NBER WORKING PAPER SERIES

THE ECONOMETRICS AND ECONOMICS OF THE EMPLOYMENT EFFECTS OF MINIMUM WAGES: GETTING FROM KNOWN UNKNOWNS TO KNOWN KNOWNS

David Neumark

Working Paper 25043
http://www.nber.org/papers/w25043

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2018

Prepared as the keynote address for the "Evaluation of Minimum Wages" conference, DIW Berlin, July 4-5, 2018. I am grateful to William Wascher for long-standing research collaboration, and to Jeff Clemens, Kyle Colangelo, Matthew Harding, Jonathan Meer, Joan Monras, David Powell, Carsten Schroeder, Evan Totty, and Weilong Zhang for helpful comments and discussions of this paper. Some of the material in this paper is covered, in much less detail, in Neumark (2018). The views expressed herein are those of the author and do not necessarily reflect the views of the National Bureau of Economic Research.

The Econometrics and Economics of the Employment Effects of Minimum Wages: Getting from Known Unknowns to Known Knowns
David Neumark
NBER Working Paper No. 25043
September 2018
JEL No. J23,J38

ABSTRACT

I discuss the econometrics and the economics of past research on the effects of minimum wages on employment in the United States. My intent is to try to identify key questions raised in the recent literature, and some from the earlier literature, that I think hold the most promise for understanding the conflicting evidence and arriving at a more definitive answer about the employment effects of minimum wages. My secondary goal is to discuss how we can narrow the range of uncertainty about the likely effects of the large minimum wage increases becoming more prevalent in the United States. I discuss some insights from both theory and past evidence that may be informative about the effects of high minimum wages, although one might argue that we first need to do more to settle the question of the effects of past, smaller increases on which we have more evidence (hence my first goal). But I also try to emphasize what research can be done now and in the near future to provide useful evidence to policymakers on the results of the coming high minimum wage experiment, whether in the United States or in other countries.

David Neumark
Department of Economics
University of California, Irvine
3151 Social Science Plaza
Irvine, CA 92697
and NBER
dneumark@uci.edu

I. Introduction

The United States is embarking, in at least some regions, on an experiment of using high minimum wages to try to increase incomes of workers and to reduce poverty. Figure 1 shows state minimum wages as of Jan. 1, 2018. There are now 29 states (plus the District of Columbia) with minimum wages above the federal minimum wage, with an average difference across states of 30.2 percent. As a result, the federal minimum wage now provides a floor for an increasingly narrow set of states, concentrated in the South (see Figure 2). Moreover, California, Massachusetts, New York, Seattle, and Washington, DC have legislated either current or future minimum wages of $15, other localities may follow, and a change in the national political alignment could result in a $15 national minimum.[1]

Some economists claim with confidence that a $15 minimum wage will not result in job loss (e.g., Reich, 2016). Others argue that a $15 minimum wage will lead to huge job losses (e.g., Even and Macpherson, 2017). These divergent views are also reflected in the media. For example, conflicting titles in articles from *Forbes* and *The American Prospect* read, respectively, "A Statewide $15 Minimum Wage is a Bad Idea,"[2] and "Why a $15 Minimum Wage is Good Economics."[3]

I regard such confidence regarding the effects of a $15 minimum wage as badly misplaced, for two reasons. First, although one might think that we know pretty much everything about the employment effects of minimum wages in the United States, given the scores of papers written, and the development of seemingly ever-richer data and more-refined empirical techniques, the debate among researchers about whether minimum wages reduce employment, and if so by how much, remains intense and unsettled. Second, even if one has a strong view of what the U.S. literature says about the employment effects of *past* minimum wage increases, this may provide much less guidance in projecting the consequences of much larger minimum wage increases than those studied in the prior literature.[4] Predicting the effects of

[1] A $15 federal minimum wage was part of the Democratic Party platform in the 2016 elections.
[2] This was in reference to Virginia. See https://www.forbes.com/sites/adammillsap/2017/03/10/a-statewide-15-minimum-wage-is-a-bad-idea/#4817ea465d4a (viewed June 24, 2018).
[3] See http://prospect.org/article/why-15-minimum-wage-good-economics (viewed June 24, 2018).
[4] The past literature may also be less informative about the effects of minimum wages at a more limited geographic scale, such as cities. For example, there may be more scope for business relocation (or choosing alternative locations for new businesses) in response to a local minimum wage.

minimum wage increases of many dollars, based on research studying much smaller increases, is inherently risky for the usual statistical reasons. But the problem is potentially exacerbated because the reduced-form estimates on which the prior literature is based may fail to capture changes in underlying behavior as high minimum wages affect a far greater share of workers.[5] The same issues carry over to large minimum wage increases elsewhere, such as the recent introduction of a minimum wage in Germany in 2015, starting at a relatively high 8.50 Euro.

My main goal in this paper is to delve into the econometrics and the economics of past research on the effects of minimum wages on employment in the United States. My intention is not to relitigate the debate about research on minimum wages and employment, which has already been synthesized and reviewed extensively.[6] Instead, my intent is to try to identify key questions raised in the recent literature, and some from the earlier literature, that I think hold the most promise for understanding the conflicting evidence and arriving at a more definitive answer about the employment effects of minimum wages.

My secondary goal is to discuss how we can narrow the range of uncertainty about the likely effects of the large minimum wage increases becoming more prevalent in the United States. I discuss some insights from both theory and past evidence that may be informative about the effects of high minimum wages, although one might argue that we first need to do more to settle the question of the effects of past, smaller increases on which we have more evidence (hence my first goal). But I also try to emphasize what research can be done now and in the near future to provide useful evidence to policymakers on the results of the coming high minimum wage experiment.

My review and discussion focus on the U.S. context and evidence. The U.S. experience dominates the literature because of three-plus decades of significant cross-state variation in minimum wages; and the very large minimum wage increases we are likely to see even more of in the United States

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[5] For example, Holtz-Eakin and Gitis (2015) estimated, based on 2014 data, that 55.1 million workers would have been directly affected by raising the federal minimum wage to $15 by 2020.

[6] See Brown et al. (1982), Card and Krueger (1995), and Neumark and Wascher (2007, 2008) for earlier reviews, and Allegretto et al. (2017) and Neumark and Wascher (2017) for an exchange on recent evidence. There are also a few recent meta-analyses of the employment effects of minimum wages. See Neumark (2016) for references to some of these, as well as criticism of their methods. For recent discussions of my views on recent prior research, see Neumark (2016), Neumark et al. (2014a, 2014b), and Neumark and Wascher (2017).

will likely spur even more work on U.S. minimum wages. But the roadmap to future research on minimum wages should go through other countries as well, for three reasons. First, such evidence will allow researchers to test, in a different context, explanations and hypotheses that arise in studying minimum wages in the United States. Second, policymakers in other countries need evidence on the effects of the minimum wages they adopt. Third, and most important, research in other countries can provide evidence that the U.S. setting cannot provide, such as effects of different institutions for setting minimum wages (like the U.K. Low-Pay Commissions, or collective bargaining in other countries), and the effects of implementing a minimum wage where there previously was not one (as in Germany).

II. A Brief Overview of Recent Research

Regardless of one's precise view of what the U.S. minimum wage literature says about the employment effects of minimum wages, and which studies one finds most convincing, it is clear that there is considerable variation in the magnitude of estimated employment effects studies. The debate is often characterized as being about whether the elasticity for low-skilled groups is equal to (or more precisely indistinguishable from) zero, or more likely in the range of −0.1 to −0.2. But there are also larger negative estimates in the literature (e.g., Clemens and Wither, 2016, and see Table 1 below), and occasional large positive estimates (most notably, Card and Krueger, 1994).[7]

In the most recent research, two key econometric issues underlie the different answers researchers obtain about whether higher minimum wages reduce employment of low-skilled workers. One concerns the proper specification of control areas (or counterfactuals), given the potential endogeneity or non-randomness of minimum wage increases. Most methods of addressing this problem continue to find negative employment effects, but methods using geographically-close controls tend not to – which leaves us with an open question as to why. The second concerns the inclusion of trends for treated and control areas, and the sensitivity of estimates to those trends. This sensitivity can leave us with little guidance as to which estimates should be preferred.

[7] While this study is frequently cited as showing no evidence of employment effects from minimum wages (e.g., Schmitt, 2015), their results indicate that the increase in New Jersey's minimum wage led to *faster* employment growth, with an elasticity of 0.73.

I focus a good deal of attention on these two issues, to try to indicate how we might progress in making sense of the conflicting estimates in this recent literature. But independently of these two issues emphasized in recent research, there is long-standing evidence of differences in estimated employment effects of minimum wages across studies. In my view, more serious considerations of the economic factors that may help explain these differences could be very fruitful in resolving some of the contradictions in past research. Moreover, since this question has more to do with economics than econometrics, consideration of the factors underlying differences in minimum wage effects across studies may prove useful in thinking about the effects of much higher minimum wages.

III. Treatment and Control Areas and Identification Strategies

Estimating causal effects of policy based on past responses to policy changes requires choosing appropriate controls to provide a counterfactual for what would have happened absent the minimum wage increase. The traditional "workhorse" in the empirical literature on the employment effects of state minimum wages in the United States is the standard two-way fixed effects model with state and time dummy variables – a continuous difference-in-differences (DD) estimator that compares changes in low-skilled employment in states where the minimum wage increased more to states where it increased less (or not at all). This estimator is the source of the oft-cited conclusion that the elasticity of employment of low-skilled groups (especially teens) with respect to the minimum wage is in the −0.1 to −0.2 range – a range of estimates that is replicated across many studies, including those discussed below that first report such estimates before criticizing the two-way fixed effects estimator and moving on to other methods.

