WORKING PAPER NO. 2014-06

Are the Effects of Minimum Wage Increase Always Small?

A Re-Analysis of Sabia, Burkhauser, and Hansen

By

Saul D. Hoffman

WORKING PAPER SERIES

UNIVERSITY OF DELAWARE | Alfred Lerner College of Business & Economics

DEPARTMENT OF ECONOMICS

Are the Effects of Minimum Wage Increases Always Small?

A Re-Analysis of Sabia, Burkhauser, and Hansen

Saul D. Hoffman
Department of Economics
University of Delaware

January 28, 2014

Abstract: In a recent article, Sabia, Burkhauser, and Hansen report very large negative employment effects of the 2004-2006 increase in the NY state minimum wage on young, less-educated workers. I re-examine their estimates using data from the full CPS, rather than the smaller MORG files they use. I find no evidence whatsoever of a negative employment impact. When the two data sources conflict, there can be no doubt that the full CPS, which is the source of official employment data, is the more appropriate. Furthermore, when I repeat their analysis using three states and the District of Columbia that also had a substantial increase in the state minimum wage, I find evidence of a small positive employment effect.

Key Words: Minimum Wage

JEL Codes: J08, J21, J38

In a recent contribution to the minimum wage literature, Sabia, Burkhauser, and Hansen (2012) ask "Are the Effects of Minimum Wage Increases Always Small?" Using evidence from the 2004-2006 increase in the New York state minimum wage from $5.15 to $6.75, they answer emphatically "no." They find that employment for less-educated workers under age 30 fell by 20%, which yields an employment elasticity of approximately -.7, far larger than estimates found in most of the more recent empirical minimum wage literature. Indeed, they conclude that "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" (p. 372). This result has been widely cited by conservative think tanks and on-line commentators as an important argument against an increase in the federal or state minimum wage.[1]

Their analysis is based on employment data from the Current Population Survey-Merged Outgoing Rotation Group (CPS-MORG) files for 2004 and 2006. They use a variety of difference methods to compare employment changes in NY to the corresponding changes in either neighboring states or a synthetic control group. While the analysis is very capably executed, it is undermined by two factors. First, the data set they use yields estimates of the employment rate in NY and the control group states that differ substantially from the official rates derived from the full CPS. The MORG files used by Sabia, Burkhauser and Hansen (hereafter SBH) are a subset of the regular CPS that includes the one-quarter of the CPS panel that is rotating out of the sample after either four or eight months in the survey.[2] The full CPS, not the MORG

[1] See, for example, Hotz-Eakin (2013) in the Huffington Post and Employment Policies Institute (2012).
[2] CPS sample members are interviewed for four months consecutively, leave the survey for eight months, and then return for another four months. They are part of the ORG files in both of their final months of interviewing.

subsample, is the source of official BLS tabulations of employment and unemployment and it is clearly the preferred data source. SBH used the MORG data because, unlike the regular CPS, they include information on wage rates for workers paid by the hour and weekly earnings for other workers. This is essential information for computing wage impacts of the minimum wage, but not for estimating employment effects. As shown below, the employment rate effects computed from the full CPS files for 2004 and 2006 yield a very different picture of the impact of the minimum wage increase in NY. While the MORG files are, in principle, an appropriate data set to use, in practice their representativeness may fail for relatively small samples, as it apparently did in this case.

Second, NY appears to be a somewhat idiosyncratic treatment state. In natural experiments like this one, it is always necessary to assume that the treatment and control groups are similar except for the treatment itself, here, the minimum wage increase. If that were true, then, by extension, states with minimum wage increases similar to that in NY would be expected to have relatively similar responses. But that is not the case. Three other large states and the District of Columbia had minimum wage increases at the same time that were quite substantial—an average increase of $1.03 or 18.7%. The employment response to the minimum wage increase in these states is substantially different from that in NY. Indeed, the employment rate of the same group analyzed by SBH increased in these states relative to states that had no increase in the minimum wage.

In this paper, I re-examine the NY minimum wage experiment analyzed by SBH using both the CPS-MORG and the full CPS data for 2004 and 2006. I also apply the same methods to

examine the impact of the minimum wage increase in the other states with a sizeable increase. The next section of the paper briefly reviews the analysis and findings of SBH and then focuses on the NY experiment. The following section provides a parallel analysis of the impact of minimum wages in the other states that also had substantial increases during the same time period.

