Minimum Wages and the Rigid-Wage Channel of Monetary Policy

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Abstract: We argue that minimum wage employment, which is subject to legislatively-rigid wages, constitutes an ideal setting to study the wage-rigidity channel of monetary policy. In the first part of the paper, we use a calibrated model to show that minimum wages can contribute substantially to non-neutrality of monetary policy. Intuitively, while expansionary monetary policy may lead prices, endogenous wages, and capital rental rates to increase, the nominal minimum wage is held fixed by legislation and therefore falls in real terms. Heterogeneity in the share of minimum wage workers generates meaningful variation in the effects of our monetary policy channel across time and regions in the U.S. In the latter part of the paper, we present empirical evidence that the short-run employment fluctuation induced by monetary policy is significantly higher in states where the minimum-wage labor share of total costs is higher: the peak effect on employment of a 1% federal-funds rate shock is 2.5 percentage-points higher where the minimum-wage share is at its 90\textsuperscript{th}-percentile value compared to its 10\textsuperscript{th}-percentile value. This result is robust to a variety of econometric specifications, including state and year fixed-effects, an IV strategy instrumenting the minimum wage share with the legislated minimum wage level, either narrative or VAR monetary shocks, an analogous specification using Canadian data, and within-state county-level analysis. Testing the mechanism, we use a triple-differences specification and find substantially larger effects on near-minimum-wage employment (relative to higher-wage employment). We address the concern that the minimum-wage share is correlated with the marginal propensity to consume by showing larger effects on employment in tradable sectors relative to non-tradable sectors. We conclude that minimum wages, and rigid wages more generally, are a crucial channel through which monetary policy is operationalized.

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1 Introduction

Minimum wages and monetary policy are major features of the modern U.S. economy and the economies of many other nations today. A core source for efficacy of monetary policy is the existence of price and wage rigidities, and the minimum wage is an example of an important legislatively-set wage rigidity. It may thus come as a surprise that little attention has been paid to the intersection of these two topics, and no systematic empirical investigation of the role that minimum wages play in mediating monetary policy efficacy has been undertaken as of yet. We aim to fill this gap in the literature.

We also argue that a systematic exploration of the minimum wage’s implications for monetary policy is an ideal setting in which to study the importance of the wage-rigidity channel of monetary policy. The empirical literature on nominal wage rigidity has yielded mixed evidence about the extent to which wages are downwardly rigid, as we discuss in our literature review; for example, recent evidence from administrative data suggests that only 7-8% of job stayers experience no year-to-year wage changes during normal times, a lower fraction than the share of total U.S. employment near the minimum wage in the late 1970s and early 1980s, and up to 30% of job stayers experience wage cuts during recessions (Kurmann and McEntarfer 2019). Thus, focusing on the minimum wage – a wage which is known by definition to be nominally-rigid and is binding for a non-trivial fraction of the population – allows for a directed examination of the wage-rigidity channel of monetary policy.

We begin by setting up a model of monetary policy in which minimum wages are the only source of non-neutrality. The key assumption is that minimum wages in each state are binding: low skill workers would like to supply more labor than firms demand. Formally, low skill workers take their labor supply as given and, because the minimum wage is exogenous, firm demand determines the quantity of low skill labor in equilibrium. Expansionary monetary policy increases capital rental rates, endogenous wages, and prices, leading to reductions in the real cost of low skill labor for firms. Factor price changes induce both substitution and scale effects: under expansionary policy, firms substitute towards more use of low skill labor and also scale up their operations. Because our mechanism is fundamentally a supply shock, the model predicts larger
effects on tradable employment than non-tradable employment; intuitively, more production shifts towards the places where it has become relatively cheaper when that production can be consumed nationally rather than just locally.

Next, we take the implications of the model to the data. To illustrate the variation we exploit, Figure 1 shows the federal real minimum wage over time and the distribution of state shares of wage and salaried employment near the minimum wage in each year. Defining a worker as near the minimum wage if their hourly wage is within 10% of their state minimum wage, it can be seen that, in 1981, 13.5% of wage and salaried employment was near the minimum wage in the median state (11.4% in the 25th percentile state and 16.1% in the 75th percentile state). 1981 would be the last time minimum wages were raised for nearly a decade, and by 1989, only 5.9% of wage and salaried employment was near the minimum wage in the median state. This decline in minimum wage shares of employment tracks the decline in the real federal minimum wage quite well over this period – it was less common for individual states to set their minimum wages then than it is now. By 2005, just before the federal minimum wage increases of 2007-2009, only 2.7% of wage and salaried employment was near the minimum wage in the median state. Our empirical work exploits both time-series and cross-sectional variation in minimum wage shares and drives our conclusion that the declining minimum wage shares may have reduced the efficacy of monetary policy over time.

More specifically, for our baseline regression specifications, we obtain data on monetary policy shocks from Coibion et al. (2017), who expanded the original Romer and Romer (2004) narrative shock series beyond 1996. We obtain monthly data on state-level employment from the Quarterly Census of Employment and Wages (QCEW). And we compute the minimum-wage labor share of total costs by state and year using the Current Population Survey (CPS) and GDP and employee compensation data from the Bureau of Economic Analysis (BEA). Combining these sources, we run a regression specification that has much in common with canonical national-level monetary policy regressions, albeit adapted for a state panel setting by featuring two-way clustering on both the time variable (year and month) and the state variable in order to robustly account for complex autocorrelation structures. That is,
\[ \Delta E_{s,t} = \alpha + \sum_{i=0}^{48} \beta_i \text{Shock}_{t-i} + \gamma M\text{WShare}_{s,t} + \sum_{i=0}^{48} \delta_i \text{Shock}_{t-i} \cdot M\text{WShare}_{s,t} + \sum_{i=0}^{48} \eta_i \Delta E_{s,t-i} + \varepsilon_{s,t}, \]

where \( \Delta E_{s,t} \) represents month-over-month employment growth and \( M\text{WShare}_{s,t} \) represents the minimum-wage labor share of total costs. From this specification, we find evidence that the short-run employment fluctuation induced by monetary policy is significantly higher in states where the share of the minimum wage workers is higher. The peak effect on employment of a 1 percentage-point Romer and Romer Federal Funds Rate shock on employment growth is approximately 2.5 percentage points stronger where the minimum wage share is at its 90\(^{th}\)-percentile value compared to its 10\(^{th}\)-percentile value.

We apply a battery of robustness checks to this finding as well. We run a version analogous to a difference-in-differences specification, adding state and year fixed-effects to the baseline regression. Observing that changes in the share of minimum wage workers can be driven either by plausibly-exogenous factors such as minimum wage changes or by more endogenous factors such as uneven growth of low-wage and high-wage industries, we construct a Bartik-type variable that accounts for the latter effect and add it to our baseline regression. In an alternative approach to isolating the plausibly-exogenous variation, we run an IV specification instrumenting the state minimum wage share with the state minimum wage. Our main result is robust to all these alternative specifications, and the magnitude of the effect is scarcely modified.

Additionally, we replace the Romer and Romer narrative shocks in our main specification with VAR shocks, and the result is not much changed. We run the baseline specification in the Canadian context – using Canadian data on provincial minimum wages, employment, and monetary shocks – again finding the same significant relationship. We proceed even further with our robustness checks, using QCEW county-level data and the publicly-available 5% samples of the 1980/1990/2000 Censuses to compute the share of minimum wage workers at the county level. Equipped with this data, we add state-by-time fixed-effects in order to pursue a within-state county-level identification strategy. Once again, the result remains statistically significant.

To test the mechanism suggested by the model more clearly, we run a triple-differences specification that compares near-minimum-wage employment to higher-wage employment, finding that the employment increases are primarily driven by near-minimum-wage workers, just
as the model suggests. Finally, we separately examine the effects on employment in tradable versus non-tradable sectors, finding that the effect is somewhat larger amongst tradable sectors, a result consistent with the implications of our model and inconsistent with the competing explanation that all effects we measure are driven by differences in the MPC across states.

