Policy Shocks and Wage Rigidities: Empirical Evidence from Regional Effects of National Shocks*

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Abstract

This paper studies the effect of wage rigidities on the transmission of fiscal and monetary policy shocks. We calculate downward wage rigidities across U.S. states using the Current Population Survey. These estimates are used to explain differences in the state-level economic effects of identical national shocks in interest rates and military expenditure. In line with the role of sticky wages in New Keynesian models, wage rigidities significantly influence the transmission of both monetary and fiscal policy shocks. We find that contractionary monetary policy shocks increase unemployment and decrease economic activity in rigid states considerably more than in flexible states. Similarly, fiscal spending multipliers are larger for states with higher wage rigidities.

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JEL classification: E52, E62, J30.

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1. Introduction

Empirical research has shown that shocks in monetary policy have persistent effects on output and employment, while estimates of fiscal spending multipliers often exceed unity. The exact transmission processes of these shocks continue to be debated. However, a majority of macroeconomic models assign a prominent role to rigidities in wages and prices. In New Keynesian models, imperfect price adjustment after demand or interest rate shocks creates short-run disequilibria with aggregate demand above or below equilibrium, while sticky wages create periods of unemployment or labor market tightness. Hence, shocks in monetary policy or fiscal spending persistently affect the real economy. In these models, higher price or wage rigidities cause both a higher effect upon impact, as well a more persistent effects of these shocks.[1] Empirical evidence in support of the role that such rigidities play as a transmission channel of policy shocks has, however, remained surprisingly scarce.

In this paper, we empirically assess the relationship between downward wage rigidities in U.S. states and the effect of national policy shocks between 1980 and 2007.[2] Based on the role played by wage rigidities in New Keynesian models, we hypothesize that equal shocks in monetary and fiscal policy have more pronounced effects in states with high rigidities. We expect a lack of wage cuts in rigid states to create greater unemployment and output loss when policy shocks reduce economic activity. We test this hypothesis using data on shocks in the federal funds rate (FFR) and exogenous changes in military expenditure. Romer and Romer (2004) calculate shocks based on a narrative approach of intended policy changes, where they isolate FFR changes not driven by developments in the Federal Reserve's internal forecasts. We also crosscheck these results using announcement shocks (see Gertler and Karadi, 2015; Gorodnichenko and Weber, 2016). For fiscal policy, we estimate rigidity-contingent multipliers of state-level changes in military expenditure (Nakamura and Steinsson, 2014). We expect that states with high rigidities exhibit a higher impact of policy shocks. The inability to adjust wages creates a temporary equilibrium with excessive demand driving increased output and employment. Recently, real wages even grew in recessions (see, e.g., Daly and Hobijn, 2014; Abbritti and Fahr, 2013).[3] We instrument state-level changes in military spending by changes in national military spending along the lines of Nakamura and Steinsson (2014) to isolate exogenous variation in spending levels.

By considering differences in the impact of national shocks across states, we exploit three favorable characteristics of the United States. First, the United States forms a fiscal and monetary union. Hence, states experience identical national shocks in economic policy (an FFR increase is identical in, e.g., California and Delaware), allowing the previously described shocks to be used. Second, within a monetary union, exchange rates do not form an automatic stabilizer across states such

[1] For details of effects of monetary and fiscal shocks on employment and output, conditional on the degree of wage rigidities, using Smets and Wouters (2007) model see appendix B.

[2] Wage rigidities are used instead of price rigidities, because evidence suggests the latter rigidities are driven by the former rigidities (e.g., Dhyne et al., 2005; for some discussion see also Christiano et al., 2005). Intuitively, wage rigidities create slow marginal cost adjustment, which translates to sluggish adjustment of marked-up prices.

[3] Also upward adjustments in prices and wages are slower after increases in aggregate demand in states with higher downward wage rigidities. The main reason for that is the inability to lower wages in recessions also limits wage increases in expansions (Elsby, 2009; Akerlof et al., 2000).

that *real depreciation* through price adjustments has an effect on economic activity. Third, states are similar from a legislative and institutional perspective, which makes our analysis less sensitive to omitted variable bias than a cross-country comparison. In addition, micro data on wages is collected in exactly the same way for all states. The Current Population Survey (CPS) provides 1.38 million observations of wage changes between 1979 and 2014, which are used to estimate downward wage rigidities by state, measured through resistance to wage cuts.

We provide evidence of considerable variation in downward *nominal* wage rigidities across states and over time. Our estimates of nominal rigidities are positively related to state minimum wages, unionization, union bargaining power, and the size of services and government in employment and negatively to labor mobility. There is little to no evidence of downward *real* wage rigidities in the United States. We therefore focus on nominal wage rigidities when assessing the transmission of policy shocks. We find that states with greater downward nominal wage rigidities experience larger increases in unemployment and declines in output after monetary policy shocks. This relationship is unveiled using local projection models and System GMM estimations, with various dependent variables (unemployment, coincidence index, and state-level GDP). This result is robust to the use of various outlier treatments as well as comprehensive controls for labor market institutions and sectoral composition. The analysis of military expenditures also suggests that fiscal multipliers depend on the degree of wage rigidities. In our flexible state, which we define as a hypothetical state that is equal to to the most flexible state in the United States in each year, the fiscal multipliers are close to 0, while they exceed 2 for our rigid state, which is defined analogously as a hypothetical state that is equal to the most rigid state in the United States in each year. Thus, rigid and flexible states should be viewed as an upper and lower bound of plausible responses for U.S. states. Combined, these results firmly corroborate the hypothesis that resistance to wage cuts deepens policy shocks.

Although wage rigidities are a standard feature in the DSGE literature, the relationship between wages and the real effect of nominal shocks is empirically assessed only in a few papers. Cross-country comparisons are found in work on the Great Depression, which, according to Friedman and Schwartz (1963), was driven by a monetary shock. Bernanke (1995) finds a negative relationship between nominal wage reduction and output loss in countries on the gold standard. His analysis builds on a similar premise because the gold standard was a system of fixed exchange rates, resembling a monetary union. Bernanke and Carey (1996) find a significantly negative relationship between real wages and industrial production using panel data on 22 countries. A more recent cross-country study by Blanchard and Wolfers (2000) examines the evolution and heterogeneity in unemployment across European countries. They document that the interaction between shocks and rigid labor market institutions helps to explain hysteresis in unemployment. Similarly, Gnocchi et al. (2015) find a negative relationship between business cycle severity and episodes of labor market reforms in OECD countries. Bauer et al. (2007) study the relationship between regional differences in wage rigidities and inflation across West German regions and conclude that incidences of wage rigidities accelerate unemployment growth. However, they point out that this effect on unemployment growth is minimized in a moderate inflation environment.[4] Direct evidence on the relationship between rigidities and policy shocks is provided in Gorodnichenko and Weber (2016) and Pischke (2016). Gorodnichenko and Weber (2016) document that after monetary policy announcements, firms with stickier prices exhibit greater unconditional volatility of stock market returns than firms with more flexible prices.[5] Pischke (2016) compares the employment reactions of real estate agents, architects, and construction workers—groups with very different wage-setting institutions—to the housing cycle shocks that serve as a proxy for a demand shock. The employment of real estate agents, whose wages are the most flexible among the three groups, indeed reacts less to the cycle than employment in the other two groups. Our paper contributes to this literature by providing evidence on the relationship between wage rigidities and the impact of policy shocks on economic activity, exclusively relying on reduced-form estimates.[6]

More generally, this paper builds on papers that study the effects of monetary and fiscal policy shocks in the United States. Romer and Romer (2004) find large effects of monetary policy on output and prices using deviations in FFR changes from standard responses to internal forecasts. Coibion (2012) has recently revisited these effects and concludes that they are consistent with the real effect of shocks derived from Taylor rules. In fact, Olivei and Tenreyro (2007) conjecture the importance of the effect of wage rigidities on the impact of monetary shocks. Olivei and Tenreyro (2007) estimate impulse response functions (IRFs) for monetary shocks in the United States occurring in the first or last two quarters of the year. They report that shocks in the last quarters have much smaller real effects than shocks occurring in the first quarters and hypothesize that wage setting at the end of calendar years explains this finding.[7] Recently, several papers have focused on fiscal multipliers. Nakamura and Steinsson (2014), for instance, show that these multipliers exceed unity after exogenous shocks in military expenditure. Others have shown that multipliers are state dependent: they are larger in recessions than in expansions (e.g., Auerbach and Gorodnichenko, 2012; Ramey and Zubairy, 2014; Michaillat, 2014).[8]

[4] In a short paper, Daly and Hobijn (2015) look at the effect of different wage rigidities on the industry-specific slope of the Phillips curve and find significant differences. In particular, industries with most downwardly rigid wages experienced the slowest wage growth in the recent recovery.

[5] Favilukis and Lin (2016) study the relationship between sticky wages and risk in an asset-pricing framework.

[6] Using a data set on immigrant workers Guriev et al. (2016) compare wage adjustments during the recent crisis in regulated and unregulated labor markets in Italy. They find that wages adjusted only in the informal sector while the employment shifted from formal to informal due to regulatory obstacles to adjust wages in the formal sector. Other papers that study the macroeconomic significance of wage rigidities include, e.g., Card and Hyslop (1997), Lebow et al. (2003), Nickell and Quintini (2003), Fehr and Goette (2007), Elsby (2009), Abbritti and Fahr (2013), Kaur (2014), and Daly and Hobijn (2014).

[7] Olivei and Tenreyro (2010) compare impulse responses to monetary shocks in quarters before and after periods of highly synchronized wage setting in Japan and Germany. Results were similar. For more general results on asymmetric effects of monetary policy over the business cycle, see Santoro et al. (2014), Matthes and Barnichon (2015), and Tenreyro and Thwaites (2016) for details.

[8] Baxter and King (1993) show that fiscal multipliers depend on whether fiscal intervention is permanent or transitory. Several authors have emphasized that the multipliers depend on the monetary policy; see, e.g., Christiano et al. (2011a). Boehm (2015) also shows that the multiplier depends on the type of goods a government purchases; for nondurable goods it exceeds unity, while for durables it is below unity.

Additionally, we contribute to the literature by extending evidence of nominal wage rigidities in the United States to the state level.[9] Our estimates are in line with studies observing greater rigidities in job-stayers than in job-changers (e.g., Devereux and Hart, 2006; Haefke et al., 2013), as states with high rates of job destruction and creation have lower rigidity. Similarly, state-level findings confirm cross-country evidence on the positive correlation between wage rigidities and institutions that affect wage bargaining, like unionization and employment protective legislation (e.g., Dickens et al., 2007; Alvarez et al., 2006).

Lastly, this paper is related to the literature studying regional business cycle differences across U.S. states. One of the problems researchers face when studying regional business cycles is the lack of indicators of economic activity at any frequency that is higher than annual. The Philadelphia Fed provides an alternative by computing the Coincidence Index (CI) for U.S. states as a monthly indicator of economic activity, which is described in Crone and Clayton-Matthews (2005). Examples of papers that focus on regional cycles include Blanchard and Katz (1992), which studies the behavior of wages and employment over regional cycles, and Driscoll (2004), which details the effect of bank lending on output across U.S. states.

The remainder of this paper is structured as follows. Section 2 presents the empirical strategy used to relate rigidity to the impact of monetary and fiscal policy shocks. Section 3 provides estimates of downward wage rigidity at the state level. Results and robustness checks are presented in section 4. Section 5 concludes.

2. Empirical Methodology

This section explains the empirical strategy used to relate variation in wage rigidity to the state-level impact of national policy shocks. Strategies regarding monetary and fiscal policy are discussed in sections 2.1 and 2.2, respectively. The selection of control variables is discussed in section 2.3.

2.1. Monetary Policy Shocks

Shocks in monetary policy provide nationwide disturbances identical across states; therefore, differences in their impact have to be related to state-specific factors.[10] To assess the premise that wage rigidities are such a factor, we estimate the effect of monetary policy shocks on state-level unemployment and the coincident index (CI), conditional on wage rigidity. CI is a composite variable for state-level economic activity based on four indicators: nonfarm employment, average hours worked in manufacturing, unemployment, and salary disbursements. The trend growth of the index is equalized to annual state-GDP growth—higher values of CI thus imply greater economic activity (for details see Crone and Clayton-Matthews, 2005). Unemployment and CI are two of few real state-level variables available at a monthly frequency, as industrial production and state GDP are

[9]Influential national studies include, e.g., Blinder and Choi (1990), Kahn (1997), Campbell and Kamlani (1997), Card and Hyslop (1997), Altonji and Devereux (1999), Fehr and Goette (2007), and Dickens et al. (2007).

[10]For employment and output response conditional on the degree of wage rigidities in the Smets and Wouters (2007) model see figure A.4 in appendix B.

measured annually.[11] This lower frequency renders them less useful in assessing the short-run impact of monetary policy shocks. If wage rigidities have the predicted effect on the impact of shocks, the absence of wage cuts in rigid states increases unemployment and decreases CI more strongly.

Monetary policy shocks are obtained from Romer and Romer (2004). Because changes in policy rates are endogenous to macroeconomic forecasts, Romer and Romer (2004) estimate these shocks in two steps. First, intended changes in the FFR are derived from narrative records of internal briefings to the FOMC. Second, predicted developments in interest rates are regressed on changes in the Federal Reserve's Greenbook forecasts to derive a typical response function. Deviations from this function are used as policy shocks. We use the data from Coibion (2012), which extend the original series through the end of 2007.[12] We rely on Romer and Romer's (2004) shocks because they impose the least possible amount of structure. Shocks from vector auto regression models (VARs), suffer from two shortcomings. First, VARs impose structure on the identification of shocks, for instance, through short- and long-term, or sign restrictions. Second, VARs may not adequately capture the forecast-dependence of decisions on policy rates, which as Coibion (2012) shows may lead to underestimation of the effect of monetary policy shocks. In addition, we crosscheck our results using announcement shocks from Gertler and Karadi (2015) and Gorodnichenko and Weber (2016), which are available for a shorter time span. The upper part of table 1 presents summary statistics for these variables.

To estimate the relationship between wage rigidities and the impact of monetary policy shocks, we employ the local projections method (Jordà, 2005). Local projections estimate impulse response profiles using separate regressions for each lead over the forecast horizon. Responses therefore do not rely on the nonlinear transformations of reduced-form parameters as in VARs. The effect of policy shocks at $t+h$ is estimated by regressing dependent variables at $t+h$ on shocks and covariates at time t. Following Auerbach and Gorodnichenko (2012) and Ramey and Zubairy (2014), we use a model that resembles the smooth transition local projection model to allow for inference in both rigid and flexible states:

$$y_{s,t+h} = F(z_{s,t})\big(\alpha_h^R + \beta_h^{R'} x_{s,t} + \gamma_h^R i_{s,t}\big) + \big(1 - F(z_{s,t})\big)\big(\alpha_h^F + \beta_h^{F'} x_{s,t} + \gamma_h^F i_{s,t}\big) + \phi_h^{'} c_{s,t} + \eta_{s,t+h}, \tag{1}$$

where subscripts refer to state s at time t, y is our variable of interest, either unemployment rate (UR), or coincidence index (CI). i denotes shocks in monetary policy, x is a vector of controls, and $c_{s,t}$ is a vector of deterministic covariates, including state fixed effects and a linear trend, respectively. z is our measure of wage rigidity, transformed along function $F(z)$, which ranges between 0 (for states with lowest rigidity) and 1 (for states with highest rigidity). Details are provided in section 4.1. The effect of shocks on unemployment and CI is captured by γ, where γ^R measures the effect in rigid state while γ^F measures the effect in flexible state. Our hypothesis implies that the absolute

[11] The BEA has recently released quarterly state-GDP data but only from 2005 onwards.

[12] The resulting shocks are plotted in figure A.1 in the appendix A. The left figure graphs historic FFR changes, while the right figure plots shocks. Data on shocks are available at a monthly frequency from 1966 to 2009, although alternatively, the availability of rigidity measures, the Volcker Disinflation, and the financial crisis in 2008 restrict our sample to either 1980–2007 or 1983–2007.

value of γ^R should exceed γ^F. Equation 1 is estimated separately for each h using least squares. Hence, the specification of T has no influence on estimates at other points on the horizon. As noted by da Rocha and Solomou (2015) and Furceri and Zdzienicka (2012), this feature marks an important advantage of local projections over auto regressive distributed lag (ARDL) models. ARDL models estimate coefficients over the forecast horizon jointly, yielding misspecification in case of nonlinearity. This advantage has made local projections an increasingly popular alternative to VARs or ARDLs.[13] Its use has, however, also been subject to criticism—Kilian and Kim (2011), for instance, note that the small sample bias of local projections is larger than in standard VARs. Similarly, Teulings and Zubanov (2014) note that local projections fail to incorporate shocks occurring after period t that affect unemployment at $t + h$, creating downward bias. This bias is limited in our case because shocks are both positive and negative and autocorrelation is low.