II. Employment Effects of the NY Minimum Wage Increase

Background. Between 2004 and 2006, the state minimum wage in New York was increased in two steps from $5.15 to $6.75, while the federal minimum was unchanged at $5.15. Three geographically-proximate states—New Hampshire, Pennsylvania, and Ohio—had minimum wage rates of $5.15 throughout the period and are used by SBH as a control group.[3] The use of geographically-proximate areas with different minimum wages was first famously used in a natural experiment context by Card and Krueger (1994) following the 1992 increase in New Jersey's minimum wage. Similar approaches have been used subsequently in research by Dube, Naidu, and Reich (2007), who compared restaurant employment in San Francisco and neighboring cities after a local increase in the minimum; Hoffman and Trace (2009), who compared Pennsylvania and New Jersey after a federal minimum wage increase that affected only Pennsylvania; and Dube, Lester, and Reich (2010), who compared restaurant employment in adjacent counties that are across state boundaries and are subject to different minimum wages.

[3] Four other neighboring states (Vermont, Massachusetts, Connecticut, and New Jersey) either had an increase in their state minimum or had a constant, but higher minimum.

SBH use primarily difference-in-difference methods, with and without control for covariates. As the group potentially most adversely affected by the minimum wage increase, they focus on 16-29 year olds without a high school degree. They also examine subgroups by age (16-19, 20-24, and 25-29). In addition to the three neighboring states, SBH also compare NY to a synthetic control group using the methods of Abadie, Diamond, and Hainmueller (2010). Finally, they apply a difference-in-difference-in-difference model to compare employment changes of the target group to the employment changes for a putatively unaffected group across the two sets of states.

Their analysis uses data from the CPS-MORG files, which are the merged annual files for the outgoing rotation groups of the regular CPS. Each month's ORG file contains one-quarter of the full CPS, so the annual file contains three times the sample size of any single month's CPS and one-quarter the sample size of the full annual CPS. Monthly sample sizes for a sample that includes just a few states and a restricted age and education range can be relatively small. For 16-29 year olds with less than a high school degree, the CPS-MORG annual file includes 989 persons in NY in 2004, 916 in 2006, and 1765 and 1499 for the control group. Monthly sample sizes average about 75-80 for NY and 125-150 for the control group. Sample sizes for subgroups by age are obviously much smaller.

SBH use the MORG files because they want to first examine whether the minimum wage increase affected the distribution of wage rates. Only the MORG file contains information on wage rates. For this reason, the annual MORG files are the data source used in the annual BLS reports on the characteristics of minimum wage workers (BLS 2013) and are occasionally also

used in analyses of inequality (David Card and John E. DiNardo 2002). While they are essential for that purpose, they are not ideal for the analysis of employment rates, because of their smaller sample size. Indeed, for employment analyses, they have no advantage whatsoever over the full CPS sample.[4] The full CPS is always the source for official tabulations of employment and unemployment. In many cases, the MORG files may be a suitable substitute for the full CPS; they are, after all, a random part of a nationally-representative sample. But with smaller sample sizes, the representativeness may not carry through. When the MORG and full CPS files yield different estimates, there is no question that the full CPS is the authoritative data source.

Analysis. To re-examine the impact of the NY state increase in the minimum wage, I downloaded the MORG files for 2004 and 2006 from the NBER site and the corresponding monthly CPS files from the US Census site using Data Ferrett. As expected, the full annual CPS files provide samples for the NY and control group states that are about four times as large as the MORG sample for persons age 16-29 without a high school degree—3,854, 3,582, 6,909, and 6,077 for the two years and two groups, respectively. In my re-analysis, I focus on the comparison to the geographically-proximate states rather than the synthetic comparison group. The results of the two analyses are quite similar.

In Table 1, I show the re-analysis of the NY v NH/PA/OH natural experiment. The first row is from their Table 3, row 1. The huge adverse employment effect is easily seen. The employment rate for the less-educated younger workers in NY plummeted from .362 to .291

[4] This point has been made previously by Addison, Blackburn, and Cotti (2013), who recommend using the MORG files only to examine wage effects and then use the full CPS for employment effects.

between 2004 and 2006, a 20% decline. Employment in the control group states was essentially unchanged, yielding a difference-in-difference estimate of -0.076 that is statistically significant at the 5% level. Since the wage increased 31%, the employment elasticity is a very sizeable -0.63. My estimates from the MORG files are identical to theirs, which confirms that their analysis does not involve any idiosyncratic coding whatsoever. There is no question that the MORG files show a very substantial adverse employment effect of the minimum wage increase in NY on this group of relatively young, less-educated workers.