We conclude that minimum wages are an overlooked but important factor in determining the efficacy of monetary policy, confirming the more general hypothesis that wage rigidity is a key contributor to monetary non-neutrality. Indeed, our empirical magnitudes suggest that a sizeable fraction of monetary policy’s effectiveness is filtered through precisely this channel. This suggests two policy implications. On the one hand, minimum wages appear to function as an additional dimension of policy space. A higher fraction of minimum wage workers induced by a higher minimum wage unleashes greater effectiveness of monetary stabilization policy. On the other hand, monetary policy may primarily be functioning to erode distortions that were themselves previously put in place by the government. The fact that this channel accounts for a non-trivial amount of monetary policy effectiveness suggests that the Fed – often conceived of as an agency fully independent from the political process – is actually relaxing legislated policies and is thus working in close conjunction with the political process.

2 Literature Review

There is an extensive literature, with a diverse methodological history, devoted to studying the effects of monetary policy on economic outcomes. A key bifurcation in the literature on the effects of monetary policy is between those papers which use a vector autoregression (VAR) framework and those which use the narrative approach. While these literatures are both impressive in depth, defying a systematic listing here, key examples of VAR papers include Bernanke and Blinder (1992), Leeper, Sims, and Zha (1996), Bernanke and Mihov (1998), Christiano, Eichenbaum, and Evans (1999), Uhlig (2005), and Bernanke, Boivin, and Eliasz (2005). Key examples of narrative papers include Romer and Romer (1989), Romer and Romer (2004), and Coibion et al. (2017). Both branches of this literature find significant effects of monetary policy on real outcomes – but the effects found in the narrative literature are typically much larger. Ultimately, our regressions
will interact monetary policy shocks derived in these literatures with minimum wage shares that we compute in the data.

Our findings contribute directly to the literature on the rigid nominal wage channel of monetary policy. While models generating non-neutrality of monetary policy through nominal wage rigidity are common in the literature, there are no empirical tests of this channel in settings where the extent of wage rigidity is not in question. This is important because the empirical evidence on the extent to which nominal wages are rigid is quite mixed.

Early microdata evidence on downward nominal wage rigidity from the PSID, which contains individual-level wage changes, was relatively unfavorable. Fallick, Villar, and Wascher (2020 WP) describe this evidence: McLaughlin (1994) and Lebow, Stockton, and Wascher (1995) do not find strong evidence of downward nominal rigidity, though Kahn (1997) finds some evidence for hourly wage workers. Later work, e.g. Altonji and Devereux (2000), found that the mixed evidence on downward nominal wage rigidity might be due to measurement error in reported wages.

The evidence on downward nominal wage rigidity in small employer surveys and case studies has also been mixed. While Wilson (1999) and Altonji and Devereux (2000) find supporting evidence, Blinder and Choi (1990) find that five of the nineteen interviewed firms had recently cut wages, despite the booming economy.

Studies using the CPS, e.g. Daly, Hobijn, and Lucking (2012) and Daly and Hobijn (2014, 2015), find an increase between 2007 and 2011 in the fraction of workers in the same job (hereafter, “job stayers”) who report no change in their wage relative to the previous year. These studies are reassuring for the rigid nominal wage hypothesis, since the ORG component of the CPS, like the PSID, contains reported hourly wages, where we may be most likely to find rigidity. One issue with these studies is they focus on the fraction of workers with no wage change rather than focusing on the fraction of workers who receive wage cuts.

More recent studies turn to large surveys of employers that are less likely to suffer from measurement error. An early example is Lebow, Saks, and Wilson (2003). They use microdata from the BLS’s Employment Cost Index (ECI) program, which collects information on compensation for thousands of jobs across thousands of establishments, and find stronger evidence
of downward nominal wage rigidity than was typically found in panel data on individual wages: from 1981 to 1999, about 14.5% percent of year-to-year wage and salary changes were negative, and about 18.5% were 0. Fallick, Villar, and Wascher (2020 WP) turn again to this data and find increased downward nominal wage rigidity during and after the Great Recession.

Administrative data point to the importance of analyzing wage cuts and wage freezes separately. Kurmann and McEntarfer (2019 WP) use data collected by the unemployment insurance office in Washington state, which covers over 95% of private-sector employment in the state. They find that, during the Great Recession, the fraction of job stayers who are paid the same wage as a year earlier increases from 7-8% to 16% and then gradually returns to its pre-recession average. The fraction of job stayers who experience wage cuts increases during the recession from 20% to 30%, and the fraction of stayers who experience declines in annual earnings increases to 39%, suggesting some role for composition effects in hours. Jardim, Solon, and Vigdor (2019) find, using the same data, that for every quarter of year-to-year wage changes they in their data, at least 20% of job stayers experienced nominal wage reductions.

Elsby and Solon (2019) survey evidence from employers’ payroll records and pay slips in multiple countries, which includes the research from Kurmann and McEntarfer (2019) and Jardim, Solon, and Vigdor (2019) cited above. They find that, except during periods of high inflation or when nominal wage cuts are legally prohibited, an average of 15-25% of job stayers receive nominal wage cuts from one year to the next.

We also contribute to the literature that develops tests of underlying economic mechanisms relying on differential effects of shocks on tradable and non-tradable employment. Intuition and our model suggest larger effects of our channel on tradable employment than non-tradable employment. If, on the other hand, the minimum wage share in a region is correlated with the marginal propensity to consume (MPC) of a region, and this MPC channel is the true underlying mechanism, we might expect that monetary policy leads to larger demand shocks in these regions. Research shows that local demand shocks often lead to larger effects on non-tradable employment than on tradable employment, the opposite of what we would expect from our minimum wage channel. In two papers, Mian, Rao, and Sufi (2013) and Mian and Sufi (2014) develop local
demand shocks using changes in housing market wealth and argue these shocks have effects on non-tradable employment but no effects on tradable employment. Chodorow-Reich et al. (2020 WP) similarly argue that local demand shocks generated from changes in stock market wealth affect non-tradable employment but not tradable employment. We think our work further validates the usefulness of analyzing tradable and non-tradable employment when testing underlying economic mechanisms.

Finally, our research is related to an extensive literature on the effects of minimum wage changes on employment. There is limited consensus in this literature on the effect of minimum wage changes on employment (Neumark 2017). It is beyond the scope of this paper to summarize this literature, but we will point to some key research. Well-known papers such as Card and Krueger (1994) and Dube, Lester, and Reich (2010) find no adverse effects of minimum wage increases on employment. More recent evidence includes Cengiz et al. (2019), which also finds no evidence of negative effects on overall employment but does find some effect on employment in tradable sectors. Neumark and Wascher (1992), on the other hand, find that a 10% increase in the minimum wage causes a 1-2% decline in employment among target groups such as teenagers and young adults. More recent work by Clemens and Wither (2019) finds that a 9% minimum wage increase reduces employment by as much as 9% in a key target group. Reich, Allegretto, and Godoy (2017) analyze Seattle’s 2015-16 minimum wage increase from $9.47 to $11 and find it led to no disemployment effects on the food services industry (argued to have a high share of minimum-wage workers). Conversely, Jardim et al. (2019) use administrative data beyond the food-services sector to study the same minimum wage increase, finding the data points to an elasticity of -0.9, and the subsequent increase to $13 point to large disemployment effects, an elasticity of -2.6.

3 Model

Since minimum wage workers make up a relatively small fraction of employment, how large should the effects of our channel of monetary policy be? Further, how much heterogeneity across states should monetary policy generate through our channel? We address these points formally in
the model, which provides quantitative estimates of how large an effect monetary policy should generate through the minimum wage channel alone. Throughout this section, the “minimum wage share” in a sector refers to the total payroll of minimum wage workers in that sector divided by total cost in that sector.\(^4\)

The share of minimum wage workers is correlated with numerous other variables that may lead to differential effects of monetary policy across regions and time, so the model also provides an opportunity for us to generate the unique implications of our channel relative to competitor explanations. The model focuses on one confound in particular: the share of minimum wage workers may be high in regions where a higher share of households is credit constrained. In this case, any effects we attribute to monetary policy relaxing the minimum wage may be due instead to monetary policy alleviating or exacerbating credit constraints. More generally, higher minimum wage share regions may be regions where there is a higher marginal propensity to consume (MPC). We address this concern by analyzing tradable and non-tradable sectors in the model. As discussed in our literature review, shocks going through the MPC channel should lead to larger effects on employment in non-tradable sectors than on employment in tradable sectors. The minimum wage channel should lead, in contrast, to larger effects in tradable employment, a result we will confirm in the model and in our empirical analysis.