III.1. Close Controls

In two recent influential papers, Allegretto et al. (2011, ADR) and Dube et al. (2010, DLR) raised the concern that cross-state minimum wage variation could be correlated with shocks that also affect outcomes. They compare estimates using the standard two-way fixed effects model in which all states could potentially serve as controls – which yield "conventional" negative elasticities – to estimates based on using only geographically-close areas in different states as controls – which yield estimates closer to and statistically indistinguishable from zero.[8]

The idea motivating the use of "close controls" is that the states (or subareas of states) affected by minimum wage increases may experience the same economic shocks to low-skill labor markets as nearby areas unaffected by these increases, and thus comparisons between the treated areas and close controls may more reliably identify the causal effects of minimum wages. The standard two-way fixed effects estimator is

$$E_{st} = \beta MW_{st} + X_{st}\gamma + D_s\theta + D_t\lambda + \varepsilon_{st}. \qquad (1)$$

Typically, E is the log of the employment rate or level for a low-skill group like teenagers. MW is the log of the state minimum wage.[9] X is a vector of controls.[10] And the D vectors are state and year dummy variables (fixed effects). I assume that the data are collapsed to the state by year level (indexed by s and t), but one could use micro-data as well.[11]

The concern raised by ADR and DLR is that ε is correlated with MW in equation (1). In the context of a state-level analysis (as in ADR), for example, let r index sub-regions that includes sets of states. Then as long as the shocks are common to regions, one can control for them by augmenting the model with interactions between year and region dummy variables, as in

$$E_{st} = \beta MW_{st} + X_{st}\gamma + D_s\theta + D_t\lambda + D_t \cdot D_r^T\eta + \varepsilon_{st}.^{12} \qquad (2)$$

As long as there is within-region variation in the minimum wage, β is identified – from these

[8] Card and Krueger (1994) is a precursor of this approach. Other studies that use close controls in a similar fashion to ADR find, not surprisingly, similar results (Addison et al., 2013; Gittings and Schmutte, 2016; and Slichter, 2016). (Addison et al. do find stronger evidence of disemployment effects for teens during the Great Recession.) An exception is Liu et al. (2016), who restrict their close controls to counties in the same BEA "Economic Areas" (but in different states); BEAs are supposed to delineate regionally-integrated markets. Liu et al. find evidence of disemployment effects for the youngest group covered in their data (14-18 year-olds) using the standard two-way fixed effects model, but these are diminished only slightly – to an elasticity of −0.17 – within Economic Areas.
[9] Some research divides the nominal minimum wage by an average wage measure.
[10] As Neumark and Wascher (1994) suggest, given the reduced-form specification, these controls should include exogenous shifters of both labor supply and labor demand.
[11] While almost every paper in the literature studies employment outcomes directly, Cengiz et al. (2017) instead estimate effects on the shares of workers with wages just below or just at or above the minimum wage. The authors suggest that this approach can avoid biases from changes in employment unrelated to the minimum wage, and generally do not find evidence of job loss. One curious finding, though – a very large positive implied employment elasticity for high school dropouts – suggests that the method may not be immune from spurious effects. I leave it to future work to consider this approach more fully.
[12] Depending on how many region-by-year interactions are omitted, these interactions could subsume the year dummy variables.

regions. The approach in DLR is the same, except that they use county-level data. In their approach, r indexes sets of bordering counties across state lines, and identification comes from the county "pairs" where the minimum wage variation differs on the two sides of the border. DLR uses employer data, and hence focuses on low-wage industries (like restaurant workers) rather than teenagers.

The implication from the evidence in both papers that estimated minimum wage effects are less negative (and insignificant) when using close controls implies that minimum wages tend to be increased where there are negative shocks to low-skill labor markets. Later, I discuss whether this is likely and what other evidence implies. The results of these and other recent studies are summarized in Table 1.

III.2. Responses to Concerns about Non-Random Minimum Wage Variation

There have been three kinds of recent responses to the legitimate concern about the non-random nature of minimum wage variation. First, research has explored the validity of the controls ADR and DLR used (Neumark et al., 2014a; Neumark and Wascher, 2017).[13] Second, researchers have pushed further the development of synthetic control methods (Abadie et al., 2010) to select or construct appropriate control areas (most notably, Powell, 2016). And third, several studies have adopted alternative identification strategies to isolate the effects of minimum wage increases from shocks that are potentially correlated with them.

In the latter category, the approach used most widely in reduced-form analyses of policy effects, generally, is triple-differences (DDD) estimators that isolate the effect of the policy change by introducing another group in the same state, which is therefore "exposed" to the same policy change but less (or not) affected by it, and which is assumed to experience the same shock – which is the identifying assumption. Thompson (2009), which predates ADR and DLR, helps motivate this approach, although he does not use a full DDD approach. Thompson focuses only on variation generated by the two federal minimum wage increases in 1996 and 1997, restricted to the states where the federal minimum wage was binding. He generates minimum wage variation by indexing counties by the extent to which the minimum wage is binding in a county. Indexing counties by c, defining H to be an indicator that

[13] See the response to this research in Allegretto et al. (2017).

minimum wages are more binding in a county, and defining POST as an indicator for period year after the federal minimum wage increases, he estimates models of the form

$$E_{cst} = \beta POST_t \cdot H_{cs} + X_{cst}\gamma + H_{cs}\psi + D_s\theta + POST_t\lambda + \varepsilon_{cst}. \quad (3)$$

Equation (3) is a difference-in-differences (DD) estimator, identifying β from the differential change in employment in counties where the minimum wage increase affected more workers (H = 1) versus fewer workers (with a focus on young people and teenagers). It may avoid endogenous minimum wage changes by comparing regions within a state, and by using only federal variation.

Thompson finds large disemployment effects in counties where minimum wages are more binding because wages are lower, and workers are lower skilled. See the second panel of Table 1, which covers strategies different from the "close-controls" approach.

One could use this approach in a DDD framework by using a period with state variation in minimum wages, and identifying the effects from differential impacts across more- and less-affected subareas of states, allowing for a full set of state-by-year interactions, as in

$$E_{cst} = \beta MW_{st} \cdot H_{cs} + X_{cst}\gamma + H_{cs}\psi + D_s\theta + D_t\lambda + D_t \cdot D_s^T\eta + \varepsilon_{cst}.^{14} \quad (4)$$

Clemens and Wither (2016) follows this strategy of including state-by-period fixed effects to control for state-specific shocks. They estimate the effects of the 2007-2009 federal minimum wage increases, comparing changes in employment for the lowest-wage workers whose wages were differentially affected by the federal increases (because of prior variation in state minimum wages), to changes in employment for workers who earned wages that were low, but high enough that the federal minimum wage increases had little impact on them.[15] Thus, H_{cs} in equation (4) becomes an indicator for the lowest-wage workers. They estimate a large employment elasticity for directly affected workers (about −0.97 based on Survey of Income and Program Participation (SIPP) data).[16]

[14] In this specification, the main effect of the minimum wage is subsumed in the state-by-year interactions. The full DDD specification would include other interactions I have omitted, such as $D_t \cdot H_{cs}$. This is the approach taken, in a slightly different context (the effects of minimum wages on automatable jobs) in Lordan and Neumark (2018).
[15] Similar to Thompson (2009), the use of contemporaneous variation generated by federal policy likely also reduces problems of endogenous minimum wage variation.
[16] Foreshadowing the discussion below, the magnitude is likely larger than other studies because it is calculated for a more directly-targeted group of workers (compared to teenagers or restaurant workers, only some of whom are paid

As an alternative strategy for addressing shocks potentially correlated with state minimum wage increases, Baskaya and Rubinstein (2015) use an instrumental variables (IV) approach (in estimating the effects of minimum wages on teen employment). Their IV is the interaction between the federal minimum wage and a measure of the historical propensity for each state to let the federal minimum wage bind, which is intended to purge the estimated minimum wage effect of bias from states endogenously choosing their minimum wage in response to shocks to state-level economic conditions. I view this IV as particularly clever; when I thought about IV strategies in the past, most candidate IVs (such as the political orientation of a state) would be fully absorbed by state and year fixed effects.

Their first-stage equation is

$$MW_{st} = \varphi F_s MW^F_t + X_{st}\gamma + D_s\theta + D_t\lambda + \varepsilon_{st}. \quad (5)$$

MW^F_t is the federal minimum wage, and F_s is the fitted value from a model for the probability that legislators allow the federal minimum to bind in the state

$$F_s = P(Z_s\pi). \quad (6)$$

Z includes measures of cross-state differences in standards of living and political preferences, as well as the proportion of years earlier in the sample when the federal minimum wage was binding.[17]

Their IV elasticity estimates for teenagers are in the range −0.3 to −0.5. These estimates exceed the OLS (standard two-way fixed effects) estimates, which is consistent with policymakers raising minimum wages when youth labor market conditions are strong, and contrasts with the direction of bias implied by the close-controls estimates in ADR and DLR.

The approaches considered thus far rely, in large part, on a priori specification of alternative controls or a priori assumptions about how to construct a valid counterfactual. For example, ADR and DLR assert that geographically-close controls are better than the larger set of control states used in the standard two-way fixed effects estimator. Baskaya and Rubinstein (2015) rely on an a priori assumption

at or near the minimum wage). Indeed, Clemens and Wither show that the elasticity is smaller when using a treatment group that includes higher-wage workers and hence is "less intensively" treated.

[17] In these earlier years, many states minimum wages lower than the federal level applied to workers not covered by the FLSA, who were quite numerous before expansions of coverage of the FLSA in 1977 (Brown et al., 1982).

regarding the validity of their instrumental variable. And Clemens and Wither (2016) rely on the assumption that slightly higher-wage workers provide valid controls.[18]

In contrast, synthetic control methods (Abadie et al., 2010) rely on a more data-driven approach to construct controls. The synthetic control model can be motivated by a factor model that is less restrictive than the standard two-way fixed effects model

$$E_{st} = \beta MW_{st} + X_{st}\gamma + \tau_t\theta_s^T + D_t\lambda + \varepsilon_{st}. \quad (7)$$

τ_t is a row vector of period factors and θ_s^T is a column vector of region factors. If, for example, $\tau_t = (1^s \ \ \varphi_t)$ – a row vector of 1's for each state s and time fixed effects – and $\theta_s = (\omega_s \ \ 1^t)$ – a row vector of state fixed effects and 1's for each year t (transposed in the equation) – then we get the standard two-way fixed effects model. However, with greater flexibility the state fixed effects need not be constant across time (for example), in which case taking differences across time does not eliminate the state fixed effects. Still, equation (7) entails some restrictions; for example, the state factors can only be multiplied by common factor in a given year.