The estimates from the full CPS, presented in the second row, show a very different picture, however. The 2004 employment rate for NY is a full 2.6 percentage points lower than the MORG estimate, while the 2006 CPS estimate for NY is about 1.6 percentage points higher. For the control states, the 2004 employment rate from the full CPS is very close to the MORG estimate, but the 2006 rate is 1.7 percentage points lower than in the MORG file. The net effect of all these adjustments is a DID estimate of the impact of the NY state minimum wage increase of less than one percentage point (-0.008) that is statistically insignificant, compared to the statistically significant -0.076 estimate from the MORG data. The DID estimates are clearly statistically different. The lower bound of the 95% confidence interval for the DID estimate from the full CPS is -0.035, which is less than half the point estimate in SBH's analysis.

A similar pattern is seen in the next two rows, which focus on 16-19 year olds who are not high school graduates. The employment rates from the MORG files yield a DID estimate of 6.3 percentage points, equivalent to an elasticity of -0.79. Again, the main factor is a very sharp decline in the employment rate in NY, while the employment rate in the control states is

essentially unchanged. With the full CPS data, the 2004 NY employment rate for this subgroup is 2.7 percentage points lower and the 2006 rate is 1.1 percentage points higher than in the MORG files. Thus, the estimated decrease in the NY employment rate is 2.6 percentage points, less than half the decrease in the MORG data. At the same time, the control state employment rate change moves in the opposite direction, from neutral in the MORG data to a 2.1 percentage point decrease in the CPS data. None of the four sets of mean differences are large enough relative to their standard error to be statistically significant, but the net result is a DID estimate of less than half a percentage point, rather than the 6.3 percentage points computed by SBH. Again, the lower bound of the 95% confidence interval for the DID estimate (-0.033) is about half the MORG point estimate.

The remaining rows of the table show the employment rates for the other age subgroups examined by SBH. The general pattern follows what has been seen in rows (1)-(4). For 20-24 year olds, SBH find a difference-in-difference of -12.4 percentage points, primarily due to an astonishing 9.3 percentage point drop in the employment rate in NY. With the CPS, the difference-in-difference estimate is about one-quarter as large (-3.7 percentage points) and is not statistically significant. The underlying employment rate decline in NY is half as large as in the MORG and it is partly offset by a two point decline in the control states, rather than the 3.5 percentage point increase seen in the MORG files. In this particular case, the small sample size of the MORG is clearly a worry. The employment rates in the MORG in conjunction with the reported sample sizes imply that the NY sample contained 94 employed 20-24 year olds in 2004 and 63 in 2006. If the employed numbers had, instead, been 91 and 68—hardly large changes—the MORG employment rates would have matched those from the full CPS.

For 25-29 year olds without a high school degree, the MORG difference-in-difference estimate of the effect of the minimum wage on employment is -5.3 percentage points. In the CPS, the estimate is actually positive (0.011), but not close to statistical significance. Again, the very small sample size in the MORG is likely an issue. Finally, in the last rows, which focuses on 20-29 year olds with at least a high school degree—a group plausibly largely unaffected by the minimum wage—the two sets of estimates are very similar. Note that this is the largest sample size in the MORG by a very substantial margin.

Difference-in-difference-in-difference estimates are shown in Table 2. Here, the comparison is between employment rate change differences for an at-risk group and one essentially unaffected by an increase in the minimum wage across the two sets of states. As the unaffected group, SBH use persons age 20-29 with a high school degree, whose employment rate changes were shown in the bottom two rows of Table 1. Their estimates are shown in their Table 4 for models including covariates or, alternatively, can be computed from the figures presented in their Table 3 without adjustment for covariates. The results are virtually identical, so to keep the analysis simple, I focus on the DIDID without covariates.

