RUHR
ECONOMIC PAPERS

Martin Micheli

It Is Real: On the Relation Between Minimum Wages and Labor Market Outcomes for Teenagers

rwi

#829

Imprint

Ruhr Economic Papers

Published by

RWI – Leibniz-Institut für Wirtschaftsforschung
Hohenzollernstr. 1-3, 45128 Essen, Germany

Ruhr-Universität Bochum (RUB), Department of Economics
Universitätsstr. 150, 44801 Bochum, Germany

Technische Universität Dortmund, Department of Economic and Social Sciences
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

Editors

Editorial Office

Ruhr Economic Papers #829

Responsible Editor: Roland Döhrn

ISSN 1864-4872 (online) – ISBN 978-3-86788-962-9

Ruhr Economic Papers #829

Martin Micheli

It Is Real: On the Relation Between Minimum Wages and Labor Market Outcomes for Teenagers

Bibliografische Informationen der Deutschen Nationalbibliothek

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available on the Internet at **http://dnb.dnb.de**

RWI is funded by the Federal Government and the federal state of North Rhine-Westphalia.

http://dx.doi.org/10.4419/86788962
ISSN 1864-4872 (online)
ISBN 978-3-86788-962-9

Martin Micheli[1]

It Is Real: On the Relation Between Minimum Wages and Labor Market Outcomes for Teenagers

Abstract

This paper reexamines the relation between minimum wages and labor market outcomes for teenagers in the US. Economic theory suggests that real minimum wages drive labor market outcomes. Instead of the commonly used nominal minimum wages, we therefore use real minimum wages to examine this relation. Increasing real minimum wages are associated with a reduction in teen employment and working hours. The correlation with real hourly wages of teenagers is positive. These results are robust to the choice of the control group, whether we compare labor market outcomes in the respective state to all other states or to spatially close states, only. This strongly suggests that interpreting nominal minimum wage changes as minimum wage shocks is not a valid identification strategy.

JEL-Code: J3, J48

Keywords: Minimum wage; teen employment; teen working hours; teen wages; state panel; US

December 2019

1 Martin Micheli, RWI. – *The author is grateful to Helge Braun, Roland Döhrn, Christopher J. Flinn, Hanna Frings and seminar participants at RWI (Essen, Germany) for helpful comments on an earlier draft.* - *All correspondence to: Martin Micheli, RWI, Hohenzollernstr. 1-3, 45128 Essen, Germany, e-mail: martin.micheli@rwi-essen.de*

1 Introduction

The effects of minimum wages on labor market outcomes are among the most controversial topics in economics. Whether or not minimum wages lower employment is of particular interest. Theoretically, negative as well as positive employment effects can occur (Flinn, 2006). The literature typically estimates employment effects for highly affected subgroups such as teenagers or for sectors with a high share of potentially affected workers by employing a reduced form.[1]

Among others, Neumark et al. (2014b), Meer and West (2016), and Clemens and Wither (2019) document negative employment effects of minimum wages. Dube et al. (2010), Allegretto et al. (2011), and Dube et al. (2016) stress the importance of controlling for local shocks. The authors show that introducing spatial controls results in insignificant estimates for the employment response.[2] These potentially contradicting findings have mainly been attributed to differences in the control groups (Neumark et al., 2014a), which started a fierce debate about the appropriate control group (Allegretto et al., 2017; Neumark and Wascher, 2017).[3]

This paper questions whether differences in the control groups are really driving the potentially contradicting empirical findings in the literature. We argue that differences in estimated employment responses are a symptom of misspecification of the estimated equation. Economic theory suggests that employment is affected by real minimum wages (Sorkin, 2015). This is well acknowledged by both sides of the debate.[4] Estimated employment effects, however, typically refer to nominal minimum wages.[5] The inclusion of time fixed effects, however, renders deflating nominal minimum wages irrelevant if an all country price index is used (Meer and West, 2016, footnote 10).

Using nominal minimum wages would be harmless, if the evolution of prices was identical across states. The well documented transmission of nominal minimum wage hikes to consumer prices (Aaronson, 2001; Aaronson et al., 2008; Allegretto and Reich, 2018; Harasztosi and Lindner, 2019) and differing stances of the business cycle, however, make

[1]For excellent surveys of the literature, see Neumark and Wascher (2007) and Card and Krueger (2015). Neumark (2019a) discusses the most recent literature.

[2]To be precise, Allegretto et al. (2011) advocate the use of census division-specific time effects and state-specific linear time trends to control for different long-run trends across states and include such controls in their subsequent research (Dube et al., 2016; Cengiz et al., 2019).

[3]Zhang (2018) and Monras (2019) argue that minimum wages affect migration flows, which might render using spatially close regions problematic.

[4]For example Allegretto et al. (2011) and Meer and West (2016) report summary statistics for real minimum wages.

[5]There are some studies that use relative wage measures such as Baker et al. (1999) and Giuliano (2013).

this a questionable assumption. One explanation for the common reliance on nominal minimum wages might be that consumer prices are not available on the state level for a sufficiently long time period.[6]

There are, however, promising alternatives to using consumer prices. The GDP deflator is a widely used indicator for price pressure. It has even been assessed to be superior to consumer prices in this respect (Bernanke and Mihov, 1998) and is available at the state level. Controlling for differences in the evolution of state level prices by using these state level GDP deflators, we reexamine the relation between minimum wages and teen employment, teen working hours, and real wages of teenagers.

For teen employment and working hours, we document an economically (and statistically) significant negative relation. This relation is independent of the control group, whether we compare teen employment to the remaining country average as e.g. suggested by Neumark et al. (2014b) and Neumark et al. (2014a) or to spatially close entities as e.g. suggested by Allegretto et al. (2011) and Dube et al. (2016). A one percent increase in real minimum wages is associated with a decrease in the teen employment share of about 0.08 percentage points. Given the average teen employment share in our sample, this translates into an employment elasticity of about -0.2, which is well in line with the old consensus of about -0.1 to -0.2 (Neumark and Wascher, 2017, p. 607).[7]

Working hours of teenagers also seem to be negatively related to real minimum wages. Our results are again robust to the introduction of state specific trends and spatial controls. A one percent increase in real minimum wages is associated with decrease in working hours of about 0.13 percent. For real wages of teenagers, we find a significant positive co-movement whit real minimum wages. A one percent increase in the real minimum wage is associated with an increase in real teen hourly wages by about 0.28 percent.

