

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 taxes. In line with the role of sticky wages in New Keynesian models, we find that contractionary monetary policy and tax shocks increase unemployment and decrease economic activity in rigid states considerably more than in flexible states. We also find larger and more persistent effects of monetary and tax policy shocks for states where the ratio between minimum and median wage is higher and for states that do not have right-to-work legislation.

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1. Introduction

Empirical research has shown that shocks in monetary policy and taxes have persistent effects on output and employment, while estimates of fiscal spending multipliers often exceed unity. The exact transmission mechanisms of these shocks continue to be debated. Most macroeconomic models, however, 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 larger effect upon impact, as well a more persistent effect of these shocks.¹ Empirical evidence in support of the role that such rigidities play in the transmission 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. We test this hypothesis using data on shocks in the federal funds rate (FFR) and federal tax changes. [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 tax policy, we use two alternative measures: exogenous changes in tax policy derived using the narrative approach ([Romer and Romer, 2010](#)) and a measure of average expected future tax rates from one to five years ahead ([Leeper et al., 2012](#)).

By considering differences in the impact of national shocks across states, we exploit three characteristics of the United States. First, the United States forms a fiscal and monetary union. Hence, states experience identical national shocks in monetary and federal tax policies (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 that *real depreciation* through price adjustments has a more pronounced 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 are 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.

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

²Evidence suggests that price rigidities are driven by the wage 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.

We find 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 and more persistent increases in unemployment and declines in output after monetary policy shocks. This relationship is revealed using local projection models, with various dependent variables (unemployment, the coincident index, and state-level GDP). Our results are robust to the use of various outlier treatments as well as comprehensive controls for labor market institutions and sectoral composition. Similar results also hold for exogenous changes in taxes, although they are slightly less robust than those for monetary policy. States with higher nominal rigidities experience larger increases in unemployment and declines in output after a tax increase compared to states that are more flexible. We further show that institutional factors that could drive wage rigidities—like minimum wages and right-to-work-legislation—have a similar effect. States with a higher minimum to median wage ratio and those without right-to-work legislation experience larger and more persistent effects of monetary and tax policy shocks. 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 in only 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 \(1996\)](#) also study the role of wage stickiness in propagating the Great Depression. Using panel data on 22 countries they find that nominal wages adjusted quite slowly to falling prices and that the resulting rise in real wages significantly reduced industrial production. A 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.³ 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

³In 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 relatively the slowest wage growth in the recent recovery.

volatility of stock market returns than firms with more flexible prices.⁴ 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.⁵

More generally, this paper builds on papers that study the effects of monetary and fiscal policy shocks in the United States.⁶ 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) revisits these effects and concludes that they are consistent with the real effect of shocks derived from Taylor rules. Olivei and Tenreyro (2007) conjecture the importance of the effect of wage rigidities on the impact of monetary shocks. They 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 Carlino and Defina (1998) examine the differential impact of monetary policy across U.S. states and regions and find that manufacturing regions experience larger reactions to monetary policy shocks than industrially-diverse regions.

Fiscal shocks considered in this paper are federal tax shocks. To calculate fiscal multipliers most studies use military spending and federal tax shocks. As state exposure to military spending is heterogeneous, we focus on federal tax multipliers.⁸ Romer and Romer (2010) show that an exogenous increase in taxes, identified using narrative methods, have a long-term negative effect on output. At its peak, an increase in taxes amounting to 1% of GDP cause a 2 to 3% reduction in GDP. Mertens and Ravn (2011, 2013) further decompose Romer and Romer (2010) shocks into different categories, including unanticipated personal income tax changes and unanticipated corporate income tax changes, and show that consumption and investment react more to personal income tax cuts than to corporate income tax cuts. Furthermore, Leeper et al. (2012) and Leeper et al. (2013) calculate a measure of expected tax changes based on the spread between federal bonds and

⁴Favilukis and Lin (2016) study the relationship between sticky wages and risk in an asset-pricing framework.

⁵Using 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 employment shifted from formal to informal due to regulatory obstacles to adjusting 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).

⁶See, for example Christiano et al. (2005), Christiano et al. (1999) and Blanchard and Perotti (2002). For a recent comprehensive survey of the literature, including results using structural vector autoregressive approach, see Ramey (2016).

⁷Olivei 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.

⁸Furthermore, state heterogeneity in exposure to military spending is potentially correlated with institutional factors behind wage rigidities.

municipal bonds and show that expectations of future tax increases raise output on impact and produce contractionary effects only after one year. [Ramey \(2016\)](#) shows that these news shocks actually explain more of the variance of the output than [Romer and Romer \(2010\)](#) shocks.

Lastly, 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 wages of job-stayers than of job-changers (e.g., [Devereux and Hart, 2006](#); [Haefke et al., 2013](#)), as states with high rates of job destruction and creation have lower rigidities. Similarly, state-level findings confirm cross-country evidence on the positive correlation between wage rigidities and institutions that affect wage bargaining, such as unionization and employment protective legislation (e.g., [Dickens et al., 2007](#); [Alvarez et al., 2006](#)).

The remainder of this paper is structured as follows. Section 2 presents the empirical strategy used to relate rigidities to the impact of monetary and fiscal policy shocks. Section 3 provides estimates of downward wage rigidities at the state level. In section 4 we discuss main results and various robustness checks. Section 5 concludes.

2. Empirical Methodology

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. 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 rigidities. The 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, such that higher values of the CI imply greater economic activity (for details see [Crone and Clayton-Matthews, 2005](#)). Unemployment and the CI are two of few real state-level variables available at a monthly frequency, as state-level GDP (GSP) is measured annually.¹⁰ 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 the CI more strongly.

We use [Romer and Romer \(2004\)](#) monetary policy shocks. Because changes in policy rates are endogenous to macroeconomic forecasts, [Romer and Romer \(2004\)](#) estimate these shocks in two steps. First, they derive intended changes in the FFR from narrative records of internal briefings to the FOMC. Second, they regress predicted developments in interest rates 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

⁹Influential national studies include, e.g., [Blinder and Choi \(1990\)](#), [Kahn \(1997\)](#), [Campbell and Kamlani \(1997\)](#), [Card and Hyslop \(1997\)](#), [Altonji and Devereux \(2000\)](#), [Fehr and Goette \(2007\)](#), and [Dickens et al. \(2007\)](#).

¹⁰The BEA has recently released quarterly state-GDP data but only from 2005 onwards.

through the end of 2007.¹¹ 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 \(2016\)](#). We also use a combined (proxy regression) approach, where the actual shocks are residuals from regressing the [Gertler and Karadi \(2015\)](#) announcement shocks on Greenbook variables by FOMC date (see [Ramey, 2016](#)). All these shocks 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. The effect of policy shocks at $t+h$ is estimated by regressing dependent variables at $t+h$ on shocks and covariates at time t . Responses therefore do not rely on the nonlinear transformations of reduced-form parameters as in VARs. Following [Auerbach and Gorodnichenko \(2012\)](#) and [Ramey and Zubairy \(2017\)](#), we use a variant of the smooth transition local projection model to allow for inference in both rigid and flexible states:

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

where subscripts refer to state s at time t , y is our variable of interest, which is either the unemployment rate (UR), or the coincident index (CI). i denotes shocks in monetary policy, x is a vector of controls, and c is a vector of deterministic covariates. z is our measure of wage rigidities, transformed along function $F(z)$, which ranges between 0 (for states with lowest rigidities) and 1 (for states with highest rigidities). Details are provided in section 4. The effect of shocks on unemployment and the CI is captured by γ , where γ^R measures the effect in the most rigid state while γ^F measures the effect in the most flexible state. Our hypothesis implies that, for example, the value of γ^R should exceed γ^F for a reaction of unemployment to contractionary monetary policy shock. For robustness we also estimate Eq. (1) on state-level GDP using the [Arellano and Bond \(1991\)](#) System GMM estimator.

We estimate Eq. (1) separately for each horizon (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

¹¹The resulting shocks are plotted in figure E.1 in the appendix E. 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 1980–2007.

advantage has made local projections an increasingly popular alternative to VARs or ARDLs.¹² 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 a 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

To assess whether wage rigidities are an integral part of the transmission of fiscal policy shocks, we analyze shocks to federal tax rates. The advantage of tax shocks is that, similarly to monetary policy shocks, their ex-ante economic effects should be relatively homogeneous across states. Alternative shocks, like local taxes or fiscal spending, may be endogenous to local economic conditions and hence less suited.

We use two measures of federal tax shocks, a narrative one from [Romer and Romer \(2010\)](#) and an expectations one from [Leeper et al. \(2012\)](#).¹³ [Romer and Romer \(2010\)](#) estimate quarterly tax shocks using a narrative record from, for instance, presidential speeches and congressional reports. Because taxes may be altered in response to economic conditions, they classify tax changes as endogenous or exogenous based on whether they target short-term or long-term growth. A tax is classified as exogenous if political reports do not mention short-term economic conditions as a reason for the change. [Romer and Romer \(2010\)](#) show that an increase in such taxes have a long-term negative effect on output. [Romer and Romer \(2010\)](#) tax shocks are the fiscal counterpart of our monetary policy shocks. Data are available up to 2007, such that our monetary policy and fiscal policy shocks can be analyzed for the same time sample.

[Leeper et al. \(2012\)](#) provide a measure of average expected future tax rates from one to five years ahead. Anticipated and unanticipated tax changes should have very different effects on macroeconomic variables, as economic agents adapt their behavior when expecting a tax increase in the future. [Leeper et al. \(2012\)](#) derive expected tax changes based on the spread between federal bonds and municipal bonds. Because municipal bonds are exempt from federal taxes, differences between risk-adjusted yields of municipal bonds and treasuries can be used to assess expected tax changes. [Leeper et al. \(2013\)](#) show (in an unpublished appendix) that expectations of future tax increases (1–5 years ahead) temporarily raise output at the time of the news. Furthermore, [Ramey \(2016\)](#) finds that these shocks produce significant contractionary effects after about three years. The middle part of table 1 presents summary statistics for these variables.

To estimate the effect of these shocks on unemployment and economic activity, we deploy Eq. (1), where we replace monetary policy shocks, i , with tax rate shocks, τ . We also amend the set of control variables in line with the literature. Details are provided in section 4.2.

¹²See, e.g., [Ho \(2008\)](#), [Furceri and Zdzenicka \(2012\)](#), [Jordà et al. \(2013\)](#), and [da Rocha and Solomou \(2015\)](#).

¹³The resulting shocks are plotted in figures E.2–E.3 in appendix E.

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

	Mean	SD	Obs.	Min.	Max.	Source	Type
<i>Dependent Variables</i>							
Unemployment Rate	5.832	2.060	17,136	2.1	18.8	BLS	
Coincident Index	110.625	28.432	16,800	57.527	232.740	Phil. Fed	
<i>Monetary policy shocks</i>							
Narrative monetary policy shocks	0.013	0.297	384	-3.259	1.885	CO (2012)	
Announcement: tight window	-0.010	0.068	191	-0.438	0.163	GW (2016)	
Announcement: wide window	-0.010	0.069	191	-0.463	0.152	GW (2016)	
Announcement: current FFR futures	-0.017	0.062	257	-0.423	0.146	GK (2015)	
Announcement: 3-month ahead FFR futures	-0.015	0.051	243	-0.290	0.092	GK (2015)	
Announcement: year-ahead fut. ED dep.	-0.011	0.058	315	-0.381	0.213	GK (2015)	
Combined: current FFR futures	0.000	0.031	216	-0.275	0.114	R (2016)	
Combined: 3-month ahead FFR futures	0.000	0.036	216	-0.264	0.128	R (2016)	
Combined: 6-month-ahead fut. ED dep.	0.000	0.037	288	-0.207	0.160	R (2016)	
<i>Tax shocks</i>							
Narrative tax shocks	-0.016	0.247	124	-1.356	0.698	RR (2010)	
1-5 years ahead expect. future tax rates	0.298	0.110	116	0.078	0.508	LRW (2012)	
<i>Control Variables</i>							
Mobility	0.287	0.046	1,836	0.184	0.694	CBS	I(1)
Firm Size	18.75	3.240	1,836	10.36	29.32	CBS	I(1)
Minimum Wage	0.424	0.062	1,683	0.257	0.670	BLS	I(0)
Unionization	0.144	0.064	1,224	0.008	0.348	CPS	I(0)
Union Power	0.562	0.496	1,938	0	1	C (2014)	I(0)
% Services	0.684	0.051	1,734	0.543	0.822	CPS	I(1)
% Government	0.056	0.027	1,734	0.024	0.233	CPS	I(1)
Education	4.058	0.226	1,734	3.000	4.547	CPS	I(1)

Notes: CO (2012) stands for [Coibion \(2012\)](#); GW (2016) stands for [Gorodnichenko and Weber \(2016\)](#); GK (2015) stands for [Gertler and Karadi \(2015\)](#); C (2014) stands for [Collins \(2014\)](#); RR (2010) stands for [Romer and Romer \(2010\)](#); R (2016) stands for [Ramey \(2016\)](#) and LRW (2012) stands for [Leeper et al. \(2012\)](#).

2.3. Control Variables

We include control variables that influence wage rigidities and that may affect policy shocks through alternative channels. Literature on labor market institutions provides a number of candidates, such as 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.¹⁴ Mobility is measured through the reallocation rate, which is the sum of job destruction and job creation rates. Business Dynamics Statistics (BDS) of the Census Bureau publishes the average number of employees per firm, a proxy for firm size. [Collins \(2014\)](#) provides data on union power, which is defined by the absence of *right-to-work* laws in a state.¹⁵ To account for differences in state minimum wages, we control for the ratio of minimum to median wages. Data are from the CPS and BLS.

¹⁴Several control variables are available at annual frequency. We interpolate these variables to obtain monthly estimates; however, we find similar results if we use in the estimation the same value within a year.

¹⁵Right-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. We measure union power as a dummy equaling 1 in states without these laws.

Other control variables relate to the structure of the economy. We include in the set of controls the share of workers employed by the government to account for the insensitivity of government expenditures to shocks. We control for sectoral composition with the share of workers employed in services, as certain industries may be more subject to wage rigidities and are thus more vulnerable to demand fluctuations.¹⁶ We include average education for a similar purpose, measured along CPS classifications. The lower part of table 1 details the summary statistics for these controls.

3. Data on Wage Rigidities

We obtain annual estimates of state-level rigidities 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.¹⁷ 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.