3.1 Households

The representative agent in each state \(s = 1, \ldots, S\) purchases tradables and non-tradables to produce a commodity, which can be invested or consumed. So, though there are two types of goods available in each state, there is only one type of capital, produced out of both non-tradables and tradables, in each state. Agents cost minimize over tradable and non-tradable inputs when producing the commodity, yielding the expenditure function

\[
E_s(p^T_t, p^{NT}_{s,t}, y_{s,t}) = \min_{y^{T}_{s,t}, y^{NT}_{s,t}} p^T_T y^T_{s,t} + p^{NT}_{s,t} y^{NT}_{s,t} \quad s.t. \quad F_s(y^T_{s,t}, y^{NT}_{s,t}) = y_{s,t}
\]

Where \(F_s\) is assumed to be constant returns to scale for each \(s\). We will call \(y_{s,t}\) “demand” in state

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\(^4\) Full details on how these are computed from CPS and BEA data, see data section.
s at time t. The commodity price is an ideal price index given by

\[ P_{s,t} = E_s(p_t^T, p_{s,t}^{NT}, 1). \]

We assume the steady state elasticity of substitution between tradables and non-tradables, denoted by \( \sigma_{NT,T} \), does not vary by state, an assumption driven by the absence of measurements of the parameter at this level of granularity.

We use lower-case variables to refer to the natural log of their upper-case variants, e.g. \( k_{s,t} = \ln K_{s,t} \). A hat denotes a variable’s deviation from its steady state value. The existence of steady state is shown later. Finally, a boldface variable refers to a vector or matrix.

The cost minimization conditions can be log-linearized as

\[
\hat{y}_t^{NT} - \hat{y}_t^T \approx -\sigma_{NT,T} (\hat{p}_t^{NT} - 1 \hat{p}_t^T)
\]

\[
\hat{y}_t \approx \eta_{NT} \hat{y}_t^{NT} + \eta_T \hat{y}_t^T.
\]

Where endogenous boldface variables are \( S \times 1 \) vectors. The steady state cost shares \( \eta_{NT} \) and \( \eta_T \) are diagonal \( S \times S \) matrices giving the share of a state’s GDP in the state’s non-tradable and tradable sectors, respectively.

The price index defined as above can be log-linearized as

\[
\hat{p}_t \approx \eta_{NT} \hat{p}_t^{NT} + \eta_T 1 \hat{p}_t^T.
\]

The agent’s dynamic problem can now be defined at the commodity level, abstracting from tradables and non-tradables.

The representative agent supplies capital \( K \) and two types of labor to the production side of the economy. Labor type \( L \) is subject to a binding wage floor \( \overline{W} \), and labor type \( H \) is paid an endogenous wage \( W \). We will refer to labor type \( L \) as “low skill” or “minimum wage” labor and to labor type \( H \) as “high skill” or “endogenous wage” labor. The model contains no uncertainty. The results are not meaningfully changed if we permit two representative agents in each state, (1) low skill agents who consume hand-to-mouth and face a binding minimum wage and (2) high-skill agents who perform all investment in the state and whose wage is endogenous. The budget constraint is

\[
P_{s,t} Y_{s,t} = P_{s,t} (C_{s,t} + I_{s,t}) = \overline{W}_{s,t} L_{s,t} + W_{s,t} H_{s,t} + R_{s,t} K_{s,t},
\]

Where the first equality links the budget constraint to the previously described cost minimization
component of the consumer problem. The law of motion for capital is

\[ \dot{K}_{s,t} = I_{s,t} - \delta K_{s,t}, \]

Where a dot refers to the time derivative of a variable, and \( \delta \) is the depreciation rate of capital. The utility function is separable and does not vary by region or time:

\[ U(c_{s,t}) - V(H_{s,t}) - V_L(L_{s,t}). \]

Our key assumption is that the wage floor is binding in each state. Thus, the representative consumer in each state would like to choose a higher value of \( L_{s,t} \) than the state can support. In the maximization problem, \( L_{s,t} \) will therefore be taken as exogenous. This is a simple application to the disequilibrium framework of Barro and Grossman (1971). We use the budget constraint to unconstrain the maximization problem, which we write as

\[
\max_{\{K_{s,t}\}_{t=0}^{\infty}, \{H_{s,t}\}_{t=0}^{\infty}} \int_0^{\infty} e^{-\rho t} \left( U \left( \frac{W_{s,t}}{P_{s,t}} L_{s,t} + \frac{W_{s,t}}{P_{s,t}} H_{s,t} + \left( \frac{R_{s,t}}{P_{s,t}} - \delta \right) K_{s,t} - \dot{K}_{s,t} \right) - V(H_{s,t}) \right) dt
\]

Where the initial capital stock is given in each state. Though the utility function does not vary by state, the inverse elasticity of intertemporal substitution, denoted \( \gamma \), and the Frisch-elasticity of labor supply, denoted \( \epsilon \), may still vary by state, since they depend on the level of consumption and skilled labor supply, respectively. We assume they do not vary by state, which could easily be micro-founded using CRRA forms for consumption utility and skilled labor disutility. Optimization yields an intratemporal and intertemporal Euler equation in each state, which we can log-linearize, respectively, as

\[-\gamma \hat{c}_t + \hat{w}_t - \hat{p}_t \approx \frac{1}{\epsilon} \hat{h}_t\]

\[\hat{c}_t \approx \frac{\rho + \delta}{\gamma} (\hat{r}_t - \hat{p}_t)\]

All of the boldface objects are endogenous \( S \times 1 \) vectors.

### 3.2 Non-tradable Sector

There is a firm in each state that produces non-tradables for use in that state. The sector first solves the cost minimization problem
The endogenous variables and minimum wage variable are again $S \times 1$ vectors, and the $\eta_i^{NT}$ are diagonal $S \times S$ matrices with entries given by the cost share of input $i$ in non-tradable production in the relevant state.

Note that these equations are the standard Slutsky equations for the firm. The first two terms denote substitution effects, and the final term denotes the scale effect. If production were Leontief, then each $\sigma$ would be 0, and we would be left only with the scale effect.

### 3.3 Tradable Sector

There is one, national tradable firm that produces in all states. We find this setup more realistic than permitting distinct tradable sectors in each state that produce a homogeneous output, since we will be able to allow parsimoniously for differences in state-level tradable output. The sector operates by producing a commodity in each state and then combining these commodities to produce final tradable output. In the first stage, it cost minimizes over production in each state:

$$E_s^T(R_{s,t}, W_{s,t}, \overline{W}_{s,t}, Y_{s,t}^T)$$

$$= \min_{K_{s,t}^T, H_{s,t}^T, L_{s,t}^T} R_{s,t} K_{s,t}^T + W_{s,t} H_{s,t}^T + \overline{W}_{s,t} L_{s,t}^T \quad s.t. \quad F_s^T(K_{s,t}^T, H_{s,t}^T, L_{s,t}^T) = Y_{s,t}^T.$$
The production function $F^T_s$ exhibits constant returns to scale in each $s$. This generates an ideal price index for the price of the state commodities required in production of the national tradable:

$$P^T_{s,t} = E^T_s \left( R_{s,t}, W_{s,t}, \overline{W}_{s,t}, 1 \right).$$

The firm then minimizes national-level costs:

$$E^T \left( P^T_{1,t}, \ldots, P^T_{S,t}, Y^T_t \right) = \min_{\{Y^T_{s,t}\}_{s=1}^S} \sum_{s=1}^S P^T_{s,t} Y^T_{s,t} \quad s.t. \quad F^T \left( Y^T_{1,t}, \ldots, Y^T_{S,t} \right) = Y^T_t$$

Finally, profit maximization yields the national tradable price,

$$P^T_t = E^T \left( P^T_{1,t}, \ldots, P^T_{S,t}, 1 \right).$$

The cost minimization conditions can be combined and stacked. This is a more complicated procedure than in the non-tradable sector, but we will give intuition after defining the relevant objects. Assume the elasticities of substitution between the state commodities in producing the national tradable are all equal and given by $\sigma_s$. This could be micro-founded by assuming $F^T$ has a CES form with a single elasticity of substitution, $\sigma_s$. Note that, had we modeled the tradable sector with distinct tradable sectors in each state that produce homogeneous output, we would implicitly be letting $\sigma_s \to \infty$, the case of perfect substitutes. Further, define $\eta^T_s$ as the cost share of the state $s$ commodity in producing the national tradable, measurable by tradable GDP in that state divided by tradable GDP in the U.S. Denote the $S \times S$ diagonal matrix of these shares by $\eta^T$. The diagonal $S \times S$ matrices of cost shares $\eta^T_i$ are analogous to those defined in the non-tradable sector: their entries are given by the cost share of input $i$ in tradable production in the relevant state. We define the $diag$ operator, which retrieves the diagonal entries of a matrix as a column vector, and the $*$ operator, which performs elementwise multiplication between two objects of the same dimension.