2.2. Fiscal Policy Shocks

The relationship between wage rigidity and the impact of fiscal shocks is assessed by estimating spending multipliers conditional on wage rigidities. Multipliers quantify the real effect of spending changes and are therefore expected to be larger in rigid states.[14] We use shocks in defense spending to estimate multipliers, in line with previous work.[15] To assure defense spending is not driven by local economic performance, the literature often relies on the effect of large wars. This approach is complicated by the rarity of wars, as well as their joint occurrence with tax increases and political instability. To prevent these issues, we follow Nakamura and Steinsson (2014) and instrument changes in state-level defense spending by changes in national spending. Under the assumption that the United States does not alter *national* spending in response to the *relative* performance of states, this identification isolates exogenous variation in state-level spending. Because federal spending affects states in different degrees, the interaction of a state dummy and national spending changes forms an exogenous change in state-level spending. The following equation presents the first stage:

$$\Delta\mu_{s,t} = \phi_s + \beta_2 \Delta\mu_t^{nat} + \beta_3 \phi_s \cdot \Delta\mu_t^{nat} + \nu_{s,t}, \tag{2}$$

where μ and μ^{nat} denote biannual changes in state and federal military spending, respectively, as a percentage of GDP. We use fitted values from this equation to estimate a multiplier of fiscal spending referred to as the *open economy relative multiplier* (henceforth ”multiplier”), which quantifies increases in state GDP relative to others after increases in military expenditure. Similarly to Eq. (2), we estimate multipliers conditional on wage rigidity along:

$$\Delta y_{s,t} = F(z_{s,t})\big(\alpha_h^R + \beta_h^{R'} x_{s,t} + \gamma_h^R \Delta\widehat{\mu}_{s,t}\big) + \big(1 - F(z_{s,t})\big)\big(\alpha_h^F + \beta_h^{F'} x_{s,t} + \gamma_h^F \Delta\widehat{\mu}_{s,t}\big) + \phi_h^{'} c_{s,t} + \eta_{s,t}, \tag{3}$$

[13] See, e.g., Ho (2008), Furceri and Zdzienicka (2012), Jordà et al. (2013), and da Rocha and Solomou (2015).

[14] Intuitively, sluggish wage convergence drives slow adjustment of marked-up prices to equilibrium, creating excess demand. Short-run output growth should therefore be larger.

[15] Examples include Burnside et al. (2004), Barro and Redlick (2011), and Nakamura and Steinsson (2014). Alternatively, Romer and Romer (2010) use tax policy changes.

where Δy measures biannual growth in state GDP while $\widehat{\mu}$ denotes the fitted value of Eq. (2), such that γ^R and γ^F capture the multipliers. If γ^R exceeds γ^F, multipliers are larger in rigid states, as hypothesized.

Data on real state GDP are obtained from the Bureau of Economic Analysis (BEA). Two variables on military spending are obtained from Nakamura and Steinsson (2014) up to 2007. The first includes prime military procurement, counting all purchases valued over $25,000. The second is a broader measure including direct financial compensation to employees. The middle part of table 1 provides summary statistics.

Table 1: Monetary Policy Shock and Fiscal Policy Shock Data Summary Statistics

	Mean	SD	Obs.	Min.	Max.	Source	Type
Dependent Variables							
Unemployment Rate	5.832	5.832	17,136	2.1	18.8	BLS	
Coincident Index	110.625	110.625	16,800	57.527	232.740	Phil. Fed	
Biannual State GDP Growth	5.384	5.148	1478	-12.82	33.50	BEA	
Monetary policy shocks							
Narrative: Monetary policy shocks	0.013	0.297	384	-3.259	1.885	RR (2004)	
Anncmt: tight window	-0.010	0.068	191	-0.438	0.163	GW (2016)	
Anncmt: wide window	-0.010	0.069	191	-0.463	0.152	GW (2016)	
Anncmt: current FFR futures	-0.017	0.062	257	-0.423	0.146	GK (2015)	
Anncmt: 3-month ahead FFR futures	-0.015	0.051	243	-0.290	0.092	GK (2015)	
Anncmt: year-ahead fut. Eurodollar dep.	-0.011	0.058	315	-0.381	0.213	GK (2015)	
Fiscal policy shocks							
Growth in Prime Military Sp. - State	0.024	0.023	1478	-5.111	3.979	NS (2014)	
Growth in Broad Military Sp. - State	0.027	0.026	1478	-5.092	3.991	NS (2014)	
Growth in Prime Military Sp. - National	0.004	0.003	29	-0.449	0.687	NS (2014)	
Growth in Broad Military Sp. - National	0.009	0.008	29	-0.534	0.808	NS (2014)	
Control Variables							
Mobility	0.287	0.046	1836	0.184	0.694	CBS	I(1)
Firm Size	18.75	3.240	1836	10.36	29.32	CBS	I(1)
Minimum Wage	0.424	0.062	1683	0.257	0.670	BLS	I(0)
Unionization	0.144	0.064	1224	0.008	0.348	CPS	I(0)
Union Power	0.562	0.496	1938	0	1	C (2014)	I(0)
% Services	0.684	0.051	1734	0.543	0.822	CPS	I(1)
% Government	0.056	0.027	1734	0.024	0.233	CPS	I(1)
Education	4.058	0.226	1734	3.000	4.547	CPS	I(1)

Note: RR (2004) stands for Romer and Romer (2004); NS (2014) stands for Nakamura and Steinsson (2014); GW (2016) stands for Gorodnichenko and Weber (2016); GK (2015) stands for Gertler and Karadi (2015); and C (2014) stands for Collins (2014).

2.3. Control Variables

We include control variables that influence both wage rigidities and the impact of policy shocks to the above specifications. Literature on labor market institutions and sectoral correlation provide relevant candidates; examples include contractual agreements (Taylor, 1983), employee bargaining power (Holden, 1994; Hall, 2005; and Christoffel and Linzert, 2006) and fear of motivational repercussion (Shapiro and Stiglitz, 1984; Akerlof and Yellen, 1990). These theories apply mainly to large firms, as monitoring costs increase with the number of employees (see Bewley, 1999). Based on this evidence, we add controls for labor mobility, firm size, unionization, union power, and minimum wages.[16] Mobility is measured through the reallocation rate, which is the sum of job destruction and job creation rates. Firm size is measured through the average number of employees per firm. All of these data are obtained from Business Dynamics Statistics (BDS) at the Census Bureau. Collins (2014) provides data on union power, which is defined by the absence of *right-to-work* laws in a state.[17] To account for differences in state minimum wages, the ratio of minimum to median wages is added. Data are from the CPS and BLS.

Other control variables relate to the structure of the economy. The share of workers employed by the government is included because government expenditures are relatively insensitive to shocks, while the share of workers employed in services is included to control for sectoral composition: Certain industries may traditionally be more subject to wage rigidities and be more vulnerable to demand fluctuations.[18] We include average education for similar purpose, measured along CPS classifications. The lower part of table 1 details the summary statistics for control variables.

3. Data on Wage Rigidity

The strategy outlined in section 2 requires annual estimates of rigidities by state. These are obtained by quantifying distributional characteristics of microdata on wages. The procedure followed in the next subsection is similar to Dickens et al. (2007). An introduction to the microdata is provided in section 3.1. Section 3.2 presents measures used to quantify wage rigidities, and section 3.3 discusses the correlation of rigidities with labor market institutions.

3.1. Microdata

Microdata are taken from the CPS. The CPS is a monthly survey organized jointly by the BLS and Census Bureau, and is used to estimate unemployment rates and labor force participation. The data set contains information on over 140,000 individuals per year between 1979 and 2014, making it the largest survey data set available for the United States.[19] Members of selected households are legally required to respond to monthly inquiries for a total of eight months. These months are divided into two cycles. The first cycle takes four months, after which all household members leave the sample for eight months. A second four-month cycle follows, after which households leave the sample entirely. Individual wage data are collected during the final month of each cycle, known as outgoing rotation.

[16] The number of control variables is obtained at annual frequency. Monthly estimates are obtained through interpolation; however, the results are not affected if we use in the estimation the same value within a year.

[17] Right-to-work laws enable firms in unionized sectors to employ non-union workers on non-union contracts, which strongly reduces a union's bargaining power. Union power is measured as a dummy equaling 1 in states without these laws.

[18] Manufacturing industries may, for instance, be more unionized and suffer deeper shocks due to the postponed consumption of durable goods (e.g., Mian et al., 2013). The Wage Rigidity Meter at the San Francisco Fed reports nominal wage rigidities using the same data set by educational attainment, by groups of industries, and by type of pay.

[19] The Panel Study of Income Dynamics (PSID) is a commonly used alternative, but it is too small for estimation of wage rigidity at the state level (it contains 60,000 individuals over the entire sample). Employer data would represent a valid alternative, but it is not publicly accessible.

To calculate wage changes, we calculate the difference between the logarithm of hourly wages at the end of the first and second cycles. Because household compositions change over time, we deploy an algorithm developed by Madrian and Lefgren (1999) to validate panel matches. Based on changes across time in age, education, race, and gender, we exclude observations that are unlikely to represent the same person. From the remaining sample we drop individuals without a reported wage in either period as well as those with absolute log-changes greater than 0.5.[20] Data from 1985 and 1996 are also dropped because most observations lack necessary panel identifiers. The remaining sample contains data on 1.37 million Americans, yielding an average of 838 observations per state per year. Summary statistics are provided in table 2. The distribution of wage changes is displayed in figure 1. The histogram of nominal changes shows a characteristic spike in the distribution around 0—that is, a disproportional number of employees endure wage freezes. The distribution is also asymmetric, in the sense that wage cuts occur less frequently than wage increases.

Table 2: CPS Microdata Summary

Variable	Mean	St. Dev.	Obs.	Min	Max	Type
Female	0.490	0.500	1,367,621	0	1	Dummy
Age	39.37	12.79	1,367,621	16	98	Discrete
Married	0.670	0.470	1,367,621	0	1	Dummy
White	0.870	0.339	1,367,621	0	1	Dummy
Wage, log change	0.040	0.200	1,367,621	-0.49	0.49	Continuous
Usual hours worked	38.80	9.010	1,342,057	0	99	Discrete
Paid hourly	0.380	0.490	1,367,621	0	1	Dummy

The large number of nominal freezes is in line with the idea that firms are hesitant to cut wages when needed. Somewhat more surprising is the frequency with which large wage changes occur. Although most changes are small, shifts of 40 to 50% are not uncommon. These shifts are likely due to the inclusion of job-changers in the CPS, which may result in an underestimation of wage rigidities. As our interest lies in relative rigidities across states, this identification is unlikely to bias our results.[21] Omitted variable bias may exist if job changes occur more frequently in states that suffer deep impacts from policy shocks, although such bias would affect our results downwards.

3.2. Measures of Downward Nominal Wage Rigidities

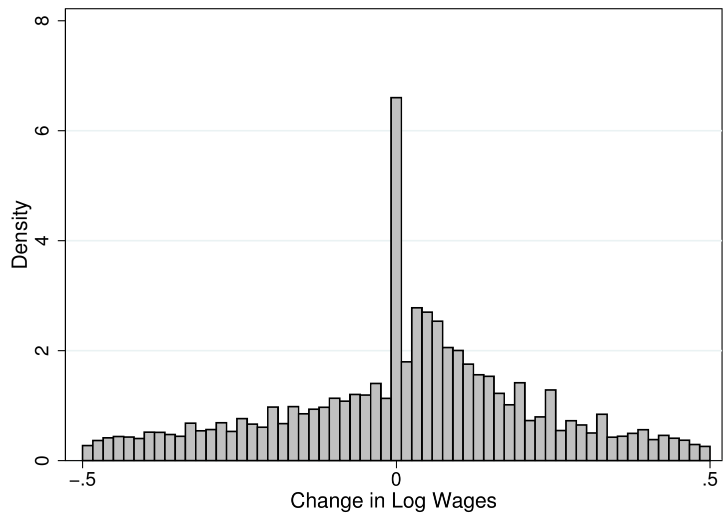
We calculate the Fraction of Wage Cuts Prevented ($FWCP$) to obtain yearly estimates of downward nominal wage rigidity by state. $FWCP$ compares the number of observations with nominal wage freezes to the number with nominal wage cuts in the sample. Under the assumption that freezes represent prevented wage cuts, $FWCP$ therefore captures the fraction of wage cuts prevented through wage rigidities. Formally,

$$FWCP_{s,t}^{n} = \frac{f_{s,t}^{n}}{c_{s,t}^{n} + f_{s,t}^{n}}, \tag{4}$$

[20] Correlations with rigidity measures using truncation between 0.4 and 0.6 exceed 0.99. Details are in appendix A.

[21] We explore robustness with respect to this assumption later in the paper.

Figure 1. Distribution of Wage Changes in CPS Microdata: 1980-2014



where f^n and c^n count the number of nominal freezes and nominal wage cuts, respectively. Higher values of $FWCP^n$ mean a greater share of wage cuts are prevented and thus represent higher degrees of downward wage rigidities. $FWCP^n$ is an increasingly popular measure of wage rigidities—it is central in estimations by the International Wage Flexibility Project (Dickens et al., 2007), and has since been used by, e.g., Holden and Wulfsberg (2008), Dias et al. (2013), and Centeno and Novo (2012).

Several measures of downward nominal wage rigidities have been proposed in the literature. Many of these measures rely on regression analysis and thus are less appropriate for use in our paper, as they are coupled with uncertainty and depend on an imposed specification. A downside of $FWCP^n$ is its sensitivity to measurement error in wage changes. Because we rely on survey data, exactly equal wages are unlikely to be reported in both cycles, resulting in underestimation of $FWCP^n$. We moderate this issue by classifying absolute wage changes smaller than 0.005 log change as freezes. Note that $FWCP^n$ implicitly assumes absent real rigidity. If workers suffer from real rigidity, real wage freezes at positive inflation levels disguise nominal freezes. Hence, $FWCP^n$ estimates apply only to the subset of observations not experiencing downward real rigidity. Real wage rigidities are unlikely to cause bias, as, in line with Dickens et al. (2007), we find little to no evidence of real wage rigidities. Analysis of real rigidities in the United States can be found in appendix C.

Table 3: Average Nominal Wage Rigidities by State

Average	*0.1949*	KY	0.1954	OH	0.1967
AL	0.1918	LA	0.1865	OK	0.1965
AK	0.1992	ME	0.2153***	OR	0.2025
AZ	0.1915	MD	0.1717***	PA	0.1954
AR	0.2031	MA	0.1886	RI	0.2046*
CA	0.1963	MI	0.2031	SC	0.1858*
CO	0.1969	MN	0.2034	SD	0.2063**
CT	0.1832**	MS	0.2056**	TN	0.1944
DE	0.1636***	MO	0.1851*	TX	0.1972
DC	0.1512***	MT	0.2200***	UT	0.2027
FL	0.1899	NE	0.2055**	VT	0.2107***
GA	0.1743***	NV	0.1945	VA	0.1834**
HI	0.1981	NH	0.1929	WA	0.1992
ID	0.2080**	NJ	0.1740***	WV	0.2009
IL	0.1824**	NM	0.1998	WI	0.2087**
IN	0.1911	NY	0.1749***	WY	0.2117***
IA	0.2001	NC	0.1831**		
KS	0.2029	ND	0.2128***		

*Note: *, ** and *** denote significance from average at the 10, 5, and 1% significance level, respectively. Estimates obtained using a mean-comparison t-test, two-sided.*

Table 3 reports average rigidities by state, from which a number of results stand out. Average nominal rigidity is 0.1918, which is lower than estimated in Dickens et al. (2007).[22] It implies that around 19% of attempted wage cuts were prevented by nominal rigidity. Second, states exhibit significantly different levels of wage rigidities. Results show that average $FWCP^n$ differs significantly from national average in the 23 states.

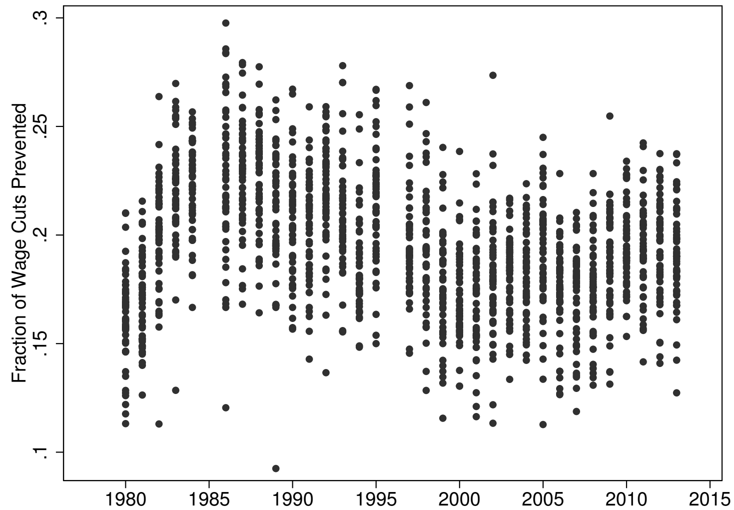
Figure 2 provides insight into the development of nominal rigidities over time. Nominal rigidities are lower in the early 1980s, reaching highs in the late '80s, steadily decreasing up to 2005, and then slowly increasing again after2005. The estimated AR(1) coefficient is 0.58. Figure 3 presents *heat maps* to get a better idea of the variability across time and states. Most states that are among the more rigid in the first half of the sample are also among the more rigid states in the second half of the sample. Generally, there is no clear division between east and west, although states on the East Coast seem to be slightly more flexible, and states in the north-central part of the United States seem to exhibit higher degrees of downward wage rigidities. However, there is also some variability within states, where, e.g., in the second half of the sample, California became relatively more rigid and Louisiana became relatively less rigid.[23] Because real rigidities seem absent, on average, for most states, the analysis on the effect of rigidities on the impact of policy shocks that follows focuses on nominal rigidities.[24]

[22]This reduction is likely due to the inclusion of job-changers in our sample. Alternatively, job-changers and job-stayers could be distinguished using information on industry of occupation in CPS data. We refrain from this approach because it *i)* is subject to measurement error if industry is misclassified in the first or second cycle and *ii)* does not account for within-industry job-changes. Excluding observations with different industries over time yields a reduction in sample size of 38.4% and increases variance by 28.8%. Nevertheless, the correlation of these estimates of nominal rigidities with our estimates is 0.91.