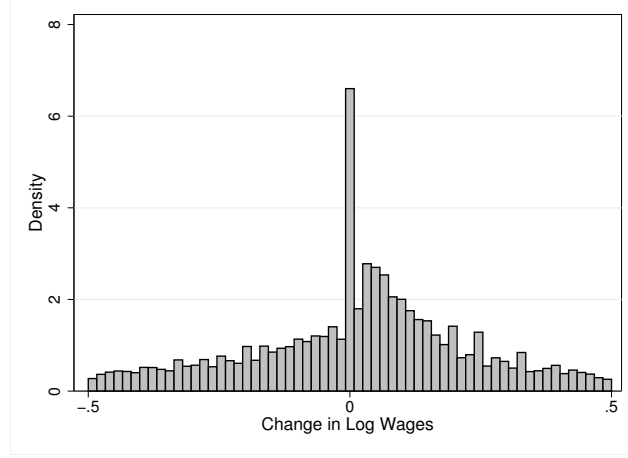
To calculate wage changes, we calculate the difference between the logarithm of hourly wages at the end of the first and second cycles. Because household compositions change over time, we deploy an algorithm developed by [Madrian and Lefgren \(1999\)](#) to validate panel matches. Based on changes across 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 0.5.¹⁸ We drop data from 1985 and 1996 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. Table E.1 in appendix E provides summary statistics. Figure 1 displays the distribution of wage

¹⁶Manufacturing 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. They provide evidence that construction workers are exposed to the highest nominal wage rigidities.

¹⁷The Panel Study of Income Dynamics (PSID) is a commonly used alternative, but it is too small for estimation of wage rigidities 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.

¹⁸Correlations with rigidity measures using truncation between 0.4 and 0.6 exceed 0.99. Details are in appendix C.

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



changes. The histogram of nominal changes shows a characteristic spike in the distribution around 0—that is, a disproportionate number of employees endure wage freezes. The distribution is also asymmetric, in the sense that wage cuts occur less frequently than wage increases.

The large number of nominal freezes is in line with the notion that firms are hesitant to cut wages when needed. Somewhat more surprising is the frequency of large wage changes. 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.¹⁹ 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,

$$FWCP_{s,t}^n = \frac{f_{s,t}^n}{c_{s,t}^n + f_{s,t}^n}, \quad (2)$$

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 prevented wage cuts 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

¹⁹See footnote 21.

Table 2: Average Nominal Wage Rigidities by State

<i>Average</i>	<i>0.1949</i>	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***		

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

since been used by, e.g., [Holden and Wulfsberg \(2008\)](#), [Dias et al. \(2013\)](#), and [Centeno and Novo \(2012\)](#).

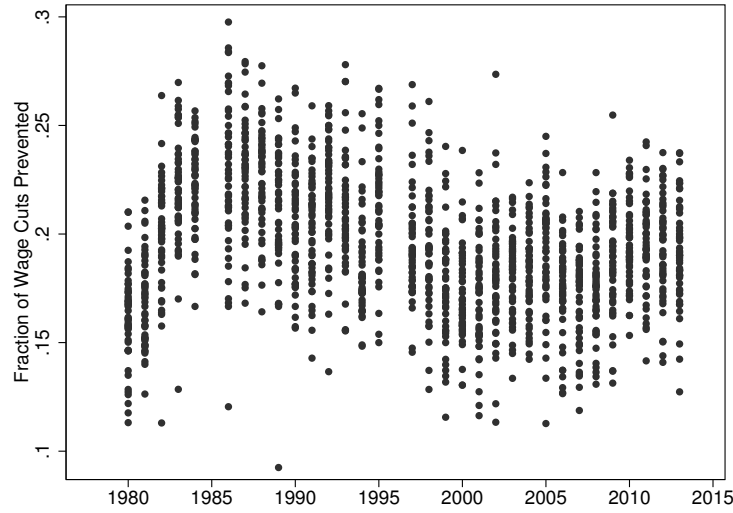
Several other measures of downward nominal wage rigidities have been proposed in the literature. Many of these measures rely on regression analysis and thus are less appropriate for use in our paper, as they are coupled with uncertainty and depend on an imposed specification. A downside of $FWCP^n$ is 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 our measure of nominal rigidities, $FWCP^n$, implicitly assumes absence of real rigidities.²⁰ In our case 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 in the United States. The analysis of real wage rigidities is in appendix B.

Table 2 reports average rigidities by state. Average nominal rigidity is 0.1918.²¹ It implies that around 19% of attempted wage cuts were prevented by nominal rigidities. Second, states exhibit significantly different levels of wage rigidities. Results show that average $FWCP^n$ differs significantly from national average in 23 states.

²⁰If real and nominal rigidities co-exist in an environment with positive inflation and employees receive positive nominal wage changes that equal the inflation rate (real rigidity is binding) then the nominal rigidity cannot be correctly measured.

²¹Our estimates are lower than reported in [Dickens et al. \(2007\)](#). 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 either the first or the 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.

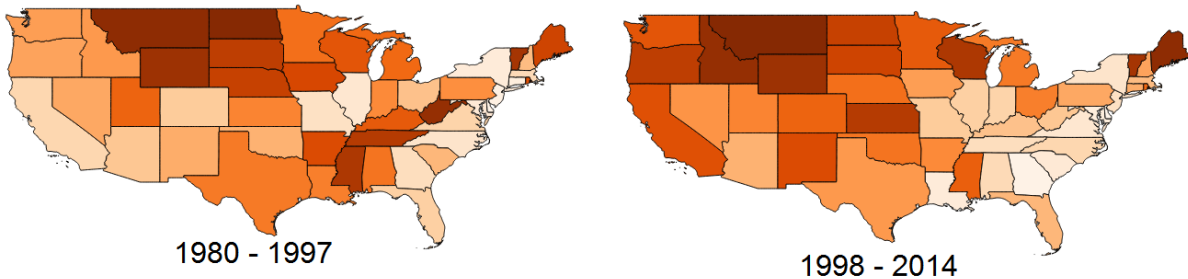
Figure 2. Distribution of Downward Nominal Wage Rigidities, 1980-2014



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

Figure 2 provides insight into the evolution 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 after 2005. 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 tend to be slightly more flexible, and states in the north-central part of the United States tend 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.²²

Figure 3. Relative Downward Nominal Rigidities across States



Light: low rigidity; Dark: high rigidity.

²²For the monthly analysis of monetary policy shocks, we interpolate rigidity measures.

Table 3: Estimations Labor Market Institutions and Nominal Wage Rigidities

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

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

3.3. Correlation with Labor Market Institutions

To confirm the validity of our rigidity measures, we verify that correlations with labor market institutions and sectoral composition run in the appropriate direction. Table 3 presents regression results using $FWCP^n$ as the dependent variable. 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 effects are highly significant and robust to the inclusion of controls for sectoral composition in columns 3 and 4. 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 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](#)). Combined, these results affirm the validity of our measures.

4. Estimation Results

This section uses the results on nominal wage rigidities to test our hypothesis of a positive correlation between rigidities and the impact of monetary and fiscal policy shocks. Section 4.1 discusses monetary policy results, while section 4.2 focuses on fiscal policy results.

For the analysis of both fiscal and monetary policy shocks, we transform our measure of rigidities such that $F(z)$ in Eq. (1) ranges between 0 and 1. This transformation assures that γ^R represents the effect of shocks in the rigid state, while γ^F represents the effect of shocks in the flexible state. We transform our measure for rigidities $FWCP$ in two 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, our estimations captures the effect of a relative position of the degree wage rigidities for a given state in a particular year compared to the mean level of rigidities, and not the effect of an absolute value of nominal rigidities. The second version is a non-linear transformation of $FWCP$, where $F(z)$ is defined as:

$$F(z_{s,t}) = \frac{\exp[\xi \frac{z_{s,t} - c}{\sigma}]}{1 + \exp[\xi \frac{z_{s,t} - c}{\sigma}]}, \quad (3)$$

where z is the standardized value for wage rigidity and c and σ are its mean and standard deviation, respectively.²³ We label this transformation as a *logistic* transformation. The logistic transformation places less weight on extreme observations compared to the standard transformation: It assigns more weight to observations that are closer to the median wage rigidity when estimating γ^R and γ^F . ξ governs how much weight we give to outliers and is calibrated to 2.²⁴ Our impulse responses are plotted for a hypothetical state that is either in all years the most flexible state or the most rigid state in our sample. Because the logistic transformation explicitly deals with potential outliers, the interpretation of flexible and rigid states is closer to the actual behavior of states that are, on average, among the most flexible and the most rigid states.

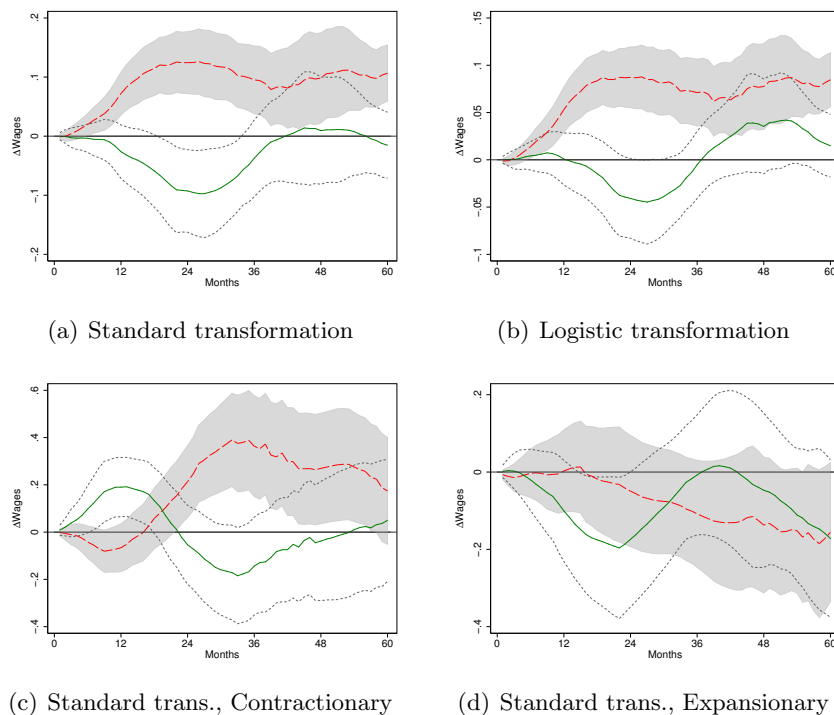
4.1. Monetary Policy Shocks

In this section we present results from estimating Eq. (1) using monetary policy shocks. We start by discussing the effect of monetary policy shocks on wages in rigid and flexible states (section 4.1.1) to confirm that monetary policy transmission in both states goes via the effect on wages. Section 4.1.2 focuses on impulse response functions of unemployment and the CI to monetary policy shocks, while sections 4.1.3–4.1.7 detail a number of robustness checks, including results conditional on two institutional factors behind wage rigidities (section 4.1.6).

²³This follows Auerbach and Gorodnichenko (2012) and Ramey and Zubairy (2017) in their approach to define recessions and expansions.

²⁴This is in the range considered by Tenreyro and Thwaites (2016) and Auerbach and Gorodnichenko (2012).

Figure 4. Monetary Policy Shocks in Rigid and Flexible States: Median Wages 1980–2007



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

4.1.1. Response of Wages to Monetary Policy Shock

We start our investigation of conditional effects of monetary policy shocks on output by looking at the wage channel of the monetary policy transmission mechanism. To assure that our measure captures the degree of rigidities and not, for example, measurement error—as wages are imperfectly measured—we check that higher values of our rigidity measure imply a smaller downward adjustment of wages after contractionary monetary policy shocks. Several authors have pointed out that measurement error can influence the rigidity measure: While [Bound and Krueger \(1991\)](#) argue that there is over-reporting of income among the low income households in the CPS, evidence from other datasets show that nominal wage cuts are over-reported and nominal wage freezes are under-reported ([Altonji and Devereux, 2000](#)). [Dickens et al. \(2007\)](#) argues that because the autocovariance of individual wage changes is positively correlated with measures of nominal wage rigidity in household-level data, rigidity measures are biased downward by measurement error in the data. However, potential concerns for our results would only arise if our measure of downward nominal wage rigidities would be correlated with measurement error: For example, if higher wage flexibility is potentially associated with higher measurement error, estimates of the effect of monetary policy on real activity could be biased down in the flexible state.

We thus estimate Eq. (1) with median wages as the dependent variable. Figure 4 presents the results. The impulse response functions (IRFs) plot the effect of a 1 percentage point contractionary

shock. All regressions include state fixed effects to account for state-specific constant factors and a time trend. Further controls include mobility, the share of the public and services sector in employment, the level of the FFR, a lagged monetary policy shock and a lagged dependent variable to account for the persistence. Standard errors are clustered at the state level to correct for within-panel correlations and heteroskedasticity. The sample runs from January 1980 to December 2007.

Figures 4(a)-(b) plot the effect of wage rigidities for standard and logistic transformations. Dashed (red) lines present results for the rigid state and solid (green) lines for the flexible state. Results for both transformations are in line with expectations: A contractionary shock only leads to a decline in median wages in the flexible state, while, surprisingly, there is even an increase in wages in the rigid state (see also, e.g., [Daly and Hobijn, 2014](#); [Abbritti and Fahr, 2013](#)). This increase in wages may be explained through a compositional effect if the monetary policy shock leads to a reduction in employment of below-median earners.

Because our measure of wage rigidities primarily works downwards, the effect of monetary policy shocks on wages should be particularly different after a contractionary shock.²⁵ In figures 4(c)-(d), we therefore study contractionary and expansionary shocks separately. Results show that the difference between flexible and rigid states is larger after contractionary shocks for most of the forecast horizon, although contractionary shocks have an opposite effect initially. For expansionary shocks, the initial effect on wages in the flexible state is also larger, but not significantly. These results confirm that our measure of wage rigidities is able to correctly identify the differentiated response of wages across U.S. states after the monetary policy shock.

4.1.2. Response of Unemployment and Output to Monetary Policy Shock

Figure 5 presents the responses of unemployment and economic activity to [Romer and Romer \(2004\)](#) monetary policy shocks. We present results using both standard and logistic transformations of our measure of wage rigidities. When estimating Eq. (1) we use the same control variables and standard errors as used in the section above. Results show that monetary policy effects are significantly different depending on the degree of wage rigidities.

Figures 5(a)-(b) plot results for unemployment, where we observe that the effect of monetary policy shocks is deeper and more persistent in the rigid state compared to the flexible state. A 1 percentage point contractionary shock raises the unemployment rate around 0.6 percentage point in the third year after the shock for the rigid state, while the effect is less than one-half that size in the flexible state. A positive monetary policy shock is initially expansionary for both rigid and flexible states for a few months. After that, the shock is contractionary, at least for the rigid state, where the effect quickly reaches its peak around 26 months after the shock. In the rigid state, the effect lasts for around four-and-a-half years. In the flexible state, the effect becomes insignificant after about two years using the standard transformation, and after three years using the outlier-robust logistic transformation. This difference is in line with expectations, as there are some outliers among the

²⁵Also upward adjustments in prices and wages can be 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](#)).

lower estimates of downward wage rigidities (see figure 2). Results are also consistent with figure 4 in the sense that changes in median wages lead the effect on unemployment by a few months—at least in the rigid state—and similarly produce large differences between the two states in the second and third year after the shock.