First, we define the scale effect

$$s_t = S \star \left( \eta^T_l \star \hat{W}_t + \eta^T_i \star \hat{W}_t + \eta^T_k \star \hat{r}_t \right) + 1 \hat{y}_t,$$

where
\[ S = \sigma_s \left ( \begin{array}{cccc} \frac{1 - \eta_1^T}{\eta_1} & 1 & \cdots & 1 \\ 1 & \frac{1 - \eta_2^T}{\eta_2} & \cdots & 1 \\ \vdots & 1 & \ddots & \vdots \\ 1 & 1 & \cdots & \frac{1 - \eta_S^T}{\eta_S} \end{array} \right ) , \quad \eta_t^{T*} = \left ( \text{diag}(\eta_t^T \eta_t^T)' \right ) \]

And we note that elementwise multiplication of \( S \) with the \( \eta \) matrices must occur before the \( \eta \) matrices multiply the factor prices. Then it follows that

\[
\tilde{l}_t^T = \sigma_{HL}^T \eta_H^T (\hat{w}_t - \hat{w}_t) + \sigma_{LK}^T \eta_K^T (\hat{r}_t - \hat{w}_t) + s_t
\]

\[
\tilde{h}_t^T = \sigma_{HL}^T \eta_L^T (\hat{w}_t - \hat{w}_t) + \sigma_{HK}^T \eta_K^T (\hat{r}_t - \hat{w}_t) + s_t
\]

\[
\tilde{k}_t^T = \sigma_{LK}^T \eta_L^T (\hat{w}_t - \hat{r}_t) + \sigma_{HK}^T \eta_H^T (\hat{w}_t - \hat{r}_t) + s_t.
\]

As in the non-tradable sector, these equations all represent Slutsky equations for the tradable sector, except now there are two substitution effects. On the one hand, when a particular input in a state becomes more expensive, the first substitution effect, and the same one we saw in the non-tradable sector, drives substitution to the cheaper inputs in that state. The second substitution effect, contained in \( s \), drives the tradable firm to substitute away from the commodity in the state that has seen the factor price increase. Thus, for tradable production in the state where the factor price has increased, the substitution effect in \( s \) is a scale effect, whereas for tradable production construed nationally, it is just another substitution effect away from the more expensive input, which in \( s \) is the state-level commodity. Finally, \( s \) also contains the standard output scale effect we also saw in the Slutsky equation for the non-tradable sector.

Note that if production at the state level and national level were both Leontief, all elasticities of substitution would be 0, and we would have \( \tilde{l}_t^T \approx 1 \hat{y}_t^T, \tilde{h}_t^T \approx 1 \hat{y}_t^T \), and \( \tilde{k}_t^T \approx 1 \hat{y}_t^T \). Note that this scale effect, unlike the scale effect in the non-tradable sector, is the same for all states. In this case, the model predicts no heterogeneity in log employment effects by state in the tradable sector. If production at the state level were Leontief, but we kept the general form for national production, we would still have \( \tilde{l}_t^T \approx s_t \) and be unable to simplify \( s_t \) to just \( 1 \hat{y}_t^T \) (and similarly for the other inputs).
3.4 Equilibrium and Steady State

The national money supply $M_t$, which is the quantity of money times its velocity, is the numeraire. It satisfies

$$M_t = \sum_s P_{s,t}^{NT} Y_{s,t}^{NT} + P_t^T Y_{s,t}^T = \sum_s E_s \left( p_{s,t}^{NT}, p_t^T, Y_{s,t} \right) = \sum_s P_{s,t} Y_{s,t} \equiv GDP_t$$

Where $GDP_t$ is nominal gross domestic product.

The rest of the equilibrium is standard. Goods markets and labor markets clear. It is worth mentioning that labor market clearing in the low skill labor market means that the low skill labor demand taken as given by households in each state equals the combined low skill labor demand of the tradable and non-tradable sectors in that state. We will solve a log-linearized version of the model and so will only be concerned with local versions of the transversality conditions.

It is shown in the appendix that a steady state in which all nominal variables grow at the same rate as the money supply exists. A key feature here is that nominal minimum wages in each state all grow at the same rate, the same rate as the money supply. Without this feature, the real minimum wage may change, leading to shifts in real variables.

3.5 Calibration and Solution

The log-linearized equations described above can be simplified to a $(3S + 1) \times (3S + 1)$ system of differential equations given by

$$\begin{pmatrix} \dot{c}_t \\ \dot{k}_t \\ \dot{w}_t \\ \dot{m}_t \end{pmatrix} \approx A \begin{pmatrix} \hat{c}_t \\ \hat{k}_t \\ \hat{w}_t \\ \hat{m}_t \end{pmatrix}.$$

It is not difficult to solve this system numerically. Now the matrix $A$ must be calibrated. We use standard parameter values where possible:
<table>
<thead>
<tr>
<th>Parameter</th>
<th>Description</th>
<th>Value</th>
<th>Notes</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\gamma$</td>
<td>Risk aversion in steady state</td>
<td>2</td>
<td>Upper bound suggested by Chetty (2006)</td>
</tr>
<tr>
<td>$\rho$</td>
<td>Discount rate</td>
<td>.01</td>
<td>Quarterly</td>
</tr>
<tr>
<td>$\delta$</td>
<td>Depreciation rate</td>
<td>.025</td>
<td>Quarterly</td>
</tr>
<tr>
<td>$\varepsilon$</td>
<td>Frisch elasticity of high skill labor supply in steady state</td>
<td>.4</td>
<td>Whalen and Reichling (2016)</td>
</tr>
<tr>
<td>$\sigma_{HL}$</td>
<td>High/low skill labor elasticity of substitution in steady state</td>
<td>1.41</td>
<td>Katz and Murphy (1992)</td>
</tr>
<tr>
<td>$\sigma_{HK}$</td>
<td>High skill labor/capital elasticity of substitution in steady state</td>
<td>.5</td>
<td>Oberfield and Raval (2020)</td>
</tr>
<tr>
<td>$\sigma_{LK}$</td>
<td>Low skill labor/capital elasticity of substitution in steady state</td>
<td>1.67</td>
<td>Krusell et al. (2000)</td>
</tr>
</tbody>
</table>

Note that we use the same elasticities of substitution in the table for both the tradable and non-tradable sectors. Our code can handle setting these separately. Now the elasticity $\sigma_s$ is somewhat nonstandard. We use the Cobb-Douglas case, $\sigma_s = 1$, as our baseline. In the appendix, we show that the results do not change much when reducing $\sigma_s$ to 0.1, a calibration close to Leontief that minimizes the ability of substitutions across state commodities in the tradable sector to drive the result that monetary policy has a larger effect on tradable employment than non-tradable employment through the minimum wage channel. We also set $\sigma_{NT,T}$, the elasticity of substitution between tradables and non-tradables in consumption, to a value consistent with the Cobb-Douglas case, or $\sigma_{NT,T} = 1$. This is used by Mian and Sufi (2014), which motivated our analysis of tradable and non-tradable employment in the first place.

