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The effect of minimum wages on employment in the US is highly disputed. We show that the differences in the literature can be explained by heterogeneous shifts in political ideology across states. We add a control for political ideology to US county-level panel studies based on 1990-2013 employment and earnings data from the Quarterly Census of Employment and Wages (QCEW). The results suggest minimum wages increase earnings in affected sectors without reducing employment. Unlike existing estimates, these new results remain consistent across specifications, even when we instrument the political ideology variable using evolving cultural values or campaign contributions. The methodology used can be extended to analyses of other state level policies.

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The minimum wage and its effects is one of the most researched topics in labour economics. Yet, even after a century of research there is no (sustained) consensus on its

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desirability.[1] It can be a powerful tool against poverty, especially for the working poor.[2] At the same time, introducing a wage floor can lead to a decrease in the demand for labour, as certain jobs might no longer be profitable to employers, reducing employment rather than poverty.

Academic research into the employment effects of minimum wages goes back many decades and can generally be split into two sets: one that finds negative employment effects and a second that does not. The first is most commonly associated with the year and state fixed effects model of Neumark and Wascher (1992) and finds economically and statistically significant disemployment effects in the US, with a 10% increase in the minimum wage leading to a 1-3% drop in employment. The difference with the no-effect set is largely a matter of specification choices. E.g. whether one should add a first or second order time trend to this twoway fixed effects model (Allegretto, Dube and Reich (2010) vs. Neumark, Salas and Wascher (2014*b*)), or whether one can compare all counties across state borders or only those that are in the same Bureau of Economic Analysis 'economic area' (Dube, Lester and Reich (2010) vs. Liu, Hyclak and Regmi (2016)).

In this paper, we show that the inconsistencies in the literature can be reconciled if we accept that minimum wage hikes are not an exogenous shock, but rather the result of political evolutions that also affect restaurant employment in ways unrelated to the minimum wage.[3] More specifically, once a measure of each state legislature's political orientation is included, the specifications proposed by both sides of the debate all point towards a lack of disemployment effects, even the twoway fixed effects model underpinning Neumark and Wascher (1992). This results holds when we instrument the political ideology variable with shifting cultural values.

We argue that those papers that find strong disemployment effects are not picking up a causal link from minimum wages to employment, but rather that states which become more

[1] In the 19th century, Macrosty (1898) describes two unions fighting for sector-specific minimum wages: the London dock workers who demand a wage which "would secure decency, if not comfort" and forward-looking miners who want their wages tied to the evolution of coal prices.

[2] In 2013, 7% of workers in the US were classified as poor despite working at least 27 weeks per year.

[3] Minimum wage research has focused on the restaurant sector as it is the only one to employ a meaningful amount of workers who could plausibly be affected by the minimum wage.

conservative tend to have lower minimum wages and higher employment rates.[4] Indeed, we find that the political measure (increasing in conservativeness) is positively correlated with restaurant employment and reduces the probability of legislatures passing a minimum wage law. We also provide arguments which suggest the statistical methods from the null effect side of the debate were already implicitly filtering out the heterogeneous shifts in political context. For example, regional clustering in the changes in political sentiment could explain why census division-specific time dummies have a similar effect to the control variable we propose.

The lack of disemployment effects combined with existing evidence that minimum wages raise wages (e.g. Allegretto, Dube and Reich (2010)) and particularly so at the bottom of the wage distribution (Autor, Manning and Smith, 2016) suggest that minimum wage policy has the capacity to reduce working poverty without detrimental effects to the labour market.

The remainder of this paper is structured as follows. Section I describes the existing inconsistencies in the minimum wage literature in more detail. Section II describes the data and institutional framework. In Section III we show why political ideology could be an important omitted variable. Then, in Section IV we show how the estimates in the literature change once one adds the political ideology control and how this explains existing conflicts. Section V provides robustness checks. Finally, Section VI concludes.

I. Related Literature

Academic interest in the minimum wage goes back at least to the 19th century, when Macrosty (1898) talks about the London dock workers' union fighting for a minimum wage "which would secure decency, if not comfort" and miners demanding their wages be tied to the evolution of coal prices. The recent conflict in the literature essentially started in the 1980's, when evidence came mainly from time series variation (Neumark and Wascher,

[4]Note for example that according to the CNBC's Top States for Business ranking, conservative states are rated more business friendly and have a lower cost of doing business. At the same time, the animosity of the Republican party towards minimum wages is no secret, whereas the Democrats added a commitment to introducing a $15 an hour minimum wage in their party platform in 2016.

1992). Brown, Gilroy and Kohen (1982) provides an excellent overview, noting that most studies found employment elasticities between -0.1 and -0.3 for teenagers.[5] Similar results were found in low-wage service and manufacturing sectors, though measured with less precision.[6]

The focus shifted to panel studies as more detailed data became available and states started topping the federal minimum wage. Neumark and Wascher (1992) exploit this variation in minimum wage regimes across states to corroborate the time series results, finding elasticities in the same -0.1 to -0.3 consensus band. A first dissenting observation comes from Card (1992), who finds that the hike in the federal minimum wage in 1990 did not lead to a decline in teenage employment rates and that this holds both in high (potentially up to 50% of teenagers affected) and low (5%) impact states.

The intuitive claim that minimum wages lead to job losses was truly challenged after the (now famous) case study by Card and Krueger (1994) of fast food restaurants in the New Jersey and Pennsylvania states. Comparing employment levels in the two states before and after a serious increase in the New Jersey minimum wage, they find no evidence of any adverse effects of the hike in New Jersey.

Many papers have debated these findings and the tide of case studies following in its wake.[7] One issue common to all these case studies is the inherent conflict between choosing a control group that is similar and one that is independent of the treatment group. Proximity might lead to more similarity[8], but at the same time increases the risk that both groups share a labour or product market.[9]

Dube, Lester and Reich (2010) generalise the case study approach by creating a panel of all county pairs crossing state borders and comparing employment evolutions across these

[5] Increasing the minimum wage by 10% would then decrease teenage employment rates by 1-3%.

[6] A broader summary can be found in Brown (1999), which also discusses the theoretical models commonly associated with minimum wage research.

[7] E.g. Ropponen (2011) find that the effects vary strongly with the size of the restaurant, Neumark and Wascher (2000) argue the Card & Krueger survey contained serious measurement error and find negative employment effects when they use payroll data instead.

[8] Although Neumark, Salas and Wascher (2014*a*) and Allegretto et al. (2017), both using synthetic control methods, reach opposing conclusions on whether proximity leads to more appropriate control groups.

[9] E.g. Philadelphia, PA is only a few minutes drive from the New Jersey cities across the river. Additionally, if labour markets overlap strongly, this might amplify the ripple-on effect beyond the treated area (Manning and Petrongolo, 2011).

borders. They argue that this contiguous border county pair method (CBCP) is more robust to spatial heterogeneity than standard panel studies as they only compare counties to their (allegedly similar) neighbours, insulating the results from unobserved regional differences.[10] They find no evidence for disemployment effects.

The synthetic control group method (Abadie, Diamond and Hainmueller, 2010) is a second method to perform more robust case studies and exploits the pre-treatment period to reduce the odds of using a dissimilar counterfactual. Unlike matching, it does not select the most similar control group(s), but rather generates an articifial one by averaging over multiple states. Dube and Zipperer (2015) provide a generalisation of this approach to allow multiple cases to be evaluated together and apply it to the minimum wage puzzle. They find that while the wage and employment elasticities differ strongly across states, overall the effect on employment is indistinguishable from zero. The downside of this approach is that it depends on a long pre-treatment period to reliably calculate the weights. Any states where minimum wages change too frequently have to be discarded - the criteria used by the authors allow them to use just 29 of the 215 state minimum wage changes between 1979-2013 (15%).

