affected individuals. Neumark and Wascher (2007: 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% 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: 2114–15; Neumark and Wascher 2007: 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

Figure 1. Employment Trends of 16- to 29-Year-Olds without High School Diploma, 2000–2007



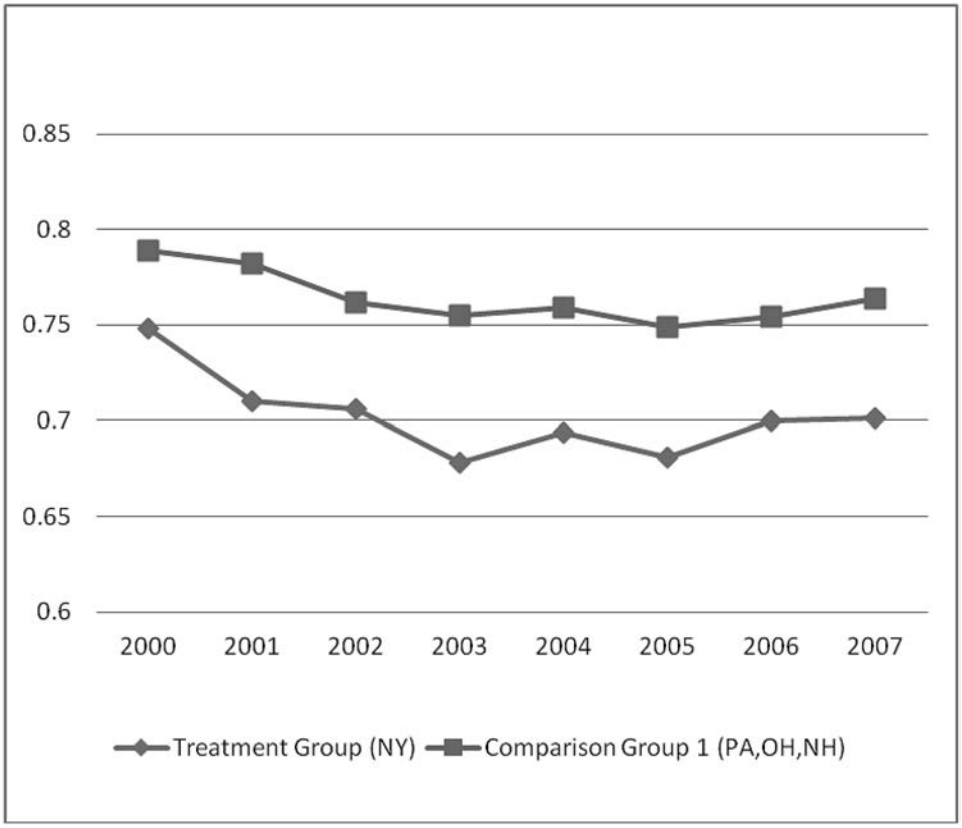
In rows 2 to 4 of Table 3, we present difference-in-difference estimates of the employment effects of the minimum wage by age. Consistent with the evidence in Table 2, we find the largest employment effects for younger individuals ages 16 to 24, the same group for whom the minimum wage was more binding. Adjusted difference-in-difference estimates suggest that estimated employment elasticities are largest for teenagers (–0.892) and decline with age (–0.844 for 20- to 24-year-old dropouts and –0.373 for 25- to 29-year-old dropouts).[10]

Could the differences in low-skilled employment trends we observe in the 2004–6 simply capture differential employment trends in New York and the comparison states that have little to do with the minimum wage increase?

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.

[10]Sabia and Burkhauser (2010) show that these results hold for white 16- to 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).

Figure 2. Employment Trends of 20- to 29-Year-Olds with a High School Degree or More, 2000–2007



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 2004–6 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 2004–6 period.