The only way our calibration will differ with the time period we analyze is in the cost shares.

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Our calibrations for the minimum wage cost shares are shown in Figure 2. These are computed as the minimum wage share of total payroll of wage and salaried workers in a given state, multiplied by the labor share in the state—more details are provided in the data section. It is clear in panel 1 of the figure that minimum wage cost shares in the tradable sector are typically lower than those in the non-tradable sector. The bulk of cost shares in both sectors are below 0.025 across states and time. We show the shares from 1976-1981 in panel 2 to highlight how much higher they are: more than half of the minimum wage cost shares in the non-tradable sector are higher than 0.05, and occasionally the shares reach the 0.10 range. Thus, in this period in certain states, 10% of production cost is subject to a price floor above the equilibrium price.

A key calibration is the magnitude of the shock to the effective money supply we feed into the model. We recall that $M_t = GDP_t$, which can be measured in the data as $P_t Y_t$. Romer and Romer (2004) studied the effects of shocks to the federal funds rate on prices and output, separately. Thus, the cumulative effects on a shock to $\ln M_t$ can be measured as the sum of the cumulative effects on $\ln P_t$ and $\ln Y_t$. The effect of a 1 percentage point increase in the federal funds rate, measured in this way, accumulates to a 4% decline in $M$ over two years (a 4% decline in output and 0% decline in prices) and a 7% decline in $M$ over four years (a 1% decline in output and a 6% decline in prices). To be conservative, since shocks other than those from Romer and Romer (2004) usually lead to smaller effects, and because we are not particularly interested in the impulse response functions resulting from this model, we will calibrate a 1 percentage point shock to the federal funds rate as a completely unanticipated and permanent 4% shock to the money supply.

3.6 Model Results

We vary our cost share calibrations by year and include all 50 states in the model, so it is infeasible to present impulse response functions to summarize our results. Instead, for each year of calibration and each outcome variable of interest, we compute the impulse response functions over a 4-year horizon and take the largest magnitude effect achieved over that 4-year horizon for each state. We summarize these maximal results in each year using a boxplot. Figure 3 shows these graphs for various outcomes of interest in two panels.
We wish to highlight several outcomes of interest, particularly in the late 1970s and early 1980s. In these years, the minimum wage channel of monetary policy contributes an overall employment decline of about -0.75% in response to a 1 percentage point increase in the federal funds rate. This effect is driven by low wage employment, which declines by nearly 6%. With some outlier exceptions, high skill employment actually increases slightly, by around 0.075%. Capital and consumption both fall somewhat, by -0.05% and -0.2%, respectively. Prices fall by less than 4%, the magnitude of the shock to the money supply, highlighting that our channel is fundamentally a supply shock. Put differently, contractionary monetary policy raises the real cost of production by increasing the nominal minimum wage relative to other prices, and so prices fall by less than what would be predicted under monetary neutrality. The heterogeneity in these effects over time is substantially less than the heterogeneity in employment, however.

We also want to highlight how our results look much closer to monetary neutrality from the late 1980s onward. This is the period where the minimum wage share has become small, meaning less economic cost is at the binding wage floor. We think the fact that a declining proportion of economic cost has been at the binding minimum wage over time contributes to findings that the effects of monetary policy may have fallen over time.

Panel 2 shows that median states experience smaller declines in tradable employment than non-tradable employment, but the minimum wage cost shares in tradable employment are often much smaller; it is also apparent that heterogeneity across states in tradable employment is much larger than in non-tradable employment. To highlight how our employment effects depend on the minimum wage share in a state, see Figure 4. Panel 1 shows how our overall employment effects increase in magnitude with the minimum wage share in the state. Panel 2 shows how our tradable employment effects increase in magnitude more quickly with the tradable minimum wage share than the non-tradable employment effects do with the non-tradable minimum wage share. Note the subtle curvature in these results, which is important for computing a slope.

4 Empirical Framework

But does this channel of effect for monetary policy highlighted by the model actually exist in
practice? To answer that question, it is necessary to conduct some empirical analysis. We begin by using standard data sources for employment – the QCEW and the CPS – and an adapted version of a very simple and standard specification from the narrative monetary policy literature. We subsequently branch out from this specification and run a broad variety of robustness checks intended to encapsulate many potential critiques of the baseline specification.

4.1 Data

We obtain data on narrative monetary policy shocks from Coibion et al. (2017). Coibion et al. follow the technique devised by Romer and Romer (2004), who obtained narrative records of the Federal Reserve’s intentions for the federal funds rate around FOMC meetings and regressed this series on internal Fed forecasts “to derive a [monthly] measure free of systematic responses to information about future developments.” The Romer and Romer (2004) series of monetary shocks has become one of the canonical sources of exogenous variation used in the monetary policy literature. Because the series initially terminated in 1996, however, Coibion et al. extended it through 2015. We also obtain an alternative VAR shocks series from Coibion (2012), a paper dedicated in part to explaining why the Romer and Romer (2004) shocks generate such large effects of monetary policy. This VAR series yields effects of monetary policy close to those found in Christiano, Eichenbaum, and Evans (1999), somewhat smaller than those found in Leeper, Sims, and Zha (1996) and somewhat larger than those found in Bernanke and Blinder (1992); in particular, these shocks lead to output effects that are roughly six times smaller than those found in Romer and Romer (2004). Further, the VAR series is much less sensitive to the inclusion of monetary policy episodes in 1980 that drive the estimated Romer and Romer (2004) shock effects to be large.

For two key reasons, we will not use monetary policy shocks derived from the high-frequency identification literature, which includes Cook and Hahn (1989), Kuttner (2001), Cochrane and Piazzesi (2002), Gürkaynak, Sack, and Swanson (2005), and, more recently, Gertler and Karadi (2015) and Nakamura and Steinsson (2018). First, we have the most power to detect our results in the late 1970s and early 1980s, when there was a relatively high share of minimum wage workers. The earliest futures data used in Nakamura and Steinsson (2018) begins in 1995,
and their measurements of real interest rates require TIPS data – TIPS were issued beginning in 1997. We cannot use the futures rate surprises from Gertler and Karadi (2015) because the key data is available from 1991 to 2012, leaving out the core period during which we want strong, exogenous monetary policy variation. Second, we question the power of these shocks in our context because some of these series are able to detect effects of monetary policy on financial variables but not real variables such as output and employment.

For data on employment, we turn to the Quarterly Census of Employment and Wages (QCEW), which has collected population data on employment by county by industry in the United States since 1937. Despite the name of the dataset, employment data is available at the monthly level. Digitized data from January 1975 onward is readily available for download on the Bureau of Labor Statistics (BLS) website. For our baseline regression specification, we use the state-level figures aggregated across all industries, but for certain alternative specifications – such as our specification with as our within-state county-level design – we make use of the underlying county-level and/or industry-specific data.

We use the CPS Outgoing Rotation Groups (CPS-ORG) to compute the share of minimum wage workers as a proportion of all workers by state and year. Households in the CPS sample respond to the questionnaire for four months in a row; they are then out of the sample for eight months; finally, they return to the sample for another four months. At the end of each of the two four-month blocks during which a household is present in the CPS sample, they are asked a specific set of questions not asked in other months. These questions – which include amongst them an explicit question about what the respondent’s hourly wage is – make up the Outgoing Rotation Groups questionnaire. Because the monthly size of the CPS is 60,000, this means that the monthly size of the ORG is (approximately) 15,000 – an annual sample size of 180,000. By merging this data with Vaghul and Zipperer’s (2016) dataset on historical state and federal minimum wages, we can identify minimum wage workers as any wage or salaried worker whose computed hourly wage is within a one dollar band around their state’s binding minimum wage (i.e., between 50

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5 The reason for the apparent discrepancy is that the census is conducted quarterly but asks employers how many workers were on their payroll at the end of each of the three months of the preceding quarter.
cents below and 50 cents above). Our results are practically identical if we instead use percentages—defining near minimum wage workers as those within 10% of the minimum wage—and they are scarcely changed if we widen to (20%) or narrow (to 5%) the band. We can then compute the minimum wage share of payroll in a state as the total payroll to minimum wage workers in the state divided by total payroll in the state.