All of the figures in Table 2 were previously presented in Table 1. The first panel shows the DID estimate (2006-2004) of -0.008 for the affected group. The next panel shows the corresponding estimate for the unaffected group (-0.0005). The DIDID estimate from the CPS is, therefore, -0.0075 with a standard error about twice as large. The estimate from SBH using the MORG is shown in the bottom row: it is -0.086 with a t-statistic of 2.6. The corresponding regression-adjusted DIDID estimate from their Table 4 is -0.078 with a t-statistic of 1.70. I do

not present the DIDID estimates for the other subgroups in Table 2, but it is obvious that they will be very similar to the DID estimates in Table 1, since the control group DID is itself very small.

Interpretation. What should we make of the differing CPS and MORG estimates? As I have argued above, the CPS data is the source for official BLS employment estimates. With the CPS data, for example, any researcher can exactly replicate national published BLS employment estimates for teens (age 16-19) in both years.[5] This includes not only the annual average (not seasonally-adjusted) employment and unemployment rate, but also the underlying monthly rates and the corresponding number of persons employed and unemployed (BLS 2014).

Estimates from the MORG do not replicate these official figures, although at the national level, the differences in the estimates between the two data sets are relatively small. The official average teen employment rate in 2004 computed from the CPS and reported by the Bureau of Labor Statistics was 36.4%, while in the MORG file, it is 35.7%, a difference of 0.7 percentage points. In 2006, the difference is 0.3 percentage points. The 2004 difference is statistically significant at the 5% level, while the 2006 is not. In both cases, the differences are quantitatively small.

At the state level, however, where sample sizes are much smaller, the differences are often much larger. Consider 2004 when, as noted above, the difference in the average annual employment rate for teens at the national level between the two data sets was 0.7 percentage points. I calculated the state-level teen employment rates with both data sets and then

[5] I use this group rather than the sample used by SBH because national published estimates are available for comparison.

computed the difference. The average absolute value of the state differences was 1.4 percentage points and the median difference was 1.5 percentage points. In 15 states, the difference was greater than two percentage points, with four states having a difference greater than three percentage points.

For the analysis sample used by SBH (age 16-29, not high school graduate), the same pattern holds. The average national employment rates in 2004 are .388 in the CPS data and a virtually identical .385 in the MORG data. But the average absolute value of the difference at the state level is 1.4 percentage points, and 15 states have a difference of two percentage points or more. Unfortunately for the SBH analysis, NY and PA are conspicuous outliers of opposite signs: for NY, the MORG employment rate, as seen in Table 1, is 2.6 percentage points higher and in PA, it is 1.4 percentage points lower (result not shown in Table 1). In OH and NH, the estimated employment rates differ by just 0.2 percentage points in the two data sets.

Figures 1 and 2 provide some insight into the differing NY employment rate estimates. The employment rate in the CPS is a weighted average of the rates for the ORG and non-ORG parts of the sample and it is easy, therefore, to back out the employment rate for the non-ORG sub-sample. The two figures plot the employment rate by month for the two subsamples. In 2004 (see Figure 1), the ORG series is clearly far more variable, which is not surprising given its smaller sample size. The average month-to-month change in the employment rates is 7.5 percentage points, compared to 2.9 points for the non-ORG sample. The two series are within 1-3 percentage points in five months, and in another four months, they differ by four to six percentage points, with the ORG higher in two and the non-ORG higher in the other two. But in

the remaining three months, they differ by 10-15 percentage points, with the ORG estimates always higher, and in each case followed in the subsequent month by a change in the ORG rate that eliminates most of the difference between the estimates. The three outlier months account for almost all of the 3.5 percentage point difference in the annual rates for the two subsamples. In the other nine months, the simple average difference is less than one percentage point. The monthly differences are statistically significant at the 5% level in two months and for the year as a whole.

In 2006 (see Figure 2), the ORG series is again much more variable from month to month; the average month-to-month change is again .075, compared to .042 for the CPS.[6] There is no clear pattern in the differences; the ORG rates are lower in eight months, higher in three, and close to the non-ORG rate in only one. Again months where the two rates differ the most are typically followed by months where the ORG rate moves toward the more stable non-ORG rate. This is true for February, April, June, September, and November. Overall, because the ORG employment rate is more often lower than the non-ORG rate, the average annual rate from the MORG is 1.7 percentage points lower than the CPS (see Table 2, rows 1 and 2).