Alternatively, there have also been efforts to make the traditional panel models more robust. A notable example is Allegretto, Dube and Reich (2010), who add either state-specific time trends or census division-specific time dummies in order to respectively filter out pre-existing trends or limit the identifying variation to a more local ('similar') level. This too leads to a lack of meaningful disemployment effects, though their findings are disputed by Neumark and Wascher (2017).[11] Totty (2015) takes this one step further by introducing multifactor models to the minimum wage debate, which theoretically encompass all the estimators described so far. In these models, there is a limited number of time varying factors that affects each panel unit differently. These factors are then filtered out

[10] I.e., they assume that although legislation is set at the state level, economic shocks vary continuously across borders (Allegretto et al., 2017).

[11] They note that the employment elasticity is still rather negative when only census division time dummies are included, that the trends-based results are sensitive both to the order of trends included and the timeframe analysed and that results including time trends are not necessarily more reliable. They argue it would be better to directly include the factors the trends are proxying for.

either by adding cross-sectional averages (CCE models) or by alternating between regular OLS and applying factor analysis on the residuals (IFE models).[12] He finds no negative employment effects.[13]

In Section IV, we will re-estimate a selection of these agnostic models and show how they are affected by the inclusion of a control for changes in the political landscape of the US.

II. The Data

A. *Employment*

We follow Dube, Lester and Reich (2010) in using data from the Quarterly Census of Employment and Wages (QCEW)[14], extending the sample to 2013. Its earnings and employment records are based on administrative records related to unemployment insurance and cover ∼97% of salaried workers in the United States.

In line with the literature, we focus on employment in the restaurant service sector[15] as it employs a disproportionate share of minimum wage workers, with 30% earning within 10% of the minimum wage in 2006 (DLR). Additionally, the sector is present in every county, although the data are at times not available for confidentiality reasons.[16] It covers both traditional (full service) restaurants as well as (limited service) fastfood and takeout joints. The sector provided employment to 10 million people in 2016, of which 30% were fastfood and counter workers (average hourly wage: $9.57), 20% were waiters ($11.57), 16% cooks ($11.28) and 7% supervisors ($16.66). Table A1 provides a more complete breakdown.

Our full sample runs from 1990-2013, representing 288 months of employment information.[17] We follow the literature in using a balanced panel, retaining the 1293 out of 3142 counties that have data for the full time period. These counties represented 88% of total

[12] See Section IV.

[13] In an earlier version of this paper, we took this another step further by making these models dynamic. The results remained qualitatively the same (Wursten, 2017).

[14] Use of this dataset in minimum wage research goes back to at least Card and Krueger (2000), when it was still known as the ES-202 program.

[15] NAICS 7221 + 7222, based on the 2002 classification.

[16] The Bureau of Labor Statistics (BLS) suppresses industry-county specific values if the industry is so small in that county the numbers might allow observations to be made on individual establishments.

[17] DLR use quarterly employment data. We find that the results are qualitatively similar regardless of the time unit chosen (results available on request).

non-farm employment in December 2013.[18]. This leaves us with 300 000 observations (out of more than half a million). The QCEW does not provide hours-worked information, only a headcount of filled positions. As a result, we are limited to discussing effects along the extensive margin. In our regressions, we will use total restaurant sector employment (as dependent variable) and total private sector employment (as control), both in logs.

B. *Minimum Wages*

Structured, regression-ready data on minimum wage levels was obtained from the personal website of Ian Salas (Neumark, Salas and Wascher, 2014*a*) and cross-checked with official data from the BLS and law (amendment) texts.[19] In total, we observe 426 minimum wage changes (counted at the state level). States are free to set the minimum wage at any level, as long as it equals or exceeds the federal minimum wage. Many have made use of this freedom: 238 of the observed changes are the result of decisions by the state legislatures, the remaining 188 are adjustments to stay in line with the federal minimum wage. Figure 1 shows the evolution of minimum wages over time.

The average increase of the mandated minimum wage was 9.5%, but this masks a wide variation in the size of the changes as depicted in Figure A1. About a quarter were smaller than 5%, often reflecting cost of living adjustments (in 2017, 18 states have their minimum wage pegged to some price index, usually the CPI). A handful of adjustments exceed 20%. Michigan spans the crown with their October 2006 increase of 35%. Colorado is the only state to have ever reduced the minimum wage (by 0.5% in 2010, following a decline of the consumer price index the year before). The minimum wage enters the regressions in logs.[20]

C. *Political measure*

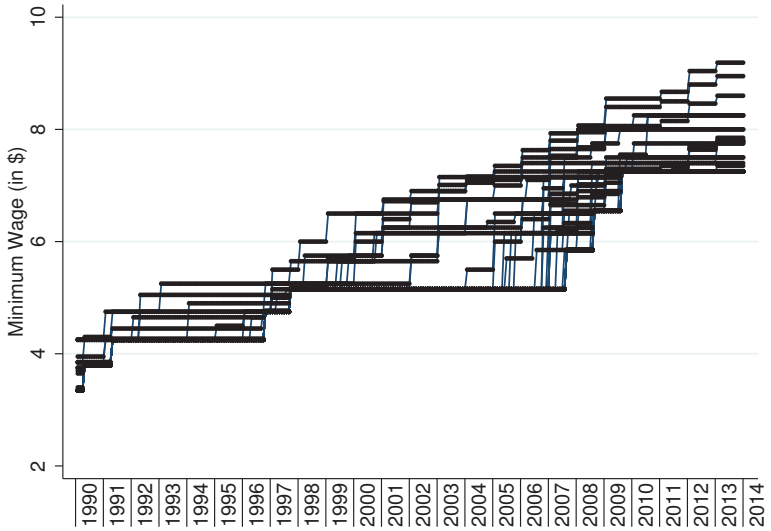
We use the political measure established in Shor and McCarty (2011) to quantify the political situation in each state. It tries to determine each legislator's 'ideal point', which

[18] Own calculations based on BLS data

[19] The original file can be found at https://sites.google.com/site/jmisalas/data-and-code.

[20] This log-log setup implies its coefficient can be interpreted as an elasticity (percentage change in X corresponds to percentage change in Y).

Figure 1. Evolution of minimum wages per state.



is their location on a liberal-conservative axis. This ideal point is construed based on both roll-call data (who votes on what) and the NPAT survey on political beliefs.[21] The first essentially characterises each legislator within its legislative body (e.g. Alabama House), but does not allow for consistent comparison across states. The survey on the other hand poses the same questions across states and years, but has only a limited response rate. Shor and McCarty (2011) show that the survey answers of the respondents can be used to rescale the roll-call based ideal points into the common NPAT space (which is consistent over states and years).[22]

The median of these individual ideal points per state chamber is then used as an indicator of the prevailing political sentiment and can be compared over time and space. Note that for many states this measure only changes every two years (the election cycle in most

[21] The National Political Awareness Test (NPAT) is administered by Project Vote Smart and poses a wide range of political questions to legislators.

[22] In other words, you know from the roll call data how conservative everyone is within his chamber. From the survey, you know for some how conservative they are relative to respondents in the rest of the country (and across years). You can then translate everyone's ideal point to this common scale (the NPAT space) using the survey respondents as indicator for how 'conservative within New York House' corresponds to 'conservative within the US'.

states). The bottom part of Figure 4 shows the evolution of this measure for the state lower chambers ('the House'). Positive (negative) values indicate a gradual shift towards more conservative (liberal) notions. There is great heterogeneity across the US, e.g. the south has become considerably more conservative, whereas the west and north-east have shifted left. The changes can be considerable, e.g. Georgia was mildly liberal in 1996, but turned into one of the most conservative states by 2012.[23] More generally, the Kendall rank correlation between a state house's median ideal point in 1996 and 2000 is only 0.59, gradually dropping to 0.31 by 2012. In the regressions, we will use the median ideology of the state's *lower* chamber, as it tends to have the most legislative power, especially when it comes to economic decision making.[24] Unfortunately, there are some gaps in the availability of this measure, which reduces our sample to 221 340 observations.