To compute the minimum wage share in the state, we multiply the payroll share computed above by the labor share in the state. The Bureau of Labor Statistics (BLS) describes how they compute the national labor share, and we implement this procedure at the state level. The statistic is computed by dividing total compensation in a state by total GDP in a state, using data published by the Bureau of Economic Analysis (BEA). This share is then adjusted upwards for proprietors’ incomes due to their own work at their businesses, which is not included in total compensation as measured by the BEA. We show how our computed labor share at the national level compares to the BLS’s labor share in the appendix.

It is worth noting that an alternative approach to using the CPS-ORG is to use the Current Population Survey’s Annual Social and Economic Supplement (CPS-ASEC) to compute the share of minimum wage workers. The ASEC commenced in 1962 and since its inception has asked respondents their total wage income, weeks worked, and hours worked per week over the last year. From this, it is possible to compute each individual’s hourly wage. However, prior to 1977, the aforementioned variables were binned, so weeks and hours worked – and thus hourly wage – can only be approximately known. And compared to the ORG’s annual sample size of 180,000, the ASEC has an annual size of 60,000. As a consequence of its lower sample size, the approximation implicit necessary as a part of the preceding process, and the fact that the QCEW data is only available from 1975 onward anyway, we use the ORG instead of the ASEC. Having said this, our results are virtually unchanged if we instead use the ASEC data.

As noted, neither the ASEC nor the ORG are of sufficient size to calculate county-level annual minimum wage shares. As such, given that state minimum wage shares are a very slow-moving variable, we turn to the Census. Using the IPUMS 5% public-use samples of the 1980, 1990, and 2000 Censuses, we compute county-level minimum wage payroll shares for use in our within-state
specifications. Because BEA data on GDP at the county level does not start until 2001, we cannot easily compute our full minimum wage share at the county level. This will motivate us showing regressions using the county-level minimum wage payroll shares and alternative specifications using minimum wage shares computed using county minimum wage payroll shares multiplied by the state labor share.

We also obtain data on some additional control variables for robustness checks. We obtain data on per-capita bank deposits by county from the FDIC and data on personal income per-capita by county from the Bureau of Economic Analysis (BEA). We obtain a Canadian narrative monetary shocks series (constructed analogously to the Romer and Romer shocks) from Champagne and Sekkel (2018), and we obtain Canadian data on monthly employment and the share of minimum wage workers by province from Statistics Canada’s Monthly Labour Force Survey Public Use Microdata File (PUMF).

4.2 Identification

Our baseline specification is an adapted version of the standard narrative-shocks monetary policy regression. We add interaction effects between the shock variable and the minimum wage share, and we additionally two-way cluster our standard errors at the state and time level in order to account by complex correlation structures induced by the fact that our dataset is a panel dataset but the monetary shock series is state-invariant.

\[ \Delta E_{s,t} = \alpha + \sum_{i=0}^{48} \beta_i \text{Shock}_t \cdot \text{MWShare}_{s,t} + \sum_{i=0}^{48} \delta_i \text{Shock}_t \cdot \text{MWShare}_{s,t} + \sum_{i=0}^{48} \gamma_i \Delta E_{s,t-i} + \epsilon_{s,t} \]  

(1)

In various robustness checks, we enhance this specification with additional control variables and/or different approaches to identification. First, we add state and time fixed-effects to the specification to account for all time-varying, state-invariant and state-varying, time-invariant confounds. Separately, we add controls for the interaction effect of a couple of other variables with the shock series: bank deposits per-capita and per-capita income (proxying for the marginal propensity to consume). The idea is that there may remain some crucial variables correlated with minimum wage share that could plausibly be the true channel for monetary policy efficacy, rather than the minimum wage share itself.

Observing that changes in the minimum wage share in a state can be driven either by
plausibly-exogenous factors such as minimum wage changes or by more endogenous factors such as changes in the share of each industry in that state’s employment, we construct a Bartik-type variable that controls for the latter effect and add it to our baseline regression. In particular, we construct the variable by computing

$$\Delta S_{s,j} = \sum_j \text{Shift}_{s,j,t} \cdot \text{Share}_{s,j,t-1},$$

where $\text{Shift}_{s,j,t}$ represents the national-level growth of employment in industry $j$ over time period $t$ (calculated as a leave-one-out average) and $\text{Share}_{s,j,t-1}$ represents the employment share of industry $j$ in total state-$s$ employment in the preceding time period $t-1$. This shift-share isolates the national-level, non-idiocyncratic component of growth in employment that stems from broader trends. Adding this control to the specification should help ensure that the effect we are finding is not driven by that more endogenous source of minimum-wage-share variation. As an alternative approach to isolating the plausibly-exogenous variation, we run an IV specification instrumenting the state minimum wage share with the state minimum wage. In particular, for our first-stage, we instrument the direct effect and the interaction effects involving the minimum wage share with the corresponding minimum wage variables as follows,

$$\begin{align*}
\text{MWShare}_{s,t} &= \omega + \rho \text{MinWage}_{s,t} + \sum_{i=0}^{48} \theta \text{Shock}_{t-i} \cdot \text{MinWage}_{s,t} + u_{s,t} \\
\text{Shock}_{t-i} \cdot \text{MWShare}_{s,t} &= \chi_i + p \text{MinWage}_{s,t} + \sum_{i=0}^{48} \theta \text{Shock}_{t-i} \cdot \text{MinWage}_{s,t} + u^i_{s,t}
\end{align*}$$

and the second-stage constitutes placing the predicted values of the left-hand-side variables from these first-stage regressions back into our baseline specification.

As another enhancement of our baseline specification, to factor out potential concerns of correlated state policymaking, we add state-by-time fixed-effects and rely on the county-level data in order to pursue a within-state identification strategy. Additionally, we run another within-state specification – analogous to a triple-differences specification – comparing near-minimum-wage employment growth to higher-wage employment growth to confirm that our effects are indeed driven by near-minimum-wage workers. That is,

$$\begin{align*}
\Delta E_{s,w,t} &= \alpha + \sum_{i=0}^{16} \delta_i \text{Shock}_{t-i} \cdot 1\{\text{MinWage}\} + \sum_{j=1}^{16} \eta_i \Delta E_{s,w,j-1} + \omega_{s,t} + \theta s_n + \varepsilon_{s,t,j}
\end{align*}$$

Note that QCEW data on employment by wage group is unavailable. So, for this specification, as
our left-hand-side variable, we compute separate series of near-minimum-wage and higher-wage employment growth using the CPS-ORG data. Also, observe that we are taking the absolute value of the shock series in this specification. This is because our model suggests that near-minimum-wage employment should respond more strongly in magnitude to monetary policy shocks – i.e., that expansionary shocks (Shock; < 0) should induce more employment growth for this group and that contractionary shocks (Shock; > 0) should induce more employment loss (less employment growth) for this group.

Finally, to be completely parallel with our model, we run the baseline specification (1) over tradables and non-tradables separately. And we similarly use a triple-differences specification to compare tradable versus non-tradable sectors within-state.

5 Results

5.1 Main Results

Beginning with the baseline specification, Figure 5 depicts its results in the form of an impulse response function cumulating the interaction effect over time. The error bands represent 90% confidence intervals. Note that the magnitude of the interaction effect peaks at -1. Thus the figure can be interpreted as follows: a 1 percentage-point higher minimum wage share corresponds to 1 percentage-point lower employment growth (at peak) as a result of a 1 percentage-point Romer and Romer contractionary monetary policy shock (i.e., a 1 percentage-point unexpected increase in the Fed Funds Rate). Stated more intuitively, a state at the 90th percentile of the minimum wage share will experience a peak employment effect of a 1 percentage-point Romer and Romer federal funds rate shock that is approximately 2.5 percentage-points higher than a state at the 10th percentile of the minimum wage share.