The same comparison for the control states also shows far more variability by month for the ORG sample than the non-ORG[7], but the difference in the rates is smaller. In 2004, for example, in eight months the two series are very close, in three months the non-ORG rate is higher, and in one month the ORG rate is higher. As we saw in Table 1, the annual rates differed by about three-quarters of a percentage point. In 2006, the two rates are similar in eight

[6] Some seasonal variability in the employment is expected for a population that includes many students.
[7] In 2004, the average month-to-month change is .022 for the CPS and .049 for the MORG. In 2006, the corresponding averages are .019 and .047.

months, but now the ORG rate is higher in three months and lower in one. On average, the months with a positive ORG difference, which are as large as 9.8 percentage points, yield a 1.7 percentage point higher employment rate.

In both years, the higher variability in the employment rate from month-to-month in the MORG than in the CPS is undoubtedly related to its smaller sample size. Why this translated into a higher employment rate for NY in 2004 and a lower one in 2006 is a puzzle, but it is genuine—and unfortunate for the SBH analysis. As I noted above, a relatively small change in the number of persons reported as employed in the MORG is all that is necessary to yield employment rates similar to the CPS, especially for the subgroups with smaller samples. One possibility is fluctuation in the proportion of the MORG NY sample that is teenaged. In 2004, this proportion ranges from under 50% to almost 75% with an average of 66.6%, while in 2006 it ranges from 63% to 79% with an average of 70%. Monthly variation in the teen population share explains 55% of the variation in the difference between the two sets of monthly employment rates in 2004, but almost none of the variation in 2006.

Whatever the explanation, when the rates differ, as they do here, there is no option but to accept the estimates from the full CPS. It is four times larger and indisputably more fully representative. It is the acknowledged gold standard for employment-based statistics. Differences in the DID estimates of the employment effect of the minimum wage increase are both quantitatively large and statistically significant. On that basis, I conclude that the natural experiment created by the increase in the minimum wage in NY shows a negligible impact on employment of persons age 16-29 without a high school degree. This result is far more

consistent with other recent studies of minimum wage effects (Dube, Naidu, and Reich 2007; Dube, Lester, and Reich 2010; Hoffman and Ke forthcoming), all of whom find either positive or very modest and statistically insignificant negative effects of state or local minimum wage increases.

III. How Representative is New York?

If the New York minimum wage natural experiment is to be of policy importance, it ought to have some predictive value for other states with minimum wage increases. Alternatively, we can conduct the same kind of natural experiment using the other states to assess the validity of using NY as a representative case. In fact, such a natural experiment can readily be done. Over the same time period, New Jersey, Florida, Illinois and the District of Columbia increased their minimum wage substantially, while another seven states increased their minimum wage by much smaller amounts.

To examine this, I again use the full CPS data for 2004 and 2006. I focus on the three states plus DC with the largest increases and compare them to all states that had no increase over this time period. More refined comparisons could undoubtedly be made, but the results of this exercise ought to be suggestive. The minimum wage increased in these states[8] by an average of $1.03, equivalent to an 18.7% increase.[9] For the same sample restrictions as in SBH (age 16-29, not a high school graduate), the CPS files include 8,000-9,000 observations for the

[8] For ease of exposition hereafter, I refer to DC as a state.

[9] The increase in the other seven states ranged from $0.25 (Maine) to $0.55 (Wisconsin) with an average increase of 7.5%.

four states with an increase in the minimum wage and approximately 65,000 in the 39 states with no increase.

Table 3 shows the employment rates in the two groups of states before and after the minimum wage increase and the corresponding difference-in-difference estimate. In 2004, the employment rate for this group in DC, IL, FL, and NJ was 36.6%, while in 2006, after the increase, the employment rate increased by 3.4 percentage points to 39.9%. In the states with no increase, the employment rate increased 0.6 percentage points. This yields a difference-in-difference estimate of 2.74 percentage points that is statistically significant at the 5% level. Panel B shows the comparable information for 20-29 year olds with at least a high school education. Employment rates in both years are very similar in the two groups of states, rising by about one percentage point, presumably for reasons having nothing to do with any change in the minimum wage and reflecting, instead, employment changes due to the overall state of the economy. The difference-in-difference estimate is a miniscule -0.0007. Interpreted as an indicator of the general state of the economy, this estimate suggests that overall conditions were quite similar in the two sets of states. Finally, Panel C combines the two sets of estimates to compute the DIDID estimate of the impact of the minimum wage increase in DC, FL, IL, and NJ. As seen there, the DIDID estimate is 0.0281 and it is statistically significant at the 5% level or better. Thus, this natural experiment suggests that the minimum wage increase in these states had a positive effect on employment of young, less-educated workers.