The synthetic control approach can provide an unbiased estimate in this more general setting (as well as even more general settings), using a weighted set of control regions that matches pre-treatment data in the treated regions to provide an estimate of how the outcome would have evolved in the treated regions absent the treatment. For example, the synthetic control model allows time-varying state effects – consistent with the more flexible factor model in equation (7) – but assumes that how they vary in the post-treatment period is similar in the treated region and the synthetic control regions.

The original application of synthetic control methods in Abadie et al. (2010) was to a single, categorical treatment in one region (a tobacco control program in California), with many possible control states.[19] However, the application to minimum wages is typically more problematic, because there are

[18] All papers present some indirect evidence in support of their approaches. For example, ADR and DLR suggest that estimators that do not use their close controls are more contaminated by "pre-trends" that could reflect endogeneity of minimum wage increases with respect to shocks to low-skill labor markets (although Neumark et al. (2014a) and Neumark and Wascher (2017) raise questions both about this evidence). And Clemens and Wither present evidence based on wage effects that are consistent with their assumption, but they cannot directly test their assumption with respect to employment.

[19] Another example is Bohn et al.'s (2014) analysis of Arizona's 2007 anti-immigration law.

potentially scores of minimum wage increases, these increases vary in magnitude, and potential pre-treatment controls may be contaminated by prior minimum wage increases (including possibly lagged effects).[20] In this setting, synthetic control methods have been used in a few ways.

Neumark et al. (2014a) did not focus directly on estimating minimum wage effects, but instead on a "first-stage" synthetic control analysis asking whether the synthetic control method would pick out the close controls that ADR and DLR advocated using. For example, ADR use state data and define their close controls as states in the same Census divisions (and hence include Census division-by-year interactions in equation (2)). If there are common shocks within Census divisions, but not across Census divisions, then the synthetic control method should put most of the weight on same-division states that would better match the prior variation in the treatment states. In contrast, if the analysis puts relatively less weight on same-division states, this would imply that those states are not the best controls, and that restricting the identifying information only to within-division variation in minimum wages may be less likely to identify the true effect of minimum wages.

As an illustration, Table 2 reports results from matching on three different forms of the dependent variable, each defined over the four pre-treatment quarters. This matching is done for states without minimum wage increases in that period. This allows 129 unique treatments in the sample period used, for which 50 have potential control donors in the same Census division, covering six divisions. The weights from the matching process on states in the same division are reported in columns (1)-(3). Except for West North Central, these weights are generally well below one. In 14 out of the 24 cases they are below 0.25, and in some cases, they are quite close to zero, implying that most of the weight chosen by the synthetic control method is on states outside the division.[21]

[20] Researcher have used synthetic control methods to estimate the effects of a single minimum wage increase. Jardim et al. (2017) and Reich et al. (2017) estimate the effects of Seattle's $2 minimum wage increase in 2016 (to $13, on the way to $15). And Sabia et al. (2012) and Hoffman (2016) study the impact of New York's 2005 minimum wage increase.

[21] Columns (4)-(6) report the average number of divisions and states in the donor pool, and the average number of states in the same division, and shows that the low weight on states in the same division is not attributable to a small number of potential donor states from the same division. For example, Pacific has a low number of potential donors from the same division relative to all potential donors, but relatively high weight, and South Atlantic has a high number of potential donors from the same division relative to all potential donors, but relatively low weight.

Calculations based on this analysis demonstrate that there is generally little reason to prefer the same-division states as controls relatively to randomly-chosen states. For the analysis in columns (1)-(3), the average weight per same-division donor state is higher than the random threshold of 1/(number of potential donors) in only 24, 17, and 19 of cases (out 50 cases in column (1), and 49 and 44 and cases in columns (2) and (3), owing to some loss of observations from the lagged variables). That is, for most Census divisions, states *outside* the Census division tend to be better control observations, militating against the ADR and DLR criticism of the two-way fixed effects model.

Other analyses have tackled the challenge of trying to use synthetic control methods to estimate employment effects using a broad set of U.S. minimum wage increases. Neumark et al. (2014a, 2014b), in a manner explicitly acknowledged as ad hoc, estimated the effects of a large set of minimum wage increases by first using the two-way fixed effects model to estimate minimum wage effects, and then matching on the estimated regression residuals; to try to bound the estimates, they used the same procedure assuming that the minimum wage effects were zero. This analysis led to evidence of disemployment effects in both the state and county context. Dube and Zipperer (2015) used a different method of using multiple (but far fewer) minimum wage increases in a synthetic control framework. They restrict attention to potential controls with no minimum wage increases in the prior two years, allowing them to study 29 minimum wage increases out of 215 in their sample period.[22] They find smaller and statistically insignificant negative employment effects.

Most recently, in what appears to be the most satisfactory and flexible approach, Powell (2016) develops and uses a method that can be applied to multiple treatments with continuous variation, and that simultaneously estimates the treatment effect and the weights on the control states. His method avoids the problem of selecting minimum wage increases with clean controls, and hence can use all the data. Powell finds a statistically significant estimated elasticity for teens of −0.44.

Totty (2017) uses a linear factor model that also does not impose close controls but is more

[22] An important feature of this approach is correct (indeed, exact) statistical inference, whereas Neumark et al. (2014a) acknowledged an inability to compute correct standard errors for their two-step approach.

restrictive than Powell's approach. He finds small (and insignificant) negative employment effects for restaurant workers and teens. However, although the idea behind this approach (see, e.g., the common correlated effects, or CCE, estimator of Pesaran, 2006) is to avoid specifying the form of the unobserved heterogeneity (see, e.g., Totty, p. 1716), Totty always includes state and year fixed effects and uses the CCE (and another estimator) to allow for other forms of heterogeneity. The standard two-way fixed effects estimator yields elasticities of −0.14 for restaurant employment and −0.18 for teen employment, while his alternative estimators yield smaller and insignificant elasticities. Totty argues for including the two-way fixed effects a priori to address the specific debate in the minimum wage literature – discussed later – about including jurisdiction specific trends along with the traditional two-way fixed effects. He states that "the factor model results are essentially unchanged if the two-way fixed effects are removed from the specification" (footnote 15). Based on estimates he provided to me, this is true for the results for teens, but not for restaurant workers, for whom the less restrictive approach yields elasticities of −0.048 to −0.066 (significant at the 5-percent or 10-percent level), in contrast to the insignificant estimates of about −0.01 to −0.02 reported in Table 3 of his paper.

In related work, Colangelo and Harding (in progress) explore the two-way fixed effects model using data from ADR. For 16-17 year-olds (they never find an employment effect for 18-19 year-olds), they show that using CCE after first assuming fixed state and year effects gives an employment effect that is smaller than the two-way fixed effects estimate (the latter is significant, the former is not), whereas using CCE without first imposing this structure on the unobservables yields a negative and significant employment effect that is very close to the two-way fixed effects estimate; the implication is that the two-way fixed effects estimate is not biased. However, the negative hours effect obtained from the two-way fixed effects specification is not robust to using the CCE estimator. Thus, there appears to be some remaining uncertainty about the implications for minimum wage effects of estimating models with weaker a priori restrictions imposed on the structure of the unobservables.

The factor model originally appeared to have some advantages, given the difficulties of applying the synthetic control approach to an unrestricted analysis of minimum wages. However, the Powell study appears to surmount these difficulties, and hence is probably more compelling.

The results discussed above, and some others from the recent literature, are summarized in Table 1. In my view, this table points to a rather clear result. Studies using close controls generally find very small disemployment effects usually indistinguishable from zero. But other identification strategies – differencing estimators that control for state-specific shocks, IV estimates that purge the minimum wage variable of correlation with these shocks, as well as the most advanced synthetic control estimator (Powell, 2016) – tend to find larger disemployment effects.

III.3. Should We Be Convinced by the Absence of Disemployment Effects from Close-Control Strategies?

This evidence poses two questions: Why do the different strategies generate different results? And which strategy or strategies is most reliable? I do not have a complete answer, but I discuss two additional lines of inquiry that have been suggested in the literature – aside from the question considered in Neumark et al. (2014a) of whether close controls are better controls.[23] These lines of inquiry have focused on the validity of the close-controls approach, although of course the validity of all the methods merits attention.

One issue, raised by Neumark et al. (2014b), is that minimum wage increases within similar geographic areas could be more endogenous with respect to economic shocks, rather than less. Other factors that differ more substantially between states in different regions, and that provide exogenous variation – such as unionization or politics – likely play less of role for close controls, implying that differences in economic conditions between treatment states and close controls, even if smaller, may matter more for determining minimum wages. As Jeffrey Clemens once remarked, "If the regions are so damn similar, why do they have different minimum wages?"

A useful analogy comes from Griliches' (1979) seminal work on twin or sibling estimates of the economic returns to schooling. The simple intuition is that if we include family fixed effects, or equivalently look only at within-family variation in schooling and wages, then bias from omitted

[23] For further exchanges on these findings, see Neumark et al. (2014b), Allegretto et al. (2017), and Neumark and Wascher (2017).

unobservables at the family level is reduced. Griliches noted, however, that whether bias in the estimated return to schooling is reduced in the within-family differences depends on what generates variation within versus across families. For example, if family influences or "background" common to both siblings or twins are relatively important in determining schooling, then the remaining within-family differences can be more reflective of ability differences to which schooling responds, in which case the within-family estimate of the return to schooling can be more biased than an estimate using across-family variation.