Now, suppose certain states are more responsive to monetary policy for reasons unrelated to the minimum wage. For example, poorer states are likely to have a higher average marginal propensity to consume, which should boost monetary policy efficacy through more traditional channels. Similarly, suppose the efficacy of monetary policy is declining over time for, again, reasons unrelated to the minimum wage. Because the share of minimum wage workers is also
declining over time, this could plausibly pollute the coefficients we estimate in the baseline model. We address these potential concerns by adding state and time fixed-effects. The resulting impulse response function is plotted in Figure 6. Notably, the effect not only survives – it is made more strongly significant than in the baseline specification. The magnitude, however, is (non-significantly) smaller by a factor of one-half.

Another concern is that the industries which have the highest share of minimum wage workers might just be the industries that are most affected by monetary policy – for reasons unrelated to the minimum wage share itself. If these industries are concentrated in specific states, that could be driving our results. To deal with this concern, Figure 7 turns to the Bartik controls, adding them to the baseline specification. The idea here is that controlling for the Bartik instrument purges the component of employment growth driven by broad industrial trends; the remaining unexplained left-hand-side variation in economic growth is not a consequence of which industries happen to be concentrated in which states. As can be seen in the left panel of Figure 7, the effect survives and, indeed, is little changed from baseline. The right panel of Figure 7 adds both the Bartik control and the state and time FE to our baseline specification, combining the desirable characteristics of both of these robustness checks. Here, too, the effect retain significance. The magnitudes are in-between the baseline specification and the specification with only state and time fixed-effects.

In Figure 8, we interact some control variables with the Romer and Romer shock series (and its 48 lags) to help demonstrate that the effect we find is not due to correlation of the minimum wage share with other important variables that affect monetary policy efficacy. We can see that doing this with bank deposits per-capita and per-capita income – two variables which are particularly likely to correlate with MPC – do not materially change our result from baseline.

Variation in the state minimum wage share may come from a variety of sources – including changing industry shares within the state. Again, this source of variation may be somewhat endogenous. As a result, a somewhat different approach from the Bartik control of factoring out this industry-correlate-driven variation in the minimum wage share is to instrument for the minimum wage share with the legislated state minimum wage. This isolates variation driven by political decisions on the part of the state legislature, plausibly a more exogenous source of
variation than changing industry shares. Figure 9 turns to this IV strategy. Again, the result survives; the magnitude of the point estimate, however, increases by a factor of approximately 2, though our previous results remain within the standard error bars of this point estimate.

The specification represented in Figure 10 makes use of the VAR shocks from Coibion (2012) instead of the Romer and Romer shocks. The monetary policy literature has proceeded along two main strands – one pursuing a narrative approach and the other pursuing a VAR-based approach. We aim to show that our result goes through regardless of which shocks series we use; it’s not an artefact of one approach or the other. While the shape of the impulse response function is somewhat different – with the effect peaking some two years later than in the baseline specification – the magnitude is the same, as is the takeaway: monetary policy is significantly more effective where the share of minimum wage workers is higher.

Figure 11 plots the results of running the baseline specification on the Canadian data. As in the case of the VAR robustness check, we do not necessarily have a reason to believe that the US data is inferior to the Canadian data (or vice versa) – we merely regard it as a second laboratory in which to test our hypothesis and provide evidence of its generality. Again, despite a somewhat modified shape of the impulse response function, the peak magnitude is nearly the same and the evidence remains that a higher share of minimum wage workers significantly boosts monetary policy efficacy.

We next turn to the within-state specification. Suppose, for example, that expansionary monetary policy causes states with a low share of minimum wage workers (i.e., where the minimum wage isn’t very binding) to increase the state minimum wage in order to prevent it from being further devalued by the price level increases induced by the monetary policy. Insofar as the increased minimum wage reduces employment growth, we could be picking up this effect in our baseline regressions. Now, it’s worth noting that this is still part of the causal chain – in this example, expansionary monetary policy is still technically causing higher employment growth in high minimum-wage-share states relative to low minimum-wage-share states – so this conjecture would not mean that the effects we estimate in the baseline specification cannot be interpreted causally. However, we might nonetheless be interested in honing in on the component of the
overall effect mirroring our model, factoring out the aforementioned side-channel. Figure 12 presents the specification that adds a state-by-time fixed-effect to the baseline specification in order to exploit county-level variation in the minimum wage share. This factors out any state policy responses. Here, too, an impulse response function similar to the baseline is reproduced – notably, though, the effect size is smaller, perhaps suggesting some evidence that the side-channel does exist. It is worth mentioning that, because we need to calculate the county-level minimum wage shares from the Decennial Census data, we use 1980 shares through 1989, 1990 shares through 1999, and 2000 shares through 2009. This introduces measurement error and, consequently, is likely to bias the result downward (due to attenuation bias) – another potential reason for the reduced magnitude.

5.2 Mechanism and Testing Model Implications

The triple-differences specification is slightly more complex in that the prediction of our model is that expansionary monetary policy should have more positive effects on near-minimum-wage employment than on higher-wage employment, whereas contractionary monetary policy should have more negative effects on near-minimum-wage employment than on higher-wage employment. Consequently, in our regression specification, we examine whether the absolute value of the effect is higher on near-minimum-wage labor. As can be seen in Figure 13, the effects on near-minimum-wage labor are massively higher than those on higher-wage labor, consistent with the mechanism laid out by the model. In the left panel, we define “near-minimum-wage” workers as those less than 125% of their state’s minimum wage; in the right panel, we use 150% as the threshold. Note that the magnitudes are quite high, but near-minimum-wage labor makes up less than 10% of total labor, so the very large coefficients in the triple-diff specification reflect that the overall 5% increase is being driven quite disproportionately by near-minimum-wage workers, as one would expect.

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6 Because our employment data begins in 1975 but the publicly-available sample of the 1970 Census is much smaller (1% sample) than the 1980 Census – and thus leaves virtually all counties unidentified in the data – we extend the 1980 minimum wage share back through 1975. Instead omitting years 1975-1979 does not substantially alter the results.
Finally, in order to further validate the mechanism of the effect, we decompose employment growth into employment growth in tradables and employment growth in non-tradables\(^7\). We run a version of the baseline specification that interacts the Romer and Romer shocks and their 48 lags with the tradable minimum wage share and, separately, a version of the baseline specification with the non-tradable minimum wage share as the interaction term. As we show in Figure 14, a higher minimum wage share significantly boosts monetary policy efficacy in both tradables and non-tradables. The effect is not driven by non-tradables. Indeed, if anything, the effect is stronger on \textit{tradable} employment, precisely the opposite of what the MPC channel would suggest. Figure 15 runs a within-state triple-differences version of this specification – comparing tradables to non-tradables – and makes it even more directly clear that the effect is not driven by non-tradables. To the extent the effects on the two groups are ever significantly different, the effect on tradable employment is larger.

\subsection*{5.3 Comparison of Effect Magnitudes between the Model and Empirics}

At this point, we have presented many different empirical specifications, some yielding differing magnitudes for how the effect of monetary policy on employment varies with the minimum wage share. While most of these magnitudes are statistically indistinguishable, we wish to discuss them in-depth here and compare them to the magnitudes from the model.

The effect size from our model can be read off of Figure 4, Panel 1, which plots the peak employment effect over 4 years in a state as a function of the minimum-wage labor share of total costs in the state. Clearly, the effect on employment of a 1 percentage point increase in the federal funds rate is decreasing and convex in the minimum wage share; it also passes through 0. To get our model’s effect size, we can regress, with no constant, the employment data in that plot against a second order polynomial in the minimum-wage labor share of total costs. This yields a maximal

\[^7\text{We define “Tradables” as the Agriculture, Mining, Manufacturing, and Finance sectors. We define “Non-tradables” as the Construction, Transportation, Communications, Utilities, Retail Trade, Wholesale Trade, Services, and Public Administration sectors. There is no one authoritative definition of these two terms, and some classifications omit Finance from the Tradables category and/or omit Wholesale Trade and parts of Services from the Non-tradables category. Omitting some or all of these sectors from our classification does not materially change our results.}\]
effect size when we consider increasing the minimum wage share a small amount from 0: to a first-order, in response to a 1pp increase in the federal funds rate, a state with no minimum wage workers will experience effect a .24 percentage point smaller change in employment than a state with a 0.01 minimum-wage labor share of total costs.