[23]Average real rigidity is constantly positioned around or below 0, although variance is higher in the mid-1980s. Its AR(1) coefficient is 0.43.

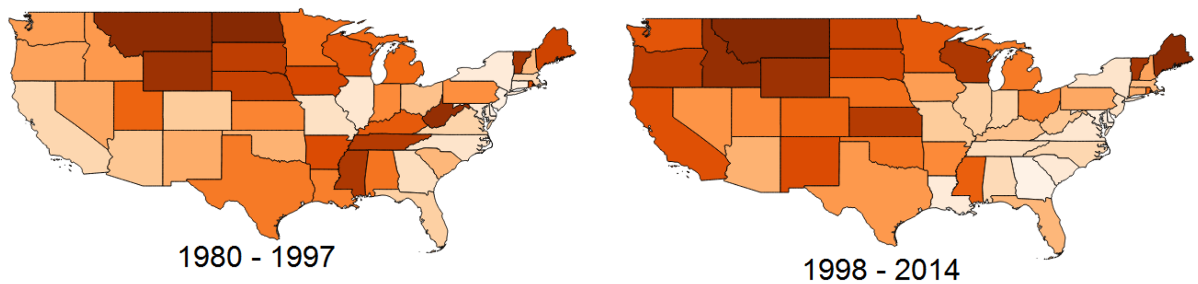
[24]For the monthly analysis of monetary policy shocks, we interpolate rigidity measures.

Figure 2. Distribution of Downward Nominal Wage Rigidities, 1980-2014



Note: 1985 and 1996 are dropped due to missing panel identifiers.

Figure 3. Relative Downward Nominal Rigidities across States



Light: low rigidity; Dark: high rigidity.

3.3. Correlation with Labor Market Institutions

To affirm the validity of our rigidity measures, we verify that correlations with labor market institutions and sectoral composition from section 2 run in the appropriate direction. Table 4 presents regression results using nominal rigidities as dependent variables. Within-panel correlation and heteroskedasticity is corrected using clustered standard errors, while state-fixed effects account for unit-specific time-invariant heterogeneity. Non-stationary explanatory variables, assessed using a Levin et al. (2002) test, are included in first differences. As expected, column 1 shows that nominal rigidities increase with state minimum wages, unionization, and union bargaining power. High worker turnover is associated with lower rigidities. These are highly significant and robust to the inclusion of controls for sectoral composition in columns 3 and 4. Interestingly, column 3 indicates that higher government employment is positively correlated with nominal rigidities. Finally, note the positive correlation of the percentage employed in services with nominal rigidities. These sectoral features are in line with expectations, as worker bargaining power in capital intensive industries is particularly high. Generally, results described in this section are in line with papers that study the determinants of nominal rigidities (e.g., Dickens et al., 2007; Alvarez et al., 2006; and Ehrlich and Montes, 2014).

Table 4: Estimations Labor Market Institutions and Nominal Wage Rigidities

	(1)	(2)	(3)	(4)
Δ Mobility	-0.063***		-0.078***	
	(0.015)		(0.017)	(0.016)
Δ Firm Size	-0.002		0.001	-0.001
	(0.002)		(0.001)	(0.002)
Minimum Wage	0.141***		0.077***	0.138***
	(0.024)		(0.022)	(0.024)
Unionization	0.071***			0.067***
	(0.025)			(0.025)
Union Power	0.010***			0.010***
	(0.002)			(0.002)
Δ % Empl. Serv.		0.202***	0.225***	0.023
		(0.039)	(0.040)	(0.044)
Δ % Empl. Gov.		0.187**	0.186**	0.008
		(0.08)	(0.082)	(0.082)
Δ Education		-0.012	-0.012	-0.047***
		(0.009)	(0.010)	(0.010)
Constant	0.116***	0.196***	0.163***	0.118***
	(0.011)	(0.001)	(0.009)	(0.010)
Observations	1,122	1,581	1,479	1,122
R^2	0.071	0.018	0.042	0.084

*Note: *, **, *** denote significance at the 10, 5, and 1%, respectively. Clustered standard errors (by state) in parentheses. Estimates obtained using Fixed Effects estimators. Non-stationary variables estimated in first difference. Sample: 1980-2013.*

Combined, these results affirm the validity of our measures. Correlations with both labor market institutions and sectoral composition are in line with expectations. Based on results in this section, we proceed with detailing the effect of downward nominal rigidities on economic activity.

4. Estimation Results

This section uses the results on nominal wage rigidities and presents estimation results regarding the effect of wage rigidities on the impact of monetary and fiscal policy shocks. Section 4.1 focuses on impulse response functions of monetary policy shocks using local projections, while section 4.2 details the estimates of conditional fiscal policy multipliers. We use data from January 1980 to December 2007.

4.1. Regressions on Monetary Policy Shocks

We test our hypothesis of a positive correlation between wage rigidities and the impact of shocks in this section. To derive inference separately for rigid and flexible states, it is convenient to transform the measure of wage rigidities to range between 0 and 1. $F(z_{s,t})$ is transformed in two different ways. The first version standardizes $F(z_{s,t})$ such that its lowest value in a given year equals 0 and its highest value equals 1, which is achieved by subtracting the minimum and dividing by the maximum value attained across states each year. We label this as a *standard* transformation. Because the extrema are annual, the resulting variable is stationary, which implies that the estimations capture the effect of having relatively high levels of wage rigidity. As extrema are sensitive to outliers, we also consider a second transformation of this measure:

$$F(z_{s,t-1}) = \frac{exp[\xi \frac{z_{s,t-1}-c_z}{\sigma_z}]}{1+exp[\xi \frac{z_{s,t-1}-c_z}{\sigma_z}]}, \tag{5}$$

where z is the standardized value for wage rigidity and c and σ are its mean and standard deviation, respectively.[25] We label this transformation as a *logit* transformation. ξ measures how much weight we give to outliers.[26] Our impulse responses are plotted for a hypothetical state that is in all years the most flexible state and the most rigid state in our sample. In the logit transformation, we explicitly deal with potential outliers, and thus the interpretation of the flexible and rigid state is closer to the actual behavior of states that are, on average, among the most flexible and the most rigid states. We are mainly interested in the relative effects—the difference between the rigid and flexible states—to test our hypothesis and detail the role of wage rigidities in monetary policy transmission mechanism—that is, it is robust to outlier treatment. Ramey (2016) shows that the impulse responses of output or unemployment to monetary policy shocks have changed over time. Furthermore, they also depend on the exact controls included in the estimation. To show some robustness of our results, we study several different cases and include a variety of different controls and sample sizes.

When estimating Eq. (1), we include the following controls: labor market institutions variables included in the analysis in section 3.3, sectoral composition in states, a lag of monetary policy shock, past level of the FFR, and a lag of dependent variable to account for its persistence. All regressions include state fixed effects to account for state-specific constant factors and a time trend. Standard errors are clustered at the state level to correct for within-panel correlations and heteroskedasticity. Resulting IRFs correspond to a 1 percentage point contractionary shock.

4.1.1. Monetary Policy Shocks: the Romer and Romer (2004) Approach

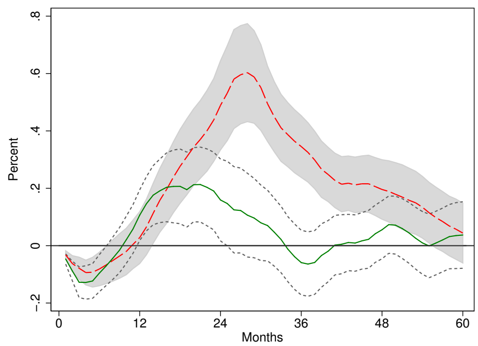
Figure 4 presents the results of monetary policy shocks effects—conditional on wage rigidities—on unemployment (figures 4(a)-(b)), and coincident index (figures 4(c)-(d)) for our full sample (1980 to 2007). It is clear that monetary policy effects are significantly different depending on the degree of wage rigidities.[27] A 1% contractionary shock in the federal funds rate raises unemployment between 0.4 and 0.8 percentage points in the third year after the shock for the rigid state, while the

[25] This follows Auerbach and Gorodnichenko (2012) and Ramey and Zubairy (2014) in their approach to define recessions and expansions.

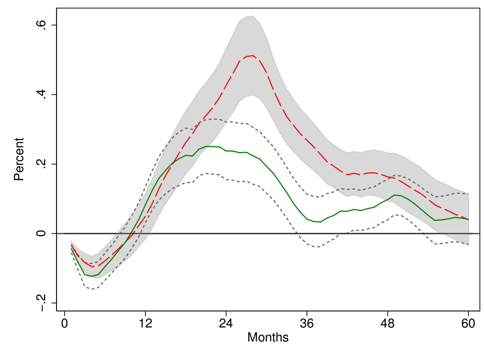
[26] We calibrate ξ to 2, which is in the range considered by Tenreyro and Thwaites (2016) and Auerbach and Gorodnichenko (2012). Note that the interpretation of ξ is different in our case.

[27] For unconditional IRFs obtained using state panel data, see figure A.3 in appendix A.

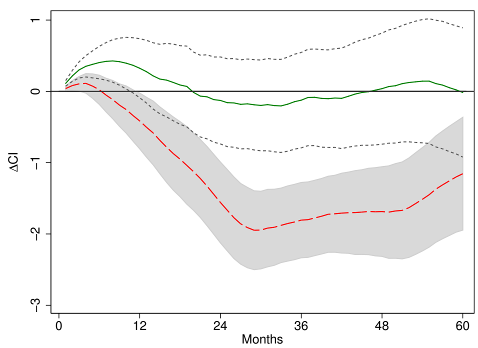
Figure 4. Monetary policy shocks in Rigid and Flexible States: Unemployment and CI, 1980–2007



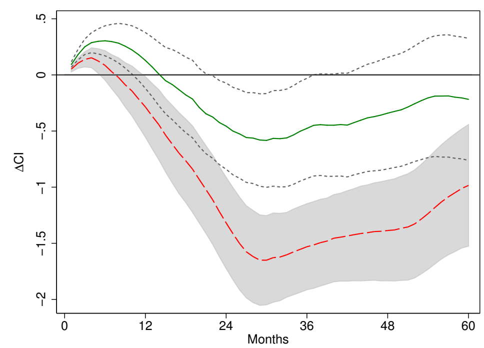
(a) Standard transformation, UR



(b) Logit transformation, UR



(c) Standard transformation, CI



(d) Logit transformation, CI

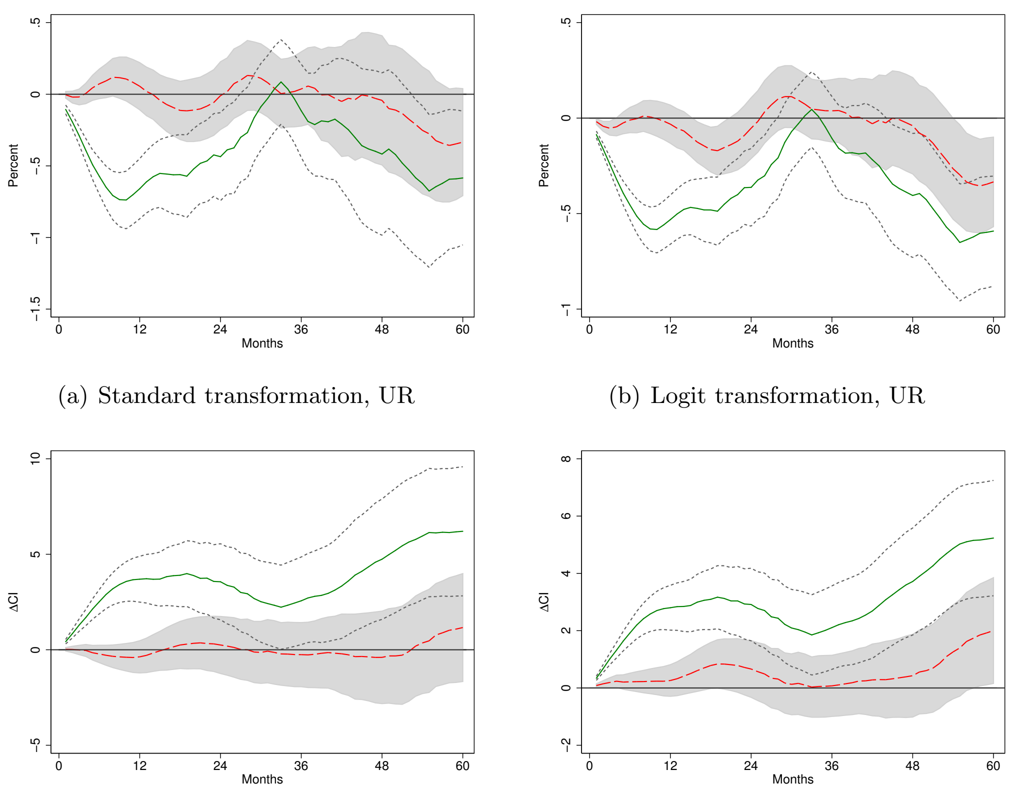
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% confidence intervals calculated using clustered standard errors by state.

effect is about one-half that size in the flexible state. The evidence corroborating our hypothesis is especially clear in the second year after the shock, and it does not depend on the transformation of wage rigidities employed in the specification.

In the full sample, see figure 4, a positive monetary policy shock is initially expansionary for both rigid and flexible states for a few months. After that, unexpected increases in the federal funds rate are contractionary, at least for the rigid state, where the effect quickly reaches its peak around 26 months after the shock. In rigid states, the effect lasts for around four-and-a-half years. In the flexible state, the effect is more sensitive to the exact transformation used for the degree of downward wage rigidity. In some specifications, positive interest rates surprises have a relatively mild contractionary effect only in the second year, and thus one could almost argue that the overall effect of monetary policy shocks is relatively small. However, in the specification with logit transformation of the rigidity measure, the duration of the effect for the flexible state is about one year shorter than in the rigid state, but the size of the effect is smaller for the flexible state than the rigid state. This difference is in line with our expectations, as our hypothesis is suggesting that wage rigidities have an effect on both the size and the persistence of monetary policy shocks. Comparing standard and logit transformations, we can observe that the differences between flexible and rigid states are smaller in the case of logit transformation. While the IRFs in the rigid state are very similar, the IRFs in the flexible state are more different. This difference is expected as there are quite some outliers among the lower estimates of downward wage rigidities, as shown in figure 2, and logit transformation corrects for that.

Results with the CI are plotted in figures 4(c)-(d). Qualitatively, the results are very similar to those on unemployment responses. The effects in the rigid state are significantly negative in most specifications. For the rigid case, the effect becomes significantly negative about three years after the shock. The effect reaches its peak after 28 months, at 1.2-2.5 index points, depending on the specification. For the flexible case, monetary policy shocks produce a contractionary effect after one-and-a-half to two years in the case of logit transformation while they are never significant for the standard transformation. The effect in the flexible state is significantly different from the rigid state for at least two-and-a-half years in all cases. Thus, we can confirm the hypothesis also for the analysis of the CI.

Figure 5. Monetary Policy Shocks in Rigid and Flexible States: Unemployment and CI, 1983–2007



(a) Standard transformation, UR

(b) Logit transformation, UR

(c) Standard transformation, CI

(d) Logit transformation, CI

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

To assess the importance of Volcker disinflation at the beginning of the 1980s on our results, we repeat the same exercise by excluding data up to 1983 from our analysis. The magnitude of shocks in this period is considerably larger than in the post-1983 sample. In fact, Ramey (2016) does not find contractionary effects of positive monetary policy innovations on industrial production and unemployment for this time sample. Results for unemployment and CI are presented in figure 5.

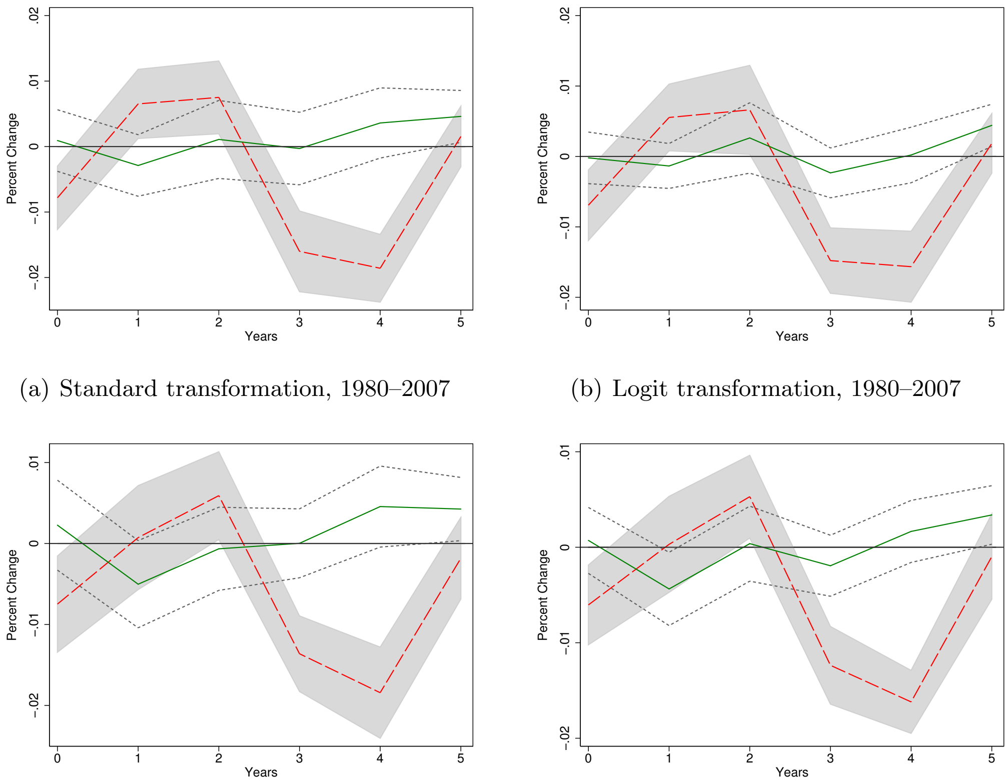
Effects on unemployment, as shown in figures 5(a)-(b), are not significant for most of the first five years, while the effects are expansionary in the flexible state, at least in the first two-and-a-half years. Impulse responses for the CI are displayed in figures 5(c)-(d). Compared with figures 4(c)-(d), we can see that in the shorter sample, even in the case with controls, we do not observe contractionary effects of positive monetary policy innovations for the flexible case. Note that the effects on CI are larger, on average, in the shorter sample. However, the relative difference between rigid and flexible state is still present and significant as put forward in our hypothesis.

