Brief comments on Mark Long et al.

Impact of Seattle’s Minimum Wage Ordinance Through 2015

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July 27, 2016

On April 1, 2015 the minimum wage in Seattle increased from the statewide level of $9.47 to $10 (a 53 cent increase, equal to a 5.6 percent change) for employers with 500 or fewer employees, and from $9.47 to $11 (a 16.2 percent change) for employers with more than 500 employees. Further increases are scheduled in future years.

Since the smaller employers account for almost all the employees receiving a minimum wage increase, the average increase among all affected employees was not much more than 6 percent. This is a very small change, especially relative to the distribution of all the minimum wage changes in the U.S. in the past 35 years (excepting those that result from indexation). It is therefore to be expected that employment and hour changes that are attributable to the mandated increase would also be very small, if they are even detectible.

These are the unsurprising principal findings in a new report from the University of Washington’s Seattle Minimum Wage Study Team. The report finds that the median wage of low-wage workers rose by 73 cents more in Seattle than outside of Seattle. This 7.7 percent increase in median earnings is consistent with the mandated increase of about 6 percent plus a modest indirect effect for some of the workers who are already paid over $10. The report claims that employment and hours also declined (1.2 percentage points out of 65 in the case of employment and about 15 minutes less per week). However, these effects are very small and perhaps not statistically different from zero. The media coverage nonetheless framed the findings as showing the harmful effects of the policy.

The report admirably provides considerable methodological detail and pays substantial attention to how to avoid the usual perils in empirical minimum wage studies. I particularly liked the synthetic control approach, using zip codes and 12 years of pre-treatment data to construct a “synthetic Seattle.” Interestingly, this approach disproportionately chose nearby zip codes, contrary to some minimum wage critics. The report’s use of individual administrative data on individual establishments, which in Washington State includes hours, also represents an improvement over the existing minimum wage literature.

But the report’s methods nonetheless fall short. The main finding of small negative effects should be treated with considerable caution. The reported declines in
employment and hours are probably too small to be counted as statistically significant. But it is difficult to know for sure, as the team does not report quarterly variation in employment prior to implementation, does not report statistical significance levels (standard errors) for their point estimates, and does not adjust for seasonality issues that are potentially quite serious. Moreover, their study is necessarily limited to single-establishment firms, which may be declining as a proportion of all firms faster in Seattle than in synthetic Seattle. Finally, they do not report standard robustness tests, such as results separately for highly-affected industries, particularly restaurants. For these reasons, the report’s conclusions should have been more muted, to state for example that they could not rule out the conclusion that there were no significant effects one way or the other.

Most of the shortcomings can be easily addressed. I urge the authors to do so in subsequent reports.

1. **Quarterly employment variation** One would expect that quarterly employment data are noisier than data that are smoothed over three quarters. Quarterly variations in differences in pre-treatment and post-treatment employment are important for gauging the significance of the reported post-treatment employment decline. But the report includes only

   The report does present quarterly variation in employment in Appendix B, in figures B4-8. But these figures include only pre-policy implementation quarters. The quarterly employment figures clearly show no difference between Seattle and synthetic Seattle from baseline to implementation, raising questions about the anticipation effects claim made in the main text. It would be informative to see quarterly data for the post-policy period, as in a standard event study framework.

2. **Statistical significance** The report discusses but then incorrectly dismisses the issue of noise in the data. The report’s argument that there is no sampling error in a near-universal data census overlooks non-sampling measurement error. Such errors are certainly present in the ESD data (some are referred to in footnote 9). Without standard errors we do not know whether the results are statistically significantly different from zero.

3. **Seasonality issues** The report compares employment changes between 2014q2 and 2015q4. However, figures B4 to B8 exhibit substantial seasonality, which likely affects Seattle differently from its suburbs or synthetic Seattle. Tourism and sports events, for example, are concentrated in Seattle and are highly seasonal. Starting the sample in 2014q2 and ending it in 2015q4 therefore risks negatively biasing the employment results. Credible short-term results need to await availability of data for 2015q2.

4. **Single-establishment versus multi-establishment firms** The report does not attempt to analyze employment changes in multi-establishment firms because location data on their individual establishment are not reported. Single-
establishment firms account for 60 percent of employment in the control group and only 50 percent of firms in the treatment area (Seattle). This limitation alone should make one very cautious about interpreting employment effects, especially when multi-establishment firms are more prevalent in the treatment area and when the proportion of businesses that are single-establishments is likely to be affected by business conditions, seasonality and longer-term trends.

5. Effects by industry It would be very informative to know whether observed differences in the effects by industry conform to expectations. Specifically, are observed wage and employment effects greater among low-paying industries—such as restaurants—than in higher-paying industries? This robustness test could be easily implemented.

A final comment on the press remarks made by one of the report’s principal authors: This author remarks that the effect of the policy on earnings (by which he means overall earnings of low-wage workers) is ambiguous, because wage increases for those who remain employed might be outweighed by employment and hours losses. This statement is difficult to square with the reported wage gains of 7.7 percent and the estimated 1.2 percent decline in employment. The positive overall effect on the low-wage community seems quite clear.