Using the economically more meaningful concept of real minimum wages instead of nominal ones, however, comes at a cost. Real minimum wage changes are on the one hand driven by governments setting nominal minimum wages and on the other hand by changes in the price level. We therefore cannot interpret the co-movement between minimum wages and labor market outcomes as causal effects running from real minimum wages to e.g. teen employment.

[6] Annual implicit price deflators on the state level start in 2008 and are published by the BEA.

[7] The old consensus refers to the employment elasticity of nominal minimum wages. Given the well documented spillovers from minimum wages to prices, a one percent increase in nominal minimum wages should transmit to real minimum wages with a coefficient smaller than one. This might be one explanation for our estimate being at the upper end of the old consensus.

2 Estimation Strategy

Starting point for our analysis is a standard two-way fixed effect model that controls for state- and time-fixed effects similar to Neumark et al. (2014b). As suggested by Allegretto et al. (2011), we stepwise augment this model by state-specific time trends and census division-specific time effects. The well documented sensitivity of the estimated minimum wage coefficient to the inclusion of these spatial controls is central to the debate on the employment effects of minimum wages.

All estimated specifications can be described by the equation

$$y_{it} = \beta \log(mw_{it}) + \alpha \; controls_{it} + \phi_{i,month} + \psi_i \cdot t + \tau_{jt} + \epsilon_{it} \; , \qquad (1)$$

where y represents either the teen employment share, log working hours of teenagers, or log real hourly wages of teenagers. mw is the real minimum wage. $controls$ represents time-varying controls for macroeconomic conditions in a respective state. It has become common practice to control for local labor market conditions by either including employment (Dube et al., 2010; Allegretto et al., 2011; Dube et al., 2016) or the states' unemployment rates (Neumark et al., 2014b). Clemens and Wither (2019) criticize this procedure as overall labor market variables might be affected by minimum wage policies. Instead, the authors propose the inclusion of state level house prices as proxies for the stance of the business cycles. We include both, states' house prices and unemployment rates, as controls. To address the concerns of Clemens and Wither (2019) with regard to a potential correlation of states' overall unemployment rates with minimum wages, we use the unemployment rate of individuals between 26 and 59. As common in the literature (Neumark and Wascher, 1992; Flinn, 2006), we assume that individuals over 25 are largely unaffected by minimum wage policies.

The indexes i and t indicate the respective state and time period. $\psi_i \cdot t$ represents state-specific time trends and τ_{jt} with $j \in \{c, d\}$ either represents common time-fixed effects (in case of $j = c$) or census division-specific time-fixed effects (in case of $j = d$) with d indicating the respective census division. ϵ represents the residuals, which are clustered on the state level. $\phi_{i,month}$ represents a state-fixed effect for each of the 12 months in a year to capture seasonality. The literature typically includes state-fixed effects, only. Seasonality is then captured by time-fixed effects. We argue that seasonality might differ across states, allowing for state-specific month-fixed effects is one way to control for this.[8]

[8]In case of allowing for census division-specific time-fixed effects, this issue might be less of a concern as seasonality is captured at the census division level. But even then, seasonality in Alaska and Hawaii is likely to differ.

3 Data

We estimate the relation between minimum wages and labor market outcomes for teenagers in the 50 US states and the District of Columbia. We use the Current Population Survey (CPS) for information on teen employment, hours worked, and wages on the state level, more precisely the Integrated Public Use Microdata Series (IPUMS) database (Flood et al., 2018). Our sample period covers the years from 1991 to 2017.[9]

Consistent with the literature, we interpret teens as individuals of age 16 to 19. The teen employment share is the number of working teenagers relative to the teen population in a respective state.[10] Teen working hours are the average working hours of teenagers during the previous week in a respective state.[11] In a robustness check, we use usual hours worked per week, which has been used by Allegretto et al. (2011). These are available starting in 1994. Teen hourly wages are the average earnings per hour for teens that are paid an hourly wage.

The state level unemployment rate for individuals that are largely unaffected by minimum wage policies (individuals between 26 and 59, we will be referring to as prime age individuals) is also calculated based on the CPS.

Nominal minimum wages in US states are taken from Neumark (2019b). This dataset includes the binding minimum wage, which is the maximum of the state specific and the federal minimum wage, for the 50 US states and the District of Columbia. The information is on a monthly frequency and available up to 2017.

For information on state level prices, we use state level GDP deflators. These are calculated based on state level nominal and real GDP, which are available at the BEA on an annual frequency. Information on house prices is taken from the Federal Housing Finance Agency (FHFA). The FHFA provides seasonally adjusted state level house price indexes based on sales price data on a quarterly frequency. Sales price indexes are available starting in 1991.

There is a mismatch in the time frequency between the monthly CPS and minimum wage data on the one hand and the annual price deflators and quarterly house price indexes on the other hand. The literature offers several procedures to convert low frequency

[9]The literature often uses samples starting in 1990 (Allegretto et al., 2011; Neumark et al., 2014b). Due to availability of sales price based house price indexes, our analysis starts in 1991.

[10]Labor force weights that replicate the countrywide teen employment share exactly are only available for 1998 and afterward. For the time period from 1991 to 1997, we use final basic weights, which seems to be the procedure in Neumark et al. (2014b). We assess this procedure to be sufficiently accurate as differences in the years prior to 1998 seem negligible. Figure A.1 plots the all country teen employment share based on our calculation and based on the officially published numbers by the BLS.

[11]For the time period from 1991 to 1997, we again use final basic weights to compute average working hours.

data to higher frequencies. In our baseline specification, we employ a Denton-Cholette transformation with a constant (Denton, 1971; Cholette, 1984). We check for the robustness of the frequency conversion by employing several alternative methods. These are i) using the low frequency observations for each period of the higher frequency, ii) a quadratic interpolation, iii) a Denton-Cholette transformation with an indicator series (Denton, 1971; Cholette, 1984), and iv) a Chow-Lin transformation (Chow and loh Lin, 1971).[12]

When employing an indicator series (cases iii and iv), we use seasonally adjusted monthly consumer prices for the four US regions to convert annual GDP deflators to the monthly frequency. These are available at the BLS. For house prices, we use seasonally adjusted monthly house price indexes based on sales prices for the nine census divisions as indicator series. These indexes are available at the FHFA.