I also computed employment rate changes using the CPS-MORG sample to see whether the same sample issues that undermined the NY v control state comparison would arise here.

Sample sizes are about one-quarter as large as the CPS, but are twice as large as the samples for NY and the control states used by SBH. In this case, the estimates from the MORG files are essentially identical to those from the CPS. The employment rate in the MORG is one percentage point lower in both years and for the control states, the rates are about 0.2-0.3 percentage points lower in both years.[10] The DID estimate for the employment change is .0275 with a standard error of .0154 and t-statistic of 1.79. The DIDID estimate from the MORG is lower than with the full CPS, because the MORG files show a more positive employment rate change for 20-29 year olds with at least a high school degree in the states with a minimum wage increase than in the states with no increase. The DIDID estimate is 0.0105 but with a t-statistic barely greater than one.

III. Conclusion

Sabia, Burkhauser, and Hansen asked "Are the Effects of Minimum Wage Increases Always Small?" and answered emphatically "no" for the case of New York's increase in the state minimum wage between 2004 and 2006. The employment rate changes they report are so large that any reasonable policy analyst would have to question the wisdom of such a policy. They are also so large that labor economists might well wonder about their accuracy.

My re-analysis of the SBH natural experiment yields results that are substantially different than theirs. I find no evidence whatsoever of a negative employment impact for young, less-educated workers in NY following the minimum wage increase. The difference in results reflects the different data sources used. SBH used the CPS-MORG, which are a one-

[10] The estimated 2004 and 2006 MORG employment rates are 35.6% and 39.1% for the four states with a large minimum wage increase and 39.1% and 39.7% for the states with no increase.

quarter subsample of the full CPS, while I used the full CPS files. In this case, the MORG files yield incorrect estimates of the employment rate changes in NY and in the control states, substantially overstating the apparent impact of the minimum wage change. When the two data sources conflict, there can be no doubt that the full CPS, which is the source of official employment data, is the appropriate one to rely on. A closer examination reveals very large month-to-month employment rate changes in the MORG files, a result that is not terribly surprising in light of the small monthly sample sizes. For example, the difference between the annual employment rates in the two data sets for NY in 2004 is fully accounted for by three outlier months, each of which is followed by a month that is very close to the CPS estimate. The huge employment decline for 20-24 year olds in the MORG data could be eliminated if 4-8 additional sample members reported a different employment status. SBH were not inherently wrong in using the MORG files. Rather, they were unlucky. The difference between employment rates at the state level from the CPS and MORG is a cautionary tale for applied labor economists, especially for analyses using a DID strategy.

I also present evidence from another natural experiment involving an increase in state minimum wages. Over the same time period, eleven states increased their minimums. I focus on three states and the District of Columbia, all of whom had quite large increases in their minimum wage, and compare employment rate changes in those states to changes in the 39 states that had no increase. I find evidence of a positive employment effect of 2.74 percentage points or 7.5%. Interestingly and perhaps reflecting the larger sample sizes involved or just the laws of sampling variability, I find exactly the same result using the MORG files.

My findings of employment effects that are either negligible, as in the case of New York, or positive, as in the case of DC, FL, IL, and NJ, are largely consistent with the newer round of minimum wage employment estimates. Dube, Lester, and Reich find no negative employment effects comparing counties across state lines with different minimum wages and Hoffman and Ke find no negative effect of the 2009 federal minimum wage increase in a comparison of individuals in states where the minimum did increase and those where the minimum did not increase because the state minimum already exceeded the new federal standard.

It is important to caution that these findings of no negative employment impacts reflect the range of minimum wage increases observed in the data. They support the idea that modest minimum wage increases in the 10-20% range phased in over a two-year period may not be problematic in terms of employment. But they are not informative about what the employment consequences might be for much larger increases. At the current $7.25 level of the minimum wage, a 20% increase would boost the minimum to $8.70. The recently proposed increase to $10.10 amounts to a 39% increase.

Bibliography

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* 105: 493-505.