For the simplest analogy to estimating minimum wage effects, suppose we have only two years of data, form the first differences between treated states (s) and bordering states (s'), and estimate

$$(\Delta E_s - \Delta E_{s'}) = \beta \Delta MW_s + (\Delta X_s - \Delta X_{s'}) \cdot \gamma + (\Delta \varepsilon_s - \Delta \varepsilon_{s'}).^{24} \qquad (8)$$

Suppose there is a shock correlated with ΔMW_s – denote it $\Delta\mu_s$. If we assume the shock in the the first difference for state s' ($\Delta\mu_{s'}$) is the same, then it drops out of equation (8) and we obtain an unbiased estimate of β . In contrast, if we use control states further away, the shocks are less likely to be the same, and estimators that do not rely solely on close controls will be biased.

But like the assumption that identical twins (the strongest case) have identical unobservables, the assumption that the shock is identical in the treatment and close-control states is likely not strictly true. That is, there is an omitted variable in equation (8) equal to $(\Delta\mu_{st} - \Delta\mu_{s't})$. The simple intuition that might still rationalize the close-controls estimator is that difference in shocks must be a good deal smaller than between a treatment state and some other (not close) state or set of states. However, this does not imply less bias. The omitted variable bias in equation (8), ignoring the X terms, is

$$\mathrm{Cov}(\Delta\mu_{st} - \Delta\mu_{s't}, \Delta MW_s)/\mathrm{Var}(\Delta MW_s).^{25} \qquad (9)$$

The only assertion about the shocks in different sets of states that is compelling a priori is that I $\mathrm{Var}(\Delta\mu_{st} - \Delta\mu_{s't})$ is smaller for nearby states than farther way states. But equation (9) shows that two different magnitudes for the bias in the close-controls estimator.

First, is $\mathrm{Cov}(\Delta\mu_{st} - \Delta\mu_{s't}, \Delta MW_s)$ necessarily lower for close states? This takes us back to the

[24] $\Delta MW_{s'} = 0$, since s' denotes the untreated states.
[25] Formally, this is the inconsistency, derived from taking probability limits.

question of what drives minimum wage variation between nearby states. Here is one possibility in which the covariance would be *higher* in nearby states. Suppose policymakers in part respond to changes in low-skill labor markets in setting minimum wages, but they also respond to other factors. In two distant states, *because they differ on many dimensions*, the other factors (or, more precisely, changes in those factors), vary more. In contrast, in bordering states, because of their assumed homogeneity, the other factors do not differ. In that case, even though $\mathrm{Var}(\Delta\mu_{st} - \Delta\mu_{s't})$ is higher in the farther state pairs, $\mathrm{Cov}(\Delta\mu_{st} - \Delta\mu_{s't}, \Delta MW_s)$ is higher for the bordering states.

Second, the denominator in equation (9), $\mathrm{Var}(\Delta MW_s)$, is generally lower in nearby states, because of a strong regional component to minimum wages; for example, New England states are more likely to border other New England states that tend to have higher minimum wages. This, in itself, will exacerbate the bias in the close-controls estimator.

This discussion about potential bias in the close-controls estimator is speculative. (At the same time, I would argue that the claim that the close-controls approach is necessary to get unbiased estimates of minimum wage effects is also speculative.) But since I am already speculating, let me take it a bit further.

My hypothesis about influences on labor market conditions on minimum wages is that some policymakers like to increase the minimum wage, maybe because they think it is a good idea, but also because minimum wages are popular.[26] But policymakers also know that they get blamed for poor economic developments that coincide with the policies they choose, regardless of whether the policies are to blame. Thus, policymakers will tend to raise minimum wages when there are positive shocks to low-skilled labor markets.[27] The average positive pre-trend prior to minimum wage increases that Monras (2015) estimates is consistent with this story, as is the stronger disemployment effect that Baskaya and

[26] See, e.g., https://www.thirdway.org/memo/americans-want-to-raise-the-minimum-wage-but-not-the-way-dc-thinks (viewed June 30, 2018).

[27] It has to be a little more complicated than this because the error term of interest in this discussion is in a model for low-skill labor market outcomes conditional on an aggregate cyclical measure. Also, the last federal increases in the United States coincided with the Great Recession, hardly a time of improving labor markets. However, my hypothesis is intended to help explain the local variation at issue in most of the recent literature.

Rubinstein (2015) estimate using their IV.

More importantly, if we couple this with the argument above – that minimum wage variation between nearby states is more correlated with shocks to low-skilled labor markets than is minimum wage variation between non-close states, then this contributes to greater *positive* bias in the estimates of β from the close-controls approach. The alternative, of course, is that the close-control estimates are the unbiased ones, in which case we would have to believe that policymakers tend to raise minimum wages when low-skilled labor markets are deteriorating, which I find less plausible.

What we need, clearly, is research that gets beyond this speculation. This research needs to better illuminate the determination of minimum wage variation, as well as how the determination of this variation influences the different estimators. Of course, if we have a means of implementing a synthetic control estimator across all minimum wage increases, as in Powell (2016), then maybe this point is now moot with respect to the U.S. evidence, because we do not need to rely on a priori assumptions about which controls are valid. Still, it would be better to uncover – if we can – an empirically-grounded, behavioral basis for specifying control areas. More generally, researchers seem to accept cross-border research designs as valid, rather uncritically.[28] It seems useful, then to think about economic analyses of cross-border policy variation that might tell us when such designs are more likely to be useful.

This is a good segue into the other potential issue related to using close controls – the potential role of spillovers between treated and control areas. A recent theoretical contribution by Zhang (2017) models such spillovers in a search model of minimum wages and demonstrates why such spillovers could generate bias against finding disemployment effects in close-control research designs. In his model, there are two areas between which workers can commute or migrate, one of which raises its minimum wage. Workers are heterogeneous (high and low skilled). Workers are randomly distributed across the two areas, but firms decide where to post jobs. Search is random, so workers in both locations are contacted by firms at the same rates. The bargained wage depends in part on the worker's quality and is truncated

[28] For two recent examples in other contexts, see de Blasio and Poy (2017) and McVicar et al. (2018). There are a number of other examples in the tax literature.

by the minimum wage. Workers also make commuting or migration decision based on the trade-off between the wage offers they received and the moving cost they need to pay.

A higher minimum wage on one side of the border attracts neighboring high skilled workers to move or commute in, due to higher wages. Local firms therefore create more job vacancies, because the greater proportion of high-quality workers makes those vacancies more likely to be filled with high-quality workers and thus become more productive. At the same time, there is some gross disemployment falling on low-skilled workers as a higher minimum wage leads marginally productive matches to end. In equilibrium, the higher minimum wage area experiences an influx of high-quality workers and an outflow of low-quality workers, and the latter generates negative externalities for low-quality workers in the area where the minimum wage did not increase.

Cross-border designs can therefore understate disemployment effects for two reasons. First, unemployed workers migrate from the treated area, obscuring the lower employment rate in the treatment areas. Second, they move to or commute to the control area, reducing the employment rate and perhaps employment there (the latter from fewer vacancies), contaminating the control group in way that further obscures job loss in the treatment area relative to the control area.

Zhang presents two types of supporting evidence. First, lower-quality workers (based on education) tend to migrate or commute from counties where minimum wages increases.[29] Second, he presents calculations suggesting that the diminution of the disemployment effect from using neighboring counties as control areas can be due to labor mobility, rather than the spatial heterogeneity that Dube et al. (2010) (and Dube et al., 2016) emphasize, and presents empirical evidence consistent with this argument, showing that the diminution of the disemployment effect from using cross-border counties as controls is stronger where moving costs are lower (because of distance).[30,31]

[29] Pérez Pérez (2018) reports similar evidence, finding that when minimum wages increase along a border, low-wage commuting into the affected area declines, and the low-wage employment share declines.

[30] The minimum wage and cross-border flows have also been studied by McKinnish (2017) – who reports evidence consistent with Zhang's – Kuehn (2016), and Shirley (forthcoming). More general evidence on minimum wages influencing migration (focusing on location decisions of low-skilled immigrants) is reported in Cadena (2014).

[31] Another potential reason that cross-border designs can understate disemployment effects is policy spillovers to wages across the border (Jardim et al., in progress).

Thus, I think there are two possible positions regarding using close-control research designs to estimate minimum wage effects. The favorable position is that these methods uncover unbiased estimates of the disemployment effects of minimum wages, which are near zero. Other methods, including those that try to account for the potential correlation between minimum wage changes and shocks to low-skilled labor markets, generate biased estimates that suggest disemployment effects. This is presumably because policymakers tend to raise local minimum wages in concert with negative shocks to low-skilled labor markets, *and* the other methods used in recent literature for some reason do not capture this mechanism. The alternative position is that the close-controls approach is biased against finding disemployment effects, perhaps because of mobility and spillover effects, and/or because close-control designs exacerbate positive endogeneity bias in estimated disemployment effects of minimum wages.

In my view, there is more evidence for the latter position. This includes the evidence in Neumark et al. (2014a) suggesting that close controls are not better controls, reinforced by the evidence from Powell (2016) that a comprehensive synthetic controls analysis produces rather strong disemployment effects. It also includes the evidence in Zhang (2017) – predicted by his model – that mobility and spillover effects generate a bias towards zero in close-controls estimates of disemployment effects. Finally, the evidence from recent research using different methods of controlling for endogenous minimum wages (i.e., not close controls) finds stronger disemployment effects of minimum wages (Baskaya and Rubinstein, 2015; and Clemens and Wither, 2016) – consistent with the opposite direction of bias generated by endogenous policy responses from that suggested by close-controls designs.

At the same time, I think we are quite far from a definitive answer. More research assessing the reasons for different estimates of employment effects of minimum wages from different research designs would be very valuable.

IV. Trends

Another issue highlighted in recent work – related to the construction of the counterfactual but not in as transparent a way – is the inclusion of trends in models estimating the employment effects of minimum wages. This issue arises in using the standard two-way fixed effects model, and in models for employment *growth* that are motivated by new theoretical models of minimum wage effects.