Summary statistics for all variables used in this paper are reported in Table 1. The average share of employed teenagers is 39 percent in our sample. Employed teenagers work approximately 24 hours per week and earn about 8.55 US-Dollars evaluated at US-Dollar values of the year 2012. Minimum wages have been 6.01 US-Dollars on average in nominal terms, which translates into about 7.03 US-Dollars evaluated at values of the year 2012. The states' average unemployment rate for prime age individuals has been about 4 percent in our sample.

The lower panel of Table 1 presents summary statistics for variables used in the various robustness checks. With 25 hours per week, usual working hours of teenagers are similar to hours worked in the previous week. The method used in the frequency conversion hardly affects summary statistics of the respective variables. Neither real teen hourly wages, nor real minimum wages, nor real house prices differ substantially from the variables used in the baseline specification.

4 Estimation Results

We estimate Equation (1) for a panel of the 50 US states and the District of Columbia. We use monthly observations from 1991 to 2017. The estimated coefficients $\hat{\beta}$ indicate the co-movement between real minimum wages and the respective dependent variable and are reported in Table 2. Column (1) represents a variant of the standard two-way fixed effect specification, which has been favored by Neumark et al. (2014b), but includes state-specific month-fixed effects to allow for differing seasonality across US states. Col-

[12]We use the R package "tempdisagg" for the Denton-Cholette and the Chow-Lin method (Sax and Steiner, 2013).

umn (2) augments this model by state-specific time trends, which has been label as the ”classic” fixed-effect specification (Meer and West, 2016). Column (3) augments Column (1) by division-specific time-fixed effects to control for spatial heterogeneity due to regional economic shocks (Allegretto et al., 2011). Column (4) presents estimation results for the fully fledged model with state-specific time trends and census division-specific time fixed-effects as proposed by Allegretto et al. (2011).[13]

For each dependent variable, we first present the estimated coefficient for the specification using nominal minimum wages as explanatory variable to ensure comparability with the literature (Allegretto et al., 2011; Neumark et al., 2014b; Meer and West, 2016; Dube et al., 2016). As we argue that real, not nominal, minimum wages drive labor market outcomes, we proceed by presenting estimation results using real minimum wages as explanatory variable.

Panel A. in Table 2 reports the estimation results for $\hat{\beta}$ with the teen employment share as dependent variable. Similar to the literature (Allegretto et al., 2011; Neumark et al., 2014b), we find a significant effect of nominal minimum wages on teen employment in the two-way fixed effect specification (Column 1). A one percent increase in the nominal minimum wage is associated with a 0.06 percentage point decrease in the teen employment share. Given an average teen employment share of 39 percent, this translates into an employment elasticity of about -0.15, which is roughly in line with estimates for the two-way fixed effect model in Neumark et al. (2014b) and Dube et al. (2016) and perfectly in line with the old consensus of about -0.1 to -0.2 (Neumark and Wascher, 2017, p. 607). It is well documented in the literature that this significant correlation between nominal minimum wages and teen employment becomes insignificant when controlling for state specific time trends and division-specific time effects (Allegretto et al., 2011; Neumark et al., 2014b; Dube et al., 2016). In our sample, the estimated coefficient reduces to about -0.02 (Column 4) and is statistically indistinguishable from zero at conventional levels of significance.

We proceed by estimating the correlation between teen employment and real minimum wages. In the two-way fixed effect model, a real minimum wage increase is associated with a significant reduction in the teen employment share of about 0.12 percentage points (Column 1). Introducing state-specific time trends (Column 2) or division specific time effects (Column 3) affects the co-movement quantitatively. The correlation, however, remains significant at conventional levels of significance. For the fully fledged model with all controls proposed by Allegretto et al. (2011), a one percent increase in real minimum

[13]For the purpose of simplicity, we will be referring to this model when stating that we add spatial controls.

wages is associated with a decrease in the state's teen employment share of about 0.08 percentage points (Column 4). This corresponds to an employment elasticity of about -0.2.

Panel B. in Table 2 presents the estimation results for teen working hours as dependent variable. Using nominal minimum wages as explanatory variable, the estimated employment elasticity in the two-way fixed effect specification is about -0.09 and statistically significant (Column 1). Introducing state-specific time trends and census division-specific time effects, the estimated elasticity becomes statistically insignificant (Column 4) at commonly used levels. Our results therefore seem in line with Allegretto et al. (2011), who document the instability of the estimated elasticity for teen working hours with respect to the inclusion of spatial controls.[14]

Using real minimum wages as explanatory variable, the estimated elasticity is significant in all specifications. A one percent increase in the real minimum wage is associated with a decrease in teen working hours of about 0.17 percent (two-way fixed effect estimation, Column 1) to 0.13 percent (fully fledged specification, Column 4).[15]

Panel C. in Table 2 presents estimated elasticities for real teen hourly wages. A one percent increase in nominal minimum wages is associated with a 0.13 percent (Column 1) to a 0.19 percent (Column 4) increase in average hourly wages of teenagers. The coefficient is significant in all specifications. Positive effects of nominal minimum wages on hourly wages of potentially highly affected groups, the literature typically uses nominal instead of real hourly wages as dependent variable, are rather uncontroversial and their robustness to the inclusion of spatial controls is well documented in the literature (Allegretto et al., 2011; Cengiz et al., 2019).

Similar to the previous cases of employment and working hours, we present estimation results using real minimum wages as explanatory variable. A one percent increase in the real minimum wage is associated with a 0.34 percent (Column 1) to a 0.28 percent (Column 4) increase in real hourly wages of teenagers. The estimated elasticity is quantitatively larger for real than for nominal minimum wages. Given the well documented price effects of minimum wage hikes (Aaronson, 2001; Aaronson et al., 2008; Allegretto and Reich, 2018; Harasztosi and Lindner, 2019), a one percent increase in real minimum wages implies a substantially larger increase in nominal minimum wages, which might

[14] Allegretto et al. (2011) use usual hours worked as explained variable. We use hours worked last week, for two reasons. First, these are available for the entire sample period used in this paper. Usual hours worked start in 1994. Second, we think it is reasonable to assume, that usual hours worked will respond with a lag to changes in actual hours worked. Hours worked last week are therefore, in our view, the superior measure. Estimated elasticities for usual hours worked as dependent variable are, however, very similar. We report the estimation results in Table A.1 in the Appendix.

[15] Estimated elasticities for usual hours worked are again reported in Table A.1 in the Appendix.

explain the quantitative difference in the estimates elasticities.