Figures 5(c)-(d) display the IRFs for the coincident index (CI). Qualitatively, the results are very similar to those on unemployment. Responses in the rigid state are significantly negative over the entire forecast horizon in both specifications. 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 logistic 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 both cases. In line with results for the unemployment rate, the responses in flexible and rigid states support our hypothesis that the effect of monetary policy shocks is deeper and more persistent in the rigid than in the flexible state.

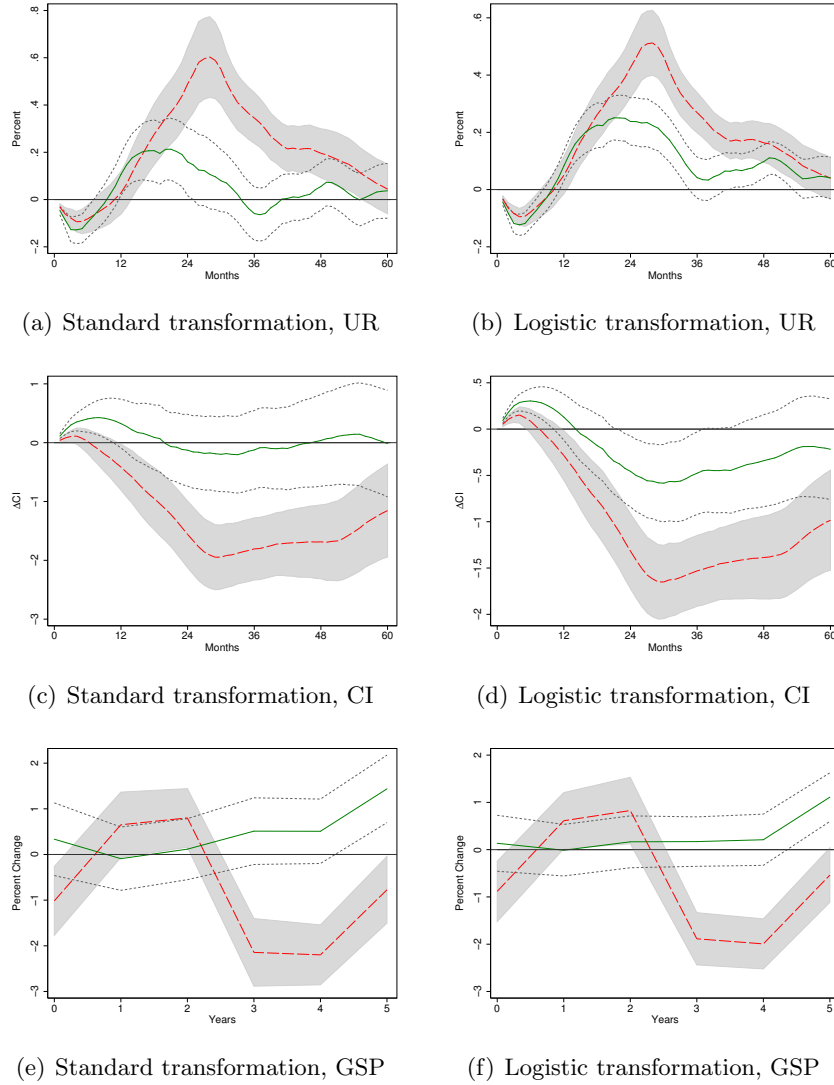
We also study the effects of monetary policy shocks on state-level GDP (GSP) to address potential endogeneity concerns about unemployment and the CI. Unemployment could be subject to endogeneity, as states where layoffs are easy to implement may have limited need for wage cuts. Furthermore, CI estimates may be biased downwards, as salary disbursements 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 the CI irrespective of real activity. Because GSP 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 may thus cancel each other out within a 12-month period.

Figures 5(e)-(f) present the results. We use the [Arellano and Bond \(1991\)](#) System GMM estimator for dynamic panels to counter the [Nickell \(1981\)](#) bias, as in the case of yearly data our time dimension is considerably smaller than our cross-sectional dimension. Instrument proliferation is limited by restricting instruments to second lags of dependent variables. Standard errors are clustered by state. Compared to other estimations in this section, we exclude labor market control variables and state fixed effects to preserve the necessary degrees of freedom. These results suggest that for the flexible state, the impact is never significantly different from zero, while for the rigid state monetary policy shocks lead to a significant contractionary effects on GSP after two years. This is in line with the evidence for unemployment and the CI. Results in figure 5 therefore firmly corroborate the hypothesis.

4.1.3. Asymmetries: Direction of Shocks

To provide some additional evidence of the causal value of our results, we consider expansionary and contractionary shocks separately. As our rigidity measure captures downward wage rigidities, the effect on the impact of shocks should be largest if wage cuts are desired. Hence, the difference

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



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

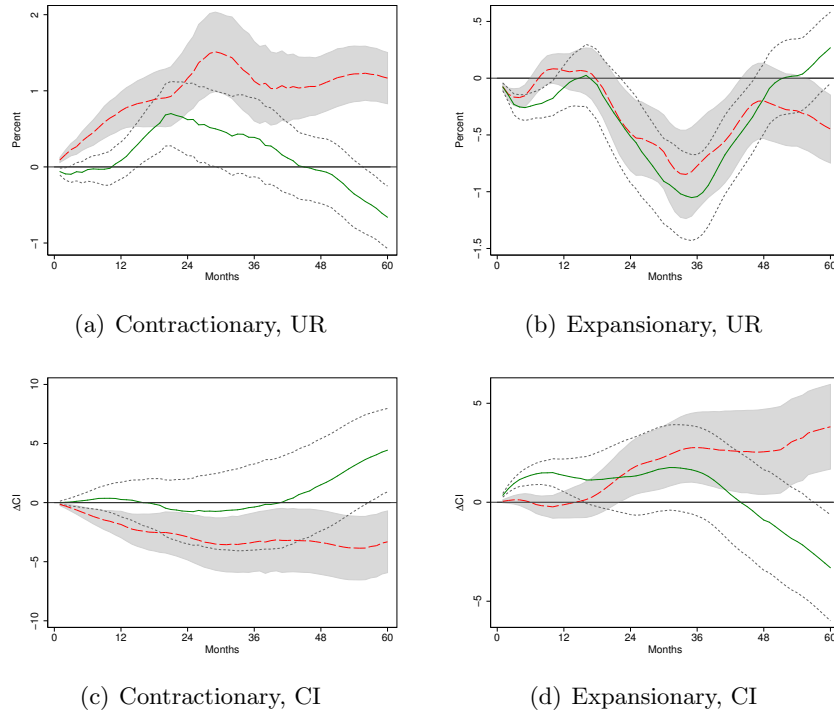
between impulse response profiles for rigid and flexible states should be larger when shocks are contractionary.²⁶

Figure 6 displays IRFs for contractionary (left-hand-side panels) and expansionary (right-hand-side panels) shocks using the standard transformation and our standard set of controls.²⁷ Results for unemployment are in figures 6(a)-(b), while results for the CI are in figures 6(c)-(d). From these panels, it is obvious that most of the differences between flexible and rigid states occur for

²⁶Note though that local projections have a downward bias when shocks run in a single direction, as discussed in Teulings and Zubanov (2014). Point estimates in this subsection should therefore be interpreted with caution.

²⁷In appendix F figure F.1 we reproduce figure 6 with the logistic transformation: results are qualitatively similar.

Figure 6. Monetary Policy Shocks in Rigid and Flexible States: Direction of Shocks, Standard transformation, 1980–2007



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

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 both 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 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 the 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. We explore the potential role for credit frictions in section 4.1.4.

4.1.4. Robustness: Additional Controls

In this subsection, we assess the sensitivity of our results to the selection of covariates. Estimates of impulse response functions are generally sensitive to the selection of control variables, as shown by [Ramey \(2016\)](#). In particular, she advocates the use of control variables that preserve the *recursive-ness* assumption—as defined by [Christiano et al. \(1999\)](#)—to structure the timing in the monetary policy transmission mechanism and to guarantee the orthogonality of monetary policy shocks to in-

flation and output (unemployment). In case the Greenbook (Tealbook) forecasts do not incorporate all relevant information used by the FOMC to make decisions on the FFR, one needs to include additional control variables to satisfy this assumption.²⁸

To ascertain that our results are robust to additional controls, we expand the set of controls in three steps. First, we add the log of national CPI and state-level house price indexes to address the recursiveness assumption. House prices are seasonally adjusted and obtained from the Federal Housing Finance Agency.²⁹ Top two panels of figure 7 present the results for the standard transformation.³⁰ The results are qualitatively robust to this expansion of the set of control variables. The response of unemployment to monetary policy shocks is very similar in both states to the ones reported in figure 5. There remain significant differences between flexible and rigid states, in particular in the second and third year after the shock. 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 IRFs. The response in the flexible state is more contractionary with additional controls compared with the baseline case reported in figure 5, where we observe less persistence compared to the ones reported in figures 7(a)-(b).

Second, we explore 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 for our exercise is that state-level financial frictions could have similar effects if states with high levels of financial frictions coincided with states with high levels of downward nominal wage rigidities. We start by introducing the quarterly regional level 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.³¹

Results in figures 7(c)-(f) display very similar IRFs to those in baseline figure 5. In fact, in figures 7(c)-(d)—that include the state average for 30-year mortgage rates—the difference between rigid and flexible states even increases. Responses in the flexible state are never significantly different from zero in the second to fourth year after the shock, while responses in the rigid state are the largest over this horizon. Results are very similar when we additionally introduce a proxy for financial frictions in figures 7(e)-(f).³²

²⁸This is affirmed in figure F.2 in appendix F, where we present results from figure 5 without controls. They show that a contractionary monetary policy shock has an *expansionary* effect in both flexible and rigid states. However, even these results still affirm our hypothesis of differential impacts of monetary policy shocks in rigid and flexible states. Only if we omit variables that are both correlated with the outcome variable and the interaction of monetary policy shocks and wage rigidities our estimates will not longer be valid.

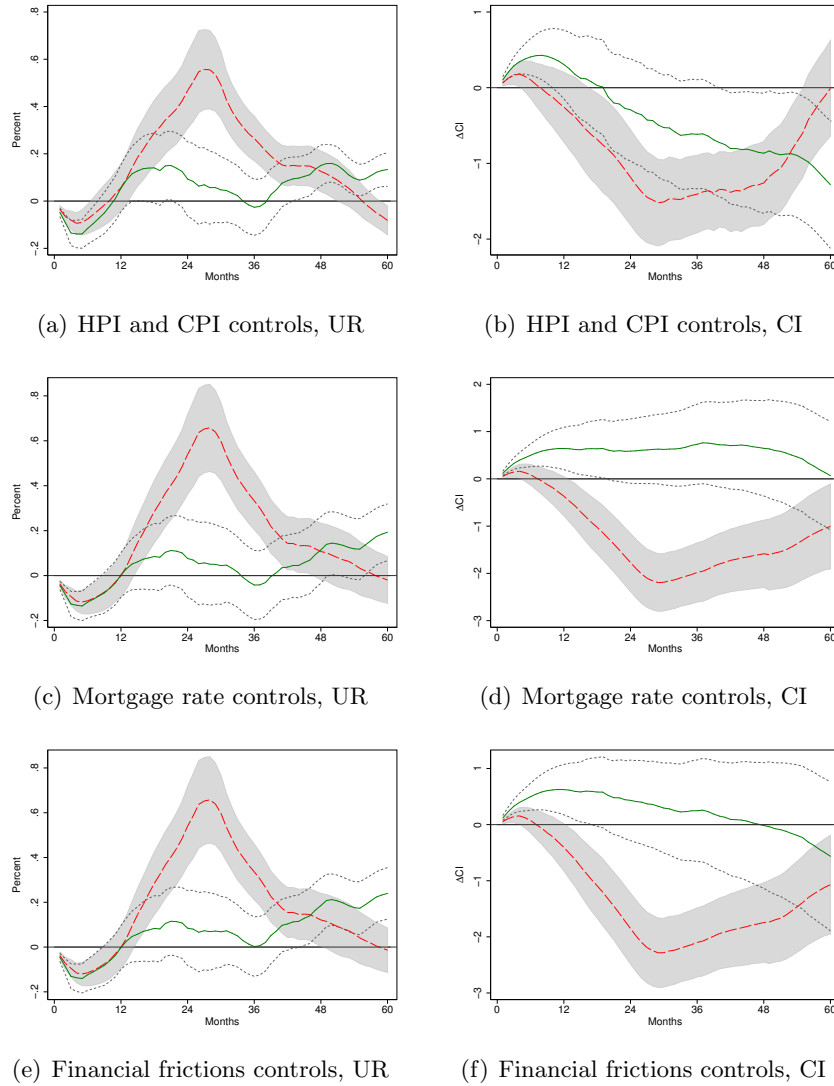
²⁹Controlling for house prices is also important from the perspective of informal unemployment insurance, as housing is a prime source of wealth (see [Den Haan et al., 2015](#)).

³⁰In appendix F we also report figure F.4 with the logistic transformation instead of the standard transformation in figure 7.

³¹We 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.

³²Results are also robust to inclusion of additional labor market controls, such as minimum to median wage ratio, union power, and firm size. Results are in figure F.3 in appendix F.

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

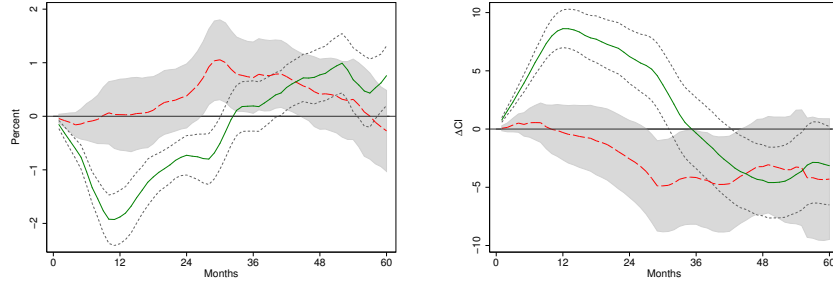


Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

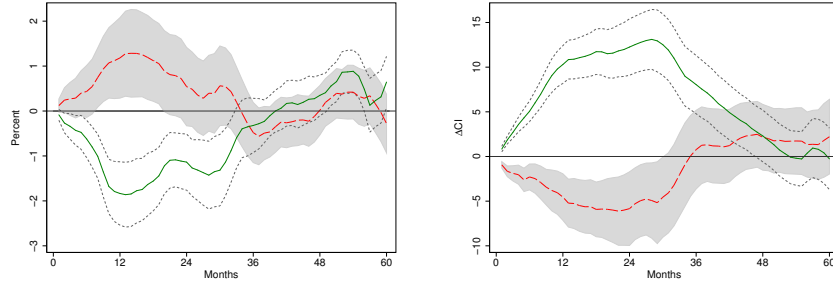
4.1.5. Robustness: Announcement Shocks

In this subsection, we analyze whether the impact of alternative measures of monetary policy shocks is also conditional on the degree of wage rigidities. These shocks account for the fact that changes to the interest rate are not the only tool of monetary policy. Press conferences, speeches, and forward guidance are becoming increasingly important (Gürkaynak et al., 2005). To capture this, we repeat the analysis in figure 5 using announcement shocks from Gorodnichenko and Weber (2016) and Gertler and Karadi (2015).