IV.1. Including Trends in the Standard Panel Data Model

In reduced-form, panel data analyses of policy effects it is quite standard to include linear time trends specific to the states (or other jurisdictions) under study. This is intended to correct for violations of the "parallel trends" assumption (in the case of DDD estimates, in differences between affected and unaffected groups), by controlling for cross-state differences in the evolution of outcomes that were present absent the policy change. However, recent work has demonstrated that estimated employment effects of minimum wages can be quite sensitive to the inclusion of state-specific time trends.[32] For example, in the standard two-way fixed effects model, ADR showed that estimated employment effects were quite sensitive to including state-specific linear trends (going to near zero in this case). However, Neumark et al. (2014a) showed that this conclusion was fragile; when higher-order trends were included, the estimated employment effects reverted to being negative. At a minimum, this illustrates that we should do more than just check the sensitivity of estimates to including linear trends.

The broader point, though, is that the inclusion of trends (especially for outcomes that are not naturally trended, like employment rates) is ultimately a profession of our ignorance, in two dimensions. (I am putting aside the notion that including trends corrects for policy endogeneity. I cannot think of a good reason why trends – linear or otherwise – would capture explicit endogeneity. And the kinds of approaches discussed in the previous section are more appropriate for addressing this issue.)

First, the appeal to including trends is typically based on the hypothesized influence of omitted variables that underlie these trends.[33] This suggests that more compelling evidence will come from expanding the variables used in minimum wage studies to include the hypothesized omitted variables. And we should have extra motivation to pursue this line of inquiry given that results are non-robust across different ways of including time effects to capture these unmeasured variables. Most employment equation specifications in the minimum wage literature use quite measured parsimonious controls, often

[32] See Allegretto et al. (2011, 2017), Neumark et al. (2014a), and Neumark and Wascher (2017).
[33] For example, Allegretto et al. (2011) who refer to unmeasured changes in technology leading to teens experiencing increased competition from adults for low-skilled jobs.

including only an aggregate labor market indicator and a relative supply variable (like the share of the young population in the total population). This is rather striking relative to research on other topics where a much more extensive list of controls is typically included. I think the reason for this parsimony is that research papers try to build incrementally on past work – changing the estimation method, for example, and not wanting to confound the effects of doing this with the effects of varying the control variables. But the sensitivity of estimates to the inclusion of trends suggests that a change in strategy might be warranted. Indeed, a couple of recent papers introduce richer sets of controls (Clemens and Wither, 2016; and Clemens and Strain, forthcoming).[34]

The second dimension of our ignorance concerns how to introduce and estimate trends when we still want to include them. One issue is functional form. There is nothing sacred about linear trends. In fact, Neumark et al. (2014a) suggest that linear trends can be particularly problematic when business cycles affect the estimated trends, and in some contexts linear trends can lead to impossible implied values of dependent variables (such as an employment rate below zero or above one). A second issue is how we distinguish between trends and treatment effects – an issue discussed in more detail in the next subsection. We should probably explore estimating trends using only the pre-treatment period and using these estimated trends to detrend the post-treatment data, to avoid confounding policy effects and estimation of trends. As an example, Monras (2015) removes the trend fitted to the pre-treatment period for a specific number of periods.[35]

IV.2. Dynamic Models

In recent work, Meer and West (2016) demonstrate in a dramatic way the problem that trends can absorb treatment effects. They do this in the context of estimating effects of minimum wages on employment growth, although the same holds true for more conventional models focused on levels.[36]

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[34] In the latter, adding richer controls tends to strengthen adverse employment effects estimated for 16-21 year-olds.

[35] Monras also estimates a separate post-treatment trend, which he alternately considers as part of the treatment effect (à la Meer and West, 2016, discussed below) or not part of the treatment effect.

[36] Of course, the effects of minimum wages in a model for employment growth also involves the issue of trends, since now the data are detrended by first-differencing, with the difference that the effect of minimum wages is modeled as an effect on the growth rate of employment (log differences).

Most prior research has not studied effects of minimum wages on employment growth. However, recent models using a "putty-clay" approach to technology have suggested that minimum wages may have small initial effects on employment but increasing effects over time, as new technology comes on line that uses less low-skilled labor (e.g., Sorkin, 2015).

To see the basic idea in Meer and West in a simple way, suppose that the minimum wage can change both the level and the rate of growth of employment, so that the correct model absent trends is

$$E_{st} = \beta \cdot MW_{st} + \beta' \cdot t \cdot MW_{st} + X_{st}\gamma + D_s\theta + D_t\lambda + \varepsilon_{st}. \tag{10}$$

If the growth effect ($t \cdot MW_{st}$) is omitted, and one instead estimates the standard two-way fixed effects model, then part of the effect of $t \cdot MW_{st}$ will load onto the simple effect of MW_{st}. To simplify further, suppose there are two states, and there is a single minimum wage that starts to increase in one state beginning in in period t', which is between t = 1 and t = T. Then as long as the panel is somewhat long, some of the growing shortfall in employment (assuming $\beta' < 0$) in the treated state will be captured in the estimate of β when $t \cdot MW_{st}$ is omitted.

But suppose, in addition to omitting $t \cdot MW_{st}$, that a researcher includes state-specific trends, so the model becomes

$$E_{st} = \beta \cdot MW_{st} + X_{st}\gamma + D_s\theta + D_t\lambda + D_s \cdot t\psi + \varepsilon_{st}. \tag{11}$$

In this case, the included trends can pick up the effect of the omitted variable $t \cdot MW_{st}$, even if there are no true state-specific trends in the model. This is less likely if t' is close to T and far from 1, because then there is a long pre-treatment period with which to identify the parameter ψ that captures the trend difference between changes in E_{st} in the treatment and control states. Put differently, if the minimum wage increases start late in the panel, then the correlation between $t \cdot MW_{st}$ and $D_s \cdot t$ is relatively weak, and less of the minimum wage effect will load onto the estimate of ψ. But if t' is close to 1, there is only a short pre-treatment period, and more of the effect of $t \cdot MW_{st}$ will load onto the estimate of ψ, obscuring the minimum wage effect.

The problem is mitigated, of course, by estimating the correct (or, at least, unrestricted) model

$$E_{st} = \beta \cdot MW_{st} + \beta' \cdot t \cdot MW_{st} + X_{st}\gamma + D_s\theta + D_t\lambda + D_s \cdot t\psi + \varepsilon_{st}. \tag{12}$$

The analysis in Meer and West is consistent with the problem highlighted by equations (11) and (12). Their standard two-way fixed effects estimate of the employment elasticity is −0.15, significant at the 1% level. But adding state time trends reduces this elasticity to a small and insignificant −0.013 (their Table 2). Of course, this evidence is equally consistent with equation (11) being the correct model, and the negative minimum wage effect being a spurious reflection of state-specific time trends.

However, Meer and West present three types of evidence against the latter interpretation, and instead in favor of model misspecification from omitting the dynamic effect of minimum wages. First, they add leading minimum wage effects to the model. These effects are small and insignificant, and do not change the estimated main effect appreciably, suggesting that "if preexisting underlying trends are in fact different between states, they are not different by very much and are unlikely to be a key driver of the overall result" (p. 513). Second, they estimate long-difference specifications over different numbers of periods. If the minimum wage effect grows over time, as in equation (10), then the long-difference estimate should grow with the length of the difference (although perhaps leveling out as a new equilibrium is reached). This is exactly what Meer and West find, with a small and insignificant −0.02 elasticity for a one-year first difference, growing to (and stabilizing at) an elasticity of around −0.05 as the differences get longer.[37] Finally, they estimated distributed lag estimates that show that, indeed, contemporaneous minimum wage effects are small, but the lags cumulate to a larger effect.

The evidence in Meer and West (2016) is a departure from the larger literature in two dimensions. The first, already discussed, is their focus on dynamic effects. That said, their evidence does point to dynamic effects consistent with "putty-clay" models. Aaronson et al. (2018) generates this type of evidence from a calibrated model for the restaurant industry, but also finds evidence consistent with this framework when it studies firm entry and exit behavior. Second, Meer and West look at aggregate employment (in three different data sets, one in the paper, and two in on-line appendices), in contrast to

[37] Moreover, when they then add state-specific time trends to these models, the long-difference estimates are sensitive to the length of the difference and become small as the difference length grows. If there were no true minimum wage effect, the estimates with trends should be consistently zero. In contrast, the diminution of the estimated effects is consistent with the trends soaking up the true effect.

the usual focus on low-skill groups or industries. However, in their on-line appendices, they show results for industries in two of the three data sets (for which industry is identified) and find that their evidence is driven more by industries with higher concentrations of low-wage workers (Meer and West, n.d.). Together, this work raises the question of whether the literature should move toward more emphasis on dynamic and hence longer-run models for estimating the employment effects of minimum wages. To be clear, if we find additional evidence of adverse effects on aggregate employment when we look at longer-run models, the policy implications of minimum wages – and especially much higher minimum wages – will likely become much more negative.

V. Variation in Results across Studies – Economic Factors

The preceding sections focus on the how econometric methods influence estimated minimum wage effects, in part to help understand the sources of variation in effects across studies. In this section, I turn to the potential role of economic factors in explaining this variation. It is harder to draw specific conclusions, but there are some hints that economic factors may also play a role. A sharper focus on understanding the relationship between estimated employment effects of minimum wages and underlying economic factors may be particularly important in the current and pending high minimum wage environment in the United States, by identifying potential channels by which the employment effects could change at much higher minimum wage levels and suggesting how simply scaling up existing elasticities by larger increases could therefore be misleading.

V.1. The "Bite" of the Minimum Wage

Perhaps most important in thinking about the effects of much higher minimum wages, and one that may inform the literature more generally, is the "bite" of the minimum wage – i.e., how much the minimum wage binds. This question has received relatively little attention in the research literature.