As discussed in Section 2, we allow for state specific seasonality due to the introduction of state-specific month-fixed effects via $\phi_{i,month}$ in Equation (1). The literature typically assumes a common seasonality, which time-fixed effects control for. To show that our results are robust to the assumption of a common seasonality, we report estimation results for Equation (1) using state fixed effects ϕ_i instead of state specific month effects $\phi_{i,month}$ in Table A.2 in the Appendix.

The literature also typically uses a quarterly frequency (Neumark et al., 2014b; Meer and West, 2016) instead of the monthly frequency employed in this paper. Table A.3 in the Appendix shows that our results are also robust to this change in the data frequency.[16]

4.1 Robustness Checks

State specific house prices are only available at a quarterly, state specific price indexes (we use GDP deflators) at an annual frequency. In our baseline specification, we use the Denton-Cholette method with a constant to transform states' house prices and price levels to a monthly frequency. As discussed in Section 3, there are alternative methods for frequency conversion in the literature. We therefore re-estimate Equation (1) where states' house prices and price levels have been converted using four different procedures. These are i) using the observed quarterly value for each month in the respective quarter, ii) a quadratic interpolation, iii) a Denton-Cholette transformation with census-division specific house prices and region-specific consumer prices as indicator series for quarterly house prices and annual GDP deflators, and iv) a Chow-Lin transformation, also using these indicators.

We report the estimated coefficients $\hat{\beta}$ in Table 3. The results are unaffected by the method used for the frequency adjustment. All coefficients are in line with estimates for the specification using real minimum wages as explanatory variable reported in Table 2.

Table 4 presents estimation results for a sub-sample, which excludes time periods for which minimum wages have been indexed to a nominal variable such as the price level. Indexation of minimum wages has become increasingly popular in recent years. This, however, might have changed the way minimum wages and labor market outcomes for teenagers are associated. It has therefore become a standard procedure to exclude time periods of indexed minimum wages to show how this affects the estimated elasticities (Allegretto et al., 2011; Meer and West, 2016). Our results are robust to this change in the sample. For teen employment (Panel A.), the significant co-movement with nom-

[16] In the frequency conversion, we use the average of the monthly observations.

inal minimum wages in the two-way fixed effect specification (Column 1) ceases to be significant when including spatial controls (Column 4). Increasing real minimum wages, however, are associated with lower teen employment in all specifications. The estimated coefficients are similar to our baseline results in Table 2. The estimated effects become smaller when adding spatial controls, but remain significant at conventional levels.

We find the same pattern for teen working hours (Panel B.). Increasing nominal minimum wages are associated with reductions in working hours in the two-way fixed effect specification (Column 1). The inclusion of spatial controls renders the coefficient insignificant (Column 4). For real minimum wages, the estimated elasticity is always negative and significant.

Real hourly wages of teenagers are positively related to minimum wages (Panel C.). This holds true for nominal as well as for real minimum wages and is robust to the inclusion of spatial controls.

In a further robustness check, we use a different measure for real minimum wages to analyze the co-movement with labor market outcomes. Cross-country comparisons of minimum wage policies typically deflate minimum wages by a measure of hourly wages of affected individuals to ensure cross-country comparability (Neumark and Wascher, 2004; Sturn, 2018). This ratio, often referred to as Kaitz index, takes differences in per capita income levels across countries into account.[17] Assuming a similar wage distribution, a low Kaitz index indicates that the minimum wage is probably not binding for many individuals. A value of one indicates that all workers are paid the minimum wage.

We follow this idea and measure the bindingness of minimum wages by deflating its level with the average hourly wage of teenagers in the respective state. Estimation results are reported in Table 5. A one percentage point increase in the relation of minimum to average teen hourly wages is associated with a reduction in teen employment of 0.02 (two-way fixed effect estimation, Column 1) to 0.01 percentage points (including spatial controls, Column 4). For working hours, we also find a negative relation with relative minimum wages. A one percentage point increase in the Kaitz index for teenagers reduces average teen working hours by about 0.12 (Column 1) to 0.11 percent (Column 4). Similar to the results using real minimum wages as explanatory variable, the inclusion of spatial controls does not affect the significance of the estimates, neither for employment nor for

[17]Using the Kaitz index to measure differences in the bindingness of minimum wages across space has also been employed in single country setups. Baker et al. (1999) evaluate minimum wage policies in Canada using the minimum wage relative to the average manufacturing wage. The Kaitz index is especially useful in the evaluation of uniform nominal minimum wages. vom Berge and Frings (2019) estimate employment effects of bargained, Ahlfeldt et al. (2018) estimate employment effects of the statutory minimum wage in Germany.

working hours.

5 Concluding Remarks

This paper reexamines the relation between minimum wages and labor market outcomes for teenagers in US states. We argue that it is crucial to distinguish between nominal and real minimum wages. The literature, however, typically estimates the effects of nominal minimum wages (Meer and West, 2016).

Most controversial in the literature are employment effects of minimum wages. We therefore start by estimating the employment elasticity of nominal minimum wages in our sample. In line with previous studies (Neumark and Wascher, 1992; Neumark et al., 2014b), we find significant dis-employment effects of nominal minimum wages in the two-way (state- and time-) fixed effect specification. The estimated elasticities, however, become insignificant when including spatial controls, as first documented by Allegretto et al. (2011) and subsequently found in the literature (Dube et al., 2016). The fragility of the estimated elasticity has been attributed to differences in the control group and started a fierce debate about the appropriate control group (Allegretto et al., 2017; Neumark and Wascher, 2017).

We argue that the fragility of the estimates might not necessarily be due to differences in the control group. Economic theory suggests that real minimum wages, not nominal ones, affect labor market outcomes. We therefore proceed by estimating the employment elasticity of real minimum wages. We find a negative relation between the two variables. The negative co-movement is significant in the two-way (state- and time-) fixed effect specification and robust to the inclusion of state-specific time trends and census division-specific time-fixed effects as suggested by Allegretto et al. (2011). An increase in the real minimum wage by one percent is associated with a decrease in teen employment of about 0.2 percent. This elasticity seems in line with the old consensus that a one percent increase in the nominal minimum wage lowers teen employment by about −0.1 to −0.2 percent (Neumark and Wascher, 2017, p. 607).[18]

We find a similar pattern for working hours of teenagers. The estimated elasticity for nominal minimum wages is significant and negative, but not robust to the inclusion of spatial controls, which confirms the finding of Allegretto et al. (2011). Using real

[18]Note that the old consensus refers to the employment response after a change in the nominal minimum wage. Given the well documented transmission of minimum wages to prices (Aaronson, 2001; Aaronson et al., 2008; Allegretto and Reich, 2018; Harasztosi and Lindner, 2019), the real minimum wage elasticity of teen employment is expected to be higher than the nominal one.

minimum wages as explanatory variable, the estimated elasticity is negative and significant at conventional levels, irrespective of the inclusion of state specific trends and census division-specific time-fixed effects.