More generally, the overall effect of monetary policy shocks depends on the set of controls. Results without any controls are presented in figure A.5 in appendix E. These results are in line with Ramey (2016), which argues that it is important to include control variables to preserve the recursiveness assumption, as defined in Christiano et al. (1999). Christiano et al. (1999) calls the identification where they assume that the first block of variables consisting of output, prices, and commodity prices does not responds to monetary policy shocks within the quarter (or month) the *recursiveness assumption*. According to Ramey (2016), this assumption is needed in the case if one believes that the Greenbook (Tealbook) forecasts do not incorporate all relevant information used by the FOMC to make decisions on the FFR. Due to the lack of price indexes available at the state level, this exercise becomes more difficult for us, and it is even more important that we choose a sample that does not exhibit too much volatility in the level of inflation. In our sample, inflation is well anchored. Average inflation expectations in the considered period were only 3.7%. In contrast, in times of double-digit inflation, workers are more likely to understand the real effect of nominal freezes and therefore are less willing to accept them. The relationship between rigidity and the impact of shocks may then shift from nominal to real rigidity (Akerlof et al., 1996). Furthermore, as a robustness exercise, we add log of national CPI and state house prices to the set of controls (see section 4.1.4).

4.1.2. Effects of Monetary Policy Shocks on State-Level GDP

The use of the unemployment rates and CI index may lead to concern. Unemployment could be subject to endogeneity, as states where layoffs are easy to implement may have limited need for wage cuts. Furthermore, CI estimates may be biased downwards, as salary imbursements are one of its components. States with flexible wages are likely to have larger wage declines after a contractionary shock, leading to a decline in CI irrespective of real activity. To see whether this bias affects our results, we reestimate the regressions above using state GDP. Because state GDP is only available annually for our sample, monthly interest rate shocks are aggregated by year. This aggregation adds to the challenge of this exercise, as innovations are not correlated and thus it is likely that they cancel out each other within a 12-month period when we aggregate them.

Figure 6. Monetary Policy Shocks in Rigid and Flexible States: State-level GDP, System GMM estimates



(a) Standard transformation, 1980–2007

(b) Logit transformation, 1980–2007

(c) Standard transformation, 1983–2007

(d) Logit transformation, 1983–2007

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

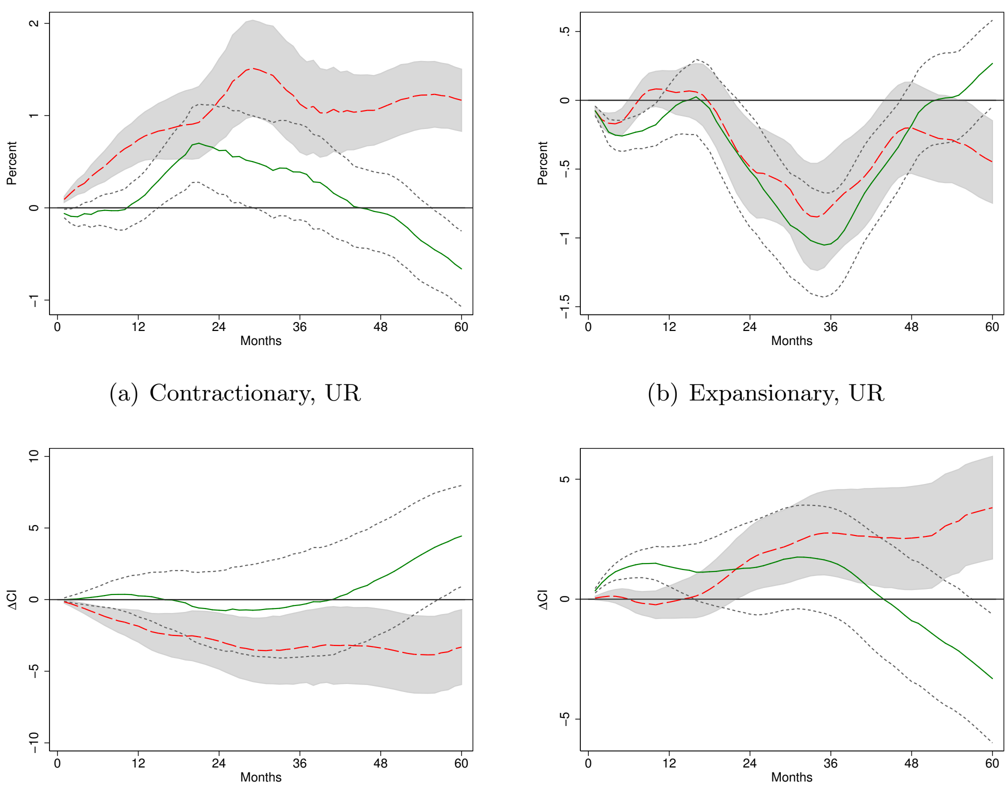
We use the Arellano and Bond (1991) system GMM estimator for dynamic panels to counter the Nickel bias, as in the case of yearly data our time dimension is considerably smaller than our cross-sectional dimension. Instrument proliferation is prevented by restricting instruments to second lags of dependent variables. However, we cannot use all our control variables in this specification; in particular, they do not include labor market controls and state fixed effects due to degrees of freedom problem. Figures 6(a)-(b) present estimates for the 1980–2007 period, while figures 6(c)-(d) present results for the 1983–2007 period. Standard errors are clustered by state. The left panels (a,c) show results for the standard transformation and the right panel (b,d) for the logit transformation.

These results suggest that for the flexible state, the impact is never significantly different from zero and thus is in line with the evidence that we presented for unemployment and CI. Figures 6(c)-(d) present the same estimations for the 1983–2007 sample. Interestingly, state-GDP results seem more robust to using this alternative sample then unemployment rates and CI. The effect of contractionary monetary policy shocks—at least in the rigid state—causes a significant decline on GDP in all estimations. Results are very similar for both standard and logit transformations.

4.1.3. Asymmetries: Direction of Shocks

To provide some additional evidence of the causal value of our results, we split our sample of policy shocks into expansionary and contractionary shocks, as in Olivei and Tenreyro (2007) and Matthes and Barnichon (2015). As our rigidity measures capture downward wage rigidities, the effect on the impact of shocks should be largest if wage cuts are desired. Hence, the difference between impulse response profiles for rigid and flexible states should be larger when shocks are contractionary. In this subsection, we present results with full sample and standard transformation.[28]

Figure 7. Monetary Policy Shocks in Rigid and Flexible States: Direction of Shocks, Standard transformation, 1980–2007



(a) Contractionary, UR

(b) Expansionary, UR

(c) Contractionary, CI

(d) Expansionary, CI

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

Figure 7 displays IRFs for contractionary and expansionary shocks. From these panels, it is obvious that most of the differences between flexible and rigid states occur for contractionary shocks, while responses for expansionary shocks are very similar for both flexible and rigid states. This pattern is particularly evident for unemployment IRFs, where the responses for two states are very similar in the case of expansionary shock. In the case of contractionary shocks, they are significantly different both in the first 15 months and after 40 months of the initial shock. Furthermore, the

[28]Note though that downward bias can be large with local projections when shocks run in a single direction. In appendix E we report also figure A.6 with logit transformation instead of standard transformation in figure 7.

response in flexible state is both smaller and less persistent, as it is below the confidence interval for the rigid state in practically all periods. Results for coincident index generally confirm those reported for the unemployment, although differences are smaller and often not significant. If our results were driven by sectoral composition, such differences are unlikely, as most confounding channels work similarly for contractionary and expansionary shocks. Alternatively, confounding channels like credit constraints may still have similar effects. The next robustness check explores the potential role for credit frictions.

4.1.4. Robustness: Additional Controls

Results in section 4.1.1 show the importance of controls for inference in this paper. In this subsection, we expand the set of controls with the log of national CPI and state-level house price indexes. Due to the lack of price indexes at the state level, we include seasonally adjusted house prices from the Federal Housing Finance Agency to the set of control variables. Controlling for house prices is also important from the perspective of unemployment insurance (see Den Haan et al., 2015). The results for standard transformation are presented in the top two panels of figure 8.[29]

Generally, the main result is robust to including additional controls. The response of unemployment to monetary policy shocks is very similar in both states to the ones reported in figure 4. We observe also that with this set of controls, there are significant differences between the flexible and rigid states, in particular in the second and third years after the shock. In this exercise, we test the relevance of the recursiveness assumption advocated by Ramey (2016) and, except for a slightly more persistent response in the flexible state, the responses are virtually unchanged. More noticeable differences occur in the case of CI IRF. Response in the flexible state is more contractionary with additional controls compared with the baseline case reported in figure 4, where we observe less persistence compared with ones reported in figures 8(a)-(b). Overall, this exercise points out also that with additional control variables, our qualitative results remain unchanged, although there is less difference between the response in the rigid state compared with the flexible state for the CI.

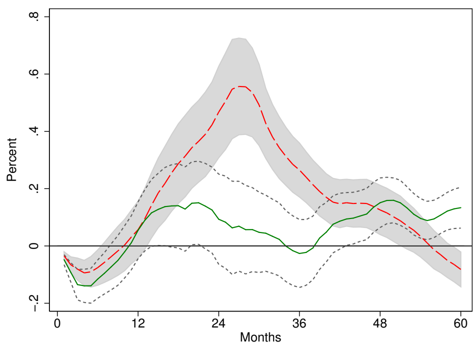
The second exercise in this section explores the role of state-level financial frictions proxies and credit channel as control variables. Recently, Gertler and Karadi (2015) found empirical evidence for a prominent role for the credit cost in the transmission of monetary policy. The rationale is that state-level financial frictions could potentially have similar effects if states with high levels of financial frictions would coincide with states with high levels of downward nominal wage rigidities. We start by introducing the quarterly state average for 30-year mortgage rates—published by Freddie Mac as part of the Primary Mortgage Market Survey—in the set of controls. In the third exercise, we additionally introduce a proxy for state-level financial frictions in the form of the value of FDIC interventions in a given state per year.[30]

Results reported in figures 8(c)-(f) display very similar IRFs to those in baseline figure 4. In fact, in figures 8(c)-(d)—that include the state average for 30-year mortgage rates—the difference between

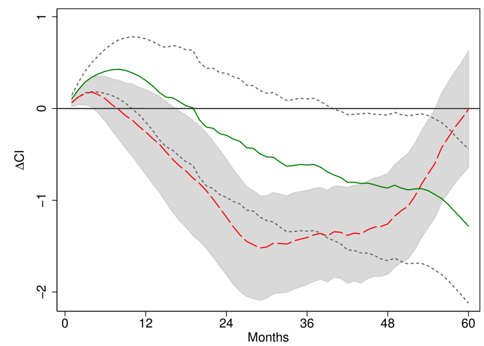
[29] In appendix E we report also figure A.7 with logit transformation instead of standard transformation in figure 8.

[30] We have explored other indicators of financial frictions as well. In particular, we have experimented with foreclosure rates by state, as detailed in Calomiris et al. (2013) and collected by the Mortgage Bankers Association Quarterly Delinquency Survey. Results are qualitatively the same.

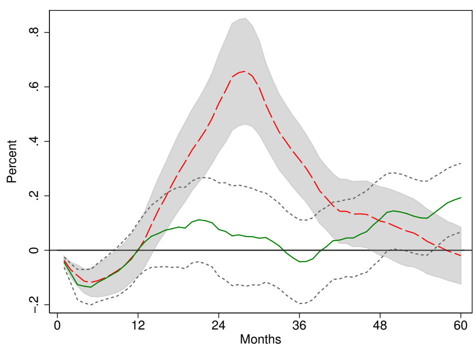
Figure 8. Monetary Policy Shocks in Rigid and Flexible States: Additional Controls, Standard transformation, 1980–2007



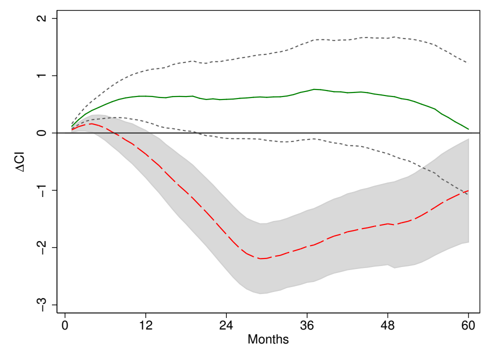
(a) Recursivness controls, UR



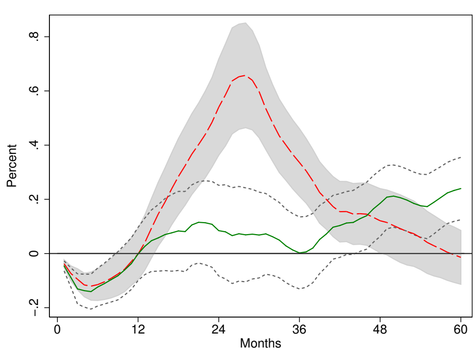
(b) Recursivness controls, CI



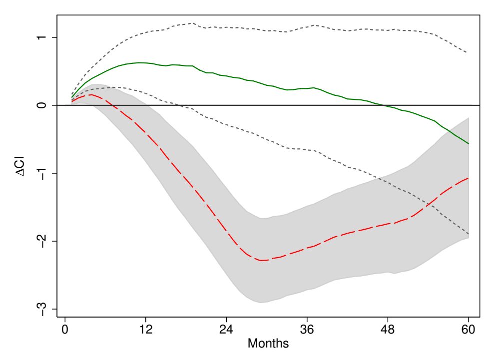
(c) Mortgage rate controls, UR



(d) Mortgage rate controls, CI



(e) Financial frictions controls, UR



(f) Financial frictions controls, CI

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

the rigid and flexible state even increases. The responses in flexible state are never significantly different from zero in the second to fourth year after the shock, while in the rigid state the responses are the highest between the second to fourth year after the shock. Results are very similar when we additionally introduce a proxy for financial frictions in figures 8(e)-(f).[31]

4.1.5. Robustness: Announcement Shocks

In recent years, changing the interest rate is not the only tool of monetary policy. Press conferences, speeches, and forward guidance are becoming more and more important (Gürkaynak et al., 2005). Therefore, we also consider announcement shocks on the monetary policy decision day. For robustness, we consider announcement shocks from both Gertler and Karadi (2015) and Gorodnichenko and Weber (2016).

Gorodnichenko and Weber (2016) announcements are available between 1994 and 2009. Their data are constructed from the federal funds futures from the Chicago Mercantile Exchange Globex electronic trading platform, where they consider a change in these futures in either 30- or 60-minute windows (tight, wide) after the announcement. In total, they calculate surprises for 137 events between 1994 and 2009. The advantage of Gertler and Karadi (2015) announcements, which follow Gürkaynak et al. (2005), is that they are available for a slightly longer time sample: Surprises to current month's federal funds rate futures are available from November 1988, surprises to three-months-ahead federal funds rate are available from January 1990, and surprises to year-ahead futures of Eurodollar deposits are available from January 1984.[32]

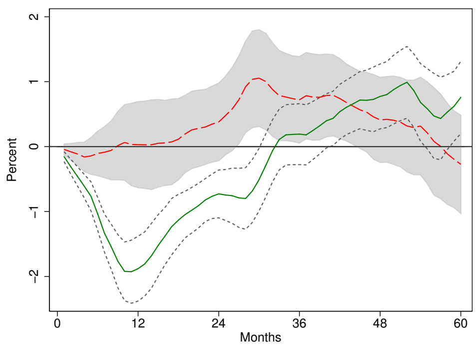
Results for the announcement shocks, presented in figures 9 and 10, confirm other results in this section. All five different announcement shocks that we consider in this paper point to significantly different responses between flexible and rigid states. For the rigid state, we can observe that Gorodnichenko and Weber (2016) contractionary announcement shocks lead to insignificant changes in unemployment in the first two years after the shock and produce small contractionary effects only in the third and fourth years after the shock (figures 9(a)-(b)). Flexible state responds expansionary, as we note already for the post-1983 sample in the case of Romer and Romer (2004) shocks.[33] In the case of Gertler and Karadi (2015) shocks, initially both flexible and rigid state responds expansionary to contractionary monetary policy shock. We observe contractionary responses only for the rigid state in the case of announcement shocks calculated from year-ahead futures of Eurodollar deposits (see figure 9(e)). Results for CI, displayed in figure 10, are qualitatively the same as in the case of unemployment. The main message, that the response is different for the flexible and rigid state, is confirmed in all cases considered.

[31] Caldara and Herbst (2016) point out that it is important to take into account the systematic component of monetary policy characterized by a significant reaction to changes in credit spreads. Our results are qualitatively the same if we consider their version of Romer and Romer (2004) type shocks in our analysis for the 1994–2007 sample and we control for additional variables discussed in this section. Results look very similar to those in figures 9(a)-(b) and 10(a)-(b) that use the same time sample.