Addison, John T., McKinley L. Blackburn, and Chad D. Cotti. 2013. "Minimum Wage Increases in a Recessionary Environment." *Labour Economics* 23: 30-39.

Bureau of Labor Statistics. 2013. Characteristics of Minimum Wage Workers: 2012." U.S. Department of Labor, http://www.bls.gov/cps/minwage2012.htm.

_____. 2014. Labor Force Statistics from the Current Population Survey, Series Id: LNU02000012, downloaded Jan 5, 2014.

Card, David and John E. DiNardo. 2002. "Skill-Biased Technological Change and Rising Wage Inequality: Some Problems and Puzzles." *Journal of Labor Economics* 20 (4): 733-783.

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* 84 (4): 772-793.

Dube, Arindrajit, T. William Lester, and Michael Reich. 2010. "Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties." *Review of Economics and Statistics* 92 (4): 945-964.

Dube, Arindrajit, Suresh Naidu, and Michael Reich. 2007. "The Economic Effects of a Citywide Minimum Wage." *Industrial and Labor Relations Review* 60 (4): 522-543.

Holtz-Eakin, Douglas. 2013. "The Mythology of the Minimum Wage," *Huffington Post*, http://www.huffingtonpost.com/douglas-holtzeakin/the-mythology-of-the-mini_b_4338234.html.

Employment Policies Institute. 2012. "New Study: Past New York Wage Hike Caused Substantial Job Loss," http://www.epionline.org/release/o335/.

Hoffman, Saul D. and Chenglong Ke. forthcoming. "Employment Effects of the 2009 Minimum Wage Increase: New Evidence from State Comparisons of Workers by Skill Level." *The B.E. Journal of Economic Analysis and Policy*.

Hoffman, Saul D. and Diane M. Trace. 2009. "NJ and PA Once Again: What Happened to Employment When the PA–NJ Minimum Wage Differential Disappeared?" *Eastern Economic Journal* 35 (1): 115-128.

Sabia, Joseph J., Richard V. Burkhauser, and Benjamin Hansen. 2012. "Are the Effects of Minimum Wage Increases always Small? New Evidence from a Case Study of New York State." *Industrial & Labor Relations Review* 65 (2): 350-376.

Table 1. Employment Rate Effects of NY State Minimum Wage Increase, 2004-2006
(Standard Error in Parentheses; Sample Size in Brackets)

Group	Data Source	NY 2004	NY 2006	NH, PA, OH 2004	NH, PA, OH 2006	Diff-in-Diff
16-29, w/o HS degree	MORG	0.362 [989]	0.291 [916]	0.409 [1765]	0.414 [1499]	-0.076** (0.029)
	CPS	0.336 [3854]	0.308 [3582]	0.417 [6909]	0.397 [6077]	-0.008 (0.014)
16-19, w/o HS degree	MORG	0.260 [685]	0.196 [659]	0.357 [1383]	0.356 [1198]	-0.064** (0.032)
	CPS	0.233 [2698]	0.207 [2547]	0.366 [5406]	0.344 [4836]	-0.005 (0.015)
20-24, w/o HS Degree	MORG	0.537 [176]	0.430 [148]	0.524 [224]	0.560 [170]	-0.124 (0.077)
	CPS	.515 [686]	0.458 [604]	0.549 [877]	0.528 [720]	-0.037 (0.038)
25-29, w/o HS Degree	MORG	.604 [128]	.620 [109]	.603 [158]	.671 [131]	-0.053 (0.034)
	CPS	0.593 [470]	0.631 [431]	0.599 [626]	0.627 [521]	0.011 (0.043)
20-29, >= HS Degree	MORG	0.694 [2082]	0.700 [1844]	0.759 [3422]	0.754 [3503]	0.010 (0.009)
	CPS	0.695 [8197]	0.701 [7323]	0.755 [13612]	0.762 [13791]	-0.001 (0.009)

MORG estimates from Sabia, Burkhauser, and Hansen, Table 3.

** Statistically significant at 5% level or better.

* Statistically significant at 10% level or better.