The effect of minimum wages on hourly wages is rather uncontroversial. The literature typically finds positive effects of minimum wages on hourly wages of potentially highly affected subgroups (Allegretto et al., 2011; Cengiz et al., 2019). We document that the elasticity for real minimum wages is larger than the one for nominal minimum wages. This seems intuitive given the well documented spillovers of minimum wages on prices (Aaronson, 2001; Aaronson et al., 2008; Allegretto and Reich, 2018; Harasztosi and Lindner, 2019).

We argue that the assumption of a uniform inflation rate across all US states, which is implicit when correlating nominal minimum wages with labor market outcomes, is highly questionable. We interpret the documented differences in the correlations of nominal and real minimum wages with labor market outcomes as evidence against this assumption. When estimating labor market effects of minimum wages, one should therefore focus on the effects of real, not nominal, minimum wages, as suggested by economic theory.

Using real minimum wages, however, has drawbacks with regard to the identification of shocks. Typically, nominal minimum wages are assumed to be exogenous. Nominal minimum wage changes can then be interpreted as exogenous variation.[19] Interpreting real minimum wage changes as exogenous shocks, however, is obviously flawed.[20] Real minimum wages are affected by nominal minimum wages as well as by macroeconomic conditions, which most likely also affect the price level. We therefore have to abstain from a causal interpretation of the correlations estimated in this paper. Future research therefore requires a strategy to identify real minimum wage shocks. Micheli (2019) employs vector autoregression to disentangle endogenous from exogenous variation in real minimum wages. We find this to be an promising approach.

References

Aaronson, D. (2001). Price pass-through and the minimum wage. *The Review of Economics and Statistics 83*(1), 158–169.

[19]Reich (2009) argues that nominal minimum wages react to the stance of the economy. The implications for estimated coefficients in the literature are still controversial (Reich, 2009; Neumark et al., 2014b).

[20]This argument has e.g. been made by (Giuliano, 2013) for relative wages.

Aaronson, D., E. French, and J. MacDonald (2008). The minimum wage, restaurant prices, and labor market structure. *Journal of Human Resources 43*(3), 688–720.

Ahlfeldt, G. M., D. Roth, and T. Seidel (2018). The regional effects of Germany's national minimum wage. *Economics Letters 172*, 127 – 130.

Allegretto, S., A. Dube, M. Reich, and B. Zipperer (2017). Credible research designs for minimum wage studies: A response to Neumark, Salas, and Wascher. *ILR Review 70*(3), 559–592.

Allegretto, S. and M. Reich (2018). Are local minimum wages absorbed by price increases? Estimates from internet-based restaurant menus. *ILR Review 71*(1), 35–63.

Allegretto, S. A., A. Dube, and M. Reich (2011). Do minimum wages really reduce teen employment? Accounting for heterogeneity and selectivity in state panel data. *Industrial Relations: A Journal of Economy and Society 50*(2), 205–240.

Baker, M., D. Benjamin, and S. Stanger (1999). The highs and lows of the minimum wage effect: A time-series cross-section study of the canadian law. *Journal of Labor Economics 17*(2), 318–350.

Bernanke, B. S. and I. Mihov (1998, 08). Measuring monetary policy. *The Quarterly Journal of Economics 113*(3), 869–902.

Card, D. and A. B. Krueger (2015). *Myth and Measurement: The New Economics of the Minimum Wage-Twentieth-Anniversary Edition.* Princeton University Press.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer (2019, 05). The Effect of Minimum Wages on Low-Wage Jobs*. *The Quarterly Journal of Economics 134*(3), 1405–1454.

Cholette, P.-A. (1984). Adjusting sub-annual series to yearly benchmarks. *Survey Methodology 10*(1), 35–49.

Chow, G. C. and A. loh Lin (1971). Best linear unbiased interpolation, distribution, and extrapolation of time series by related series. *The Review of Economics and Statistics 53*(4), 372–375.

Clemens, J. and M. Wither (2019). The minimum wage and the great recession: Evidence of effects on the employment and income trajectories of low-skilled workers. *Journal of Public Economics 170*, 53 – 67.

Denton, F. T. (1971). Adjustment of monthly or quarterly series to annual totals: An approach based on quadratic minimization. *Journal of the American Statistical Association 66*(333), 99–102.

Dube, A., T. W. Lester, and M. Reich (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *The Review of Economics and Statistics 92*(4), 945–964.

Dube, A., T. W. Lester, and M. Reich (2016). Minimum wage shocks, employment flows, and labor market frictions. *Journal of Labor Economics 34*(3), 663–704.

Flinn, C. J. (2006). Minimum wage effects on labor market outcomes under search, matching, and endogenous contact rates. *Econometrica 74*(4), 1013–1062.

Flood, S., M. King, R. Rodgers, S. Ruggles, and J. R. Warren (2018). Integrated public use microdata series, current population survey: Version 6.0 [dataset]. Minneapolis, MN: IPUMS, 2018. https://doi.org/10.18128/D030.V6.0, retrieved 15.4.2019.

Giuliano, L. (2013). Minimum wage effects on employment, substitution, and the teenage labor supply: Evidence from personnel data. *Journal of Labor Economics 31*(1), 155–194.

Harasztosi, P. and A. Lindner (2019). Who pays for the minimum wage? *American Economic Review 109*(8), 2693–2727.

Meer, J. and J. West (2016). Effects of the minimum wage on employment dynamics. *Journal of Human Resources 51*(2), 500–522.

Micheli, M. (2019). Labor market effects of minimum wage shocks. Ruhr Economic Papers 830, RWI, Essen.

Monras, J. (2019). Minimum wages and spatial equilibrium: Theory and evidence. *Journal of Labor Economics 37*(3), 853–904.

Neumark, D. (2019a). The econometrics and economics of the employment effects of minimum wages: Getting from known unknowns to known knowns. *German Economic Review 20*(3), 293–329.