III. Omitted Variable Hypothesis

There are essentially four conditions that need to be met for political ideology to be an important omitted variable. First, political ideology should affect the level of the minimum wage. There is some literature that shows this (e.g. Leigh, 2008; Baskaya and Rubinstein, 2012), but surprisingly little research exists that looks into the determinants of the minimum wage. To fill this gap, we trawled through two decades of state legislation to determine which legislature passed which minimum wage legislation and under which political conditions.

$$MW\_law\_passed_{st} = \delta * POL_{st}^{0to1} + \gamma_1 * e_{st}^{TOT} + \gamma_2 * pop_{st} + \alpha_s + \tau_t + \epsilon_{st} \quad (1)$$

Equation 1 summarises how we use that information to determine whether ideology matters in minimum wage policy. $MW\_law\_passed_{st}$ is a binary variable indicating whether state s passed a minimum wage law in year t. POL_{st}^{0to1} is the political ideology measure,

[23] Anecdotally, it might be interesting to note that Georgia never exceeded the federal minimum wage in our sample, whereas Colorado has done so since 2007.

[24] Results are nonetheless very similar when one uses the upper chamber ('the Senate') instead.

rescaled such that a value of one refers to the most conservative legislature in our sample (0: most liberal). We add total private employment (e_{st}^{TOT}) and population (pop_{st}) as controls. In order to filter out confounding state-level factors and changing nationwide effects we include state and year fixed effects (in line with the political science literature, e.g. Besley and Case, 2003; Leigh, 2008).

Results are shown in Table 1. In Column (1), we omit the control variables, including only the state and year fixed effects. The point estimate of -0.35 (significant at 5%) indicates that going from the most liberal to the most conservative legislature reduces the probability of introducing a minimum wage law by 35 percentage points.[25]

The estimated relation does not change if we add the total employment and population controls in Column 2 (-0.36, 5%). It is reassuring to see that those controls appear uncorrelated to the minimum wage. Finally, in Column (3) we interact the political ideology variable with a dummy that indicates whether the same parties control both chambers (state House and Senate). One would expect ideology in the House to matter less if the Senate is controlled by the opposing party. Indeed, we only see a significant effect under unified government (-0.42, 10%). When government is divided, the coefficient drops to -0.04 with a large standard error as well.

A second requirement is that liberal governments behave differently in ways that could affect restaurant employment. This is directly visible in the main regressions in Section IV and also commonly observed in the political science literature. For example, liberal governments spend more on education and family assistance (Beland and Oloomi, 2017; Hill and Jones, 2017; Besley and Case, 2003; Leigh, 2008; Soss et al., 2001), they raise more state revenues (Besley and Case, 2003; Reed, 2006; Warren, 2009; Alt and Lowry, 1994; Bjørnskov and Potrafke, 2013; Chang, Kim and hsiang Ying, 2009; Gu et al., 2017; Pickering and Rockey, 2013) and are associated with lower unemployment rates (Beland, 2015; Chang, Kim and hsiang Ying, 2009; Leigh, 2008).

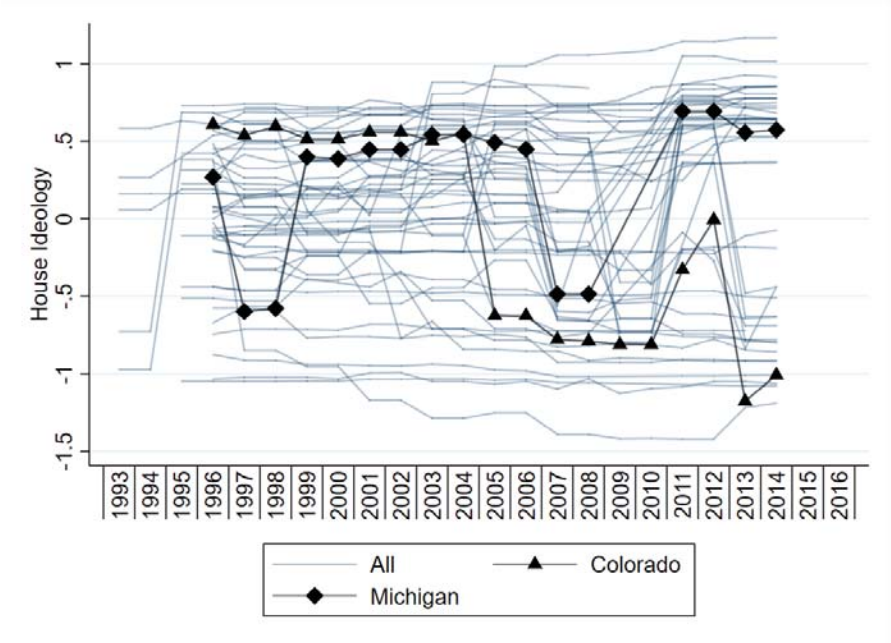
[25] In terms of predicted probabilities, this results in a 32% chance of introducing minimum wage legislation in the most liberal legislature versus -3% in the most conservative one. We consider the negative predicted probability close enough to zero to stick to a linear model (and thus avoid the complications that arise from including fixed effects in probit/logit models).

Table 1. Impact of Politics on Minimum Wage Laws

	(1)	(2)	(3)
Rescaled Political Ideology	-0.35**	-0.36**	
	(0.17)	(0.17)	
Unified government ×			-0.42*
Rescaled Political Ideology			(2.09)
Divided government ×			-0.04
Rescaled Political Ideology			(0.31)
Divided government			-0.31
			(0.22)
Total Private Sector		-0.27	-0.16
Employment		(0.53)	(0.52)
Population		0.06	0.01
		(0.49)	(0.49)
State FE	✓	✓	✓
Year FE	✓	✓	✓
N	564	564	560

Standard errors statistics in parentheses, errors are clustered at the state level. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 2. Median Political Ideology over Time

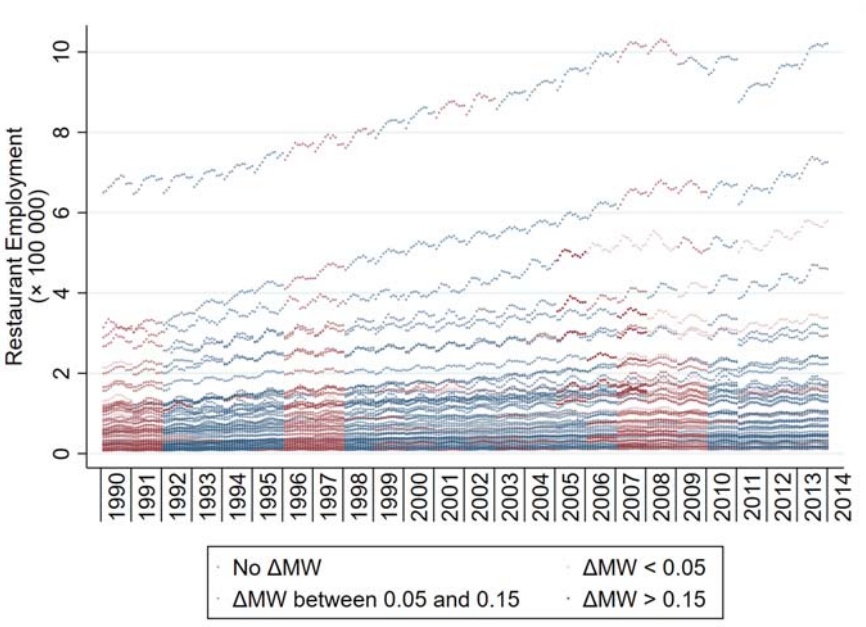


Third, the median ideology of state governments should change heterogeneously over time, else its effects could be filtered out by state and time fixed effects. This was partly covered in Section II and should become even clearer once we look at Figure 2, which shows our political variable for each state over time. There is considerable variation over the years and no overall trend. We have highlighted two states. First, Colorado (triangles), which went from being one of the most conservative states to one of the most liberal ones and Michigan (diamonds) which was mostly conservative early on, but had a short liberal stint in 2006/08 which is also when it started exceeding the federal MW. Then it turned back conservative and never raised its MW again. In that same liberal legislative session, they also increased the state income tax, invested $2bn in a jobs fund, cut government services and introduced a "No Worker Left Behind" act which provides 2 years of free training/community college to the unemployed.