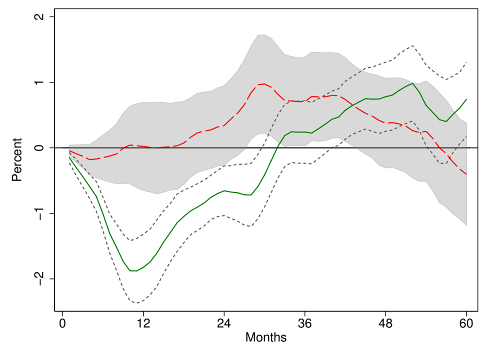
[32] Gertler and Karadi (2015) consider also six- and nine-month-ahead futures of Eurodollar deposits. Results are qualitatively the same for those announcement shocks as well.

[33] In figures A.9 and A.10 in appendix E, we reproduce these figures by additionally including state levels of 30-year mortgage rates. In that case for the rigid state the contractionary effects are larger, and persistence is slightly increased for the flexible state.

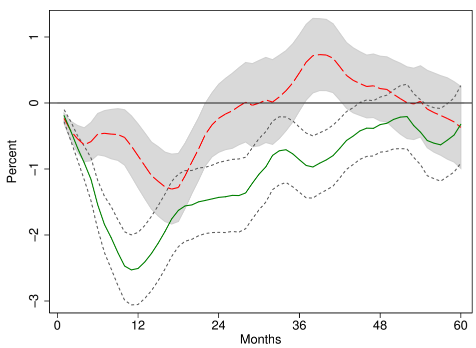
Figure 9. Monetary Policy Shocks in Rigid and Flexible States: Announcement shocks, Unemployment, Standard transformation



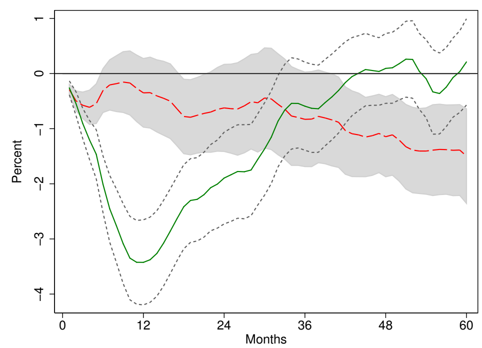
(a) GW (2016), tight interval, 1994-2007



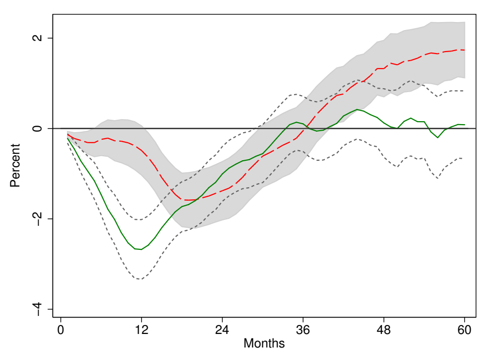
(b) GW (2016), wide interval, 1994-2007



(c) GK (2015), current FFR futures, 1988-2007



(d) GK (2015), 3-month ahead FFR futures, 1990-2007



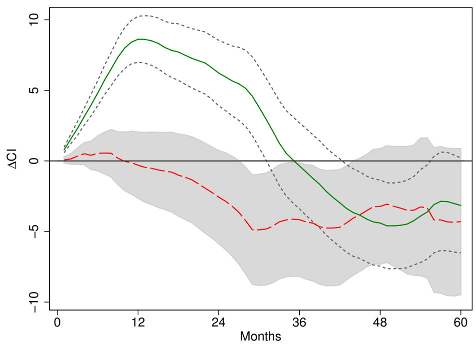
(e) GK (2015), year-ahead futures of Eurodollar deposits, 1984-2007

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. GW (2016) stands for Gorodnichenko and Weber (2016); and GK (2015) stands for Gertler and Karadi (2015).

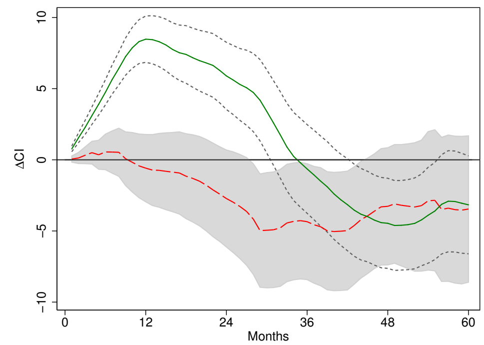
4.1.6. Robustness: Standard Errors

Most papers in the literature where local projections are applied to the panel data implement robust clustered standard errors at the cross-sectional dimension—in our case, U.S. states (see Jordà et al.

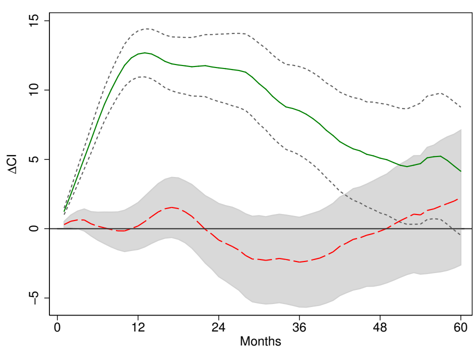
Figure 10. Monetary Policy Shocks in Rigid and Flexible States: Announcement shocks, Coincident, Standard transformation



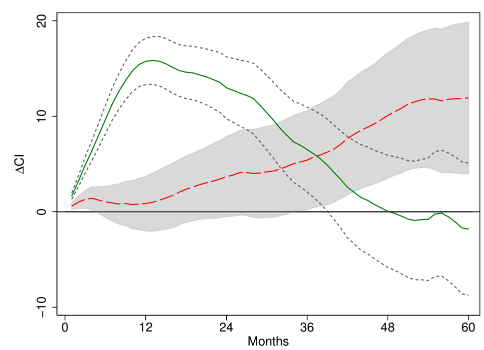
(a) GW (2016), tight interval, 1994-2007



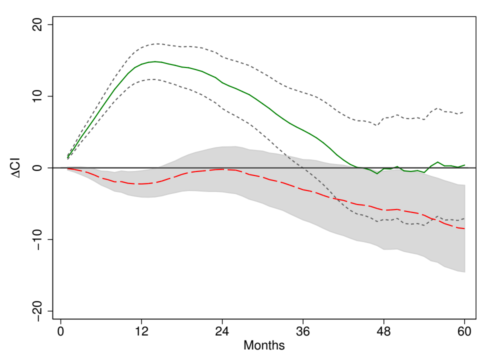
(b) GW (2016), wide interval, 1994-2007



(c) GK (2015), current FFR futures, 1988-2007



(d) GK (2015), 3-month ahead FFR futures, 1990-2007



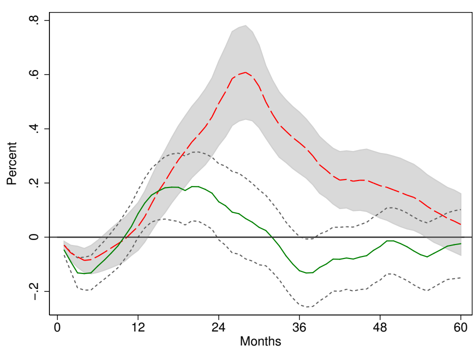
(e) GK (2015), year-ahead futures of Eurodollar deposits, 1984-2007

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. GW (2016) stands for Gorodnichenko and Weber (2016); and GK (2015) stands for Gertler and Karadi (2015).

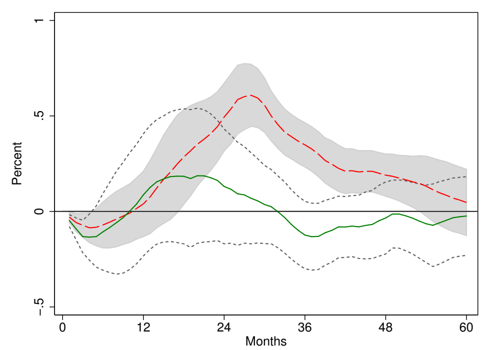
2015a, 2015b, 2016). However, a complication that arises from using the Jordà (2005) method is the serial correlation in the error terms generated by the successive leading of the dependent variable. Furthermore, there could be a time dependence of the impulse responses. Following Pfajfar et al.

(2016), we estimate standard errors using the SURE estimator, where we obtain simultaneous (co)variance matrix of the sandwich/robust type for all leads h corrected for clusters in both states and time.[34] To show the importance of different assumptions, we present standard errors with the SURE estimator with clustering by state in figure 11(a), the SURE estimator with clustering by time in figure 11(b), and the SURE estimator with clustering by state and time in figure 11(c).

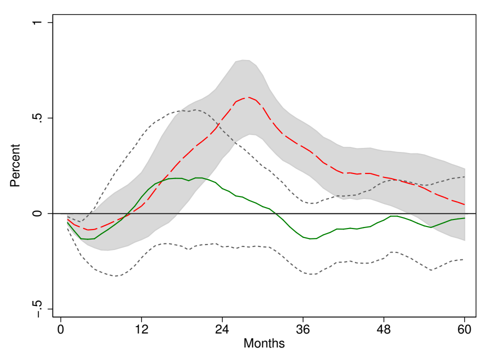
Figure 11. Monetary Policy Shocks in Rigid and Flexible States; Unemployment with Standard Transformation: Standard errors, 1980–2007



(a) SURE estimator, clustered by state



(b) SURE estimator, clustered by time



(c) SURE estimator, clustered by state and time

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

As we can observe in figure 11, standard errors increase after we take into account clustering by time. This increase is not surprising considering the results from previous sections, where we show that the shape of the impulse response changes considerably depending on the start date. A lot of identification of monetary policy shocks comes from the beginning of the sample, especially Volcker disinflation. Nevertheless, we can still observe that our main conclusions are robust, as the response for the flexible state is significantly different from the response for the rigid state. This difference is particularly evident in the third year after the monetary policy shock. In fact, the responses are never different from zero only in the flexible state.

[34] Gourio et al. (2016) also use the SURE estimator and cluster standard errors by time. Banerjee and Zampolli (2016) use clustered standard errors by state and time.

4.1.7. Other Robustness Checks

We have performed several other robustness checks, including removing the five (ten) smallest states. Results are essentially the same. We also explore additional controls for industry, as the average rigidities vary across industries and some states may have higher shares of those industries. Daly and Hobijn (2015) point out that in particular, construction is subject to higher wage rigidities. Our results are robust to excluding the five (ten) states with the highest share of construction among all industries.

The second set of robustness checks were with respect to our measure of wage rigidities. If we condition on industry of occupation in CPS data when computing the measure of wage rigidities, we also obtain very similar results. This result is not surprising, as the correlation between these two measures is 91%. An additional test that we perform is to consider a proxy for the measure of wage rigidities that relies on the ratio between minimum and median wage. The differences between flexible and rigid state are smaller for this proxy than in our baseline regressions but still statistically significant. Ratio between minimum and median wage is "only" one of the features of downward wage rigidities.[35]

4.2. Regressions on Fiscal Policy Shocks

This section presents results for fiscal multipliers conditional on wage rigidities. As a benchmark, Nakamura and Steinsson (2014) estimate that the fiscal multiplier across U.S. states independent of wage rigidity equals 1.43. This implies that a 1% increase of relative military spending as a percentage of state GDP increases its GDP relative to other states by 1.43% within two years of the increase in spending. Estimates including direct compensation yield an estimated multiplier of 1.62.

4.2.1. Main Results

Table 5 presents results from two-stage least-squares regressions along Eq. (3). The dependent variable is biannual growth in state GDP while the explanatory variable is growth in state-level prime defense spending as a percentage of GDP. The first row in the column presents estimates for coefficient β^R, while the second row presents β^F. The difference between these multipliers identifies the effect of wage rigidities. We use the standard transformation of the measure of wage rigidities.[36] Standard errors are clustered by state. Columns 1 presents the baseline estimates without controls for state fixed effects and characteristics. Year-fixed effects are included to control for common shocks such as changes in monetary policy.[37] Results in column 1 support the hypothesis. In the rigid state, fiscal multipliers exceed 1.7, while in the flexible state, they are slightly negative and insignificant. Column 2 displays the results, where we add a control variable for the level of

[35] Figure A.11 in appendix E reports IRFs to monetary policy shocks using the ratio between minimum and median wage as a proxy for downward wage rigidities.

[36] Results with the logit transformation are very similar and therefore not shown. They are available upon request.

[37] This specification is in line with Nakamura and Steinsson (2014). It is feasible to include these fixed effects because differences in military spending growth across states are substantial within years. Estimations on monetary policy shocks do not include these fixed effects, as shocks themselves are identical across states each period.

Table 5: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Prime Spending							
Rigid State Multiplier	1.737*	2.197**	1.554**	2.484**	2.159***	1.993***	1.442**
	(0.945)	(1.009)	(0.769)	(1.010)	(0.756)	(0.774)	(0.605)
Flexible State Multiplier	-0.888	-0.586	-0.396	0.439	-0.0654	-0.0279	-0.291
	(1.033)	(0.883)	(0.911)	(0.895)	(0.816)	(0.808)	(0.669)
Military Spending		-0.0361		-0.323	-0.289	-0.223	0.167
		(0.125)		(0.513)	(0.443)	(0.441)	(0.277)
Avg. Mortgage Rate					0.0304*	0.0314*	0.0324*
					(0.0165)	(0.0166)	(0.0191)
Minimum Wage					0.132***	0.128***	0.156***
					(0.0489)	(0.0487)	(0.0470)
Δ Firm Size					0.0180***	0.0192***	0.0168***
					(0.00525)	(0.00541)	(0.00564)
Δ Mobility					0.000484	0.000434	0.000419
					(0.000477)	(0.000476)	(0.000385)
Δ % Services					-0.109	-0.108	-0.0433
					(0.0728)	(0.0727)	(0.0658)
Δ % Government					-0.176	-0.176	-0.119
					(0.133)	(0.133)	(0.123)
Union Power					-0.0442***	-0.0465***	-0.0293***
					(0.00613)	(0.00612)	(0.00655)
House Prices						0.0394*	-5.961**
						(0.0236)	(2.551)
LDV							0.257***
							(0.0444)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	1,223	1,223	1,223	1,223	1,172	1,172	1,121
R-squared	0.237	0.230	0.389	0.367	0.409	0.413	0.490

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state. Dependent variable is two-year growth in state GDP.*

military expenditure as a percent of state GDP. Defense spending may affect certain industries more strongly than others. Particularly, the auxiliary benefits of spending shocks are likely to be larger in states with industries that rely on defense spending. If these industries are also characterized by high degrees of wage rigidities, this would jeopardize causal inference. Results in column 2 are in line with column 1: The multiplier in rigid states exceeds 2 while the multiplier in flexible state is slightly below 0. Columns 3 and 4 detail results where we control for state fixed effects. This additional controls reduce standard errors and yield similar estimates for the multipliers. Adding labor market institutions, sectoral composition, and financial frictions to the set of controls, as shown in column 5, has little effect on qualitative results in this exercise. Column 6 presents the specification that contains all additional control variables from regressions on monetary policy shocks, including controls for state-level house prices. These results predict a multiplier of 2 for the rigid state and 0 for the flexible state. This specification is our preferred. In the last column, column 7, we also explore the effects of controlling for lagged dependent variable. These results suggest slightly smaller multipliers in both the flexible and rigid state, while leaving the difference between them similar to results in column 6. Table 6 presents results from identical specifications but using a broader definition of military expenditure that includes direct compensation. Results across columns are (qualitatively) similar, indicating that the estimated effect of wage rigidity on fiscal multipliers is robust to changes to the definition of military spending.

Table 6: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Incl. Direct Comp.							
Rigid State Multiplier	1.560*	1.809**	2.052***	2.730***	2.387***	2.300***	1.604***
	(0.808)	(0.845)	(0.735)	(0.970)	(0.700)	(0.705)	(0.504)
Flexible State Multiplier	-0.956	-0.767	-0.221	0.536	0.00570	0.0460	-0.401
	(0.937)	(0.809)	(0.877)	(1.008)	(0.926)	(0.909)	(0.839)
Military Spending		-0.0230		-0.429	-0.371	-0.316	0.136
		(0.127)		(0.575)	(0.479)	(0.476)	(0.304)
Avg. Mortgage Rate					0.0329*	0.0342**	0.0339*
					(0.0172)	(0.0172)	(0.0194)
Minimum Wage					0.127**	0.123**	0.153***
					(0.0502)	(0.0501)	(0.0466)
Δ Firm Size					0.0178***	0.0191***	0.0168***
					(0.00523)	(0.00536)	(0.00563)
Δ Mobility					0.000503	0.000450	0.000421
					(0.000476)	(0.000477)	(0.000386)
Δ % Services					-0.111	-0.109	-0.0450
					(0.0733)	(0.0731)	(0.0667)
Δ % Government					-0.172	-0.169	-0.118
					(0.136)	(0.137)	(0.124)
Union Power					-0.0447***	-0.0471***	-0.0296***
					(0.00625)	(0.00622)	(0.00659)
House Prices						0.0420*	-5.724**
						(0.0232)	(2.540)
LDV							0.254***
							(0.0437)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	1,223	1,223	1,223	1,223	1,172	1,172	1,121
R-squared	0.239	0.236	0.384	0.364	0.409	0.411	0.492

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state. Dependent variable is two-year growth in state GDP.*

Results for fiscal multipliers conditional on wage rigidities are consistent with those using macroeconomic models. Estimated, or realistically calibrated, New Keynesian models like Smets and Wouters (2007) predict multipliers around unity. Neoclassical models without nominal rigidities are below unity and in some calibrations negative, in line with our low-multiplier estimates for the flexible state (e.g., Baxter and King, 1993). Keynesian models, depending on the amount of frictions assumed, can yield multipliers far exceeding 2 (e.g., Galí et al., 2007), in line with our estimates for the rigid state.[38] As the estimates follow this pattern in all specifications, in both subsamples and for both measures of defense spending, the results firmly corroborate the hypothesis.