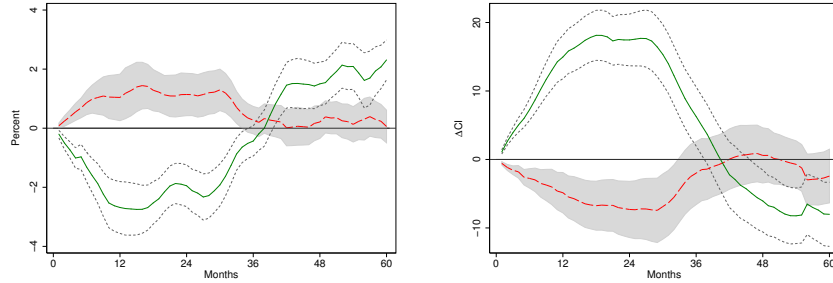
Figure 8. Monetary Policy Shocks in Rigid and Flexible States: Announcement Shocks, Standard Transformation



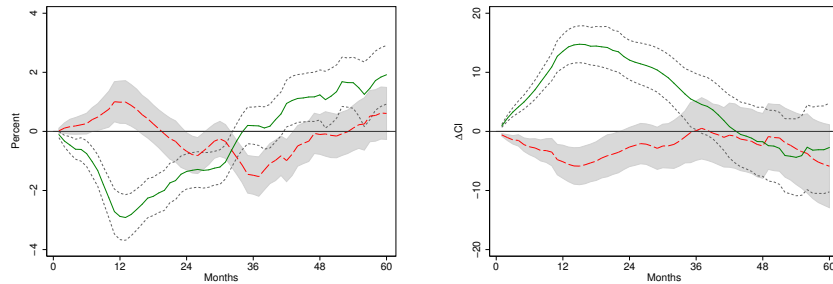
(a) GW (2016), tight int., UR, 1994-2007 (b) GW (2016), tight int., CI, 1994-2007



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



(e) GK (2015), 3-month ahead FFR futures, UR, 1990-2007 (f) GK (2015), 3-month ahead FFR futures, CI, 1990-2007



(g) GK (2015), 6-month ahead of Eurodollar deposits, UR, 1984-2007 (h) GK (2015), 6-month ahead futures of Eurodollar deposits, CI, 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 \(2015\)](#).

Gorodnichenko and Weber (2016) construct their announcement shocks using the federal funds futures from the Chicago Mercantile Exchange Globex electronic trading platform, where they consider changes 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. To obtain monetary policy shocks for Gertler and Karadi (2015) announcements shock, we use a proxy regression approach, where the announcement shocks are regressed on Greenbook variables.³³ An 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 six-months-ahead futures of Eurodollar deposits are available from January 1984.³⁴

Figure 8 presents the results: Figures 8(a)-(b) plot the response to Gorodnichenko and Weber (2016) shocks (tight interval), while figures 8(c)-(h) detail the response to the various shocks derived using Gertler and Karadi (2015) announcements. Control variables and standard errors follow those in figure 5. Results for the announcement shocks also point to significantly different responses between flexible and rigid states. For the rigid state, we observe that contractionary Gorodnichenko and Weber (2016) 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 8(a)). There is a surprisingly large expansionary effect in the flexible state for both unemployment and the CI.³⁵ In the case of Gertler and Karadi (2015) shocks, the contractionary response in the rigid state is more pronounced and in most cases more persistent than for the Gorodnichenko and Weber (2016) shocks, while in the flexible state it is still expansionary. Results for the coincident index (CI), displayed on the right-hand side in figure 8, are qualitatively the same as for unemployment. The main message, that the response is different for flexible and rigid states, is confirmed in all cases considered.

4.1.6. Institutional Factors and Wage Rigidities

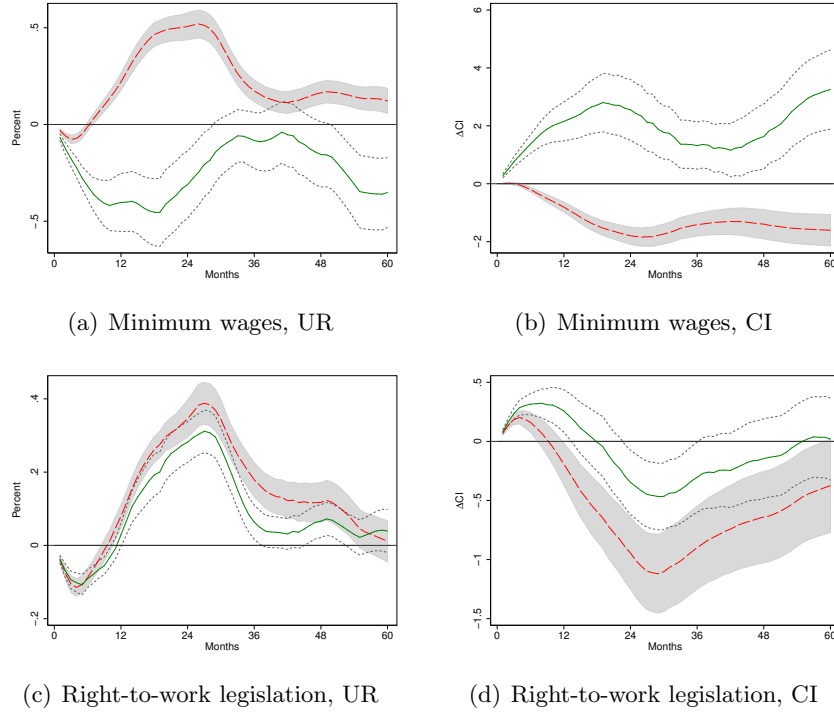
In this subsection we perform robustness checks with respect to our measure of wage rigidities. We present the effects of institutional factors *behind* wage rigidities that are likely to be exogenous to the potential relation between wages and economic activity over the business cycles. We focus on two institutional factors: the ratio between minimum and median wage and the presence of right-to-work legislation. They are both in the domain of the states, as any state can either decide to adopt the federal minimum wage or to raise it. Several states have exercised this option and raised their minimum wages. As of January 2017, 29 states have minimum wage higher than the federal minimum. Right-to-work legislation determines the union power in respective states. Most right-

³³Regressions using direct Gertler and Karadi (2015) announcement shocks and wide interval Gorodnichenko and Weber (2016) shocks are available in figure F.5 in appendix F.

³⁴Gertler and Karadi (2015) consider also year- and nine-month-ahead futures of Eurodollar deposits. Results are qualitatively the same for those announcement shocks as well.

³⁵In figures F.6 and F.7 in appendix F, we additionally include regional 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: Institutional Factors, 1980–2007



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

to-work laws prohibit labor unions and employers from agreeing to only employ unionized workers. Approximately 56% of state-months in our sample have this legislation implemented. As of January 2017, 28 states have right-to-work laws. For right-to-work legislation we set $F(z_{s,t})$ equal to 1 when these laws are absent. In the case of ratio between minimum and median wage, we use this ratio as z in Eq. (3). Compared to previous logistic transformations of our rigidity measure, note that we do not standardize minimum wages before using it as z : The minimum-to-median ratio is naturally bound between 0 and 1, and we also preserve the time-variation of the minimum to median wage for our results. For the minimum-to-median ratio the time variation seems to be more important than relative position of a state compared to other states in a given year.³⁶

Figure 9 displays the effects of monetary policy shocks conditional on the ratio between minimum and median wage and the presence of right-to-work legislation.³⁷ Control variables and standard errors follow those in figure 5. The rigid state (red dashed line) in this figure corresponds to the case where the ratio of minimum to median wage is higher and thus more binding (figures 9(a)-(b)), and to the case where right-to-work legislation is absent (figures 9(c)-(d)). Figures 9(a)-(b) display an eye-popping difference between the responses conditional on the ratio between minimum and median wage. This demonstrates the importance of minimum wages, but we have to bear in mind

³⁶Even if we standardize the measure, the differences would still be significantly different between the less binding and more binding states, although smaller.

³⁷The response of wages to a monetary policy shock under a high or low value of both variables is presented in figure F.8 in appendix F. They display a reaction in line with expectations.

that responses in flexible and rigid states are the extreme cases. Only when this ratio is high (the rigid state), we observe contractionary effects of positive innovations to monetary policy, while in the flexible state the response is expansionary for both unemployment and the CI. In fact, the IRFs in two states are statistically different at virtually all horizons that we consider. The differences between cases with and without right-to-work legislation are smaller than in our baseline regressions (figure 5) but still statistically significant. In the flexible state, where right-to-work legislation is implemented, the effects of monetary policy shocks are smaller at the peak of the impact and less persistent. This holds both for the response of unemployment and the response of the coincident index.

4.1.7. Other Robustness Checks

Appendix D presents additional robustness checks. Section D.1 analyzes the robustness of our results to the time sample considered, while section D.2 repeats the main exercise with alternative standard errors. We perform several other robustness checks, including removing the ten smallest states. Results are essentially the same (see figures F.9(a)-(b) in appendix F). 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 construction in particular is subject to higher wage rigidities. Our results are robust to excluding the ten states with the highest share of construction among all industries (see figures F.9(c)-(d) in appendix F).

4.2. Fiscal Policy Shocks

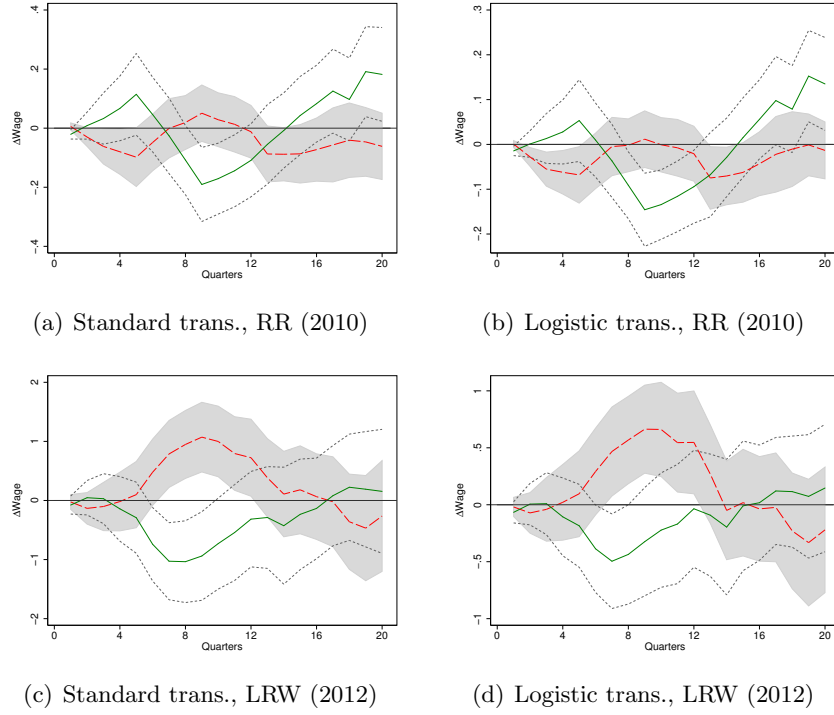
In this section we present results from estimating Eq. (1) using federal tax shocks. We estimate the wage response to tax shocks in section 4.2.1, followed by the response of unemployment and economic activity in section 4.2.2. Robustness checks are discussed in sections 4.2.3–4.2.5.

4.2.1. Response of Wages to Tax Shocks

To assess the role of median wages in the transmission of federal tax policy shocks, we estimate Eq. (1) with median wages as the dependent variable. If wages play an important role also for the transmission of tax policy shocks then wages should experience a larger decline in the flexible state compared to the rigid state after a contractionary tax shock. Control variables follow [Ramey \(2016\)](#) and include two lags of nominal tax receipts, nominal federal purchases and a quadratic time trend, in addition to the controls used for monetary policy shocks. We cluster standard errors at the state level. The sample spans from 1980 to 2007 at quarterly frequency.

Figure 10 presents the results: Figures 10(a)-(b) plot the response of wages to a [Romer and Romer \(2010\)](#) shock, while figures 10(c)-(d) plot the effect of a [Leeper et al. \(2012\)](#) shock. The IRFs for [Romer and Romer \(2010\)](#) shocks plot the effect of an increase in tax revenue equal to 1 percent of GDP, while the IRFs for [Leeper et al. \(2012\)](#) plot the effect of an increase in federal tax rates by 100 percentage points. Left-hand-side panels use the standard transformation of wage rigidities, while right-hand-side panels use the outlier-robust logistic transformation.

Figure 10. Tax Shocks in Rigid and Flexible States: Median Wages, 1980–2007



Note: Rigid state in red dashed line; Flexible state in green solid line. 90% confidence intervals calculated using clustered standard errors by state. RR (2010) stands for [Romer and Romer \(2010\)](#) and LRW (2012) stands for [Leeper et al. \(2012\)](#).

The conditional response of median wages to [Romer and Romer \(2010\)](#) shocks is not as pronounced as in the case of monetary policy shocks, but still displaying significant differences about 2 years after the shock, in line with the hypothesized role of wages in fiscal transmission mechanism. In the rigid state the response is not significantly different than zero in most periods. In the flexible state, an increase in tax rates has a slightly positive effect on wages initially, but between second and third year they display a significantly negative effect. The patterns are similar under both standard and logistic transformations of wage rigidities. Conditional responses of wages after the [Leeper et al. \(2012\)](#) shock are distinctly different between flexible and rigid states. As in the case of monetary policy shocks, wages in the flexible state decline significantly after about one-and-a-half years, while they actually increase in the rigid state after the contractionary tax shock. In the rigid state the effect is significantly positive in the second and third year of the effect. This confirms that wages play an important role also in the transmission of fiscal policy shocks and that our measure of wage rigidities is able to identify these effects.