In the first row of Table 4, we use a triple-difference approach to examine whether the difference-in-difference estimates of employment effects for low-skilled workers are significantly different from those for more highly skilled workers. 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,

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) Treatment Group: Age 16 to 29	–0.078*
Without a HS Degree	(0.046)
	[16,020]
Elasticity	*–0.693*
(2) Treatment Group: Age 16 to19	–0.077**
Without a HS Degree	(0.039)
	[14,776]
Elasticity	*–0.953*
(3) Treatment Group: Age 20 to 24	–0.148*
Without a HS Degree	(0.078)
	[11,569]
Elasticity	*–0.887*
(4) Treatment Group: Age 25 to 29	–0.071
Without a HS Degree	(0.061)
	[11,377]
Elasticity	*–0.378*

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. *Statistically significant at the .10% level; **at the .05 level; ***at the .01 level.

an elasticity that is once again large relative to consensus estimates (Neumark and Wascher 2008).[11]

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 age 16 to 24 (rows 2 and 3) and are much smaller for individuals age 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 subpopulations with a relatively greater share of workers that earned wages between $5.15 and $6.74 per hour. In Table 5, we define 12 subgroups of lower-skilled and more highly skilled individuals disaggregated by age and

[11]In Appendix Table A.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.

Subgroups of our highly skilled population could be directly affected by the minimum wage. As Table 5 shows, 11.6 percent of 20- to 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 here (see column 1 of Appendix Table A.3).

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

Subgroup	*Share New York Workers Earning \$5.15–\$6.74 in 2004*	*Diff-in-Diff*		*Employment Elasticity*
No High School Degree				
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
At Least High School Degree but No Bachelors				
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
More than a High School Degree				
30- to 34-year-olds	0.032	0.017	(0.022)	0.071
35- to 39-year-olds	0.025	0.019	(0.021)	0.080
40- to 44-year-olds	0.024	0.024	(0.020)	0.096
45- to 49-year-olds	0.026	–0.026	(0.020)	0.103
50- to 54-year-olds	0.024	0.006	(0.022)	0.025
At Least a Bachelor's Degree				
20- to 24-year-olds	0.051	–0.025	(0.032	–0.135
25- to 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)	

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 subgroup in the first column who reside in the geographically proximate states of Pennsylvania, Ohio, and New Hampshire. All estimates are weighted. Standard errors corrected for heteroskedasticity are in parentheses.
*Statistically significant at the .10% level; **at the .05 level; ***at the .01 level.

education and present difference-in-difference estimates of the employment effects for each subgroup. Consistent with the results already reported, we find the largest adverse employment effects for those individuals with larger shares of affected workers. For instance, for teenagers age 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 subgroup on the share of New York workers in each subgroup 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.[12]

[12]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 consis-

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

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.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

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 age 16 to 29 and the more highly skilled comparison group during the 2004–6 minimum wage window. As we discussed earlier, the declines in low-skilled employment in New York are strongest for younger, less-educated individuals ages 16 to 24 (columns 1 and 2) 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 2002–4 pre-treatment period.[13]

In the third row of Table 6, we examine the period 2006–7, just after the 2004–6 minimum wage hike when, in addition to New York, each of the comparison states raised its minimum wage. On January 1, 2007, Pennsylvania raised its minimum wage from $5.15 per hour to $6.15; Ohio raised its

tent with expectations that populations with more affected workers experience larger adverse employment effects.

[13]Although 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 2003–4 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.

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 to 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 here 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 the 25 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 counterfactual 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 1) to generate weights to place on each regressor. Appendix Table A.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. Using each of these characteristics and their respective weights, we chose a synthetic control state as a weighted average of all states that 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) when we used the share employed as the outcome variable, only four states received a positive weight: Ohio and Pennsylvania receive 38% and 29% respectively; Maryland receives 27%; and Michigan receives 6 %. Notably, in our synthetic control design that does not include geographical prox-

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 to 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.

imity 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 Table A.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.[14] 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 (2002–4) 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

[14]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 to 2004.