[38]For a detailed summary of theoretical results in Neo-Classical and Keynesian models, see Ramey (2011).

4.2.2. Robustness Checks

To further assess the sensitivity of our results, we perform several robustness checks.[39] We re-estimate specifications from tables 5 and 6 for a subsample excluding the Volcker disinflation. Tables A.4 and A.5 present these results. Although point estimates are somewhat different, the difference between the rigid and flexible multiplier is not meaningfully different from previous estimates in any specification. Column 5 in table A.4 yields the strongest result so far. Controlling for state- and year-fixed effects, as well as sectoral composition, labor market institutions, and financial frictions through state-level mortgage rates, the multiplier in the rigid state is 2.9, while in the flexible state it is close to -1.4.

An additional robustness check that we perform involves estimating fiscal multipliers on subsets of states. In particular, we remove states in which military spending is either very sensitive or insensitive to changes in national spending. In appendix tables A.6 and A.7 we replicate tables 5 and 6 on the subsample of states excluding the five most sensitive (California, Connecticut, Kansas, Massachusetts, and Missouri). The difference between multipliers in the flexible and rigid state lies around 2 across various specifications in both tables, in line with our previous results. Standard errors are slightly larger though, as expected for a smaller sample. Appendix E tables A.10 and A.11 repeat the exercise for a sample that excludes the five states with the least sensitive of local spending to national spending (Idaho, North Carolina, Oregon, South Carolina, and West Virginia). The difference between flexible and rigid multipliers lies between 2.0 and 2.5 in these specifications, in line with our previous results.

Furthermore, performing exercises similar to those in section 4.1.7 yield qualitatively the same results to our baseline specification. We also check for robustness of standard errors—we two-way cluster them by time and state. Actually, these standard errors (see tables A.10 and A.11) are narrower than those reported in tables 5 and 6.[40]

5. Conclusion

This paper provides empirical evidence on the role of wage rigidities as a transmission channel of monetary and fiscal policy shocks to the real economy. It shows that there exists a positive relationship between wage rigidities and the impact of shocks in monetary and fiscal policy. New Keynesian DSGE models predict a positive correlation between wage rigidity and the impact of government spending and monetary policy shocks, as sluggish wage changes result in poor adjustment of consumer prices and larger fluctuations in unemployment.

[39] The asymmetry in response to downward and upward shocks is not found for fiscal multipliers. These results are not provided because, in contrast to the monetary policy shocks, changes in military spending show clear trends. Spending increases for most of the first half of the sample, while it decreases for most of the second (as shown in figures A.1 and A.2 in appendix A). A comparison of positive and negative shocks is essentially a comparison of fiscal multipliers across different time samples.

[40] Alternatively, we can use Romer and Romer (2010) tax policy changes as a proxy for fiscal policy shocks in a specification similar to the one for monetary policy shocks. These results also corroborate our hypothesis. Results are available upon request.

We relate variation in the state-level impact of identical shocks in national policy to differences in wage rigidities. Using microdata from the Current Population Survey, we calculate state-level downward nominal wage rigidity between 1980 and 2007. Rigidities are lower in states with high labor mobility and a large fraction of small firms, while they are higher in states with a greater share of employment in service and government sectors and high minimum wages, as well as when unions are more powerful.

Our results show that monetary policy shocks affect state-level unemployment and output only if wages are rigid. Estimates suggest that states with high rigidities experience significantly greater output reductions and unemployment increases after an interest rate shock than states with low rigidities. Similarly, we show that multipliers of fiscal spending shocks are considerably larger in states with high wage rigidities than states with low rigidities. Multipliers in states with low rigidities are close to 0, while rigid states have multipliers larger than 2. These results are robust to the inclusion of controls for labor mobility, firm size, minimum wages, and unionization as well as measures of sectoral composition. Some evidence of causality is provided by splitting our sample into contractionary and expansionary monetary policy shocks. Wage rigidities only affect the impact of contractionary shocks, as expansionary shocks have very little effect on real variables.

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Appendix A. Additional Figures

Figure A.1. Changes and Shocks in Federal Funds Rates (FFR)

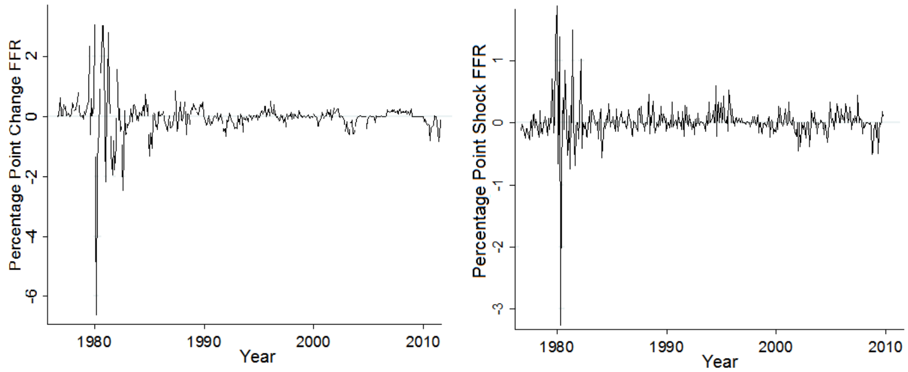
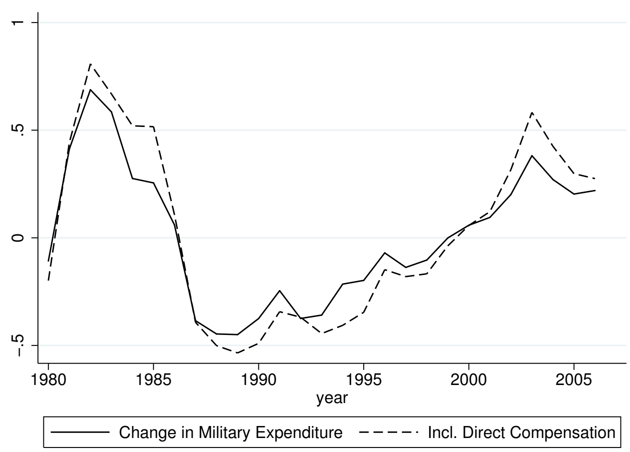


Figure A.2. Changes in Military Expenditure



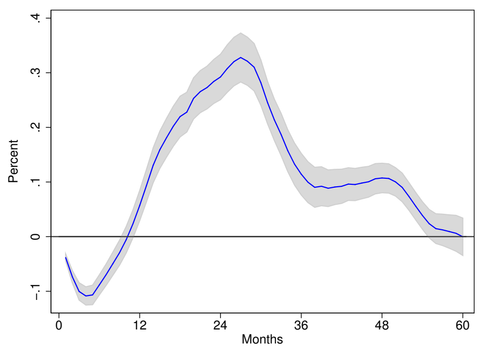
Appendix B. Role of Wage Rigidities in New Keynesian Models

To formalize the hypothesis of our empirical sections, we present results from Smets and Wouters (2007) model, which we estimate using a 1965–2007 sample, with different degrees of wage rigidities.[41] The latter is defined à la Calvo (1983), which we vary between high and low values compared to the estimate of 0.77. In Figure A.4 we plot impulse responses of employment and output to monetary and fiscal policy (exogenous spending) shocks.

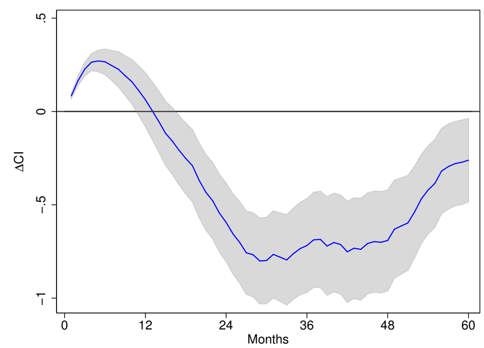
The responses are in line with our hypothesis of a positive relationship between the impact of policy shocks and rigidity. Indeed, the higher wage rigidities, the larger the response of employment and output to fiscal and monetary policy shocks. Furthermore, the persistence of shocks in increasing in the degree of wage rigidity.

[41]Smets and Wouters (2007) can be straightforwardly extended to an open economy with a monetary and fiscal union, where two countries would differ in only the degree of wage rigidities. This extension would produce the same qualitative results. Details regarding the estimation are available upon request.

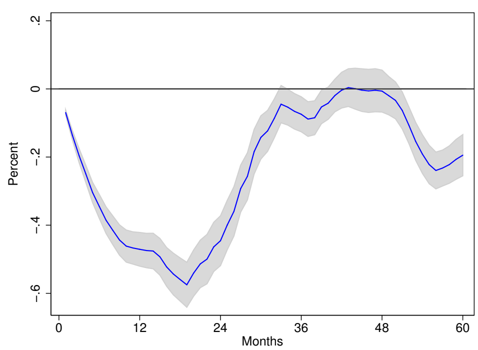
Figure A.3. Unconditional monetary policy shocks: Unemployment and CI



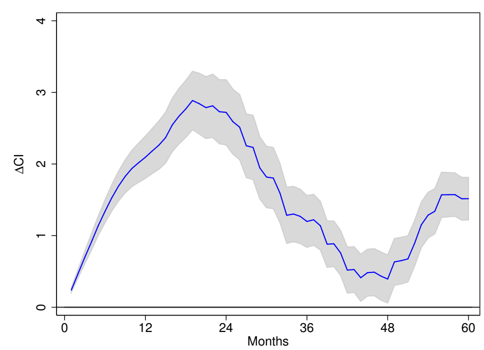
(a) Unemployment, 1980–2007



(b) Coincidence Index, 1980–2007



(c) Unemployment, 1983–2007



(d) Coincidence Index, 1983–2007

Note: 90% intervals.

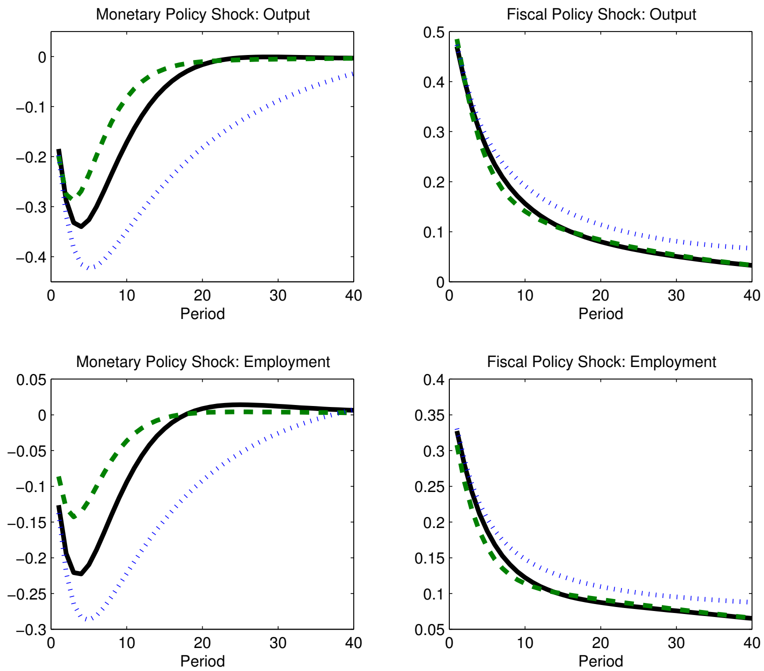
These results are in line with other New Keynesian Dynamic Stochastic General Equilibrium (DSGE) models.[42] Galí (2014), for instance, shows that seigniorage has positive effects on output, conditional on the presence of rigidity. Monetary injections are beneficial as real interest rates decrease in Galí's model, because inflation expectations are dampened by sticky prices. Similarly, Christiano et al. (2011b) show that fiscal spending multipliers depend positively on wage rigidity in a model designed to explain economic behavior around the zero lower bound. In the remaining of the paper, we corroborate the hypothesis without imposing structure, which facilitates the causal interpretation of our results.

Appendix C. Measure of Real Rigidity

To measure real rigidity, one could simply replace nominal wage freezes and cuts in Eq. 4 by real counterparts. This approach is flawed in the presence of heterogeneous inflation expectations. For example, a firm may expect 2% inflation and accordingly offer employees a 2% wage increase,

[42]For example, Gertler et al. (1999), Smets and Wouters (2002), Christiano et al. (2005), Galí and Monacelli (2005), and Blanchard and Galí (2010).

Figure A.4. Impulse Responses to Monetary and Fiscal Policy Shocks.



Note: Black solid line represents impulse responses with the estimated coefficients, green dashed line with low degree of wage rigidities, and blue dotted line with high degree of wage rigidities.

which yields a real freeze from the firm's perspective. This freeze would not be counted as a freeze, however, if average inflation expectations are 1%. Hence, we use a redesigned measure of real rigidity from Dickens et al. (2007) that accounts for variation in inflation expectations:

$$FWCP^r_{s,t} = \frac{f^r_{s,t}}{c^r_{s,t} + f^r_{s,t}} = \frac{2(h_{s,t} - c^r_{s,t})}{h_{s,t}}, \tag{6}$$

where superscripts r refer to real values based on average inflation expectations. $h_{t,s}$ counts the number of observations with wage changes greater than the sum of median and median real change ($\Delta M_{t,s} + [\Delta M_{t,s} - \pi^e_{t,s}]$, where ΔM denotes median change while π^e denotes average inflation expectations). The numerator counts expectation-corrected real wage freezes. To see this, assume that in the absence of real rigidity the distribution of wage changes on either side of the median is symmetric. If no wage rigidity is present, this implies that the number of observations in $c^r_{t,s}$ and $h_{t,s}$ are equal, as both lie equally far from the median. Wages freezes at various inflation expectations create asymmetry such that $c^r_{t,s} < h_{t,s}$. On the left-hand side of the median wage change, $c^r_{t,s} - h_{t,s}$ thus quantifies the number of missing real wage cuts. When assuming symmetric inflation expectations, an equal number of real wage cuts is missing right of the median. We therefore multiply $c^r_{t,s} - h_{t,s}$ by 2. The denominator $c^r_{t,s} + f^r_{t,s} = h_{t,s}$ follows from assuming wage-change symmetry in absence of rigidity. The number of intended real wage cuts in left tail $c^r_{t,s} + f^r_{t,s}$ therefore equals the actual number in right tail $h_{t,s}$.

Estimates of average inflation expectations are taken from the *Survey of Consumers and Attitudes* conducted by the University of Michigan. Thereby, we assume that national prices are used in state-level wage bargaining.[43]

Table A.1: Average Wage Rigidity by State

	$FWCP^n$	$FWCP^r$		$FWCP^n$	$FWCP^r$		$FWCP^n$	$FWCP^r$
Average	*0.1949*	*-0.0691*	KY	0.1954	-0.1021	OH	0.1967	-0.0922
AL	0.1918	-0.1963***	LA	0.1865	-0.1918**	OK	0.1965	-0.1213
AK	0.1992	-0.1296	ME	0.2153***	-0.0218	OR	0.2025	0.0250
AZ	0.1915	0.0080**	MD	0.1717***	0.0152*	PA	0.1954	-0.0966
AR	0.2031	-0.1098	MA	0.1886	0.0081	RI	0.2046*	-0.0049
CA	0.1963	-0.0465**	MI	0.2031	-0.0806	SC	0.1858*	-0.1608*
CO	0.1969	-0.0013***	MN	0.2034	0.0656***	SD	0.2063**	-0.0235
CT	0.1832**	-0.0191	MS	0.2056**	-0.3255***	TN	0.1944	-0.2001***
DE	0.1636***	-0.0255	MO	0.1851*	-0.0359	TX	0.1972	-0.1154
DC	0.1512***	0.0485**	MT	0.2200***	-0.1162	UT	0.2027	0.0767***
FL	0.1899	-0.0235	NE	0.2055**	-0.0182	VT	0.2107***	0.0283**
GA	0.1743***	-0.1902	NV	0.1945	-0.1678**	VA	0.1834**	-0.0525
HI	0.1981	-0.2365	NH	0.1929	-0.0025	WA	0.1992	-0.0506
ID	0.2080**	-0.0368	NJ	0.1740***	-0.0099	WV	0.2009	-0.1893**
IL	0.1824**	-0.0744	NM	0.1998	0.0180*	WI	0.2087**	-0.0203
IN	0.1911	-0.0919	NY	0.1749***	-0.0546	WY	0.2117***	-0.1309
IA	0.2001	-0.0836	NC	0.1831**	-0.1208**			
KS	0.2029	0.0185*	ND	0.2128***	-0.0670			

*Note: *, ** and *** denote significance from average at the 10, 5, and 1% significance level, respectively. Estimates obtained using a mean-comparison t-test, two-sided.*

First, average nominal rigidity exceeds real rigidity for every state. In fact, $FWCP^r$ is negative in many states, implying median real wage growth was often negative. These estimates indicate that for most states real rigidities are not of concern. Note that this increases the validity of $FWCP^n$, because it assumes absence of real rigidity. Point correlation between $FWCP^n$ and $FWCP^r$ equals -0.19. 17 states have significantly different average $FWCP^r$.