Table 2. Difference-in-Difference-in-Difference Estimates of Employment Rate Effect of Minimum Wage Increase, NY v NH/OH/PA, 2004-2006
(Standard Error in Parentheses; Sample Size in Brackets)

Group	NY	NH/PA/OH
16-29, w/o HS degree		
2004	0.336 [3854]	0.417 [6909]
2006	0.308 [3582]	0.397 [6077]
Difference-in-Difference	-0.008 (0.014)	
20-29, high school degree or more		
2004	0.695 [8197]	0.755 [13612]
2006	0.701 [7323]	0.762 [13791]
Difference-in-Difference	-0.0005 (0.009)	
Diff-in-Diff-in-Diff (CPS)	-0.0075 (0.017)	
Diff-in-Diff-in-Diff (MORG)	-0.086** (0.033)	

Source: Current Population Survey, 2004 and 2006. MORG estimates from Sabia, Burkhauser, and Hansen, Table 3.

Table 3. Employment Rate Effects of State Minimum Wage Increase, 2004-2006, DC, FL, IL, and NJ and States with No Increase
(Standard Error in Parentheses; Sample Size in Brackets)

Group	DC, FL, IL, NJ	States with No MW Increase
A. Age 16-29, w/o HS degree		
2004	0.3664 [9014]	0.3945 [66548]
2006	0.3999 [8673]	0.4006 [66534]
Difference	0.0335** (0.0073)	0.0061** (.0027)
Difference-in-Difference	0.0274** (.0078)	
B. Age 20-29, at least HS degree		
2004	0.7416 [19017]	0.7464 [148336]
2006	0.7520 [18697]	0.7575 [131314]
Difference	0.0104** (0.0025)	0.0111** (.0016)
Difference-in-Difference	-0.0007 (.0048)	
C. Diff-in-Diff-in-Diff	0.0281** (0.0091)	

Source: Current Population Survey, 2004 and 2006.

Fig. 1. Employment Rate by Month, Persons 16-29 without HS degree, NY, 2004, ORG and Non-ORG CPS Samples

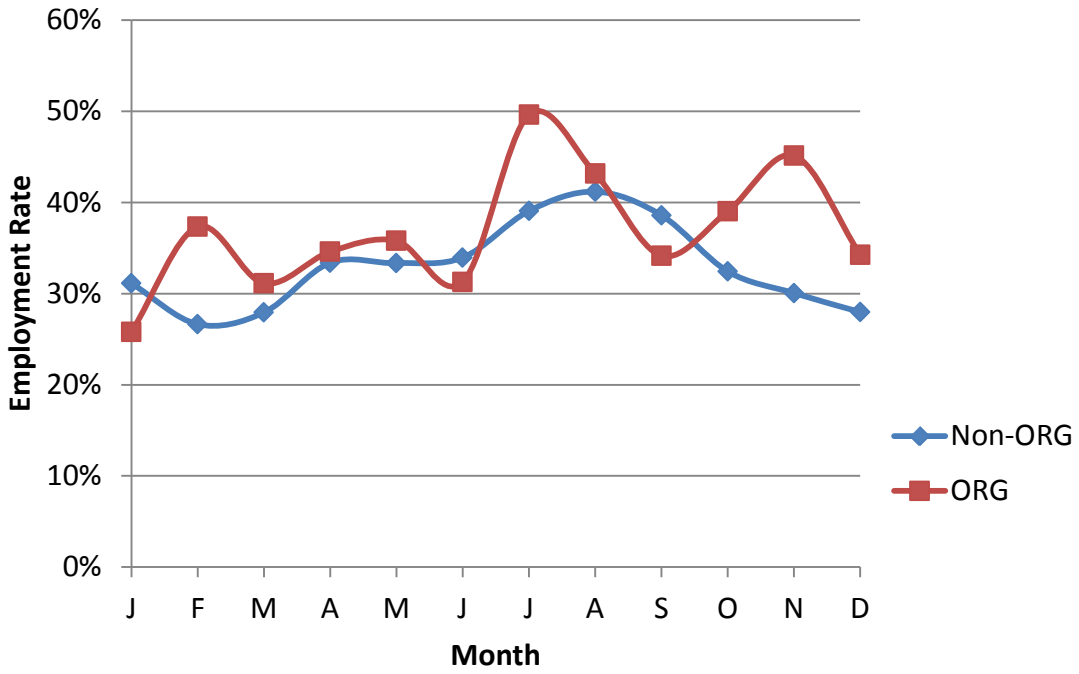

Fig. 2. Employment Rate by Month, Persons 16-29 without HS degree, NY, 2006, ORG and Non-ORG CPS Samples