Neumark, D. (2019b). Minimum wage/living wage data set. https://www.socsci.uci.edu/~dneumark/datasets.html, last checked: May 15th, 2019.

Neumark, D., J. I. Salas, and W. Wascher (2014a). More on recent evidence on the effects of minimum wages in the united states. *IZA Journal of Labor Policy 3*(1), 24.

Neumark, D., J. M. I. Salas, and W. Wascher (2014b). Revisiting the minimum wage-employment debate: Throwing out the baby with the bathwater? *ILR Review 67*(3_suppl), 608–648.

Neumark, D. and W. Wascher (1992). Employment effects of minimum and subminimum wages: Panel data on state minimum wage laws. *ILR Review 46*(1), 55–81.

Neumark, D. and W. Wascher (2004). Minimum wages, labor market institutions, and youth employment: A cross-national analysis. *ILR Review 57*(2), 223–248.

Neumark, D. and W. Wascher (2017). Reply to ”Credible research designs for minimum wage studies”. *ILR Review 70*(3), 593–609.

Neumark, D. and W. L. Wascher (2007). Minimum wages and employment. *Foundations and Trends® in Microeconomics 3*(1–2), 1–182.

Reich, M. (2009). Minimum wages in the United States: Politics, economics, and econometrics. In C. Brown, B. J. Eichengreen, and M. Reich (Eds.), *Labor in the Era of Globalization*, Chapter 11, pp. 353–74. Cambridge University Press.

Sax, C. and P. Steiner (2013). Temporal disaggregation of time series. *The R Journal 5*(2), 80–87.

Sorkin, I. (2015). Are there long-run effects of the minimum wage? *Review of Economic Dynamics 18*(2), 306 – 333.

Sturn, S. (2018). Do minimum wages lead to job losses? Evidence from OECD countries on low-skilled and youth employment. *ILR Review 71*(3), 647–675.

vom Berge, P. and H. Frings (2019). High-impact minimum wages and heterogeneous regions. *Empirical Economics*.

Zhang, W. (2018). Distributional effects of local minimum wage hikes: A spatial job search approach. Cambridge Working Papers in Economics 1889, University of Cambridge, Faculty of Economics.

Table 1: Descriptive statistics, sample period 1991-2017

	Mean	Std dev	Observations
Teen employment share	0.3924	0.1258	16524
Last week's working hours of teenagers	24.0761	3.9325	16524
log(Last week's working hours of teenagers)	3.1679	0.1640	16524
Real wage of teenagers[a]	8.5539	1.4183	16456
log(Real wage of teenagers)[a]	2.1346	0.1496	16456
Minimum wage	6.0085	1.4588	16524
log(Minimum wage)	1.7646	0.2377	16524
Real minimum wage[a]	7.0261	0.8335	16524
log(Real minimum wage)[a]	1.9428	0.1161	16524
Unemployment Rate (26-59)	0.0405	0.0213	16524
Real seasonally adjusted house price index[a]	202.8989	47.4834	16524
log(Real seasonally adjusted house price index)[a]	5.2880	0.2185	16524
Usual working hours of teenagers	24.8585	4.0334	14688
log(Usual working hours of teenagers)	3.1999	0.1640	14688
Minimum wage/Hourly Wage	0.8334	0.1184	16456
Real wage of teenagers, frequency conversion by			
Constant	8.5539	1.4198	16524
Quadratic	8.5538	1.4180	16524
Denton-Cholette	8.5540	1.4183	16524
Chow-Lin	8.5540	1.4182	16524
log(Real wage of teenagers, frequency conversion by)			
Constant	2.1346	0.1498	16524
Quadratic	2.1346	0.1495	16524
Denton-Cholette	2.1346	0.1496	16524
Chow-Lin	2.1346	0.1496	16524
log(Real minimum wage, frequency conversion by)			
Constant	1.9428	0.1162	16524
Quadratic	1.9428	0.1161	16524
Denton-Cholette	1.9428	0.1162	16524
Chow-Lin	1.9428	0.1162	16524
log(Real seasonally adjusted house price index, frequency conversion by)			
Constant	5.2879	0.2186	16524
Quadratic	5.2880	0.2184	16524
Denton-Cholette	5.2880	0.2186	16524
Chow-Lin	5.2881	0.2184	16524

The base year for GDP deflators is 2012 (2012=100). The base period for house prices is the first quarter of 1991 (1991Q1=100). [a] Prices are converted to the monthly frequency using the Denton-Cholette method with a constant.

Table 2: Minimum wages and labor market outcomes

	(1)	(2)	(3)	(4)
A. Dependent variable: Teen employment share				
log(Nominal MW)	-0.0584***	-0.0331**	-0.0496*	-0.0218
	(0.0188)	(0.0160)	(0.0252)	(0.0207)
log(Real MW)	-0.1195***	-0.0754***	-0.1352***	-0.0774***
	(0.0196)	(0.0181)	(0.0282)	(0.0263)
Sample Period	1991 - 2017			
Observations	16524			
B. Dependent variable: Last week's working hours of teenagers				
log(Nominal MW)	-0.0867**	-0.0521	-0.1092**	-0.1011*
	(0.0338)	(0.0420)	(0.0431)	(0.0522)
log(Real MW)	-0.1679***	-0.0903**	-0.1931***	-0.1279***
	(0.0353)	(0.0398)	(0.0422)	(0.0459)
Sample Period	1991 - 2017			
Observations	16524			
C. Dependent variable: Real wage of teenagers				
log(Nominal MW)	0.1318***	0.1356***	0.1971***	0.1897***
	(0.0419)	(0.0293)	(0.0706)	(0.0472)
log(Real MW)	0.3413***	0.2182***	0.4435***	0.2789***
	(0.0500)	(0.0332)	(0.0628)	(0.0519)
Sample Period	1991 - 2017			
Observations	16456			
State specific month effects	Yes	Yes	Yes	Yes
Time effects	Yes	Yes		
State trends		Yes		Yes
Division-specific time effects			Yes	Yes

Robust standard errors are clustered by state and reported in parentheses. Each specification controls for the unemployment rate of individuals between 26 and 59 and real house prices. Quarterly seasonally adjusted purchase-only house price indexes and annual GDP deflators are converted to the monthly frequency employing the Denton-Cholette method without an indicator series. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Different measures for real minimum wages and labor market outcomes