Finally, many of the channels in our omitted variable hypothesis are indirect, with poten-

tially only small effects on the employment elasticity estimate. If the direct minimum wage effect on employment were expected to be large, we could possibly still ignore the political aspects. However, this is not the case, as even the more extreme estimates suggest only a drop of 1-2% in total restaurant employment after a 10% increase in the minimum wage (see Section I). In Figure 3 we show aggregated restaurant employment in our sample per state over time, with minimum wage change years highlighted in red to illustrate how fine a comb is required to detect the minimum wage effect. Note that most of these arguments apply to other state policies as well, suggesting our methodology would also be of use in other fields.

FIGURE 3. RESTAURANT EMPLOYMENT BY STATE, MINIMUM WAGE CHANGE YEARS HIGHLIGHTED



IV. Results

We start our analysis by re-estimating the most common methods in the literature and then add the political ideology variable to determine whether it is a meaningful omitted

variable in practice. Equation (2) shows the basic regression structure as inspired by Neumark and Wascher (1992) and DLR. e_{it}^R represents the log of restaurant employment in county i during month t. mw_{it} is the log minimum wage, β is then the employment elasticity we are interested in. The common controls e_{it}^{TOT} and pop_{it} refer to the logs of total private sector employment and population (at the sector level this time). The term between large brackets is the median ideology of the state lower chamber. Table 2 shows results both with and without this variable. Finally, $controls_{it}$ represents the particular statistical setup of the various papers as detailed in the set of Equations (3).

$$e_{it}^R = \beta * mw_{it} + \gamma_1 * e_{it}^{TOT} + \gamma_2 * pop_{it} + \text{controls}_{it}\Big[+ \delta * POL_{it}\Big] + \epsilon_{it} \tag{2}$$

$$\begin{aligned}
&\text{(1) 2FE} && \text{controls}_{it} = \alpha_i + \tau_t \\
&\text{(2) Trends} && \text{controls}_{it} = \alpha_i + \tau_t + \psi_s * t \\
&\text{(3) CBCP} && \text{controls}_{ipt} = \alpha_i + \tau_{pt} \\
&\text{(4) IFE} && \text{controls}_{it} = \alpha_i + \tau_t + \sum_{r=1}^{R} \lambda_{i,r} F_{t,r}
\end{aligned} \tag{3}$$

The simplest model listed is the twoway fixed effect model (2FE), which includes month dummies as well as county fixed effects and represents the side of the debate that finds negative employment effects. The remaining models then represent the null effect side. The trends model (Trends), as popularised by Allegretto, Dube and Reich (2010) adds state-specific linear time trends (ψ_s identifies the state). The contiguous border county pair model (CBCP), introduced by DLR adds separate time dummies per county pair that straddles a state border, with p identifying the pair.[26]

The interactive fixed effects model (IFE), linked to the minimum wage debate by Totty

[26] We surrendered some accuracy here for ease of exposition. The CBCP method involves a modified sample, where each county appears once per cross-border pair it belongs to. This way, we only retain 281 unique counties represented in 289 pairs. As a result, the error term should be e_{ipt} to indicate a particular county might be represented multiple times per time period if it borders multiple states. In the regressions, errors are clustered both on state and state border segment. See DLR for more details.

(2015) is perhaps the most elaborate statistical model included. Rather than explicitly specifying the structure of the controls (e.g. Equations 1-4), the IFE model uses factor analysis to filter out structural elements in the residual (that are potentially correlated with the explanatory variables). In the notation of Equation (5), $F_{t,r}$ is a time-varying factor, with corresponding county-specific factor loading $\lambda_{i,r}$. R is then a finite number indicating the number of factors present in the model. This combination of multiple products of i and t terms allows for very complex patterns to be captured, theoretically including all models mentioned earlier. The downside of this flexibility is that without further research it remains unclear what exactly is being filtered out, making it hard to evaluate the credibility of the results. Estimation happens in two stages. First, the model is estimated without any regard for the factors, leading to a set of coefficients and residuals. These residuals are then subjected to principal component analysis. The estimated factors are subtracted from the dependent variable. At this point, the estimator starts over, re-estimating the coefficients (but now with the dependent variable transformed). The residuals this produces are then again subjected to principal components analysis, etc. until the coefficients remain sufficiently stable.[27]

The results in the top panel (A) of Table 2 summarise the existing conflict in the literature. The traditional twoway fixed effects models (column 1) predicts economically[28] and statistically significant negative employment elasticities, suggesting that employment goes down by 1.1% after a 10% increase in the minimum wage, akin to roughly 0.1 million food service jobs lost nationwide.[29] By contrast, the richer models do not support the disemployment effect story. Simply adding state-specific trends already reduces the coefficient to zero (column 2). Likewise, the cross border county pair method (CBCP, column 3) and the interactive fixed effects model (IFE, column 4) both lead to a null effect.

Things change rather drastically once we include the political ideology measure in panel B. Now, none of the point estimates suggest economically or statistically significant dis-

[27] The IFE estimator is implemented in Stata as `regife` (Gomez, 2015).

[28] We consider results economically significant if they are in the initial -0.1 to -0.3 consensus band described in Section I.

[29] Based on 2013 employment levels in the restaurant sector (8 730 219).

Table 2. Employment elasticity of the minimum wage in the restaurant sector

		(1) 2FE	(2) Trends	(3) CBCP	(4) IFE
A	Employment elasticity	-0.112**	-0.016	-0.037	0.003
		(0.049)	(0.020)	(0.051)	(0.019)
	N	221 340	221 340	90 552	221 340
B	Employment elasticity	-0.049	-0.016	-0.043	0.001
		(0.033)	(0.020)	(0.052)	(0.019)
	Median political ideology of house	0.042***	0.001	-0.008	-0.005
		(0.011)	(0.004)	(0.011)	(0.004)
	N	221 340	221 340	90 552	221 340

Panel A shows results without controlling for political ideology. Panel B shows results with the political ideology variable included.
Standard errors in parentheses, errors are clustered at the state level. All regressions include total private sector employment and population controls, as well as monthly time dummies. Model (4) uses three factors, results are robust to using a different number of factors. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

employment effects, from the twoway fixed effects model (column 1) up to the interactive fixed effects model (column 4).[30] The 95% confidence interval of the employment elasticity in the 2FE model (column 2) equals [-0.11, 0.016], overlapping only ever so slightly with the -0.1 to -0.3 consensus band described in Section I. The political measure is highly significant in the twoway fixed effects model which initially predicted strong disemployment effects (column 1), providing empirical backing for the omitted variable hypothesis. Rather than showing a causal link from minimum wage to employment, these results indicate the traditional fixed effects estimates were picking up that states which have become more conservative tend to see more growth in restaurant employment but fewer increases in the minimum wage.

One concern is that our efforts to reduce omitted variable bias have exposed us to a problem of endogenous controls (Frölich, 2006). Indeed, the process determining the median ideology in a particular state is a complex one, determined by as many factors as there are constituents. Some of these may be related to restaurant employment, e.g. a cook losing his job might vote more left-wing as a result (reverse causality), but the links can also be more convoluted. Maybe states vote more right-wing and spend less money in restaurants as they become richer (omitted variable). There seem to be few ex ante reasons to assume that these effects are systematic. Even in the given examples, the cook might just as well vote more right-wing if he believes regulations are to blame. Likewise, states can also vote more right-wing as they become poorer (Huber, Stanig and Brancati, 2007).

Nevertheless, it can still be interesting to see what happens when we instrument the political ideology variable. As instruments we take cultural values which affect whether the Democrat or Republican candidate enters the state house, but which do not plausibly affect restaurant employment. We obtain state-year level polling data (with gaps) from the American National Election Studies (ANES) and use the combination of plausibly exogenous opinions that maximises the first stage predictive power.

[30]Results for the census division-specific time dummies model, the BEA economic area-specific time dummies model (Liu, Hyclak and Regmi, 2016), as well as dynamic versions of each of the models are available upon request - none of them suggest economically and statistically significant disemployment effects.