One type of evidence on a much larger bite comes from an earlier study by Castillo-Freeman and Freeman (1992), who estimated the effects of the minimum wage in Puerto Rico – a U.S. territory that is bound by the U.S. federal minimum wage but has much lower wage levels, and hence where the minimum wage has much more bite. They reported very large aggregate employment effects and particularly adverse effects on low-wage industries, consistent with stronger disemployment effects where the minimum wage binds strongly. This evidence was revisited by Krueger (1995), who found evidence of disemployment effects from time-series data but not cross-industry analyses and concluded that evidence of disemployment effects was fragile. But, surprisingly, to the best of my knowledge the evidence on Puerto Rico has not been revisited.[38]

Neumark and Wascher (2002) take a different approach, adopting techniques from the market disequilibrium literature (applied to labor markets in, e.g., Rosen and Quandt, 1978). They specify a labor demand and labor supply curve and fit a model that estimates the parameters of these curves as well as the probability that an observation is on the demand curve (the short side of the market when, in the standard model, the minimum wage is set too high), or instead at market equilibrium. The estimates of this model are used to compute these probabilities for samples and data used in other studies, asking whether the absence of minimum wage effects (in particular, in Card (1992a and 1992b)) could be attributable to minimum wages being largely non-binding.[39]

However, the approach is based on homogeneous labor, and as such misses what is likely the key issue regarding much higher minimum wages – how the effect changes as the share of workers affected increases. Card's (1992a) approach of specifying the minimum wage variable as the fraction affected by given minimum wage increases, rather than the minimum wage level or its ratio relative to a measure of mean or median wages, may be more useful for projecting the effects of much higher minimum wages, especially if we think we can reliably capture potential non-linearities in the effect of the fraction

[38] Of course, evidence for one jurisdiction suffers from the absence of a control group – the same concern regarding the earlier time-series evidence for the United States that fueled the interest in minimum wage research using the cross-state variation in state minimum wages that emerged in the late 1980s (and which motivated the cross-industry analysis in the two Puerto Rico studies). The same challenge arises in estimating minimum wage effects in European and other countries with only a national minimum wage. As a consequence of this problem, research on European countries often distinguishes between areas or industries strongly affected by a minimum wage increase and areas or industries not so much affected, with the latter serving as controls for shocks potentially correlated with minimum wages. For example, recent research on the implementation of Germany's new minimum wage focuses on regional variation in the bite of the minimum wage – which was much greater, generally, in formerly East Germany (e.g., Caliendo et al., 2017).

[39] A second model introduced the three regimes in the textbook "company town" monopsony model – the marginal cost of labor curve, the labor supply curve, and the labor demand curve, and found some evidence that the monopsony model fits the data better – although the textbook monopsony model is a far less plausible depiction of labor markets than more modern monopsony models that come out of search models (Manning, 2005).

affected. On the other hand, Baskaya and Rubinstein (2015) suggest that this kind of fraction affected variable is particularly prone to endogeneity with respect to local labor market shocks, and is procyclical and hence leads to bias against finding a disemployment effect. Thus, incorporation of measures of the bindingness of minimum wages may not be straightforward.

Of course, the fraction-affected approach (putting aside endogeneity concerns) would still run into problems in projecting the effects of minimum wages well outside the range of sample variation. However, in very recent years, variation in minimum wages across states has become sufficiently strong that it should be possible, using recent data, to start to obtain more reliable estimates of the effects of minimum wages that bind for a much larger share of workers. Still, the highest minimum wages have been applied in higher-wage states, leaving extrapolation to lower-wage states problematic.

A different perspective on the bite of the minimum wage that has been explored in recent work is for how long firms expect a minimum wage increase to increase the relative cost of low-skilled labor. This issue has been highlighted by Sorkin (2015), who notes that firms may have reasonably expected the kind of non-indexed, often infrequent minimum wage increases enacted in the United States to be offset by rising nominal wages (and prices) over time, reducing the incentive for firms to invest in alternative production technologies that economize on low-skilled labor. In contrast, indexed minimum wages, which are becoming increasingly common in American states,[40] may generate more adverse longer-run employment effects for low-skilled workers. Recent evidence consistent with stronger disemployment effects of indexed minimum wages is reported in Brummund and Strain (2016). Much larger minimum wage increases, especially in a low-inflation environment, could well be perceived by firms as creating large, longer-term relative increases in the cost of low-skilled labor, even aside from indexation.

V.2. Affected Workers

Closely related to the question of the bite of the minimum wage is the extent to which studies identify the effects of minimum wages on affected workers. Understanding how minimum wages impact the employment of the most directly affected workers is a substantively important policy question.

[40] See http://www.ncsl.org/research/labor-and-employment/state-minimum-wage-chart.aspx (viewed June 13, 2018).

Minimum wage-employment elasticities for teenagers or other low-skill groups are often characterized as "small" or "modest." Although this is a vague characterization, I believe what most economists mean by this characterization is that because estimated employment elasticities in the range −0.1 to −0.2 are well below 1 in absolute value, the earnings of affected workers, on the whole, will rise substantially when the minimum wage is raised (e.g., Freeman, 1996).

But the fact that the existing research often does not focus solely on affected workers means that the relevant elasticity for asking how minimum wages influence the incomes of affected workers must be larger in absolute value. For example, simplifying, we can write the minimum wage elasticity estimated for all teenagers (the most common type of estimate) as a weighted average of the elasticity for teenagers directly affected by a change in the minimum wage and the elasticity for teenagers currently earning above the minimum wage, or:

$$e = e^{A} \cdot p^{A} + e^{NA} \cdot (1-p^{A}) \tag{13}$$

where e is the estimated elasticity for teenagers as a whole, e^A and e^{NA} are the minimum wage elasticities for affected and unaffected teens, and p^A is the proportion directly affected by the change in the minimum wage. If we simplify and assume that the elasticity for unaffected workers is zero, then the minimum wage elasticity for affected teens (e^A) can be written:

$$e^{A} = e/p^{A} \tag{14}$$

It follows that the minimum wage elasticity for affected teenage workers is greater than the elasticity estimated for teenagers as a whole. The estimated elasticity from the usual minimum wage study will also tend to understate the elasticity of demand for affected workers because the size of the average wage increase associated with a higher minimum wage will be smaller than the minimum wage increase itself, given that some affected workers already earn more than the old minimum wage. Letting ΔW^A denote the average wage change of those workers whose wages are directly affected by the change in the minimum wage, and ΔMW the legislated increase, the demand elasticity for affected workers (that is, the elasticity with respect to the induced change in their wage) is

$$e^{A} = (e/p^{A})/(\Delta W^{A}/ \Delta MW) \tag{15}$$

Given that ($\Delta W^A / \Delta MW$) < 1, the elasticity in equation (14) clearly gets blown up to some extent. The possibility that the elasticity for affected worker exceeds −1 is consistent with the evidence from Seattle showing average earnings declines (Jardim et al., 2017). And as noted earlier, Clemens and Wither (2016) estimate an elasticity of −0.97; because they focus on directly affected workers, e and e^A may be quite close, and ΔW^A may be closer to ΔMW.

Thus, empirical research providing a tighter link between workers affected by the minimum wage and the employment effects they experience can sharpen our understanding of the policy implications of higher minimum wages. Neumark and Wascher (2007), in their narrative review of minimum wage research on employment effects since the early 1990s, argued that studies that focused on the least-skilled workers tended to find the sharpest evidence of disemployment effects. However, this argument was based on a qualitative assessment of the evidence across studies, rather than systematic empirical evidence comparing studies. More systematic evidence would be useful.

The most common group considered in studies of the employment effects of minimum wages is teenagers. This is a logical group to study, as teenagers generally earn very low wages because of their low skills and represent a vastly disproportionate share of minimum wage workers.[41] However, with the rich microdata now available to labor economists, it is possible to focus directly on workers affected by the minimum wage. Examples of minimum wage studies that try to identify impacts on affected workers, based on their wages, include Neumark et al. (2004) and, more recently, Clemens and Wither (2016). One limitation of this, however, is that we cannot as easily classify non-workers as affected or not, because we do not observe their wages, which can lead us to miss the effects of minimum wages on transitions from non-employment to employment. Changes in the rate of entry *into* employment, however, could be a quite important channel of employment adjustments. First, low-skill workers have

[41] For example, in 2016, teens were nearly 21 percent of workers paid hourly whose wages were at or below the federal minimum wage, but less than 6 percent of the total of workers paid hourly. (See https://www.bls.gov/opub/reports/minimum-wage/2016/home.htm, viewed May 10, 2017.) The representation of teens among minimum wage workers would, of course, decline at much higher minimum wages, and is likely to be lower in states with higher minimum wages.

very high turnover.[42] Second, there is evidence from data on worker flows that minimum wages lower the rate at which workers separate from firms and lower the rate at which workers are hired (Dube et al., 2016; Gittings and Schmutte, 2016).[43] Thus, ignoring the effects of minimum wages in reducing the flows of workers into jobs may well miss a potentially important channel by which higher minimum wages reduce employment of low-skilled workers.

One can of course study the effects of minimum wages on transitions from non-employment to employment (see Clemens and Wither, 2017). But our ignorance of likely offer wages for non-employed workers is a challenge. Selection-type models that predict wages for the low-skilled, non-employed could in principle be used, although given the relatively low explanatory power of wage regressions, it seems unlikely that such methods would accurately identify the lowest-wage workers. Longer-term panel data can tell us something about wages workers earned on previous jobs, which could potentially prove useful, although that information, too, may be available only for a subset of currently non-employed workers.

There may be other dimensions that influence how likely minimum wages are to affect certain groups of workers. For example, Lordan and Neumark (2018) study the effects of minimum wages on low-skill workers in jobs that are more easily automated, and find adverse employment effects for older workers, and for workers in some industries where we tend not to think minimum wages have much effect, like manufacturing.