	(1)	(2)	(3)	(4)
A. Dependent variable: Teen employment share				
log(Real MW)	-0.1190***	-0.0744***	-0.1348***	-0.0763***
(Constant)	(0.0196)	(0.0180)	(0.0283)	(0.0264)
log(Real MW)	-0.1197***	-0.0757***	-0.1354***	-0.0776***
(Quadratic)	(0.0196)	(0.0182)	(0.0283)	(0.0264)
log(Real MW)	-0.1196***	-0.0755***	-0.1353***	-0.0773***
(Denton-Cholette)	(0.0195)	(0.0181)	(0.0281)	(0.0263)
log(Real MW)	-0.1196***	-0.0756***	-0.1354***	-0.0776***
(Chow-Lin)	(0.0196)	(0.0181)	(0.0282)	(0.0263)
Sample Period	1991 - 2017			
Observations	16524			
B. Dependent variable: Last week's working hours of teenagers				
log(Real MW)	-0.1677***	-0.0894**	-0.1919***	-0.1252***
(Constant)	(0.0352)	(0.0396)	(0.0422)	(0.0459)
log(Real MW)	-0.1680***	-0.0902**	-0.1930***	-0.1275***
(Quadratic)	(0.0353)	(0.0398)	(0.0422)	(0.0460)
log(Real MW)	-0.1680***	-0.0902**	-0.1934***	-0.1281***
(Denton-Cholette)	(0.0353)	(0.0397)	(0.0421)	(0.0459)
log(Real MW)	-0.1680***	-0.0902**	-0.1934***	-0.1281***
(Chow-Lin)	(0.0353)	(0.0397)	(0.0421)	(0.0459)
Sample Period	1991 - 2017			
Observations	16524			
C. Dependent variable: Real wage of teenagers				
log(Real MW)	0.3398***	0.2154***	0.4435***	0.2778***
(Constant)	(0.0498)	(0.0329)	(0.0631)	(0.0523)
log(Real MW)	0.3404***	0.2166***	0.4432***	0.2778***
(Quadratic)	(0.0499)	(0.0329)	(0.0629)	(0.0520)
log(Real MW)	0.3414***	0.2187***	0.4432***	0.2787***
(Denton-Cholette)	(0.0500)	(0.0329)	(0.0627)	(0.0517)
log(Real MW)	0.3417***	0.2193***	0.4435***	0.2792***
(Chow-Lin)	(0.0500)	(0.0330)	(0.0629)	(0.0519)
Sample Period	1991 - 2017			
Observations	16456			
State specific month effects	Yes	Yes	Yes	Yes
Time effects	Yes	Yes		
State trends		Yes		Yes
Division-specific time effects			Yes	Yes

Robust standard errors are clustered by state and reported in parentheses. Each specification controls for the unemployment rate of individuals between 26 and 59 and real house prices. Quarterly seasonally adjusted purchase-only house price indexes and annual GDP deflators are converted to the monthly frequency as indicated. When employing the Denton-Cholette and Chow-Lin method, we use seasonally adjusted monthly consumer prices on the regional level as indicator series for GDP deflators. For the frequency conversion of house prices, we use house transaction based house price indexes on the census division level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Minimum wages and labor market outcomes in non-indexed states

	(1)	(2)	(3)	(4)
A. Dependent variable: Teen employment share				
log(Nominal MW)	-0.0477**	-0.0268	-0.0347	-0.0137
	(0.0208)	(0.0176)	(0.0228)	(0.0207)
log(Real MW)	-0.1127***	-0.0727***	-0.1181***	-0.0656***
	(0.0195)	(0.0179)	(0.0275)	(0.0217)
Sample Period	1991 - 2017			
Observations	15000			
B. Dependent variable: Last week's working hours of teenagers				
log(Nominal MW)	-0.0903**	-0.0712	-0.0822	-0.1073*
	(0.0362)	(0.0467)	(0.0513)	(0.0573)
log(Real MW)	-0.1600***	-0.1121**	-0.1488***	-0.1360**
	(0.0355)	(0.0435)	(0.0457)	(0.0527)
Sample Period	1991 - 2017			
Observations	1500			
C. Dependent variable: Real wage of teenagers				
log(Nominal MW)	0.1292***	0.1245***	0.2035**	0.1675***
	(0.0456)	(0.0310)	(0.0790)	(0.0532)
log(Real MW)	0.3461***	0.2145***	0.4419***	0.2510***
	(0.0547)	(0.0322)	(0.0669)	(0.0524)
Sample Period	1991 - 2017			
Observations	14935			
State specific month effects	Yes	Yes	Yes	Yes
Time effects	Yes	Yes		
State trends		Yes		Yes
Division-specific time effects			Yes	Yes

Robust standard errors are clustered by state and reported in parentheses. Each specification controls for the unemployment rate of individuals between 26 and 59 and real house prices. Quarterly seasonally adjusted purchase-only house price indexes and annual GDP deflators are converted to the monthly frequency employing the Denton-Cholette method without an indicator series. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

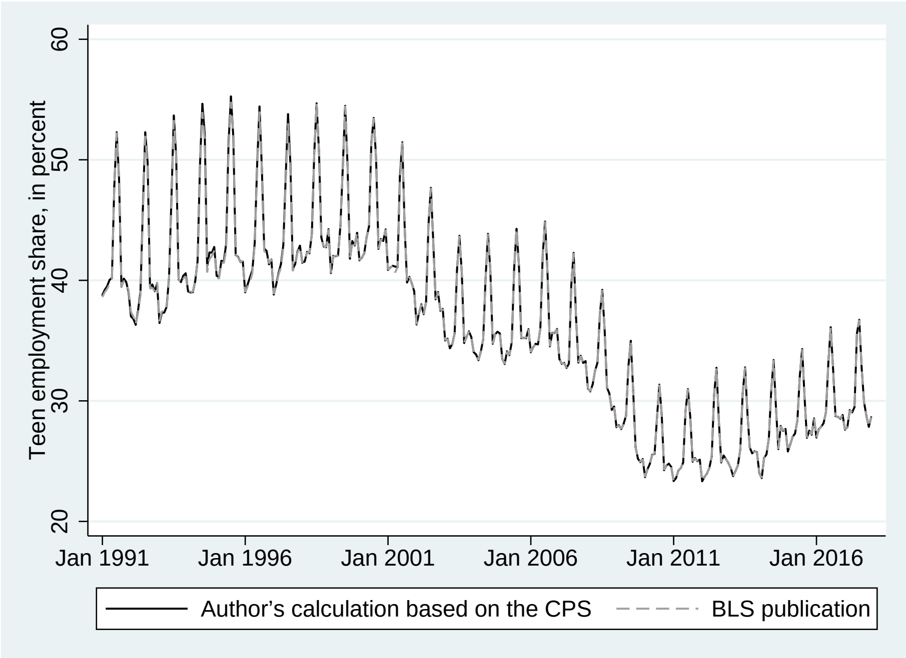
Table 5: Relative minimum wages and labor market outcomes