Table 3. Political Ideology Instruments

Instrument	N	Mean	SD	Min	Max
Women should always be able to get an abortion	614	0.29	0.16	0.00	0.69
Atheist	614	0.35	0.13	0.00	0.79
Federal crime fighting spending should be higher	614	0.64	0.12	0.20	1.00
Negative feelings towards the military	614	0.07	0.05	0.00	0.39
Negative feelings towards gays	614	0.35	0.14	0.07	0.80
We have gone too far pushing equal rights	614	0.41	0.12	0.11	0.90
Positive feelings towards big business	614	0.44	0.13	0.00	0.94
Gov't should provide only little support	614	0.48	0.12	0.07	1.00
Blacks have not been discriminated against	614	0.53	0.12	0.06	1.00
New lifestyles contribute to society's breakdown	614	0.64	0.10	0.32	0.95

Table 3 shows the set of instruments, which includes the share of atheists in the population, opinions on gays, the military, abortion, big business, the prevalence of racial discrimination and whether new lifestyles are hurting society. Figure A2 in the Appendix shows the evolution of the latter opinion for each state to illustrate the kind of variation present in the data.

Results are shown in Table 4. In Panel A we repeat the estimates from the literature for the sample that takes into account the gaps in the polling data. We still see the same pattern: a significant disemployment effect for the 2FE model (-0.12, column 1) and none for the others (columns 2-4). In Panel B we add our instrumented political ideology measure. As before, the richer models remain unaffected, producing essentially identical point estimates (columns 2, 3).[31] Most interesting is what happens to the 2FE estimate (0.04, column 1). Again, we see that any evidence for a significant disemployment effect of the minimum wage disappears. The 95% confidence interval is [-0.09, 0.18], which no longer encompasses the -0.1 to -0.3 band of consensus that used to exist in the literature. The SW F- and χ^2 tests reject respectively their nulls of weak and underidentification (Sanderson and Windmeijer, 2016). First stage results for the instrumented 2FE model are shown in column (1) of Table A2 in the Appendix.

[31] We do not produce IV estimates for the IFE model, as it is not yet clear how to consistently combine the two methods.

TABLE 4. EMPLOYMENT ELASTICITY OF THE MINIMUM WAGE IN THE RESTAURANT SECTOR - INSTRUMENTED RESULTS

		(1) 2FE	(2) Trends	(3) CBCP	(4) IFE
A	Employment elasticity	-0.118**	-0.022	-0.056	-0.000
		(0.046)	(0.018)	(0.053)	(0.020)
	N	187 092	187 092	67 488	187 092
B	Employment elasticity	0.044	-0.018	-0.056	.
		(0.067)	(0.032)	(0.058)	.
	Median political ideology of house	0.100***	0.005	-0.001	.
		(0.031)	(0.031)	(0.025)	.
	N	187 092	187 092	67 488	.
	SW F-test pvalue	0.001	0.064	0.000	.
	SW χ^2-test pvalue	0.000	0.031	0.000	IV not defined
	95% CI on MW	[-0.09, 0.18]	[-0.08, 0.04]	[-0.17, 0.06]	.

Panel A shows results without controlling for political ideology. Panel B shows results with the instrumented political ideology variable.
Standard errors in parentheses, errors are clustered at the state level. All regressions include total private sector employment and population controls, as well as monthly time dummies. Model (4) uses three factors, results are robust to using a different number of factors. The pvalues for the SW F- and χ^2 tests represent the Sanderson and Windmeijer (2016) tests for weak identification and underidentification respectively. The null is that the endogenous regressor is respectively weakly or underidentified. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The insignificance of the political measure in the models derived from the null effect camp suggests those methods were already implicitly filtering out the effects of the changing political conditions across the US. Take the trends model (2) as example. In Figure 4, we plot the estimated coefficients of the state-specific trends and underneath it those of the slope of the trend in political ideology over time (positive [darker] values indicate a shift towards conservative values). The two slope coefficients are moderately correlated at 0.35, suggesting that the shift in the political landscape is at least one of the major aspects filtered out by the trends model. Note that the clear regional clustering of slope coefficients could explain why the census division specific time dummies approach led to similar results.[32] In Figures A3 and A4 in the appendix we show that the same applies to the IFE model (4) as the first factor it estimates strongly represents a seasonally adjusted linear time trend.

V. Robustness Checks

In this section, we compare the performance of the various existing estimators to that of the politically augmented twoway fixed effects estimator, as well as its instrumented version.[33] As is common in the literature, we start by checking whether the models lead to plausible wage estimates ('find an effect when they should'). Then, we move to the finance sector to see if there we find a significant disemployment effect when we should not. Finally, we add data on campaign contributions to the set of instruments for the political ideology variable.

A. Restaurant Earnings

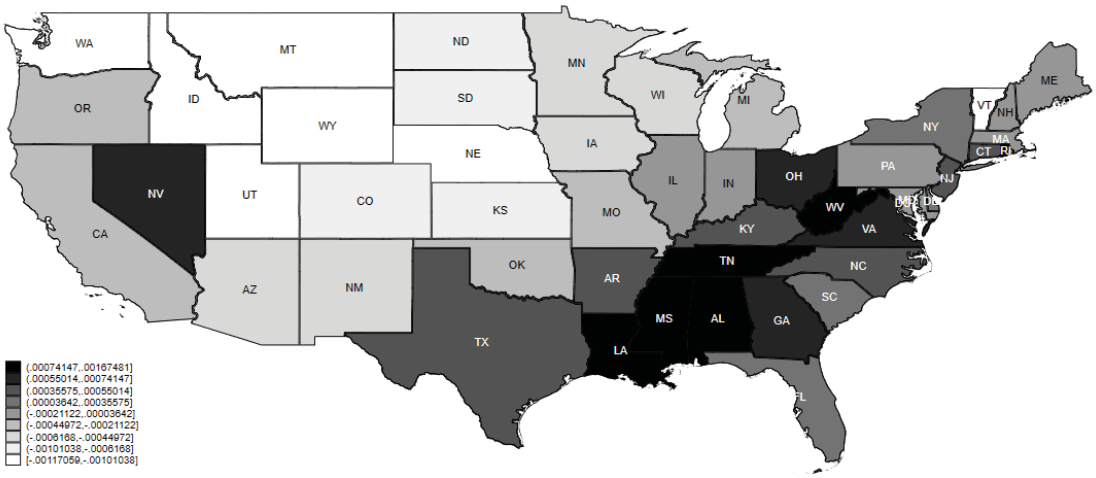
Panel A of Table 5 shows the estimated wage elasticities based on average weekly wages from the QCEW (see Section II).[34] All models lead to statistically significant positive

[32] A variance decomposition using multi-level modeling suggests 20% of variation in the political measure is explained by the census division. 52% is due to 'fixed' state differences and the remaining 28% is time variation.

[33] Results for the other methods augmented with the political measure, as well their dynamic versions are available on request.

[34] These include tips and other gratuities.

FIGURE 4. COEFFICIENTS OF THE STATE TIME TRENDS.



SLOPES OF THE TREND IN POLITICAL IDEOLOGY OVER TIME BY STATE.