Understanding how estimated minimum wage effects depend on variation in the identification of the impact from affected workers, and which affected workers, may also help clarify the underlying economics. For example, if the evidence of disemployment effects really is stronger and more robust when studies focus on directly affected workers, this would bolster the predictions of the standard neoclassical model's prediction of job loss from substitution away from the least-skilled workers (as well as scale effects), and also help explain why some studies find weaker evidence of job loss (for example, in studies that focus on industries that pay low average wages but have a good share of higher-wage

[42] See Choi and Fernández-Blanco (2016).
[43] Like with the general literature on employment effects, there is conflicting evidence on whether or not the relative magnitudes of these two effects lead, on net, to employment declines.

workers). In contrast, if there is an absence of a consistent pattern of larger disemployment effects when these effects are estimated for the lowest-skilled workers whose wages are pushed up by minimum wages, the neoclassical model's prediction would be harder to sustain.

In addition, sharper evidence on how minimum wage effects vary depending on the extent to which effects are identified from affected workers may give us a better handle on predicting effects of much higher minimum wages that will affect more workers.[44] I view this as a critical challenge. At a minimum, labor economists should be using the available microdata to try to identify skill and demographic groups likely to be affected by minimum wages, moving beyond just teenagers or workers in low-wage industries.[45] And it may be particularly useful to explore using panel data with wage information, or other methods, to directly identify workers and non-workers most affected by higher minimum wages and how minimum wage increases affect their flows into and out of employment. One potentially large-scale source of data that could be used is Unemployment Insurance records for the subset of states that report quarterly hours as well as earnings, from which wages can then be estimated – conditional on states making the data available to researchers, like in the recent study of the Seattle minimum wage.[46]

V.3. Labor-Labor Substitution

Studies that include both affected and unaffected workers can do even more to mask disemployment effects of minimum wages if there is labor-labor substitution. In a model with workers of different skill levels, a minimum wage that is binding for some workers is likely to generate some substitution towards higher-skill workers. One implication is that evidence on the employment effects of minimum wages that combine negative employment effects for the least-skilled with positive employment effects for those who benefit from labor-labor substitution will understate the net effects on the first

[44] Still, a potential difficulty in predicting such effects is that the impacts on affected workers may also depend on the share of workers affected, as we might anticipate that firms find it easier to make adjustments other than employment levels for small changes in minimum wages than for large changes in minimum wages. That is, there can be nonlinear effects of the share affected by minimum wage increases.
[45] For example, Monras (2015) presents some evidence of negative employment effects on the share of employment or full-time employment among those with a high school degree or less, without regard to age. (This is apparent only from de-trended estimates, using a method described earlier.)
[46] See Jardim et al. (2017).

group. A second implication is that such evidence will obscure the positive impacts on those workers who benefit from labor-labor substitution.

There is some evidence of labor-labor substitution, from research on both minimum wages (e.g., Neumark and Wascher, 2003; Clemens et al., 2018) and on living wages (Fairris and Bujunda, 2008).[47] But there is virtually no research that tries to use information on workers across a larger swath of the skill distribution to provide a fuller accounting of who gains and who loses from a higher minimum wage. Neumark et al. (2004) estimate wage, hours, employment, and income effects at different points of the wage distribution and find some evidence of hours increases a bit above the minimum, but also of wage declines (which could be attributable to labor supply increases, or to scale effects outweighing substitution effects).

Note, also, that this kind of evidence is also likely to be informative about the effects of much higher minimum wages. The ability to substitute away from labor whose price is directly increased by the minimum wage seems likely to be diminished as the minimum wage affects the wages of a larger share of workers. At the same time, the larger price increases implied by less labor-labor substitution imply may make it more likely that scale effects become important.

V.4. Predictions from Monopsony Models

Finally, spurred in part by studies that do not find evidence of disemployment effects of minimum wages, and occasionally even find positive effects, minimum wage researchers have sometimes appealed to monopsony search models as a better characterization of the low-skill labor market (beginning with Card and Krueger, 1995, and developed to a much greater extent in Manning, 2005). Understanding the underlying model is obviously central to identifying economic factors that can explain variation in the employment effects of minimum wages across studies.

Search models can, indeed, predict a positive effect of minimum wages over some range. This

[47] Living wages were a policy that arose in many cities (and other local jurisdictions) in the United States in the mid-1990s. Living wages typically imposed wage floors much higher than minimum wages but limited to much narrower sets of workers (city contractors, and firm receiving financial assistance from cities). For details and recent evidence, see Neumark et al. (2012).

was first pointed out in Stigler (1946), albeit in the case of a textbook single-buyer monopsony model. Brown et al. (2014) show in a fairly simple way how this result emerges in a modern search model that instead generates rising marginal costs of labor from frictions.

It is possible that search-monopsony models can account for the variation in estimated employment effects of minimum wages across studies. However, establishing this requires much more than noting that these models are consistent with such variation. As I have emphasized above, there are many reasons to expect variation in employment effects when the neoclassical model characterizes low-skill labor markets. I would find more convincing the claim that monopsony models can account for the variation in estimates – and therefore also the implication that minimum wages can sometimes increase employment – if there were evidence that directly tied variation in minimum wage effects to the predictions of these models. Christl et al. (forthcoming) report evidence of a nonlinear minimum wage effect – first increasing and then decreasing – which is potentially consistent with these models. But evidence on more direct implications of these models would be more compelling. In particular, can we find evidence that the studies that find zero or even positive effects do this in settings where monopsony search models predict positive effects, and similarly find negative effects when the models predict negative effects – based, perhaps, on variation in the extent of frictions, in the level of the minimum wage, in the time frame (short- versus longer-run), etc.?

Of course, by the same token, the neoclassical model should not simply be taken as the default in the absence of more compelling evidence that search-monopsony models can explain the variation in employment effects across studies. In line with much of the discussion above, the neoclassical characterization of low-skill labor markets would be enhanced by more convincing evidence that variation across studies in estimated employment effects can be explained in the context of this model.

There is a direct way in which more evidence on the appropriate model may help in predicting the effects of much larger minimum wage increases. While the empirical methods used in past research on the employment effects of minimum wages are useful in studying the effects of past increases, a well-known limitation of these reduced-form methods is that they are less valuable in predicting the effects of different kinds of policy changes than are present in past data. Although large minimum wage increases naturally share some features with smaller increases, some of the considerations discussed above – such as the likely closing off of other margins of adjustment to higher minimum wages – suggest that structural models may have some value in projecting the effects of large minimum wage increases. One example is the calibrated model in Reich et al. (2015), although these authors have not provided, to the best of my knowledge, a detailed explication of the model they use, and have focused more on the predictions it generates. One problem, however, at least with models that are calibrated rather than estimated, is that the evidence on past minimum wage increases needed to calibrate the model is contested.[48] It would therefore surely be useful to gauge the sensitivity of these kinds of exercises to calibrations that reflect the larger employment elasticities that many recent studies find, and perhaps even more useful to push this approach further, including estimation of structural models that could at least provide complementary evidence on predicted effects of out-of-sample minimum wage increases.

VI. Questions for a Continuing Research Agenda on the Employment Effects of Minimum Wages

Given that my goal is to identify important questions to improve our understanding of the employment effects of minimum wages, I conclude by listing, in this section, what I view as the most productive questions to pursue, based on the research I have reviewed:

1. Why do identification strategies based on close geographic controls tend to find weak or no evidence of disemployment effects of minimum wages, in contrast to other methods?
2. Which type of identification strategy should be viewed as most convincing, and why should conflicting evidence from other strategies be viewed more skeptically?
3. Can we develop a better understanding of what determines minimum wage policy, and can this help us narrow the set of compelling strategies for identifying the employment effects of

[48] This is evidenced, for example, by the numerous simulation studies Michael Reich and co-authors have written, appealing to a "structural model" to project the effects of proposed high minimum wage increases in various cities. The studies typically project little if any job loss. For example, Reich et al. (2016) predict that a $15 minimum wage in San Jose and Santa Clara Counties (California), phased in by 2019, would cost 960 jobs in San Jose, and only 80 fewer jobs over the broader region. However, as they point out, their model is calibrated to "be consistent with the very small effects that researchers find for the smaller pre-2015 increases in federal and state minimum wages" (Reich et al., 2016, p. 20). Clearly this view of the evidence is not shared by everyone.

minimum wages?

4. To what extent can theoretical modeling, such as search models, help us understand variation in results across identification strategies?
5. How can we move beyond the inclusion of little-understood trends in our models of employment effects, to capture influences predicted by our models that can be measured in the data, or to refine estimates when we are limited to including trends in some way?
6. Should we move away from models focusing on short-term effects of the minimum wage on the level of low-skilled employment, and towards a focus on longer-term dynamic effects?
7. If we move to dynamic models, what does the evidence say, and do we really find robust evidence of effects of minimum wages on aggregate employment?
8. How can we generate more systematic evidence on the relationship between the bite of the minimum wage and estimated employment effects?
9. Does variation across studies in the focus on affected workers help explain variation in results, and how can we use longitudinal data on workers to better isolate affected workers?
10. Does labor-labor substitution help explain variation in estimated employment effects across studies?
11. Can monopsony in labor markets really account for the variation in estimated employment effects across studies?
12. Can we use evidence on the bite of the minimum wage, the share of affected workers, labor-labor substitution, and monopsony models to help predict the effects of much larger minimum wage increases?