4.2.2. Response of Unemployment and Output to Tax Shocks

Figure 11 presents IRFs of unemployment and the CI to [Romer and Romer \(2010\)](#) and [Leeper et al. \(2012\)](#) shocks. Panels on the left-hand side present results using the standard transformation of

wage rigidities, while panels on the right-hand side use the logistic transformation. Regressions use the same control variables and standard errors as those in figure 10.³⁸

Figures 11(a)-(b) plot results for unemployment. The shape of the IRFs is in line with estimates by Romer and Romer (2010) and Ramey (2016), but contrary to our hypothesis, the response of unemployment rate is similar for both rigid and flexible states. However, results for the coincident index—in figures 11(c)-(d)—clearly support our hypothesis, as the response in the rigid state is always below the one for the flexible state: While the response in the rigid state is contractionary at all horizons, the response in the flexible state is mostly small and insignificant in the first three-and-a-half years and then expansionary.

Figures 11(e)-(h) show our results for tax expectations shocks. Contrary to results using Romer and Romer (2010) shocks, wage rigidities affect the response of unemployment more than the response of the coincident index to tax expectations shocks. Unemployment results in figures 11(e)-(f) display that the shock is expansionary in the first two years—as already found by Leeper et al. (2013) and Ramey (2016)—in both flexible and rigid states. After the median wages begin to rise (figures 10(c)-(d)) in the second year, the effect on unemployment becomes contractionary in the rigid state, while it remains expansionary in the flexible state. In line with our hypothesis, the IRFs are significantly different between the two states in the second half of the horizon. Results are very similar for both standard and logistic transformations.

The IRFs of the CI, presented in figures 11(g)-(h), are less different between rigid and flexible states than in the case of unemployment. The initial expansionary effect appears slightly larger in the rigid state, although the difference is not significant when controlling for outliers in the logistic transformation.

4.2.3. Robustness: Additional Controls

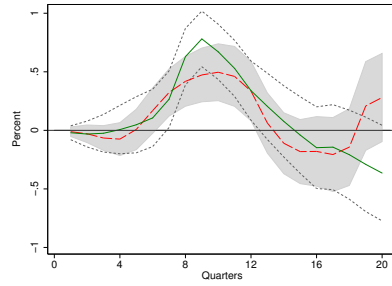
In this subsection we introduce additional control variables to check robustness of our results in figure 11. Analogous to section 4.1.4 on monetary policy shocks, we add three different sets of controls to the main specification while maintaining all other assumptions unchanged. Figures 12 (Romer and Romer, 2010 shocks) and 13 (Leeper et al., 2012 shocks) present the results with standard transformation.³⁹

Figures 12(a)-(b) present the response of unemployment and the CI to Romer and Romer (2010) shocks with additional controls for house prices and inflation. These additional controls produce IRFs for unemployment that are not aligned with our hypothesis. The contractionary response of unemployment is larger in the flexible than in the rigid state after about 7 quarters. Contrary, results for the coincident index show a larger contractionary effect in the rigid than in the flexible state, although the difference is not significant for most of the forecast horizon. In figures 12(c)-(d) we additionally control for average mortgage rates to assess the effect of credit costs in the transmission of the tax shock. Controlling for average mortgage rates further increase the reaction of unemploy-

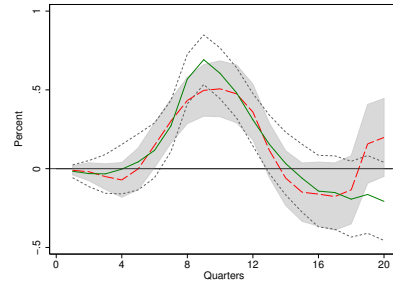
³⁸We do not report results for GSP, as the limited number of tax shocks in our sample leave inadequate degrees of freedom to estimate their effect in an annual sample. If we nevertheless estimate the effect of tax shocks in our GSP sample, there is no significant effect on output in either the rigid or flexible state. Results are available upon request.

³⁹Figures F.11 and F.12 in appendix D display results using the logistic transformation.

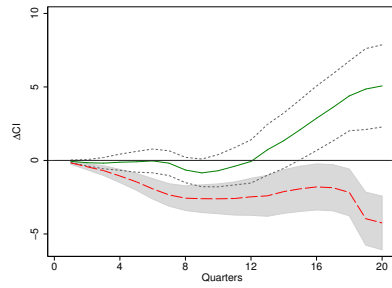
Figure 11. Tax Shocks in Rigid and Flexible States: [Romer and Romer \(2010\)](#) and [Leeper et al. \(2012\)](#) Shocks, 1980-2007



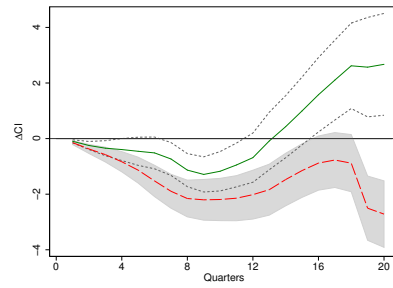
(a) Standard trans., RR (2010), UR



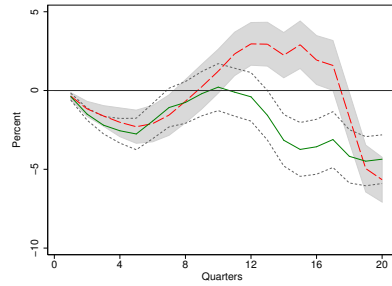
(b) Logistic trans., RR (2010), UR



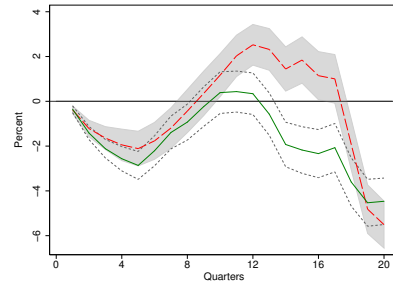
(c) Standard trans., RR (2010), CI



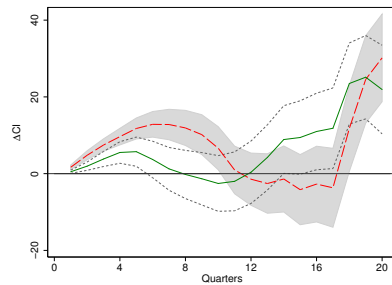
(d) Logistic trans., RR (2010), CI



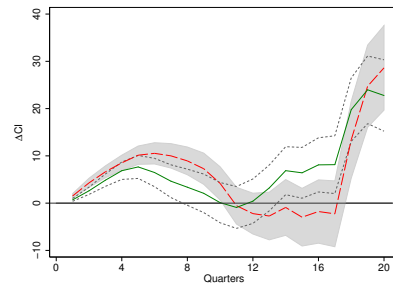
(e) Standard trans., LRW (2012), UR



(f) Logistic trans., LRW (2012), UR



(g) Standard trans., LRW (2012), CI



(h) Logistic trans., LRW (2012), CI

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. RR (2010) stands for [Romer and Romer \(2010\)](#) and LRW (2012) stands for [Leeper et al. \(2012\)](#).

ment and the CI to the tax shocks in the flexible state, so that the difference between flexible and rigid states becomes even more pronounced in the direction that we would not expect. Furthermore, when adding a proxy for financial frictions—in the form of state-level FDIC interventions—to the control set in figures 12(e)-(f), we find similar results to those in figures 12(c)-(d). Results using Romer and Romer (2010) tax shocks seem to be more affected by the exact set of controls than our results for monetary policy shocks. This is partly expected, as the response of wages is not as different between flexible and rigid states as for other shocks used in this paper.

Figure 13 details the results using Leeper et al. (2012) tax expectations. In contrast to figure 12, these results firmly corroborate the hypothesis, as one would expect given the results for wage responses in figure 10. Figures 13(a)-(b) show that expanding the set of controls with inflation and house prices amplifies the difference between IRFs for rigid and flexible states, where only the rigid state produces contractionary responses. These results are significant for most of the horizon and very similar when additionally controlling for mortgage rates and financial frictions in figures 13(c)-(f).⁴⁰

4.2.4. Institutional Factors and Wage Rigidities

We also discuss the role of institutional factors behind wage rigidities, as we do for monetary policy shocks. We study systematic variation in response to tax shocks across states that have different ratios of minimum to median wage and depending on adoption of right-to-work legislation.

Figures 14(a)-(d) present results for the ratio of minimum to median wage and figures 14(e)-(h) for the adoption of right-to-work legislation. Left-hand-side panels plot the response of unemployment rates, while right-hand-side panels display the response of the coincident index.⁴¹ Most of the results are in line with our hypothesis.

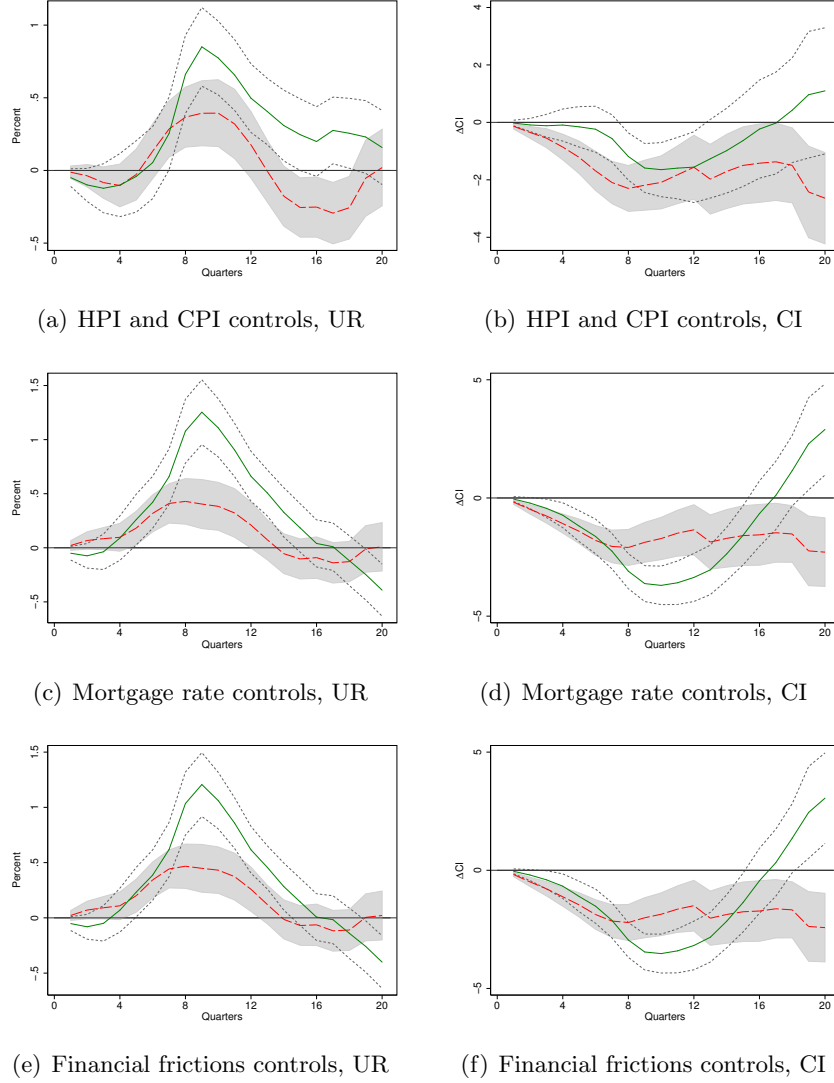
Figures 14(a)-(b) show the effect of Romer and Romer (2010) tax shocks on the unemployment rate and the coincident index. The unemployment rate in the rigid state, where the ratio between minimum and median wage is high, responds contractionary to tax increases after one year, increasing unemployment by one percentage point two years after the shock. In the flexible state, the effect on unemployment is never contractionary after the first year. Results for the CI are qualitatively the same as for the unemployment, where the difference between rigid and flexible states is especially evident from the second year onward, although this difference is not as pronounced as in the unemployment case.

Results for Leeper et al. (2012) tax expectation shocks, figure 14(c)-(d), are very similar to those for Romer and Romer (2010) exogenous tax shocks. Like in figure 13, these shocks initially have an expansionary effect in both states. Yet again, the difference is most evident in the third year of the effect, where in the rigid state the effect is contractionary and in the flexible state it is either slightly expansionary or not significantly different from zero.

⁴⁰Results are also robust to inclusion of additional labor market controls, such as minimum to median wage ratio, union power, and firm size. Results are in figure F.13 in appendix F.

⁴¹Figure F.14 in appendix F present the effect on median wages.

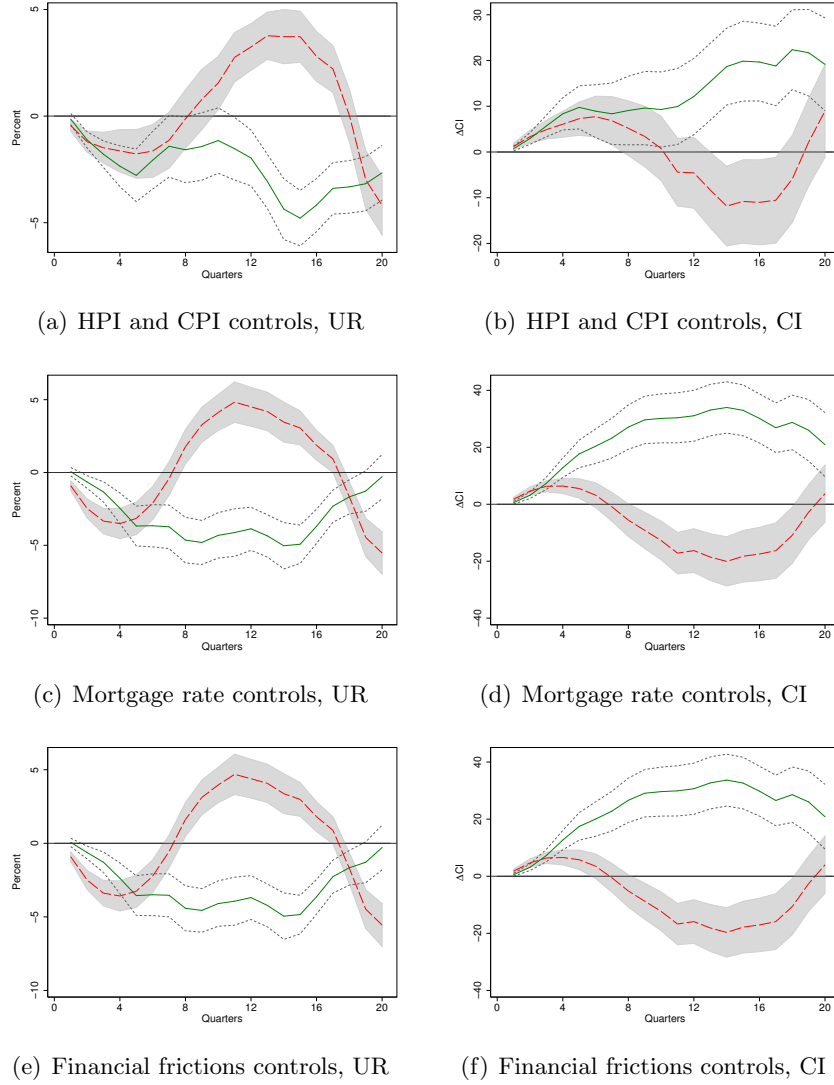
Figure 12. Tax Shocks in Rigid and Flexible States: [Romer and Romer \(2010\)](#) Shocks, Additional Controls, Standard Transformation, 1980–2007



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

Figures 14(e)-(f) show that the effect of [Romer and Romer \(2010\)](#) tax shocks on the unemployment rate and the coincident index is larger in states without right-to-work legislation. As already noted for monetary shocks, the effects of right-to-work legislation are not as markedly different for rigid and flexible states, but still significantly different at some horizons. The response of unemployment and the CI is always less contractionary in the flexible state, where right-to-work legislation is not implemented. Figures 14(g)-(h) show that the difference due to right-to-work legislation in response to [Leeper et al. \(2012\)](#) expectation shocks are not significant in most period. In some periods the response of unemployment in states with such legislation is slightly more contractionary.