	(1)	(2)	(3)	(4)
A. Dependent variable: Teen employment share				
Minimum wage/Hourly Wage	-0.0249***	-0.0151**	-0.0242***	-0.0140**
	(0.0080)	(0.0057)	(0.0085)	(0.0062)
Sample Period	1991 - 2017			
Observations	16456			
B. Dependent variable: Last week's working hours of teenagers				
Minwage/Hourly Wage	-0.1184***	-0.1048***	-0.1248***	-0.1147***
	(0.0126)	(0.0127)	(0.0134)	(0.0117)
Sample Period	1991 - 2017			
Observations	16456			
State specific month effects	Yes	Yes	Yes	Yes
Time effects	Yes	Yes		
State trends		Yes		Yes
Division-specific time effects			Yes	Yes

Robust standard errors are clustered by state and reported in parentheses. Each specification controls for the unemployment rate of individuals between 26 and 59 and real house prices. Quarterly seasonally adjusted purchase-only house price indexes and annual GDP deflators are converted to the monthly frequency employing the Denton-Cholette method without an indicator series. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

Figure A.1: Comparison of the teen employment share



Source: Flood et al. (2018), BLS, author's calculation.

Table A.1: Minimum wages and labor market outcomes

	(1)	(2)	(3)	(4)
Dependent variable: Usual working hours of teenagers				
log(Nominal MW)	-0.0719*	-0.0297	-0.1021**	-0.0692
	(0.0371)	(0.0384)	(0.0449)	(0.0487)
log(Real MW)	-0.1551***	-0.0736*	-0.1932***	-0.1061**
	(0.0414)	(0.0380)	(0.0470)	(0.0461)
Sample Period	1994 - 2017			
Observations	14688			
State specific month effects	Yes	Yes	Yes	Yes
Time effects	Yes	Yes		
State trends		Yes		Yes
Division-specific time effects			Yes	Yes

Robust standard errors are clustered by state and reported in parentheses. Each specification controls for the unemployment rate of individuals between 26 and 59 and real house prices. Quarterly seasonally adjusted purchase-only house price indexes and annual GDP deflators are converted to the monthly frequency employing the Denton-Cholette method without an indicator series. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Minimum wages and labor market outcomes, common seasonality

	(1)	(2)	(3)	(4)
A. Dependent variable: Teen employment share				
log(Nominal MW)	-0.0594***	-0.0359**	-0.0478*	-0.0217
	(0.0184)	(0.0155)	(0.0247)	(0.0196)
log(Real MW)	-0.1187***	-0.0758***	-0.1320***	-0.0743***
	(0.0187)	(0.0170)	(0.0272)	(0.0248)
Sample Period	1991 - 2017			
Observations	16524			
B. Dependent variable: Last week's working hours of teenagers				
log(Nominal MW)	-0.0878***	-0.0561	-0.1054**	-0.0997*
	(0.0324)	(0.0396)	(0.0412)	(0.0497)
log(Real MW)	-0.1649***	-0.0896**	-0.1859***	-0.1214***
	(0.0339)	(0.0374)	(0.0404)	(0.0440)
Sample Period	1991 - 2017			
Observations	16524			
C. Dependent variable: Real wage of teenagers				
log(Nominal MW)	0.1324***	0.1364***	0.1977***	0.1907***
	(0.0408)	(0.0283)	(0.0695)	(0.0460)
log(Real MW)	0.3417***	0.2193***	0.4442***	0.2802***
	(0.0487)	(0.0322)	(0.0616)	(0.0510)
Sample Period	1991 - 2017			
Observations	16456			
State specific effects	Yes	Yes	Yes	Yes
Time effects	Yes	Yes		
State trends		Yes		Yes
Division-specific time effects			Yes	Yes

Robust standard errors are clustered by state and reported in parentheses. Each specification controls for the unemployment rate of individuals between 26 and 59 and real house prices. Quarterly seasonally adjusted purchase-only house price indexes and annual GDP deflators are converted to the monthly frequency employing the Denton-Cholette method without an indicator series. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Minimum wages and labor market outcomes, quarterly regression

	(1)	(2)	(3)	(4)
A. Dependent variable: Teen employment share				
log(Nominal MW)	-0.0548***	-0.0309*	-0.0431	-0.0178
	(0.0190)	(0.0159)	(0.0260)	(0.0199)
log(Real MW)	-0.1135***	-0.0695***	-0.1270***	-0.0703***
	(0.0190)	(0.0174)	(0.0279)	(0.0253)
Sample Period	1991 - 2017			
Observations	5508			
B. Dependent variable: Last week's working hours of teenagers				
log(Nominal MW)	-0.0830**	-0.0517	-0.1035**	-0.1002*
	(0.0324)	(0.0410)	(0.0415)	(0.0507)
log(Real MW)	-0.1623***	-0.0866**	-0.1846***	-0.1221***
	(0.0343)	(0.0384)	(0.0408)	(0.0447)
Sample Period	1991 - 2017			
Observations	5508			
C. Dependent variable: Real wage of teenagers				
log(Nominal MW)	0.1242***	0.1301***	0.1919**	0.1892***
	(0.0416)	(0.0281)	(0.0719)	(0.0454)
log(Real MW)	0.3399***	0.2119***	0.4460***	0.2758***
	(0.0480)	(0.0333)	(0.0608)	(0.0520)
Sample Period	1991 - 2017			
Observations	5508			
State specific quarter effects	Yes	Yes	Yes	Yes
Time effects	Yes	Yes		
State trends		Yes		Yes
Division-specific time effects			Yes	Yes

Robust standard errors are clustered by state and reported in parentheses. Each specification controls for the unemployment rate of individuals between 26 and 59 and real house prices. Annual GDP deflators are converted to the quarterly frequency employing the Denton-Cholette method without an indicator series. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.