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Figure 1: Percent Differences between State and Federal Minimum Wages, 2018

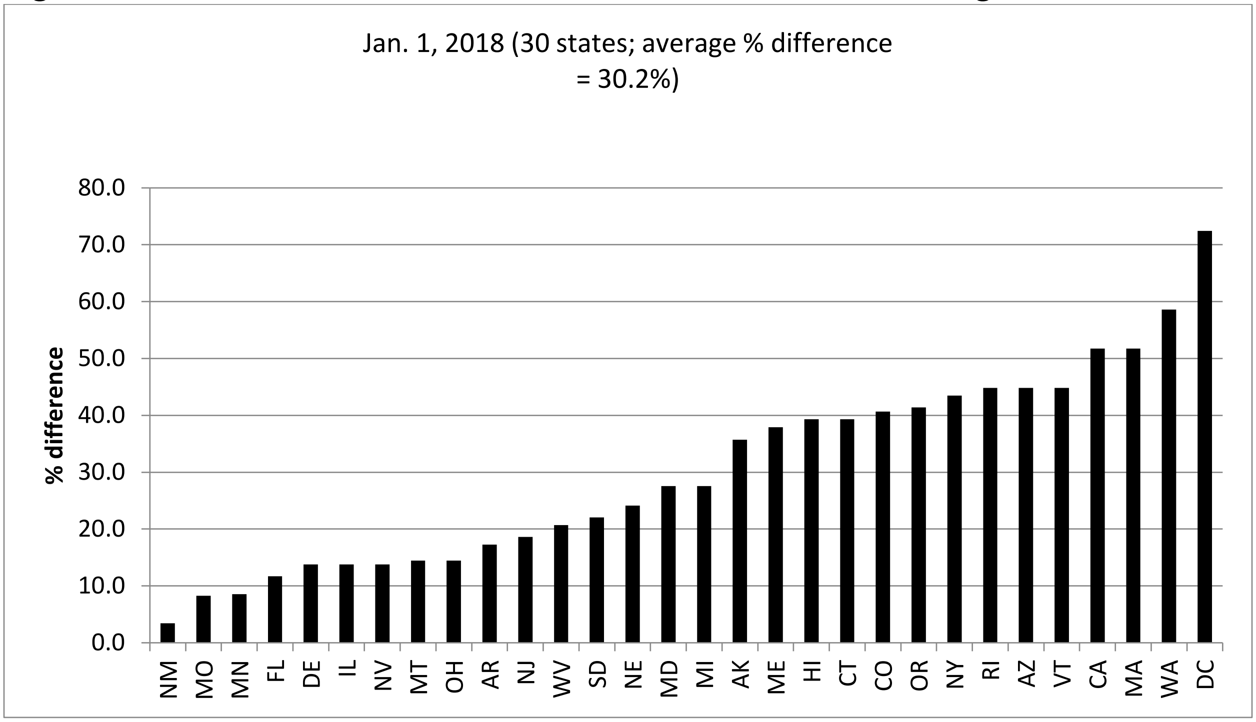
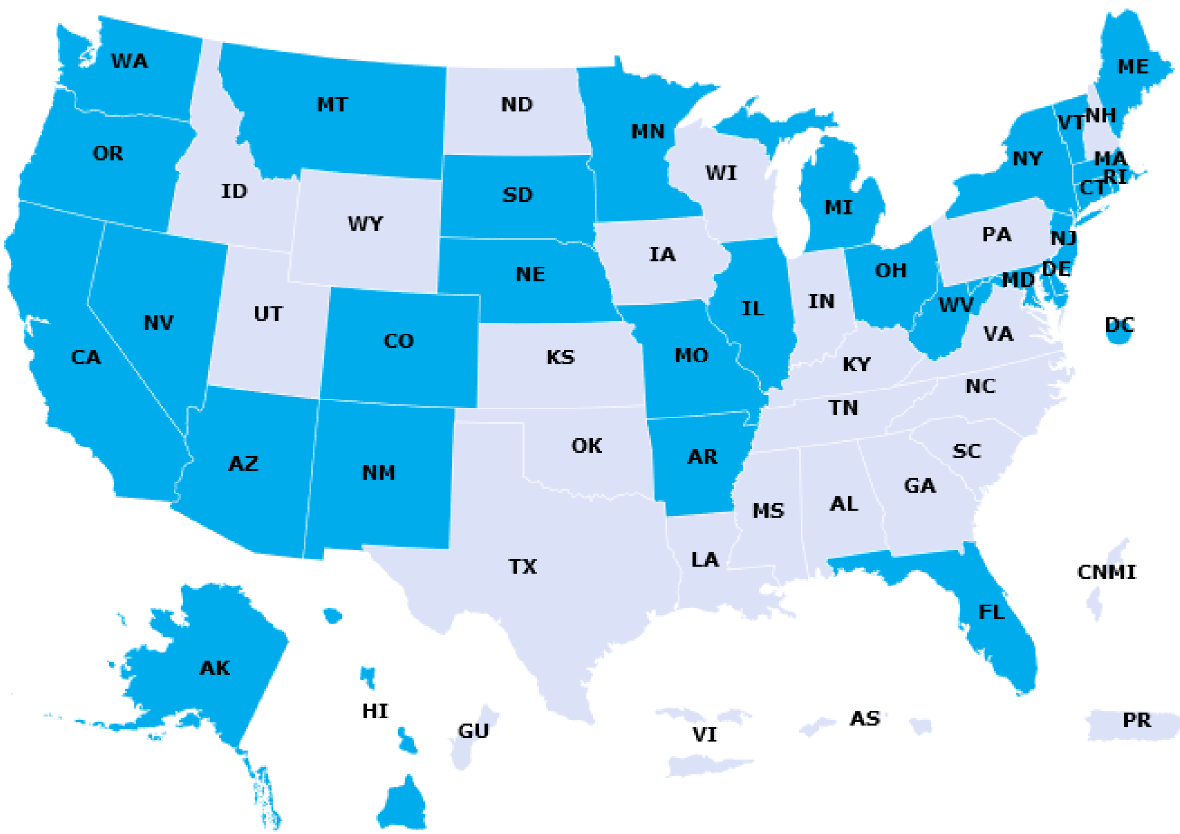


Figure 2: States (and Territories) with Higher vs. Federal Minimum Wage, Jan. 1, 2018



Source: https://www.dol.gov/whd/minwage/america.htm (viewed June 24, 2018). In states shaded light gray, the federal minimum wage prevails for workers covered by the Fair Labor Standard Act. The state minimum wage is higher in the other states.

Table 1: Recent Estimates of Minimum Wage Effects on Unskilled Employment

Authors	Employment elasticity and groups studied	Data/approach
Geographically-proximate designs		
Dube, Lester, and Reich (2010)	Near zero for teens and restaurant workers	Paired counties on opposite sides of state borders
Allegretto, Dube, and Reich (2011)	Near zero for teens	States compared only to those in same Census division
Gittings and Schmutte (2016)	Near zero for teens; larger negative elasticities in markets with short non-employment durations (−0.1 to −0.98) and smaller positive elasticities in markets with long non-employment durations (0.2 to 0.46)	States compared only to those in same Census division
Addison et al. (2013)	Varying sign, more negative, generally insignificant for restaurant workers and teens; stronger negative at height of Great Recession (−0.34)	Similar methods to Dube et al. (2010) and Allegretto et al. (2011) restricted to 2005-10 period
Slichter (2016)	-0.04 (teens)	Comparisons to bordering counties and other nearby counties
Liu et al. (2016)	−0.17 (14-18 year-olds)	Comparisons within Bureau of Economic Analysis (BEA) Economic Areas (EA) that cross state lines, with controls for EA-specific shocks
Other approaches		
Thompson (2009)	−0.3 (for teen employment share)	Low-wage counties vs. higher-wage counties in states
Clemens and Wither (2016)	Appx. −0.97, for those directly affected by minimum wage increase	Targeted/affected workers versus other low-wage workers in states affected by federal increases
Baskaya and Rubinstein (2015)	−0.3 to −0.5 for teens	States, using federally-induced variation as instrumental variable
Neumark et al. (2014a, 2014b)	−0.14/−0.15 for teens, −0.05/−0.06 for restaurant workers	States compared to data-driven choice of controls (synthetic control), and state panel data
Dube and Zipperer (2015)	−0.051 (mean) and −0.058 (median) for teens	States compared to data-driven choice of controls (synthetic control)
Powell (2016)	−0.44 for teens	States compared to data-driven choice of controls (synthetic controls, estimated simultaneously with employment effect)
Totty (2017)	−0.01 to −0.03 for restaurant workers; −0.03 to −0.07 for teens	States compared to data-driven choice of controls (factor model)

Notes: The table reports my best attempts to identify the authors' preferred estimates reported in the papers. The Thompson estimate cannot be compared directly to other elasticity estimates because there is no population count in the data source used. The Clemens/Wither elasticity is based on a 6.6 percentage point decline (p. 27), divided by a 70.2 percent employment rate (or a 9.4 percent employment decline), divided by a 9.7% minimum wage increase (50 cents, from p. 14, divided by $5.15). (These numbers are reported in a 2016 version of the study.)

Table 2. Weights on States in Same Census Division from Synthetic Control Method, CPS Data at State by Quarter Level, 1990 – 2011:Q2

	Proportion of weight on states in same division					
	Matching on:					
Division	Log teen employment-to-population ratio (1)	One-quarter difference in log teen employment-to-population ratio (2)	Four-quarter difference in log teen employment-to-population ratio (3)	Avg. # divisions in donor pool (4)	Avg. # states in donor pool (5)	Avg. # states in donor pool in same division (6)
New England	0.209	0.163	0.185	6.9	30.4	1.9
Middle Atlantic	0.134	0.455	0.168	5.5	20.0	1.0
East North Central	0.000	0.016	0.015	9.0	39.5	3.5
West North Central	0.823	0.698	0.464	3.7	7.7	1.7
South Atlantic	0.290	0.075	0.222	6.9	26.8	4.9
Pacific	0.339	0.279	0.297	5.3	21.1	2.1
Aggregate	0.323	0.264	0.251	6.1	24.0	2.5

Notes: Results are reported for the 50 unique minimum wage treatments (out of a total of 129 increases based on criteria described in the text) for which there is at least one potential donor state from the same Census division. The numbers in columns (4)-(6) refer to the matching on residuals or the log teen employment-to-population ratio. There are somewhat fewer minimum wage treatments when matching on the one- or four-quarter differences in the employment-to-population ratio because the earliest lags are not available at the beginning of the sample period. The aggregate row reports the means across all treatment units.
Source: Neumark et al. (2014a).