This maximal effect size is four times smaller than the effect size from our baseline regression, displayed in Figure 5, and roughly two times smaller than the effect size from our cross-sectional and Bartik analyses in Figure 6 and Figure 7, though it is within the standard error bands of the latter analyses. It is important to note that the model is measuring the change in employment *hours*, whereas our empirics using the QCEW data are analyzing employment *counts*. This would cause magnitudes to differ to the extent that minimum wage workers work fewer hours on average than higher wage workers. The key driver of this difference in magnitudes, however, is that changes in the minimum wage are known to increase wages of workers higher up in the wage distribution (see, e.g., Autor et al. 2016); in this sense, the minimum wage shares we used in our model and in our empirical analyses, which focused on workers very close to the minimum wage, may be at times substantially less than the shares of all workers affected by changes in the minimum wage. Unlike in the model, where using smaller shares will lead to monetary policy effects closer to monetary neutrality, using smaller shares in the empirics will lead to effect magnification. We still prefer using shares of workers *near* the minimum wage because that object is much easier to measure than shares of workers *affected* by the minimum; the latter definition requires causally identified estimates of which workers’ wages increase when the minimum wage increases.

6 Conclusion

We observe that the standard theoretical and empirical understanding of monetary policy suggests that it should erode real minimum wages. Our model establishes this point formally, providing quantitative predictions about how differences in the minimum wage share across states and time generates heterogeneity in the effects of monetary policy. The model also predicts that our channel of monetary policy should lead to larger changes in low-wage and tradable
employment compared to high-wage and non-tradable employment, respectively. To test empirically whether the minimum wage channel is indeed an important channel through which monetary policy is operationalized, we turn to the data. Using QCEW data on employment growth, CPS and BEA data on the share of minimum wage workers by state, and Romer and Romer narrative monetary policy shocks, we find that, indeed, this channel is crucial. This result is robust to a variety of different identification strategies – including a within-state county-level technique – and the inclusion of a variety of controls. The relationship also manifests itself using VAR shocks instead of Romer and Romer narrative shocks. It is present in the Canadian data as well. A triple-differences specification reveals that the employment growth effect does indeed proceed primarily through near-minimum-wage workers, as expected. And analysis of tradable and non-tradable employment shows that our channel of monetary policy goes more strongly through the tradable sector, as predicted by our model.

Taken as a whole, these findings suggest that minimum wages are an overlooked but important factor in determining the efficacy of monetary policy. Our results imply that monetary policy is minimally effective in the absence of minimum wages. This suggests that, on the one hand, higher minimum wages function as an additional dimension of “policy space” that boosts the ability of monetary policy to stabilize the economy as desired by policymakers. But on the other hand, the Fed – typically thought of as an independent agency – is actually relaxing legislated policies and thereby highly dependent on the political process. In any case, these findings suggest that the interaction effects of monetary policy and minimum wages are a highly understudied topic with substantial room for future exploration.

References


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Figure 1: Real Minimum Wages and Minimum Wage Employment Shares
Figure 2: Cost Share Calibrations

Distribution of Minimum Wage Cost Shares across States and Time

Distribution of Minimum Wage Cost Shares across states and years 1976 - 1981
Figure 3: Model Outcomes

Panel 1

Peak Effect over 4 Years of a 1pp Unexpected Increase in the Federal Funds Rate
when the shock occurs in the x-axis denominated year

- Capital
- Consumption
- Employment
- High Skill Employment
- Low Skill Employment
- Prices

Log Point Change (times 100)
Panel 2

Peak Effect over 4 Years of a 1pp Unexpected Increase in the Federal Funds Rate
when the shock occurs in the x-axis denominated year

- Non-tradable Output
- Non-tradable Prices
- Non-tradable Employment
- Rental Rate
- Log Four Change (100)
- Tradable Employment
- Tradable Output
- Tradable Prices
- Wages

Year

1980  1990  2000
Figure 4: How Employment Effects Vary with the Minimum Wage Share

Panel 1

Panel 2
Figure 5: Baseline Specification

Figure 6: Baseline with State and Time FEs
Figure 7: Bartik Specification

Shift-Share Control (Baseline)

Shift-Share Control (State+Time FEs)

Figure 8: Other Controls

Per-Capita Income

Per-Capita Deposits

Log Per-Capita Income

Log Per-Capita Deposits
Figure 9: IV Specification

![IV Effects of Monetary Policy Shock Interaction Coefficient](image)

Figure 10: VAR Shocks Specification

![Effects of VAR Monetary Policy Shock Interaction Coefficient](image)
Figure 11: Canada Specification

Effects of Canadian Monetary Policy Shock
Interaction Coefficient

Figure 12: Within-State County-Level Specification

Effects of Monetary Policy Shock
Within-State County-Level Specification
Interaction Coefficient
Figure 13: Triple-Differences Specification

![Graph showing empirical growth rate in different thresholds with 25% and 50% thresholds.]

Figure 14: Baseline Specification for Tradable and Non-Tradable Employment

![Graph showing empirical growth rate in tradable and non-tradable employment with timelines for each sector.]
Figure 15: Tradables/Non-Tradables Triple-Differences Specification
Appendix

A.1 Existence of Steady State

Steady state in our model requires constant real variables. We show that there exists a steady state with a constant growth rate of the money supply. This steady state requires that the exogenous minimum wages in each states grow at the same rate as the money supply. We start with the intertemporal Euler equation, which says

$$ \dot{c}_t = \frac{1}{y} \left( \frac{R_{st}}{P_{st}} - (\delta + \rho) \right). $$

For all $s$ and for all $t$ in steady state, it must therefore hold that

$$ \frac{R_{st}}{P_{st}} = \delta + \rho. $$

Thus, in each state $s$, $R$ and $P$ must change at the same rate in steady state. The intratemporal Euler equation is

$$ U'(c_t) \frac{W_{s,t}}{P_{s,t}} = V'(H_{s,t}) $$

This tells us that in each state $s$, $W$ and $P$ must change at the same rate in steady state. Note now that the consumer substitution equation in steady state,

$$ \dot{y}_{s,t}^{NT} - \dot{y}_{s,t}^T = 0 = -\sigma_{NT,T}(\dot{p}_{s,t}^{NT} - \dot{p}_t^T), $$

tells us that all non-tradable prices must grow at the same rate as the national tradable price in steady state. This in turn tells us that the state price indices grow at the same rate in all states, since

$$ \dot{p}_{s,t} = \frac{GDP_s^{NT}}{GDP_s} \dot{p}_{s,t}^{NT} + \frac{GDP_s^T}{GDP_s} \dot{p}_t^T. $$

Our above analysis then yields that $R$ and $W$ also grow at the same rate in all states, the same rate as the state price indices. This rate of price growth is given by the rate of money growth, since

$$ \dot{m}_t = \frac{GDP_t}{GDP_t} \dot{p}_t. $$

Now the profit maximization equations of the tradable and non-tradable sectors tell us that it must be that minimum wages grow at the same rate in all states, a rate that is given by the rate of money growth.
growth.

A.2 Additional Plots for Calibrations

Below we show how our computed labor share at the national level compares to the BLS share. Adjusting for proprietor’s income as the BLS suggests brings our calculation much closer to theirs.

Below, we compute our labor shares using the BLS methodology at the state level, separately for tradables and non-tradables. Shares are much lower in the tradable sector.

Below we show the minimum wage cost shares at the national level. These are substantially lower
than the share of total employment at the minimum wage, since these shares are weighed down both by the fact that minimum wage workers earn less than other workers and by the labor share.

A.3 Additional Model Outcomes

Below, we show all model plots in the paper for an alternative specification, \( \sigma_z = .1 \):
Peak Effect over 4 Years of a 1pp Unexpected Increase in the Federal Funds Rate when the shock occurs in the x-axis denominated year.
Peak Effect over 4 Years of a 1pp Unexpected Increase in the Federal Funds Rate
when the shock occurs in the x-axis denominated year

Non-tradable Output

Non-tradable Prices

Non-tradable Employment

Rental Rate

 Tradable Employment

Tradable Output

Tradable Prices

Wages

Log Point Change (basis 100)