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

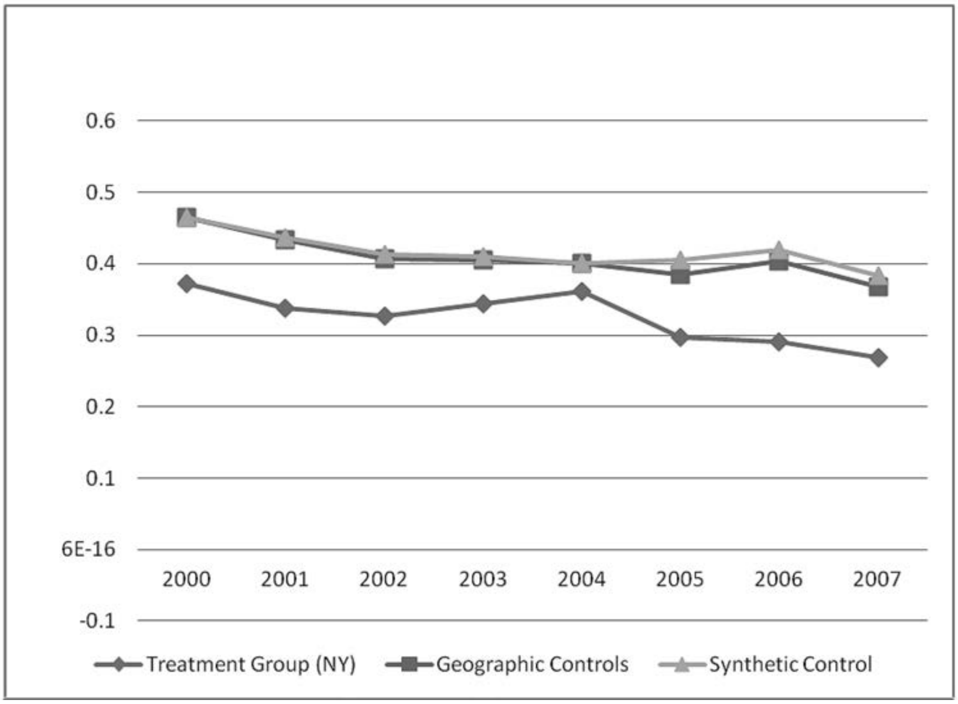


Table 8. Placebo Tests of New York State versus Synthetic Control State in Pre-Treatment (2002–4) Period

	Pre-Treatment 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)

Notes: Difference-in-difference estimates are calculated using 2002 as the pre-treatment 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 by using employment as an outcome in 2004.

as the comparison group.[15] Using the synthetic control group, we find that the increase in New York State's minimum wage is associated with a 11.0

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

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

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 2004–06 with a placebo law change introduced at the beginning of 2005.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

percentage-point decrease in the share of workers age 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 = –0.701).[16] Consistent with our findings in Table 4, the estimated employment elasticity is largest for younger individuals age 16 to 19 (–1.010) and smallest for older dropouts age 25 to 29 (–.314).

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 signifi-

[16]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 United States, 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 use placebo groups chosen from the other states that did not alter their minimum wage between 2004 and 2006, and we introduce a placebo law change in 2005. We use the point estimate of these placebo effects 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.

cantly 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 %, 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.

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 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.

Appendix

Table A.1. Summary Characteristics of NY and Counterfactual Groups in 2004

	New York	*PA, OH, NH*	*All of US (outside NY)*	*States with $5.15 MW*	*Synthetic Control Group*
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

continued

Table A.1. Continued.

	New York	*PA, OH, NH*	*All of US (outside NY)*	*States with $5.15 MW*	*Synthetic Control Group*
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.

Table A.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 (1)	*Second Phase from $6.00 in 2005 to $6.75 in 2006[1] (2)*
Effect of Minimum Wage Increase on Employment of 16- to 29-year-olds without HS Degree	–0.045*	–0.031**
	(0.027)	(0.015)
	[5,345]	[5,006]

Notes: Estimates in columns (1) and (2) are obtained using data from the 2004 and 2005 Current Population Survey Outgoing Rotation Groups. Estimates 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.

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

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

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

	Baseline Year = 2004 (1)	*Baseline Year = 2003* (2)	*Baseline Year = 2002* (3)
Comparison Group I: 25- to 29-year-old college grads	–0.097**	–0.090*	–0.041
	(0.047)	(0.052)	(0.048)
	[7,226]	[7,375]	[7,398]
Elasticity	–0.863	–0.762	–0.352
Comparison Group I: 20- to 29- year-olds with ≥ HS Degree	–0.078*	–0.055*	–0.030
	(0.046)	(0.030)	(0.025)
	[16,020]	[16,932]	[16,526]
Elasticity	–0.693	–0.466	–0.257
Comparison Group I: 30- to 54-year-olds with > HS Educ	–0.080*	–0.059*	–0.046
	(0.042)	(0.032)	(0.029)
	[27,030]	[27.796]	[28,251]
Elasticity	–0.711	–0.500	–0.395

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.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table A.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

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

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