Figure 13. Tax Shocks in Rigid and Flexible States: [Leeper et al. \(2012\)](#) Shocks, Additional Controls, Standard Transformation, 1980–2007

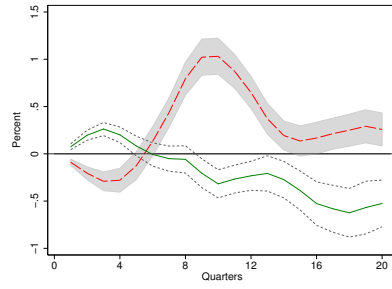


Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

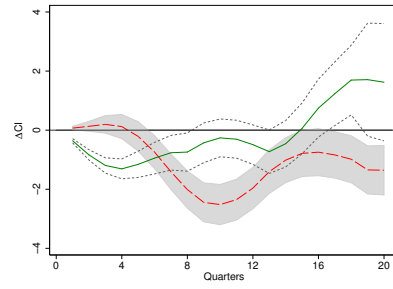
4.2.5. Robustness: Other Robustness Checks

Due to a relatively few tax events in the last decades, it does not make sense to study subsamples or to consider separately expansionary versus contractionary shocks. The sample is already quite short for the purpose of studying the effects of tax shocks. Section D.3 in appendix D studies the robustness of our standard errors. Results are in figures D.4–D.5, where we observe that standard errors increase relatively more than in the case of monetary policy shocks. An additional robustness check includes removing the ten smallest states. Results are essentially the same (see Figure F.15(a)–(d) in appendix F). We also explore additional controls for industry: Our results are

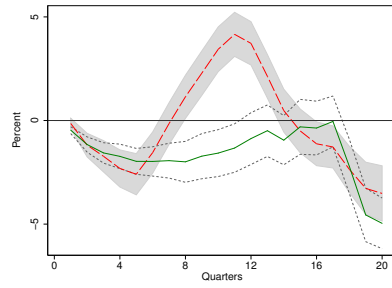
Figure 14. Tax Shocks in Rigid and Flexible States: Institutional Factors, 1980–2007



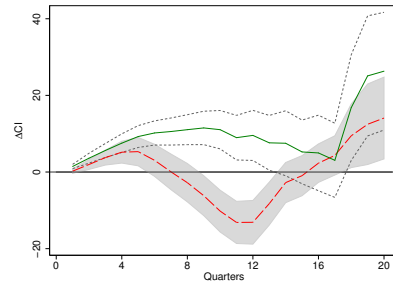
(a) Minimum wages, UR, RR(2010)



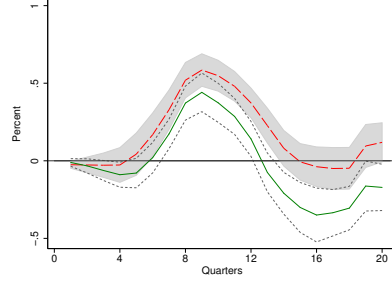
(b) Minimum wages, CI, RR(2010)



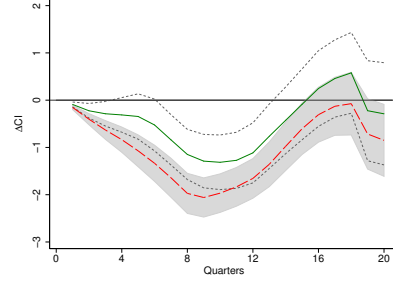
(c) Minimum wages, UR, LRW(2012)



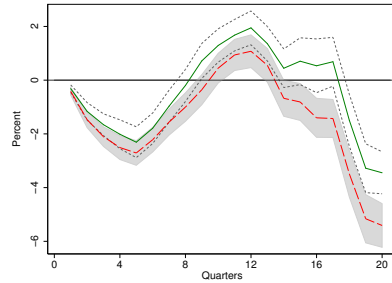
(d) Minimum wages, CI, LRW(2012)



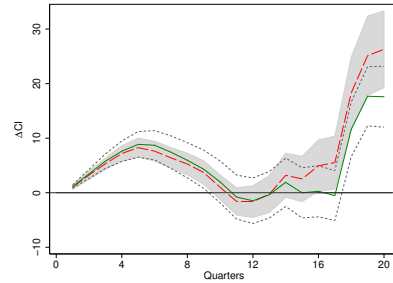
(e) Right-to-work, UR, RR(2010)



(f) Right-to-work, CI, RR(2010)



(g) Right-to-work, UR, LRW(2012)



(h) Right-to-work, CI, LRW(2012)

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. RR (2010) stands for [Romer and Romer \(2010\)](#) and LRW (2012) stands for [Leeper et al. \(2012\)](#).

robust to excluding the ten states with the highest share of construction among all industries (see Figure F.15(e)-(h) in appendix F).

5. Conclusion

This paper provides empirical evidence on the role of wage rigidities in the transmission of monetary and fiscal policy shocks. New Keynesian DSGE models predict a positive correlation between wage rigidities and the impact of government spending and monetary policy shocks, as sluggish wage changes result in poor adjustment of nominal quantities and larger fluctuations in unemployment. This paper provides empirical evidence for a positive relationship between wage rigidities and the response of economic activity to policy shocks.

We relate variation in the state-level impact of 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. We also provide some evidence of causality by studying responses of median wages in different states and by considering contractionary and expansionary monetary policy shocks separately. Wage rigidities only affect the impact of contractionary shocks, as expansionary shocks have very little effect on real variables. We also show that federal tax shocks produce more pronounced contractionary effects in states with higher rigidities, although results are sensitive to the type of tax shock considered and the specification of control variables. Furthermore, we analyze the effects of institutional factors behind wage rigidities. We find larger and more persistent effects of monetary and tax policy shocks for states where the ratio between minimum and median wage is higher and for states that do not have right-to-work legislation.

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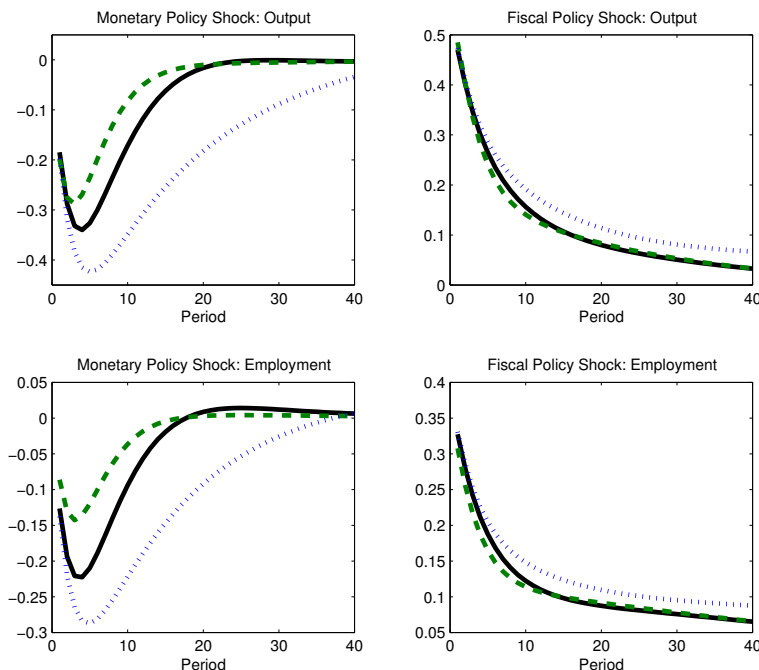
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Appendix A. Role of Wage Rigidities in New Keynesian Models

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.⁴² 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.1](#) we plot impulse responses of employment and output to monetary and fiscal policy (exogenous spending) shocks.

Figure A.1. 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.

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 is increasing in the degree of wage rigidity.

These results are in line with other New Keynesian Dynamic Stochastic General Equilibrium (DSGE) models.⁴³ [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,

⁴²[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.

⁴³For example, [Gertler et al. \(1999\)](#), [Smets and Wouters \(2002\)](#), [Christiano et al. \(2005\)](#), [Galí and Monacelli \(2005\)](#), and [Blanchard and Galí \(2010\)](#).

Christiano et al. (2011) 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 B. Measure of Real Rigidity

To measure real rigidity, one could simply replace nominal wage freezes and cuts in Eq. 2 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, 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:

$$FWCP_{s,t}^r = \frac{f_{s,t}^r}{c_{s,t}^r + f_{s,t}^r} = \frac{2(h_{s,t} - c_{s,t}^r)}{h_{s,t}}, \quad (4)$$

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 ($\Delta M_{t,s} + [\Delta M_{t,s} - \pi_{t,s}^e]$, 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 $c_{t,s}^r$ and $h_{t,s}$ are equal, as both lie equally far from the median. Wages freezes at various inflation expectations create asymmetry such that $c_{t,s}^r < h_{t,s}$. On the left-hand side of the median wage change, $c_{t,s}^r - h_{t,s}$ 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 $c_{t,s}^r - h_{t,s}$ by 2. The denominator $c_{t,s}^r + f_{t,s}^r = h_{t,s}$ follows from assuming wage-change symmetry in absence of rigidity. The number of intended real wage cuts in left tail $c_{t,s}^r + f_{t,s}^r$ 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.⁴⁴

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 $FWCP^r$.

⁴⁴Local indicators of inflation are only available at MSA level, which is likely to poorly reflect inflation at state level.

Table B.1: Average Wage Rigidity by State

	$FWCP^n$	$FWCP^r$		$FWCP^n$	$FWCP^r$		$FWCP^n$	$FWCP^r$
<i>Average</i>	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			

Notes: *, ** 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 B.2: Estimations Labor Market Institutions and Wage Rigidity

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

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

Appendix C. 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 C.1 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 it 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 C.1. It presents correlation coefficients 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 C.1: 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 D. Additional Robustness Checks

D.1. Monetary Policy: Time sample

Ramey (2016) has shown in her survey that responses of output to monetary policy shocks have changed over time and that it is difficult to observe contractionary effects of monetary policy shocks in the post 1983 sample. To assess the importance of the time sample for our result we focus—in line with recent work by Caldara and Herbst (2016)—on the 1994–2007 sample (Great Moderation). They point out that it is important to take into account the systematic component of monetary policy that includes a significant reaction to changes in credit spreads in the Great Moderation sample: A failure to account for this reaction results in attenuation in the response of all variables to monetary policy shocks. If we run our regression with standard controls on this shorter sample the effects of monetary policy are barely, if at all, contractionary after a positive monetary policy shock. In particular, the response in the flexible state is expansionary. To reconcile these responses with economic theory we proceed in line with Caldara and Herbst (2016) and expand our set of controls by including two lags of the BAA to 10-year Treasury bond spread, and one lag of the stock market value and Gilchrist and Zakrajšek (2012) spread to control for systematic developments in monetary policy. In addition, we also include two lags of the spread between regional level of 30-year mortgage rate and 10-year Treasury bond rate to get some variability between states.

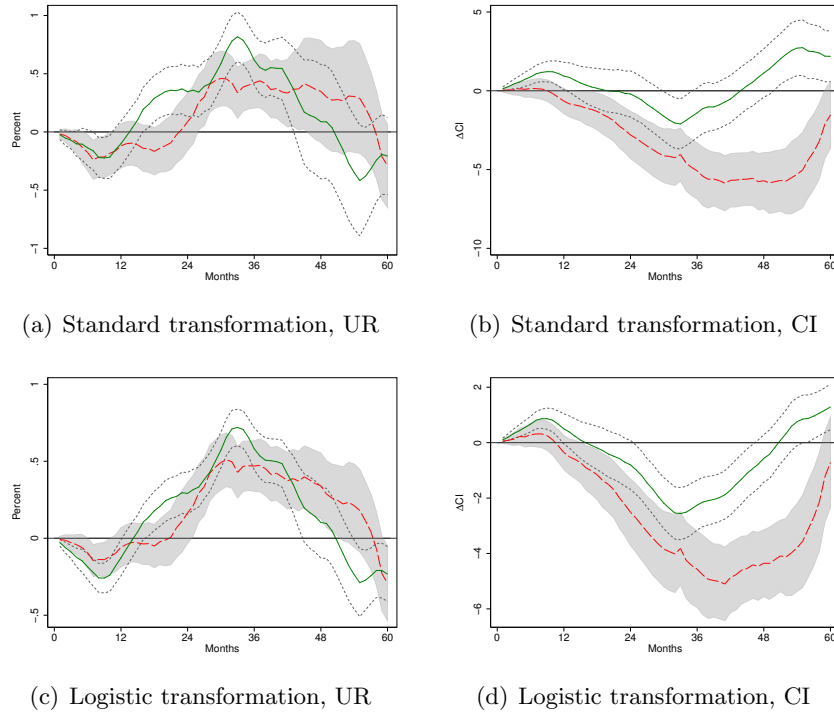
The effect on unemployment, as shown in figure D.1(a), is not significantly different between rigid and flexible states for most of the first five years. Possibly, the effect in the flexible state becomes contractionary slightly earlier and it dies out faster. Impulse responses for the CI, displayed in figure 6(b), show significant differences between flexible and rigid state as put forward in our hypothesis. For the rigid state, compared with figure 5(c), we can see that in the shorter sample, the effects on the CI are larger, on average. The impulse response in the flexible state is very similar to the ones in figure 5(a), as they are not different from zero for most of the horizon.⁴⁵

D.2. Monetary Policy: 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. 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 a simultaneous (co)variance matrix of the sandwich/robust type for all leads h corrected for clusters in both states

⁴⁵We also repeat the same exercise for 1983–2007 sample, thus excluding the Volcker disinflation at the beginning of the 1980s. Policy shocks in this period are 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. We find similar results and still observe significant differences between flexible and rigid state. Results are presented in figure F.10 in appendix F.