Appendix D. Sensitivity Test Wage Rigidity Measures

This appendix analyzes robustness of our rigidity measures. The first test involves truncating the micro sample at absolute log changes log wage changes between 0.4 and 0.6. Panel A in Table A.3 presents correlation of resulting rigidity estimates with the truncation of 0.5 used above. Correlations for $FWCP^n$ are all above 0.99. The second row provides corresponding correlations for $FWPC_r$. With a minimum of 0.95 these measures too seem stable and insensitive to changes in truncation. This insensitivity is relevant for two purposes. First, it lends support to the use of our truncation as an outlier treatment, in the sense that is is unlikely to affect results. Second, it provides an indication of our measures' stability to changes in the underlying sample. When truncating at a log change of 0.4 for instance, the number of wage cuts is reduced by 7.7%. The second sensitivity test is summarized in Panel B of Table A.3. It presents correlation coefficients

[43] Local indicators of inflation are only available at MSA level, which is likely to poorly reflect inflation at state level. A sensitivity test to measurement error of average inflation expectations is provided in appendix A.

Table A.2: Estimations Labor Market Institutions and Wage Rigidity

	(1) $FWCP^n$	(2) $FWCP^n$	(3) $FWCP^n$	(4) $FWCP^n$	(5) $FWCP^r$	(6) $FWCP^r$	(7) $FWCP^r$	(8) $FWCP^r$
Δ Mobility	-0.063***		-0.078***	-0.063***	0.114		0.206	0.101
	(0.015)		(0.017)	(0.016)	(0.191)		(0.182)	(0.193)
Δ Firm Size	-0.002		0.001	-0.001	0.048***		0.042***	0.043***
	(0.002)		(0.001)	(0.002)	(0.014)		(0.013)	(0.014)
Minimum Wage	0.141***		0.077***	0.138***	0.397**		0.579***	0.436**
	(0.024)		(0.022)	(0.024)	(0.185)		(0.146)	(0.184)
Unionization	0.071***			0.067***	-0.233			-0.223
	(0.025)			(0.025)	(0.219)			(0.218)
Union Power	0.010***			0.010***	-0.082***			-0.083***
	(0.002)			(0.002)	(0.017)			(0.014)
Δ % Empl. Serv.		0.202***	0.225***	0.023		-0.906**	-0.736**	-0.835**
		(0.039)	(0.040)	(0.044)		(0.363)	(0.339)	(0.350)
Δ % Empl. Gov.		0.187**	0.186**	0.008		-0.789	-0.101	-0.151
		(0.08)	(0.082)	(0.082)		(0.848)	(0.894)	(0.984)
Δ Education		-0.012	-0.012	-0.047***		0.082	0.043	0.081
		(0.009)	(0.010)	(0.010)		(0.119)	(0.123)	(0.109)
Constant	0.116***	0.196***	0.163***	0.118***	-0.131	-0.0673***	-0.308***	-0.146*
	(0.011)	(0.001)	(0.009)	(0.010)	(0.085)	(0.002)	(0.061)	(0.084)
Observations	1,122	1,581	1,479	1,122	1,122	1,581	1,479	1,122
R^2	0.071	0.018	0.042	0.084	0.016	0.004	0.019	0.020

*Note: *, **, *** denote significance at the 10, 5, and 1%, respectively. Clustered standard errors in parentheses. Estimates obtained using Fixed Effects estimators. Non-stationary variables estimated in first difference. Sample: 1980-2013.*

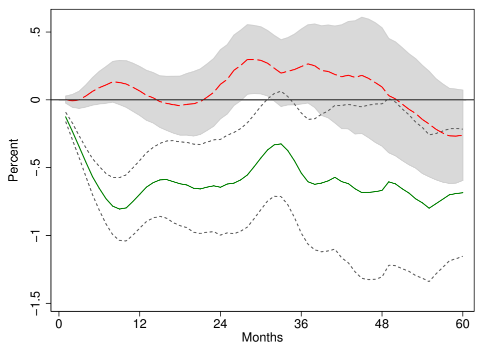
obtained when calculating $FWCP^r$ using values for π^e that diverge from the Michigan Survey. Within a 1 percentage point bandwidth, correlation across estimates always exceed 0.95.

Table A.3: Sensitivity Wage Rigidity Measures, Correlation with Baseline

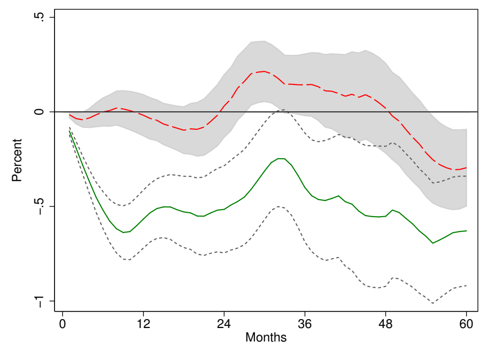
A. Truncation:	0.40	0.42	0.44	0.46	0.48	0.5	0.52	0.54	0.56	0.58	0.60
Correlation $FWCP_n$	0.994	0.996	0.997	0.998	0.999	1	0.999	0.998	0.998	0.997	0.996
Correlation $FWCP^r$	0.950	0.981	0.984	0.989	0.995	1	0.995	0.992	0.965	0.986	0.960
B. Inflation Deviation:	-1%	-0.8%	-0.6%	-0.4%	-0 .2%	0%	0.2%	0.4%	0.6%	0.8%	1%
Correlation $FWCP^r$	0.951	0.952	0.952	0.972	0.974	1	0.995	0.955	0.953	0.953	0.952

Appendix E. Additional Figures and Tables for Robustness

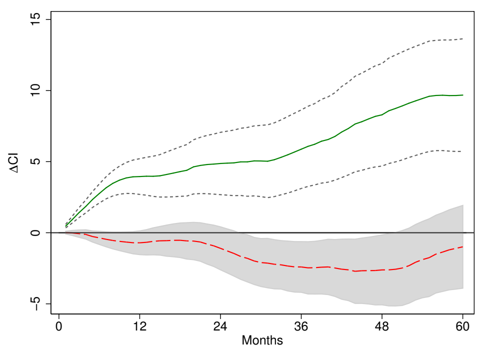
Figure A.5. Monetary Policy Shocks in Rigid and Flexible States: Unemployment and CI, 1980–2007



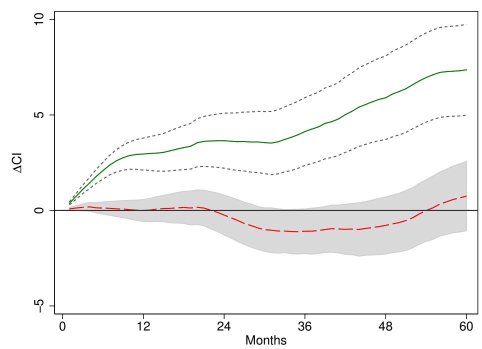
(a) Standard transformation, No controls



(b) Logit transformation, No controls



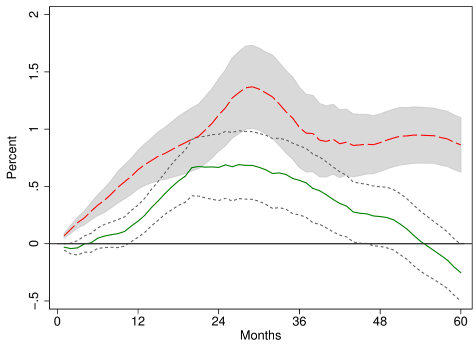
(c) Standard transformation, No controls



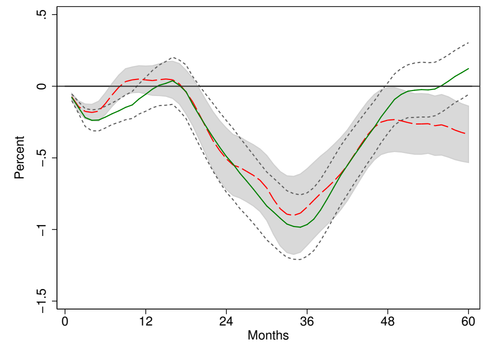
(d) Logit transformation, No controls

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

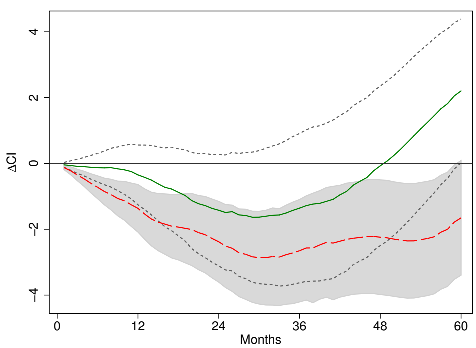
Figure A.6. Monetary Policy Shocks in Rigid and Flexible States: Expansionary vs Contractionary Shocks, Logit transformation, 1980–2007



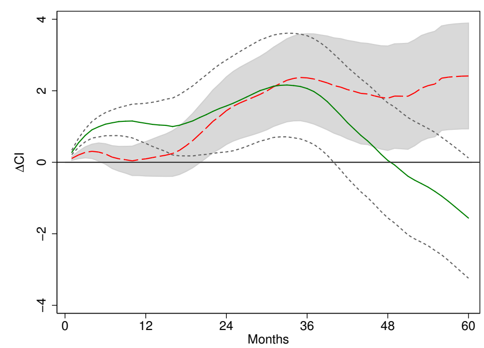
(a) Contractionary, UR



(b) Expansionary, UR

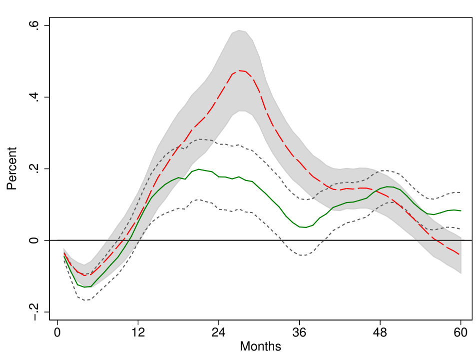


(c) Contractionary, CI

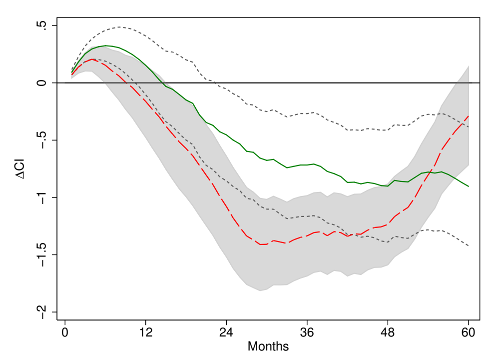


(d) Expansionary, CI

Figure A.7. Monetary Policy Shocks in Rigid and Flexible States: Robustness, 1980–2007



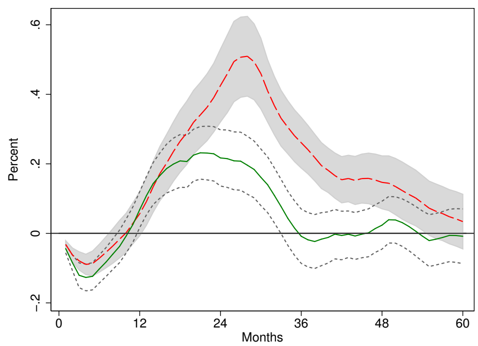
(a) Logit transformation, Additional controls, 1980–2007 UR



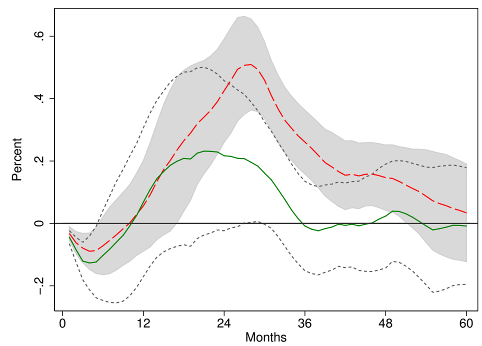
(b) Logit transformation, Additional controls, 1980–2007 CI

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

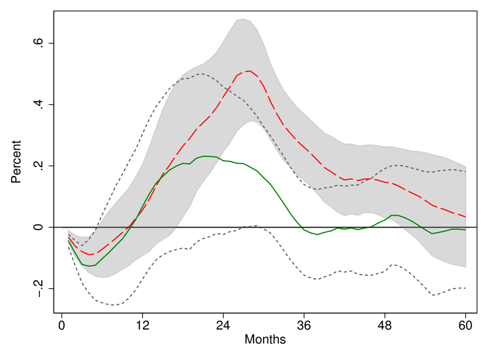
Figure A.8. Monetary Policy Shocks in Rigid and Flexible States; Unemployment with Logit Transformation: Standard errors, 1980–2007



(a) SURE estimator, clustered by state



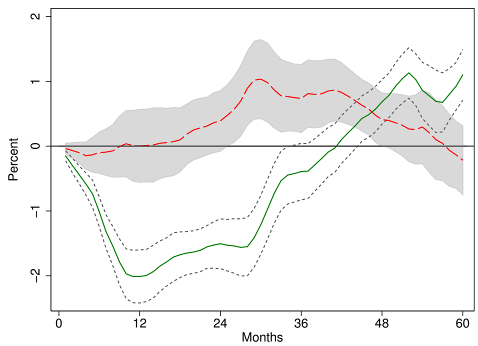
(b) SURE estimator, clustered by time



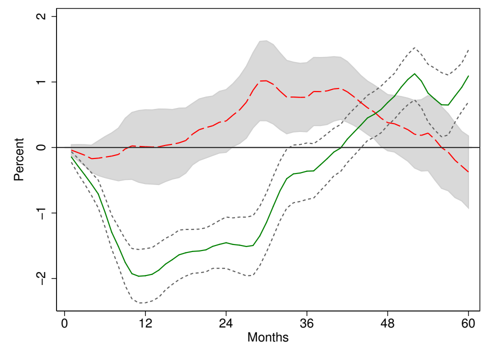
(c) SURE estimator, clustered by state and time

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

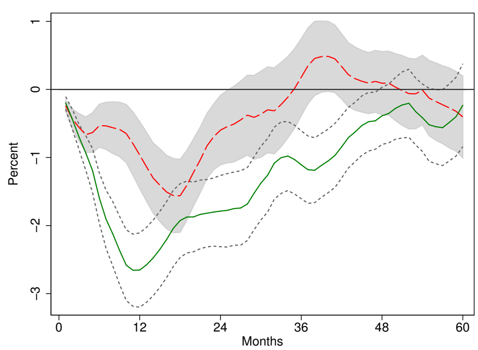
Figure A.9. Monetary Policy Shocks in Rigid and Flexible States: Announcement shocks with additional controls, Unemployment, Standard transformation



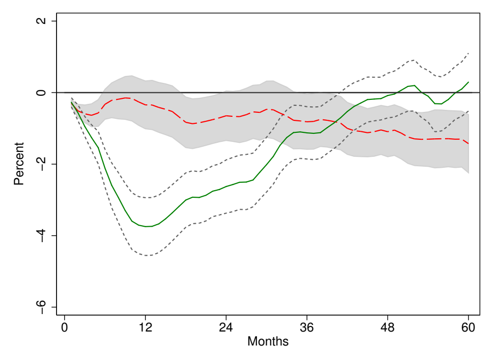
(a) GW (2016), tight interval, 1994-2007



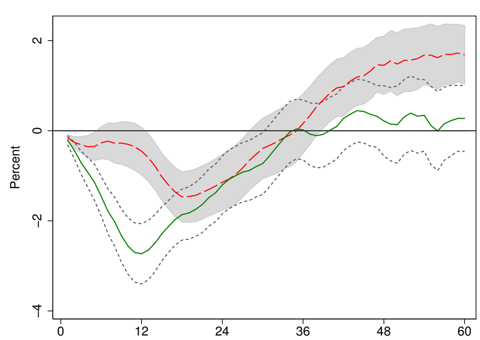
(b) GW (2016), wide interval, 1994-2007



(c) GK (2015), current FFR futures, 1988-2007



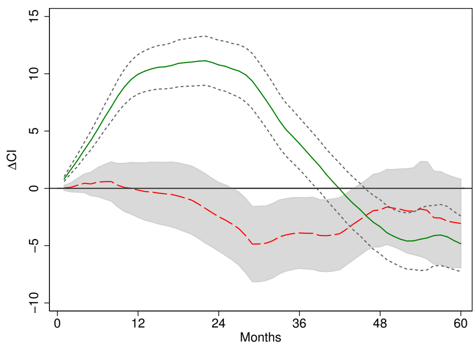
(d) GK (2015), 3-month ahead FFR futures, 1990-2007



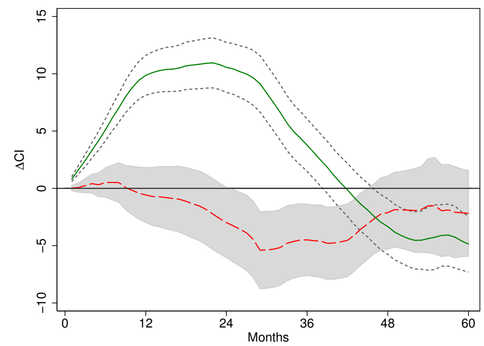
(e) GK (2015), year-ahead futures of Eurodollar deposits, 1984-2007

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. GW (2016) stands for Gorodnichenko and Weber (2016); and GK (2015) stands for Gertler and Karadi (2015).