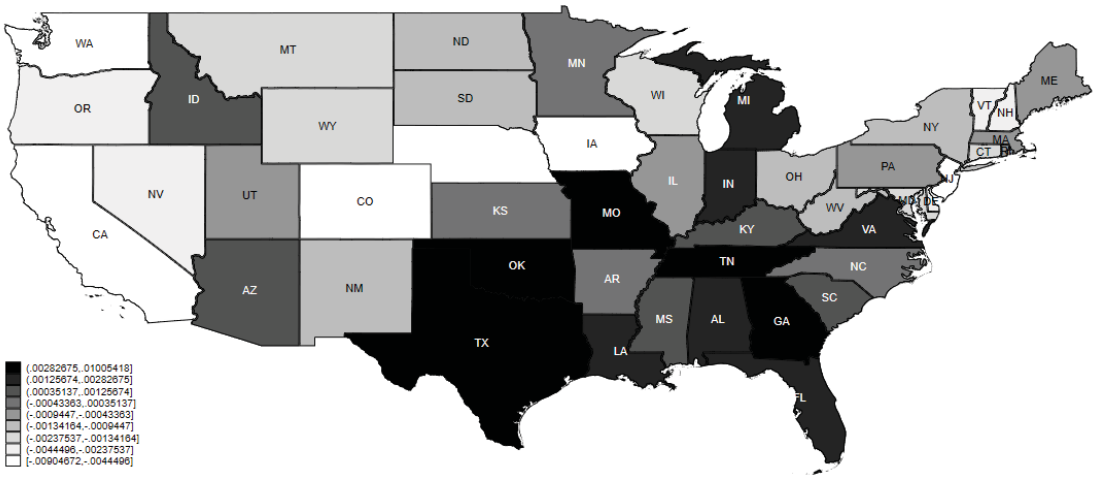


Table 5. Robustness checks (Earnings and Finance results)

		(1) 2FE	(2) Trends	(3) CBCP	(4) IFE	(5) POL2FE	(6) IVPOL2FE
A Earnings	Wage elasticity	0.231***	0.179***	0.140***	0.170***	0.222***	0.169***
		(0.028)	(0.025)	(0.030)	(0.017)	(0.022)	(0.038)
	Median political ideology of house					-0.005	-0.038***
						(0.005)	(0.012)
	SW F-test pvalue						0.001
	SW χ^2-test pvalue						0.000
	N	62 364	62 364	22 496	62 364	62 364	62 364
B Finance	Employment elasticity	-0.091*	0.035	-0.11	-0.015	-0.067	-0.019
		(0.046)	(0.035)	(0.09)	(0.028)	(0.043)	(0.051)
	Median political ideology of house					0.016	0.047**
						(0.012)	(0.018)
	SW F-test pvalue						0.000
	SW χ^2-test pvalue						0.000
	N	357 552	357 552	173 856	357 552	357 552	357 552

See notes of Table 4. The earnings results are based on quarterly data and thus include quarterly time dummies. First stage results for the POLIV2FE results are available in columns (2) and (3) of Table A2 in the Appendix. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

results, though there is noticeable variation in effect size, ranging from 0.140 (CBCP, 3) to 0.231 (2FE, 1). None of the models lead to implausibly small or insignificant elasticities, suggesting there is enough minimum wage variation left to identify its effects. If anything, the estimates seem somewhat on the high side: A back of the envelope calculation based on the Current Population Survey Merged Outgoing Rotation Groups data (CPS-MORG) suggests a wage elasticity of 0.10, assuming everyone below the new MW had their wage topped up and those above were unaffected (and no endogeneity).[35] Estimates from the politically augmented twoway fixed effects model (5) sit between the results from both sides of the debate, implying that a 10% increase in the minimum wage raises average weekly wages by 2.22%. Its instrumented version suggest a rather smaller raise (1.69%, column 6). Note that these positive *weekly* earnings effects suggest that any unobserved reductions in hours worked are bound to be limited in size.

B. Finance Employment

Next, we perform a placebo test by applying the various models to the finance sector (NAICS 52). Only 2-3% of employees in that industry earn up to 1.25 times the minimum wage (the corresponding number for the restaurant sector is 30%).[36] Panel B of Table 5 shows the relevant employment elasticities. The 2FE model predicts a remarkably similar disemployment effect (-0.09, column 1) in this plausibly unaffected sector as in the restaurant sector, significant below 10% (5.4% to be exact). The median wage of tellers, the lowest paid occupation in the finance sector, was $12.69 in 2015, almost twice the federal minimum wage and more than $2 higher than the highest state minimum wage at the time.[37] As a result, it would take some severe ripple-on effect for the minimum wage to affect employment (through wages) that far up the distribution. The CBCP model (column 3) also produces a negative point estimate, but it is very imprecisely estimated (the 95%

[35] The existence of wage spillovers above the wage floor is a subject of ongoing debate, see e.g. Grossman (1983), Autor, Manning and Smith (2016).

[36] If we compare current earnings to the upcoming minimum wage, we get respectively 4% and 40%. Calculations based on CPS-MORG data.

[37] Occupation level earnings data come from the Occupational Employment Statistics database.

CI is [-0.29,0.07]). The other models (Trends, IFE, POL2FE and IVPOL2FE) are more consistent in the sense that they never point to economically and statistically significant disemployment effects.

Table 6. Campaign Contribution based Political Ideology Instruments

Instrument	N	Mean	SD	Min	Max
Republican donations (count)	928	12673.38	17548.22	0	246094
Democrat donations (count)	928	14076.60	19516.20	17	272162
Party contribution (R, millions)	928	1.38	2.41	0	14.73
Party contribution (D, millions)	928	1.35	2.67	0	23.41
Total amount donated (R, millions)	928	15.20	23.74	0	248.22
Total amount donated (D, millions)	928	15.02	23.58	0	203.50
Individual vs PAC donations (share)	926	0.55	0.21	0.05	0.99

C. Campaign Contribution Instruments

Finally, we expand our set of instruments by adding campaign contribution data from the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica, 2015). The dataset provides the number of donations to each candidate in state elections as well as their total value. Donors are split between individuals, Political Action Committees (PAC) and party committees. Table 6 lists some summary statistics for the seven extra instruments we derive from DIME. These are the number of donations per party (per election cycle per state), contributions by the respective party committees, total amount of donations per party and the share of contributions derived from individual donations relative to PAC donations.

All of these are plausibly exogenous to restaurant employment levels and provide an alternative source of variation, which is also tied more directly to the election cycle. However, relative to the cultural values, they are a lot closer to the world of economics and there is thus more chance of some convoluted link between the contribution instruments and our dependent variable. For this reason, we excluded them in the main regressions but still evaluate the conclusions they entail.

TABLE 7. FIRST AND SECOND STAGE OF INSTRUMENTED 2FE REGRESSIONS - WITH CONTRIBUTION INSTRUMENTS

		(1) Ideology	(2) Ideology	(3) Ideology
1^{st} Stage	Abortion always allowed	0.110	0.112	0.037
		(0.284)	(0.286)	(0.339)
	Atheist	0.788***	0.795***	0.795***
		(0.285)	(0.285)	(0.272)
	Increase federal crime spending	0.371	0.374	0.290
		(0.345)	(0.345)	(0.332)
	Dislike military†	0.624	0.631	1.049*
		(0.579)	(0.580)	(0.604)
	Dislike gays†	-0.538	-0.543	-0.463
		(0.398)	(0.397)	(0.380)
	Equal Rights gone too far	0.002	0.004	0.181
		(0.455)	(0.454)	(0.448)
	Like Big Business†	0.440*	0.445*	0.458*
		(0.257)	(0.257)	(0.252)
	Govt should offer little support	0.585**	0.591**	0.550***
		(0.237)	(0.236)	(0.209)
	Blacks have not been discriminated	-0.183	-0.182	-0.012
		(0.320)	(0.321)	(0.308)
	New Lifestyles hurt society	0.588	0.591	0.646**
		0.306	0.309	0.381
	# of Republican donations (log)	0.102	0.102	0.120*
		(0.074)	(0.074)	(0.072)
	# of Democrat donations (log)	-0.175**	-0.175**	-0.174**
		(0.070)	(0.070)	(0.076)
	Total amount donated (R, log)	0.112**	0.112**	0.105**
		(0.056)	(0.056)	(0.052)
	Total amount donated (D, log)	-0.017	-0.017	-0.015
		(0.060)	(0.061)	(0.057)
	Republican Party contributions (log)	-0.011	-0.011	-0.019
		(0.030)	(0.030)	(0.029)
	Democrat Party contributions (log)	0.027**	0.027**	0.026*
		(0.012)	(0.012)	(0.015)
	Individual vs PAC donations (share)	-0.426*	-0.426*	-0.381*
		(0.223)	(0.224)	(0.219)
	Minimum Wage (log)	-0.811*	-0.819*	-0.764*
		(0.460)	(0.463)	(0.423)
	Population (log)	0.035**	0.037**	0.037**
		(0.016)	(0.015)	(0.017)
	Total Private Employment (log)	0.043		-0.014
		(0.129)		(0.144)
	Total Private Earnings (weekly, log)		0.150	
			(0.168)	
		Emp Rest	Earnings Rest	Emp Finance
2^{nd} Stage	Median political ideology of house	0.102***	-0.025***	0.037*
		(0.024)	(0.009)	(0.019)
	Minimum Wage (log)	0.056	0.165***	0.039
		(0.057)	(0.027)	(0.064)
	Population (log)	0.046***	0.006***	0.053***
		(0.006)	(0.002)	(0.005)
	Total Private Employment (log)	0.574***		0.379***
		(0.056)		(0.044)
	Total Private Earnings (weekly, log)		0.186***	
			(0.028)	
	N	171576	57192	324744
	SW F-test pvalue	0.000	0.000	0.000
	SW χ^2-test pvalue	0.000	0.000	0.000

See notes of Tables 4, 5 and A2.