Figure D.1. Monetary Policy Shocks in Rigid and Flexible States: 1994–2007



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

and time.⁴⁶ To show the importance of different assumptions, we present standard errors with the SURE estimator with clustering by state and time in figure D.2.

As we can observe in figure D.2, 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. Much of the identification of monetary policy shocks comes from the beginning of the sample, especially the 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, when the responses are never different from zero only in the flexible state.

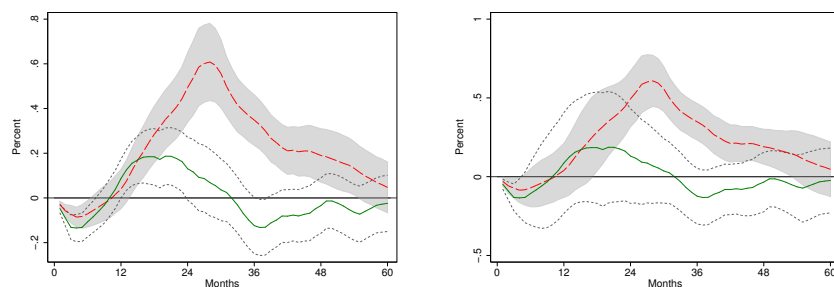
D.3. Fiscal Policy: Standard Errors

Figures D.4 and D.5 present the fiscal counterparts of the SURE results in Figure D.2. Figure D.4 plots responses to Romer and Romer (2010) tax shocks while figure D.5 presents responses Leeper et al. (2012) tax expectations.

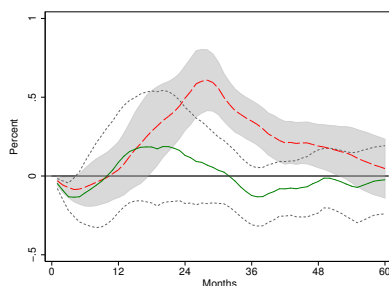
Results show that clustering standard errors by state and time using the SURE estimator leads to an increase in the confidence bounds in all figures. The difference between flexible and rigid states

⁴⁶Gourio 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.

Figure D.2. 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

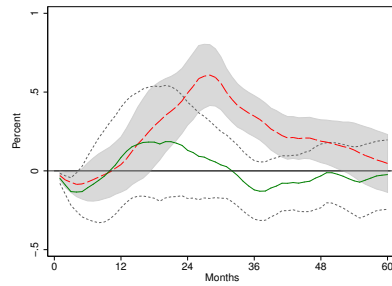


(c) SURE estimator, clustered by state and time

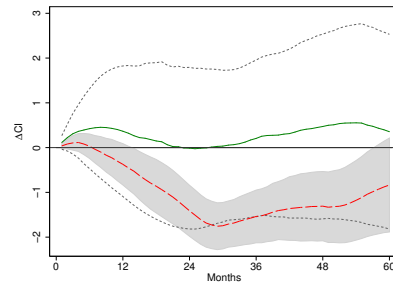
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

is now only significant in figures that use the minimum-to-median ratio to approximate the degree of wage rigidity. The negative result in Figures D.4(a), where unemployment responds more strongly in the negative state, is now highly insignificant. Overall, results affirm our previous conclusion that the relationship between wage rigidity and the impact of shocks is stronger for monetary policy shocks than for fiscal shocks.

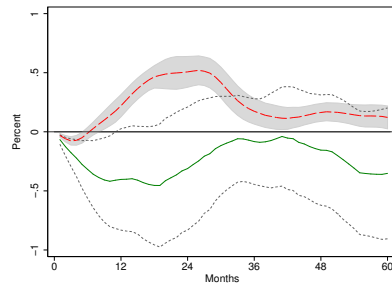
Figure D.3. Monetary Policy Shocks in Rigid and Flexible States; Unemployment with Standard Transformation: SURE estimator with errors clustered by state and time, 1980-2007



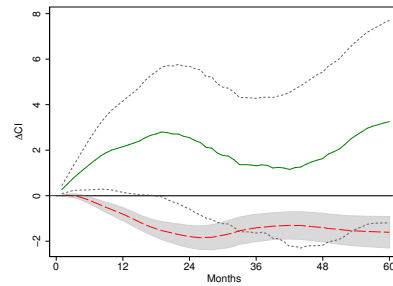
(a) Unemployment, Standard



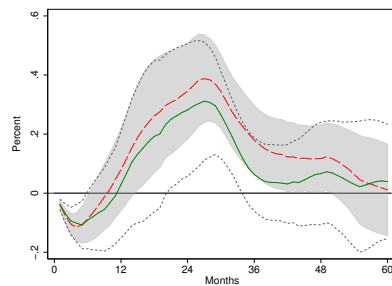
(b) CI, Standard



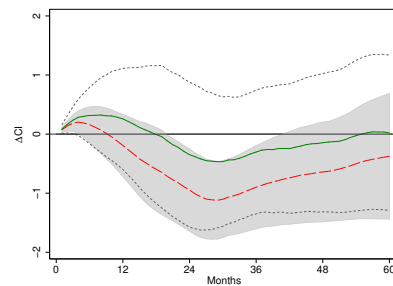
(c) Unemployment, Min. wage



(d) CI, Min. wage



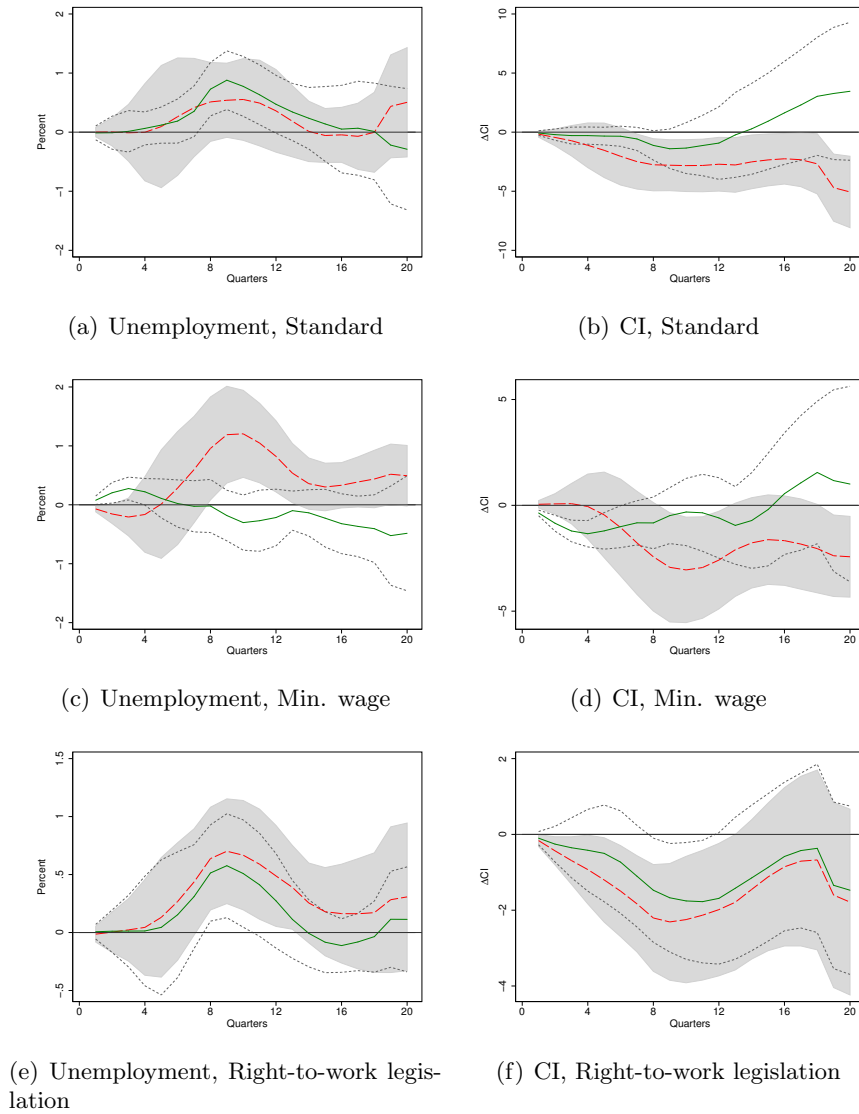
(e) Unemployment, Right-to-work legislation



(f) CI, Right-to-work legislation

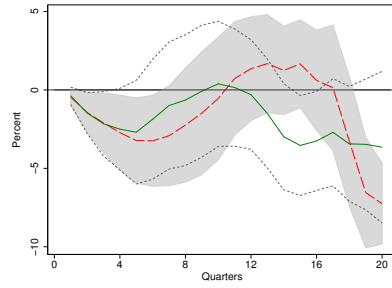
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

Figure D.4. Tax Shocks in Rigid and Flexible States: [Romer and Romer \(2010\)](#) Shocks: SURE Estimator with Errors Clustered by State and Time, 1980-2007

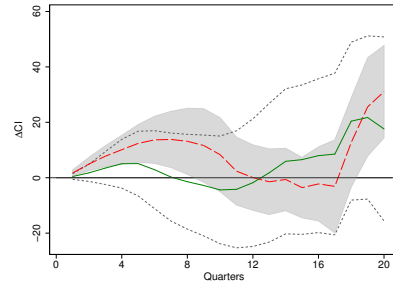


Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

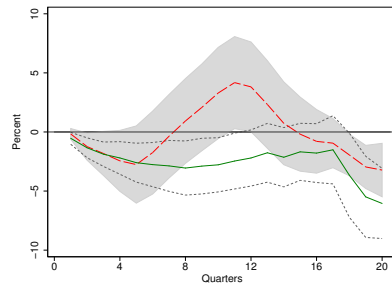
Figure D.5. Tax Shocks in Rigid and Flexible States: [Leeper et al. \(2012\)](#) Shocks: SURE Estimator with Errors Clustered by State and Time, 1980-2007



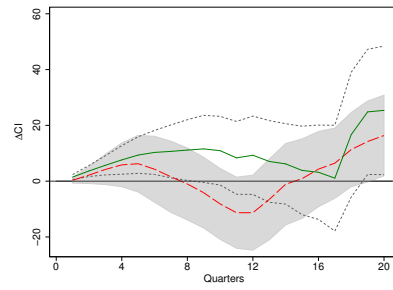
(a) Unemployment, Standard



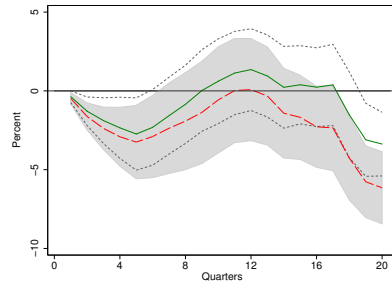
(b) CI, Standard



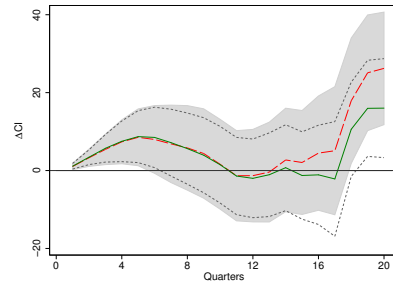
(c) Unemployment, Min. wage



(d) CI, Min. wage



(e) Unemployment, Right-to-work legislation



(f) CI, Right-to-work legislation

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

Appendix E. Additional Figures and Tables

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

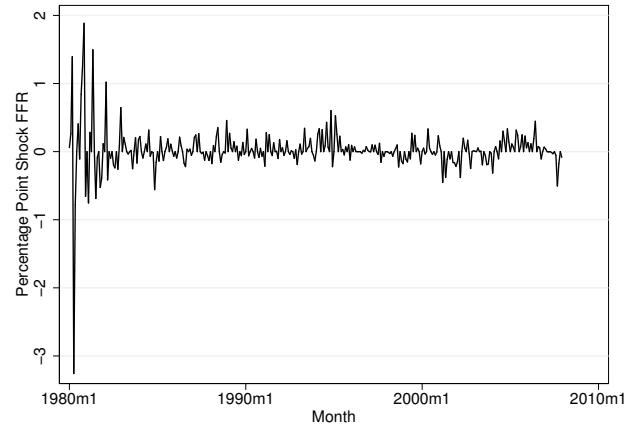


Figure E.2. Shocks in Federal Tax Rates: [Romer and Romer \(2010\)](#)

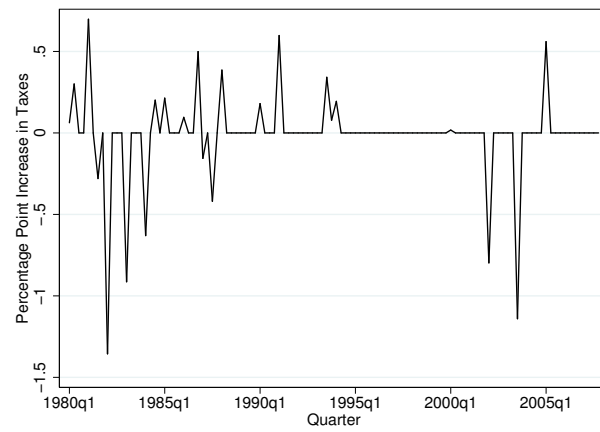


Figure E.3. Federal Tax Expectations: [Leeper et al. \(2012\)](#)

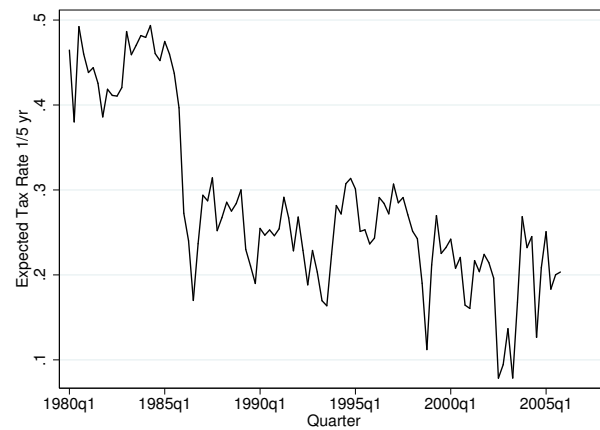
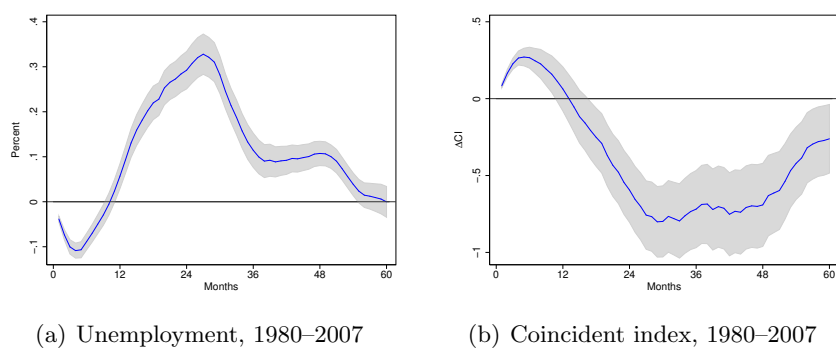


Table E.1: CPS Microdata Summary

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

Figure E.4. Unconditional Monetary Policy Shocks: Unemployment and CI



Note: 90% intervals.