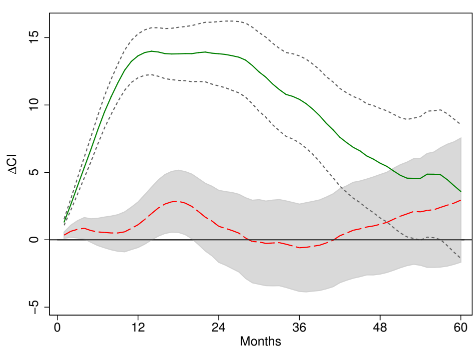
Figure A.10. Monetary Policy Shocks in Rigid and Flexible States: Announcement shocks with additional controls, Coincident, Standard transformation



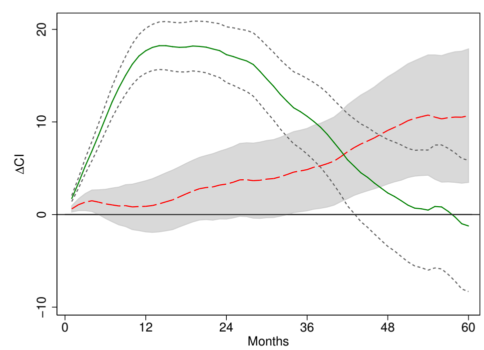
(a) GW (2016), tight interval, 1994-2007



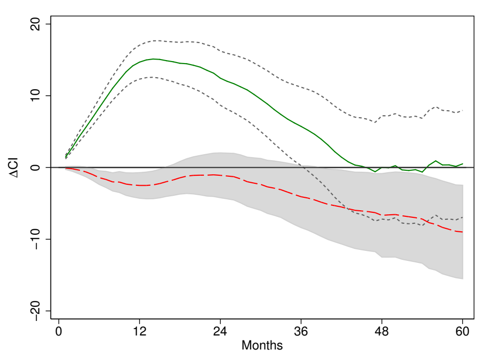
(b) GW (2016), wide interval, 1994-2007



(c) GK (2015), current FFR futures, 1988-2007



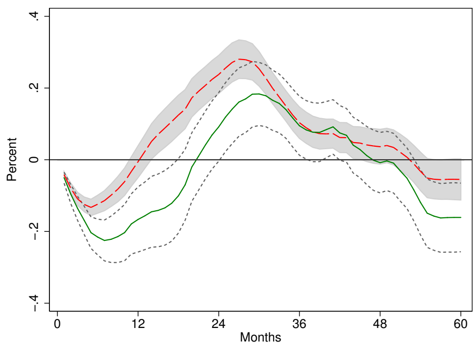
(d) GK (2015), 3-month ahead FFR futures, 1990-2007



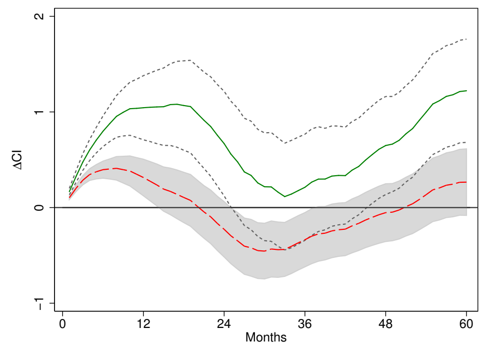
(e) GK (2015), year-ahead futures of Eurodollar deposits, 1984-2007

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. GW (2016) stands for Gorodnichenko and Weber (2016); and GK (2015) stands for Gertler and Karadi (2015).

Figure A.11. Monetary Policy Shocks in Rigid and Flexible States: Ratio between Minimum to Median wages, Standard transformation, 1980–2007



(a) Minimum wages, UR



(b) Minimum wages, CI

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

Table A.4: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity

Excl. Volcker Prime Spending	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Multiplier	1.520*	2.583***	1.466*	1.763**	2.878***	2.824***	2.499***
	(0.856)	(0.879)	(0.774)	(0.786)	(0.877)	(0.878)	(0.794)
Low Multiplier	-1.050	-0.377	-0.291	-0.0877	-1.419*	-1.375*	-1.252*
	(0.942)	(0.843)	(0.895)	(0.881)	(0.738)	(0.739)	(0.676)
Military Spending		-0.107		0.243	-0.111	-0.0995	-0.272
		(0.144)		(0.310)	(0.304)	(0.309)	(0.268)
Avg. Mortgage Rate					0.0319	0.0321	0.0228
					(0.0212)	(0.0213)	(0.0213)
Minimum Wage					0.154***	0.153***	0.0911*
					(0.0558)	(0.0573)	(0.0525)
Δ Firm Size					0.0168***	0.0171***	0.0171***
					(0.00613)	(0.00618)	(0.00650)
Δ Mobility					0.000286	0.000273	0.000703*
					(0.000450)	(0.000443)	(0.000423)
Δ % Services					-0.0510	-0.0511	-0.0842
					(0.0718)	(0.0717)	(0.0728)
Δ % Government					-0.137	-0.137	-0.174
					(0.119)	(0.119)	(0.121)
Union Power					-0.0481***	-0.0486***	-0.0348***
					(0.00501)	(0.00526)	(0.00491)
House Prices						0.0105	-1.416
						(0.0294)	(2.666)
LD V							0.272***
							(0.0480)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	1,121	1,121	1,121	1,121	1,070	1,070	1,019
R-squared	0.222	0.203	0.412	0.408	0.404	0.405	0.448

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state. Dependent variable is two-year growth in state GDP.*

Table A.5: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity

Excl. Volcker Incl. Direct Comp.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Multiplier	1.228*	2.096***	2.031***	2.086***	2.776***	2.763***	2.550***
	(0.708)	(0.692)	(0.607)	(0.648)	(0.816)	(0.813)	(0.778)
Low Multiplier	-1.237	-0.581	-0.400	-0.363	-1.249	-1.203	-0.855
	(0.886)	(0.876)	(0.917)	(1.016)	(0.894)	(0.887)	(0.740)
Millitary Spending		-0.0926		0.193	-0.139	-0.134	-0.364
		(0.147)		(0.345)	(0.331)	(0.333)	(0.279)
Avg. Mortgage Rate					0.0339	0.0342	0.0244
					(0.0219)	(0.0219)	(0.0218)
Minimum Wage					0.151***	0.149***	0.0890*
					(0.0545)	(0.0562)	(0.0524)
Δ Firm Size					0.0167***	0.0172***	0.0171***
					(0.00610)	(0.00613)	(0.00641)
Δ Mobility					0.000274	0.000258	0.000701
					(0.000450)	(0.000445)	(0.000428)
Δ % Services					-0.0536	-0.0534	-0.0831
					(0.0725)	(0.0725)	(0.0732)
Δ % Government					-0.135	-0.135	-0.171
					(0.120)	(0.120)	(0.122)
Union Power					-0.0479***	-0.0486***	-0.0359***
					(0.00484)	(0.00503)	(0.00482)
House Prices						0.0140	-0.896
						(0.0287)	(2.721)
LDV							0.268***
							(0.0473)
Constant	0.00815	0.00838	0.00107	-0.00922	-0.223	-0.239*	-0.137
	(0.00681)	(0.00966)	(0.00703)	(0.0172)	(0.137)	(0.145)	(0.140)
Observations	1,121	1,121	1,121	1,121	1,070	1,070	1,019
R-squared	0.224	0.214	0.410	0.410	0.410	0.410	0.450

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state. Dependent variable is two-year growth in state GDP.*

Table A.6: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity

Excl. Most Sensitive Prime Spending	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Multiplier	1.027	1.499	1.065	2.826*	2.486**	2.253**	1.320*
	(1.042)	(1.036)	(0.925)	(1.452)	(1.078)	(1.117)	(0.772)
Low Multiplie r	-1.345	-0.942	-0.903	0.892	0.499	0.456	-0.508
	(1.295)	(0.960)	(1.193)	(1.248)	(1.152)	(1.134)	(0.900)
Military Spending		-0.0229		-0.901	-0.919	-0.795	0.0427
		(0.138)		(0.820)	(0.759)	(0.775)	(0.479)
Avg. Mortgage Rate					0.0312*	0.0318*	0.0358*
					(0.0177)	(0.0177)	(0.0201)
Minimum Wage					0.139*	0.135*	0.144**
					(0.0711)	(0.0706)	(0.0621)
Δ Firm Size					0.0195***	0.0208***	0.0178***
					(0.00549)	(0.00563)	(0.00587)
Δ Mobility					0.000371	0.000305	0.000296
					(0.000458)	(0.000451)	(0.000383)
Δ % Services					-0.0916	-0.0915	-0.0346
					(0.0730)	(0.0729)	(0.0693)
Δ % Government					-0.191	-0.190	-0.125
					(0.134)	(0.134)	(0.124)
Union Power					-0.0464***	-0.0486***	-0.0301***
					(0.00622)	(0.00594)	(0.00667)
House Prices						0.0457*	-0.0483*
						(0.0252)	(0.0274)
LDV							0.243***
							(0.0480)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	1,104	1,104	1,104	1,104	1,058	1,058	1,012
R-squared	0.230	0.228	0.391	0.360	0.403	0.409	0.488

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state. Dependent variable is two-year growth in state GDP.*

Table A.7: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity

Excl. Most Sensitive Incl. Direct Compensation	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Multiplier	1.104	1.239	1.871**	3.127**	2.836***	2.703***	1.603***
	(0.866)	(0.862)	(0.854)	(1.287)	(0.875)	(0.885)	(0.618)
Low Multiplier	-1.399	-1.276	-0.511	0.896	0.513	0.516	-0.577
	(1.129)	(0.860)	(1.074)	(1.346)	(1.203)	(1.175)	(1.054)
Military Spending		-0.00632		-1.075	-1.081	-0.985	-0.0458
		(0.138)		(0.854)	(0.731)	(0.736)	(0.495)
Avg. Mortgage Rate					0.0347*	0.0355*	0.0374*
					(0.0186)	(0.0186)	(0.0203)
Minimum Wage					0.134*	0.131*	0.142**
					(0.0713)	(0.0709)	(0.0621)
Δ Firm Size					0.0193***	0.0207***	0.0179***
					(0.00546)	(0.00559)	(0.00585)
Δ Mobility					0.000393	0.000326	0.000295
					(0.000459)	(0.000453)	(0.000387)
Δ % Services					-0.0940	-0.0923	-0.0345
					(0.0739)	(0.0738)	(0.0702)
Δ % Government					-0.185	-0.182	-0.121
					(0.138)	(0.138)	(0.125)
Union Power					-0.0470***	-0.0495***	-0.0307***
					(0.00620)	(0.00596)	(0.00683)
House Prices						0.0475**	-0.0468*
						(0.0235)	(0.0272)
LDV							0.239***
							(0.0481)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	1,104	1,104	1,104	1,104	1,058	1,058	1,012
R-squared	0.232	0.232	0.386	0.360	0.402	0.407	0.489

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state. Dependent variable is two-year growth in state GDP.*

Table A.8: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity

Excl. Least Sensitive Prime Spending	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Multiplier	1.205	1.708*	1.002	2.064**	1.994**	1.818**	1.327**
	(0.930)	(0.954)	(0.758)	(1.026)	(0.807)	(0.823)	(0.649)
Low Multiplier	-1.207	-0.890	-0.975	-0.0263	-0.409	-0.392	-0.500
	(1.087)	(0.890)	(1.002)	(0.946)	(0.894)	(0.886)	(0.726)
Military Spending		-0.0329		-0.331	-0.320	-0.251	0.166
		(0.126)		(0.531)	(0.459)	(0.459)	(0.295)
Avg. Mortgage Rate					0.0338*	0.0347*	0.0337*
					(0.0183)	(0.0183)	(0.0204)
Minimum Wage					0.110**	0.107**	0.157***
					(0.0479)	(0.0478)	(0.0484)
Δ Firm Size					0.0208***	0.0219***	0.0187***
					(0.00614)	(0.00635)	(0.00662)
Δ Mobility					0.000466	0.000424	0.000529
					(0.000538)	(0.000540)	(0.000443)
Δ % Services					-0.114	-0.114	-0.0330
					(0.0765)	(0.0768)	(0.0684)
Δ % Government					-0.321***	-0.322***	-0.256**
					(0.122)	(0.122)	(0.111)
Union Power					-0.0386***	-0.0405***	-0.0315***
					(0.00607)	(0.00589)	(0.00592)
House Prices						0.0349	-0.0671***
						(0.0235)	(0.0255)
LDV							0.265***
							(0.0479)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	1,103	1,103	1,103	1,103	1,057	1,057	1,011
R-squared	0.220	0.216	0.372	0.358	0.398	0.401	0.482

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state. Dependent variable is two-year growth in state GDP.*

Table A.9: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity

Excl. Least Sensitive Incl. Direct Compensation	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Multiplier	1.053	1.343*	1.505**	2.180**	2.106***	2.020***	1.370**
	(0.774)	(0.777)	(0.704)	(0.955)	(0.741)	(0.746)	(0.553)
Low Multiplier	-1.331	-1.124	-0.859	-0.105	-0.517	-0.485	-0.828
	(0.977)	(0.805)	(0.920)	(1.025)	(0.966)	(0.950)	(0.889)
Military Spending		-0.0158		-0.369	-0.344	-0.291	0.199
		(0.126)		(0.582)	(0.490)	(0.489)	(0.321)
Avg. Mortgage Rate					0.0354*	0.0366*	0.0342*
					(0.0192)	(0.0193)	(0.0208)
Minimum Wage					0.106**	0.103**	0.154***
					(0.0489)	(0.0489)	(0.0475)
Δ Firm Size					0.0205***	0.0218***	0.0187***
					(0.00611)	(0.00630)	(0.00660)
Δ Mobility					0.000462	0.000420	0.000510
					(0.000538)	(0.000541)	(0.000444)
Δ % Services					-0.117	-0.116	-0.0371
					(0.0766)	(0.0767)	(0.0696)
Δ % Government					-0.319**	-0.317**	-0.259**
					(0.125)	(0.126)	(0.110)
Union Power					-0.0386***	-0.0408***	-0.0311***
					(0.00592)	(0.00582)	(0.00603)
House Prices						0.0366	-0.0657***
						(0.0227)	(0.0251)
LDV							0.239***
							(0.0481)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	1,103	1,103	1,103	1,103	1,057	1,057	1,011
R-squared	0.222	0.221	0.372	0.361	0.401	0.403	0.486

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state. Dependent variable is two-year growth in state GDP.*

Table A.10: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity: 2-way clustering

Excl. Least Sensitive Prime Spending	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Multiplier	1.737**	2.197**	1.554**	2.484**	2.159**	1.993**	1.442**
	(0.757)	(0.981)	(0.604)	(1.039)	(0.852)	(0.795)	(0.729)
Low Multiplier	-0.888	-0.586	-0.396	0.439	-0.0654	-0.0279	-0.291
	(1.362)	(1.152)	(1.397)	(1.176)	(1.260)	(1.226)	(0.856)
Military Spending		-0.0361		-0.323	-0.289	-0.223	0.167
		(0.142)		(0.596)	(0.548)	(0.521)	(0.380)
Avg. Mortgage Rate					0.0304*	0.0314*	0.0324
					(0.0170)	(0.0170)	(0.0214)
Minimum Wage					0.132**	0.128**	0.156***
					(0.0604)	(0.0629)	(0.0596)
Δ Firm Size					0.0180***	0.0192***	0.0168***
					(0.00497)	(0.00531)	(0.00574)
Δ Mobility					0.000484	0.000434	0.000419
					(0.000873)	(0.000858)	(0.000819)
Δ % Services					-0.109	-0.108	-0.0433
					(0.0912)	(0.0901)	(0.0726)
Δ % Government					-0.176	-0.176	-0.119
					(0.147)	(0.147)	(0.128)
Union Power					-0.0442	-0.0465***	-0.0293***
						(0.00256)	(0.00778)
House Prices						0.0394	-5.961
						(0.0451)	(3.766)
LDV							0.257***
							(0.0551)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	1,223	1,223	1,223	1,223	1,172	1,172	1,121
R-squared	0.237	0.230	0.389	0.367	0.409	0.413	0.490

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state and time. Dependent variable is two-year growth in state GDP.*

Table A.11: 2SLS Estimates of Military Expenditure Multiplier and Wage Rigidity: 2-way clustering

Excl. Least Sensitive Incl. Direct Compensation	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Multiplier	1.560**	1.809**	2.052***	2.730**	2.387***	2.300***	1.604**
	(0.719)	(0.804)	(0.617)	(1.062)	(0.828)	(0.787)	(0.649)
Low Multiplier	-0.956	-0.767	-0.221	0.536	0.00570	0.0460	-0.401
	(1.173)	(0.969)	(1.202)	(1.099)	(1.078)	(1.048)	(0.762)
Military Spending		-0.0230		-0.429	-0.371	-0.316	0.136
		(0.140)		(0.659)	(0.573)	(0.543)	(0.375)
Avg. Mortgage Rate					0.0329*	0.0342**	0.0339
					(0.0170)	(0.0169)	(0.0211)
Minimum Wage					0.127**	0.123*	0.153**
					(0.0623)	(0.0650)	(0.0600)
Δ Firm Size					0.0178***	0.0191***	0.0168***
					(0.00493)	(0.00520)	(0.00574)
Δ Mobility					0.000503	0.000450	0.000421
					(0.000872)	(0.000857)	(0.000817)
Δ % Services					-0.111	-0.109	-0.0450
					(0.0914)	(0.0900)	(0.0729)
Δ % Government					-0.172	-0.169	-0.118
					(0.152)	(0.151)	(0.131)
Union Power					-0.0447	-0.0471***	-0.0296***
						(0.00250)	(0.00770)
House Prices						0.0420	-5.724
						(0.0453)	(3.823)
LDV							0.254***
							(0.0550)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	1,223	1,223	1,223	1,223	1,172	1,172	1,121
R-squared	0.239	0.236	0.384	0.364	0.409	0.411	0.492

*Note: *, **, *** denote significance at the 10, 5, and 1% level, respectively. Estimates obtained using 2SLS. Standard errors are in parentheses, clustered by state and time. Dependent variable is two-year growth in state GDP.*