Table 7 shows the first and second stage regression results for restaurant employment, restaurant weekly earnings and finance employment. The coefficients in the first stage appear plausible: more Democrat donations are associated with more liberal governments (-0.175***), higher Republican contributions with more conservative ones (0.105 to 0.112**) and elections where individual donations are more important (relative to PAC money) produce more liberal legislatures (-0.381 to -0.426*). In the second stage, we find the same results as we did in the main regressions: conservative governments are associated with higher restaurant (+0.10***) and finance sector employment (+0.037*), but lower restaurant earnings (-0.025***). The minimum wage has no effect on employment, neither in the restaurant nor in the finance sector, but does raise earnings in the restaurant sector (a 10% increase leads to a 1.65% increase in average weekly earnings).

VI. Conclusion

The impact of minimum wage policy on employment levels is hotly debated, with elasticity estimates very sensitive to the exact specification. These conflicting findings can be reconciled once we account for the impact changes in the state-level political situation have on both employment and the level of the minimum wage. In particular, we show that introducing a measure of political sentiment to the twoway fixed effects specification, the only to consistently point to negative employment effects, causes any evidence of a statistically and economically significant disemployment effect in the restaurant sector to vanish. At the same time, we provide arguments to suggest that it is indeed these political shifts that are filtered out by the statistical methods espoused by e.g. Dube, Lester and Reich (2010) and Allegretto, Dube and Reich (2010).

Our preferred specification, the twoway fixed effects estimator augmented with an instrumented political measure as control variable, leads to credible wage elasticities, no disemployment effects in the restaurant sector and unlike the traditional 2FE estimator, none in the (unaffected) finance sector either. As a result, we conclude based on current data and modelling techniques that there is very little credible evidence to suggest minimum wages adversely affect employment opportunities. Given their positive impact on

bottom decile earnings, this implies minimum wage policy can be a valuable tool against (working) poverty.

There are two major caveats. First, the impact of a large minimum wage shock (e.g. the Living Wage Adjustment, from $7.25 to $15) might be very different from the moderate changes observed so far. Second, minimum wages may have different effects in different contexts. We find no average effect, but a high federal minimum wage could still be detrimental in places with low cost of living and wages, which appears an interesting venue for future research.

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Appendix

Table A1. Occupational distribution of the restaurant sector workforce in 2016

Occupation (abbreviated OCC titles)	# of employees in restaurant sector	% of restaurant employment	Average hourly wage (in $)
Fastfood and Counter Workers	3,031,790	29.47	9.57
Waiters and Waitresses	2,105,830	20.47	11.57
Cooks	1,659,270	16.13	11.28
Supervisors	742,610	7.22	16.66
Food Preparation Workers	424,590	4.13	10.57
Dishwashers	376,130	3.66	9.97
Hosts and Hostesses	360,060	3.50	10.15
Cashiers	313,850	3.05	9.79
Attendants and Helpers	273,830	2.66	10.44
Bartenders	269,980	2.62	12.88
Sales Workers and Drivers	217,080	2.11	10.39
Food Service Managers	143,040	1.39	25.58
Assorted Managers	70,460	0.68	36.35
Others (bakers, cleaners, ...)	286,600	3.59	13.62

Based on data from the Occupational Employment Statistics database. The restaurant sector is defined as NAICS 7225 from the 2012 edition of the NAICS classification.

TABLE A2. FIRST AND SECOND STAGE OF INSTRUMENTED 2FE REGRESSIONS

		(1) Ideology	(2) Ideology	(3) Ideology
1^{st} Stage	Abortion always allowed	0.189	0.193	0.184
		(0.296)	(0.299)	(0.371)
	Atheist	0.837***	0.839***	0.816***
		(0.259)	(0.257)	(0.259)
	Increase federal crime spending	0.532	0.535	0.342
		(0.327)	(0.328)	(0.347)
	Dislike military†	0.734	0.731	0.962
		(0.650)	(0.651)	(0.662)
	Dislike gays†	-0.782*	-0.787**	-0.523
		(0.402)	(0.401)	(0.391)
	Equal Rights gone too far	0.144	0.144	0.383
		(0.436)	(0.434)	(0.434)
	Like Big Business†	0.210	0.207	0.209
		(0.267)	(0.269)	(0.266)
	Govt should offer little support	0.801***	0.804***	0.725***
		(0.264)	(0.262)	(0.242)
	Blacks have not been discriminated	-0.395	-0.391	-0.263
		(0.257)	(0.257)	(0.254)
	New Lifestyles hurt society	0.588	0.591	0.646**
		(0.402)	(0.402)	(0.324)
	Minimum Wage (log)	-1.274**	-1.287**	-1.259***
		(0.504)	(0.511)	(0.479)
	Population (log)	0.032*	0.032**	0.039*
		(0.019)	(0.015)	(0.023)
	Total Private Employment (log)	-0.006		-0.087
		(0.156)		(0.182)
	Total Private Earnings (weekly, log)		0.000	
			(0.249)	
		Emp Rest	Earnings Rest	Emp Finance
2^{nd} Stage	Median political ideology of house	0.100***	-0.038***	0.047**
		(0.031)	(0.012)	(0.018)
	Minimum Wage (log)	0.044	0.169***	-0.019
		(0.067)	(0.038)	(0.051)
	Population (log)	0.045***	0.007***	0.053***
		(0.006)	(0.002)	(0.004)
	Total Private Employment (log)	0.575***		0.387***
		(0.055)		(0.045)
	Total Private Earnings (weekly, log)		0.183***	
			(0.027)	
	N	187092	62364	357552
	SW F-test pvalue	0.001	0.001	0.000
	SW χ^2-test pvalue	0.000	0.000	0.000

See notes of Tables 4 and 5. †Instruments marked with a dagger are based on thermometer questions. Respondents were classified as liking a particular group if they gave it a score larger than 50 out of 100. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

FIGURE A1. HISTOGRAM OF PERCENTAGE MINIMUM WAGE CHANGES (1 PERCENT BINS).

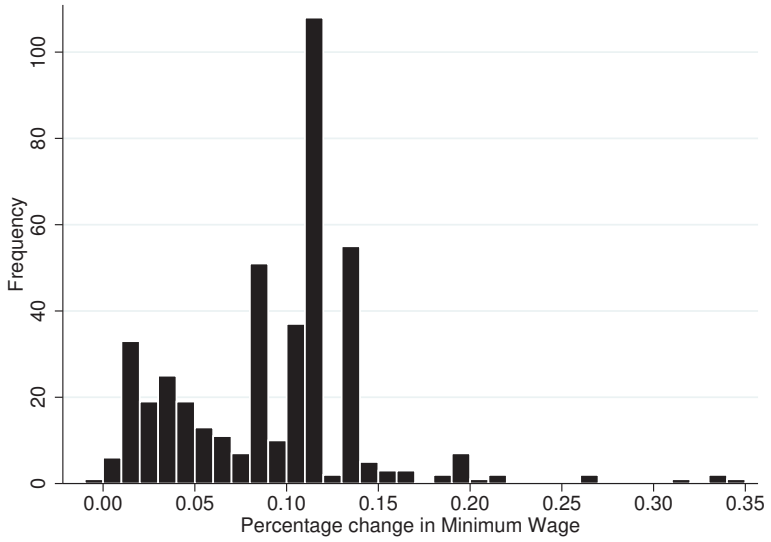


FIGURE A2. EXAMPLE OF ONE OF THE INSTRUMENTS.

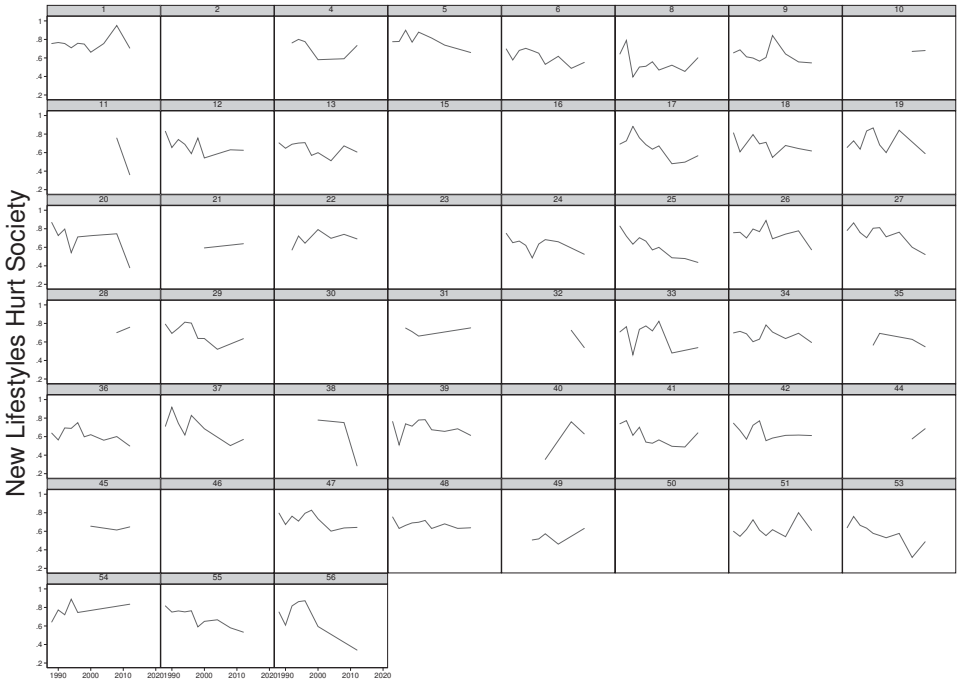
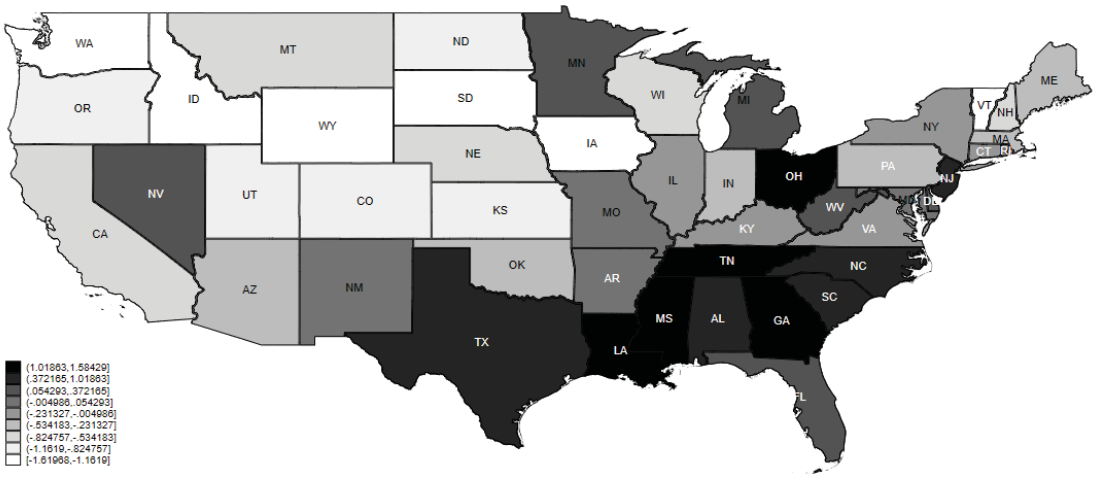


FIGURE A3. LOADINGS OF THE FIRST FACTOR (λ_1) AVERAGED BY STATE.



SLOPES OF THE TREND IN POLITICAL IDEOLOGY OVER TIME BY STATE.

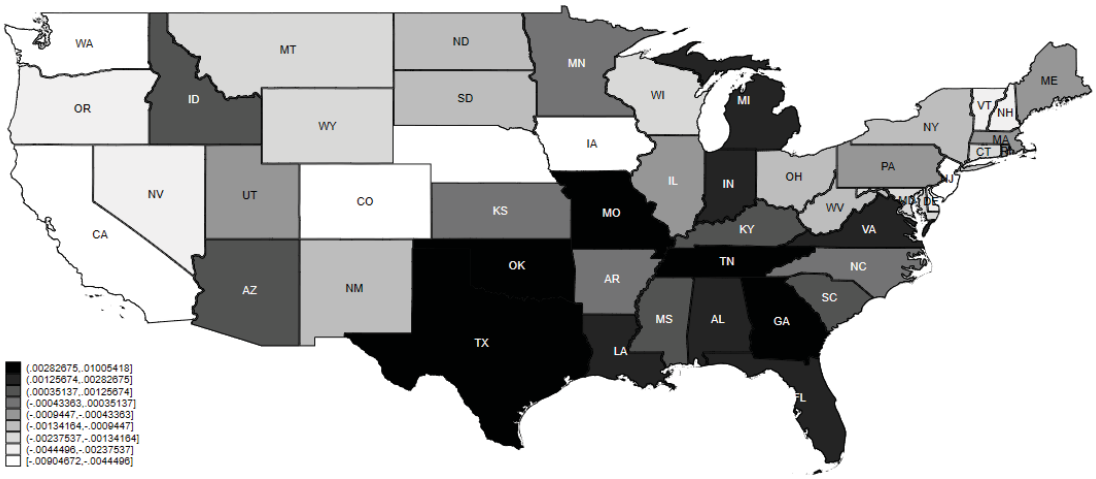
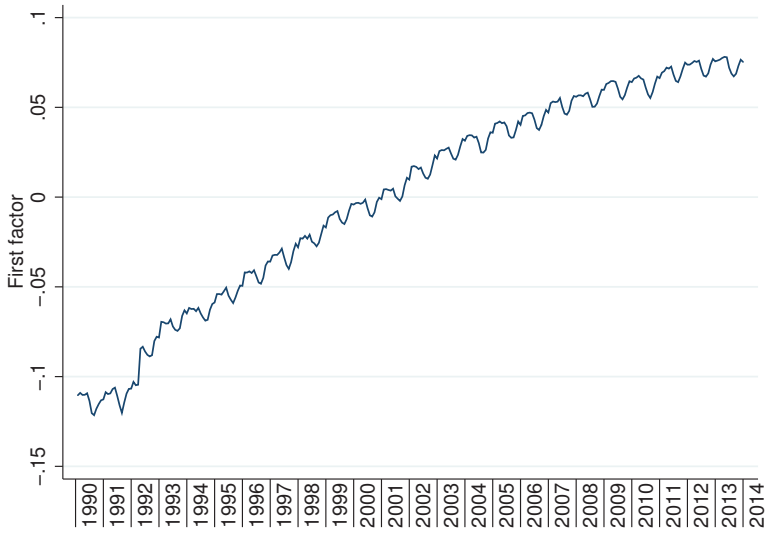


FIGURE A4. EVOLUTION OF THE FIRST FACTOR (F_1) OVER TIME.



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