Appendix F. Additional Figures and Tables for Robustness

Figure F.1. Monetary Policy Shocks in Rigid and Flexible States: Direction of Shocks, Logistic Transformation, 1980–2007

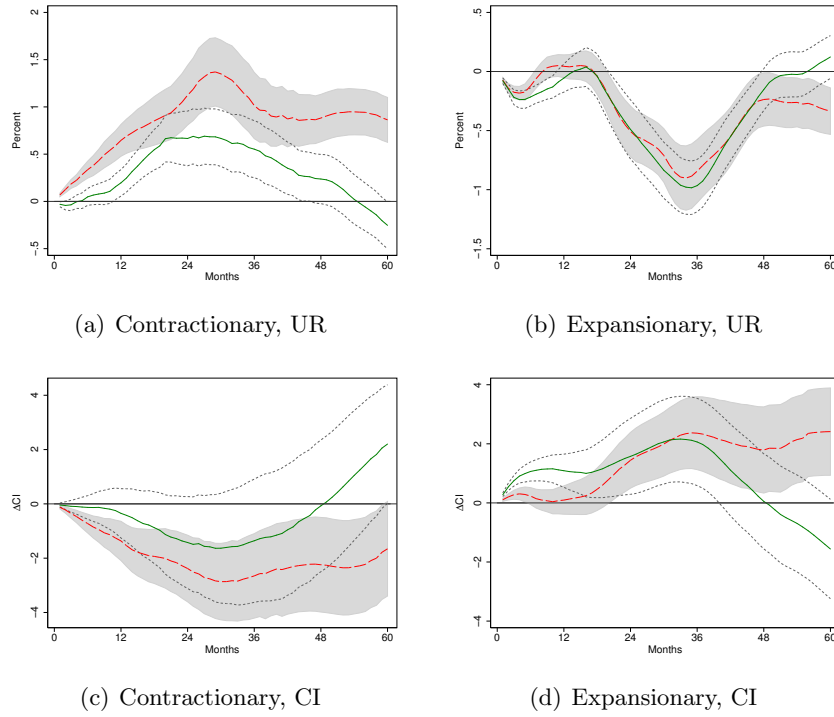
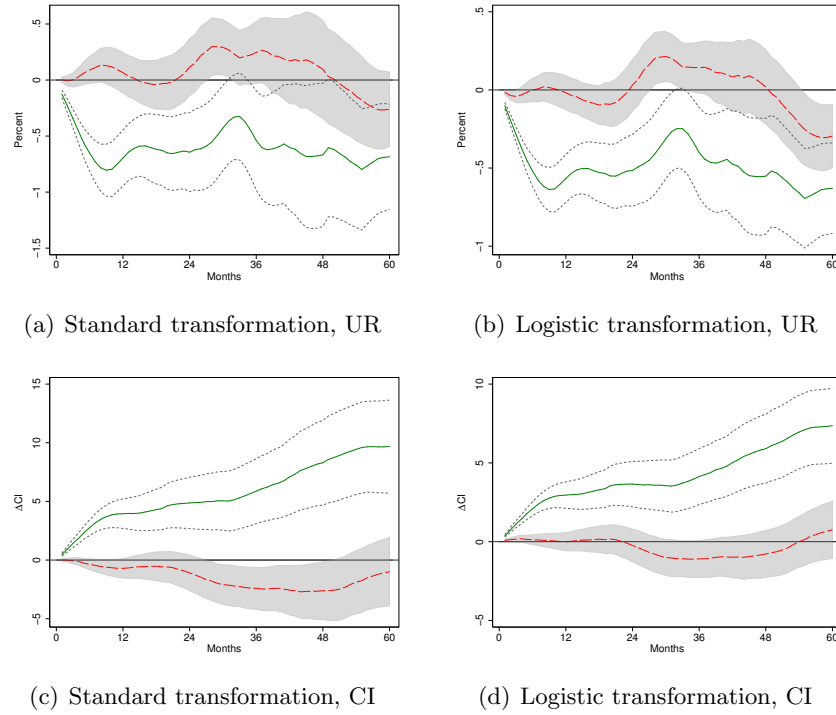
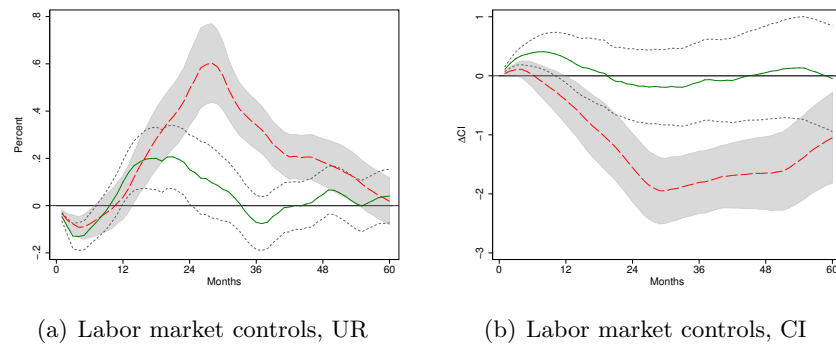


Figure F.2. Monetary Policy Shocks in Rigid and Flexible States: No Controls, 1980–2007



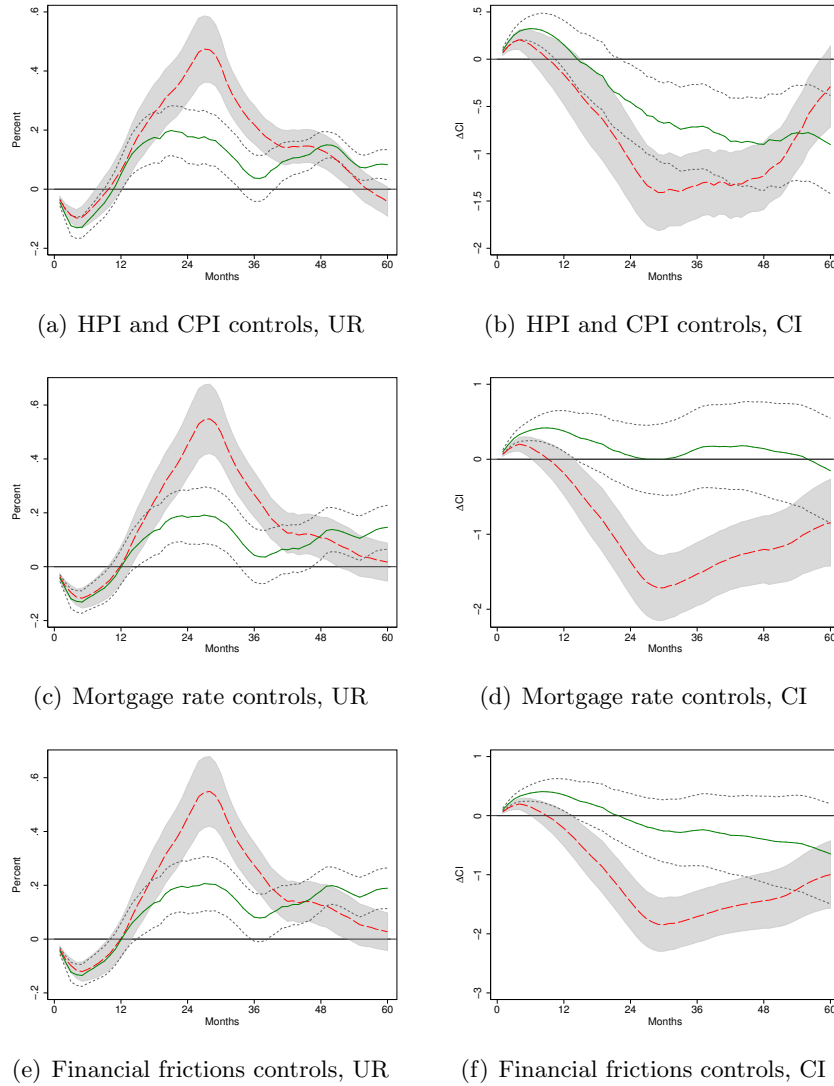
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

Figure F.3. Monetary Policy Shocks in Rigid and Flexible States: Additional Labor Market Controls, Standard Transformation, 1980–2007



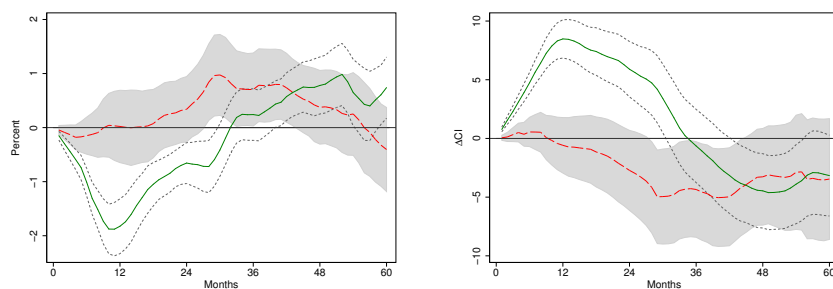
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

Figure F.4. Monetary Policy Shocks in Rigid and Flexible States: Additional Controls, Logistic Transformation, 1980–2007

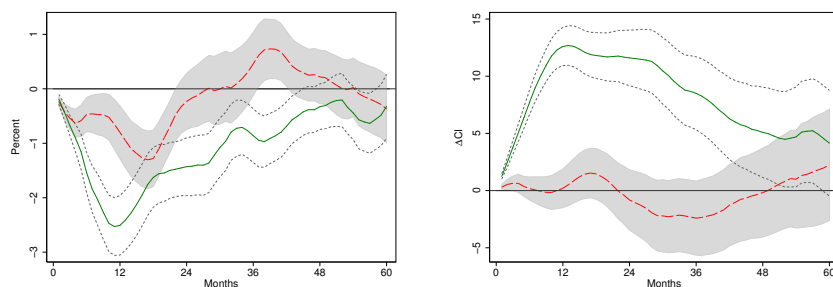


Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

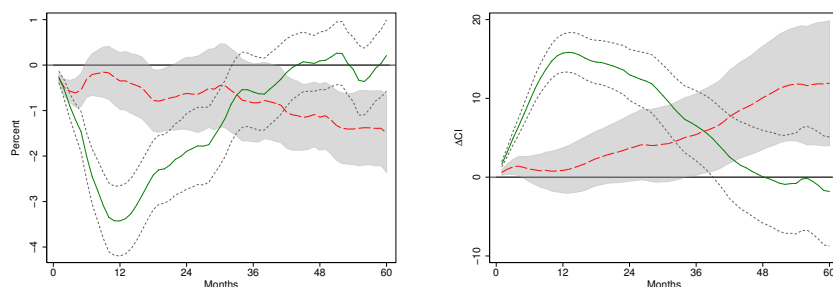
Figure F.5. Monetary Policy Shocks in Rigid and Flexible States: Announcement shocks, Standard Transformation



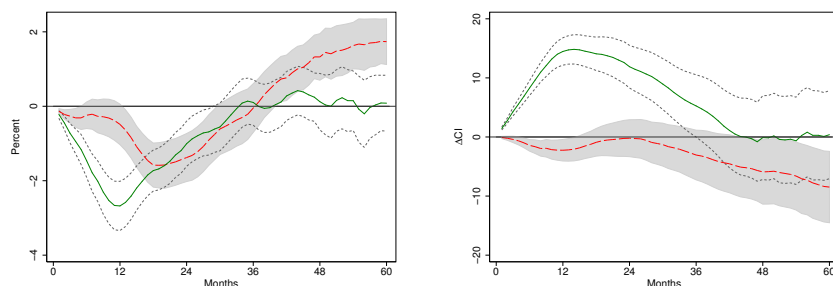
(a) GW (2016), wide interval, UR, 1994-2007 (b) GW (2016), wide interval, CI, 1994-2007



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



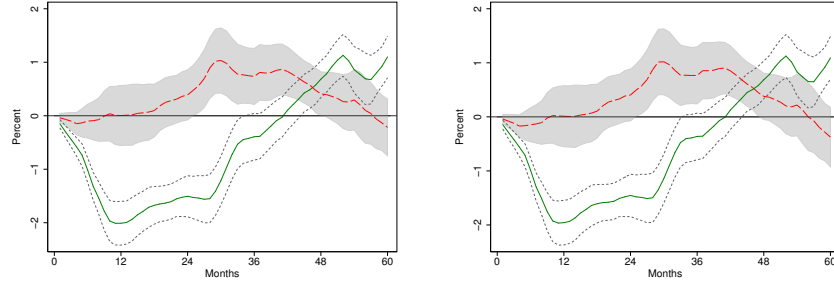
(e) GK (2015), 3-month ahead FFR futures, UR, 1990-2007 (f) GK (2015), 3-month ahead FFR futures, CI, 1990-2007



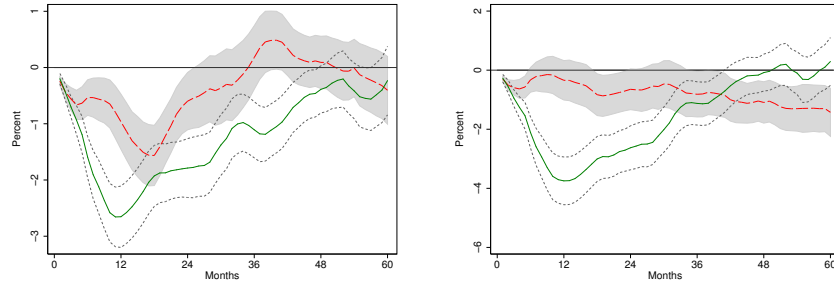
(g) GK (2015), year-ahead futures of Eurodollar deposits, UR, 1984-2007 (h) GK (2015), year-ahead futures of Eurodollar deposits, CI, 1984-2007

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 \(2015\)](#).

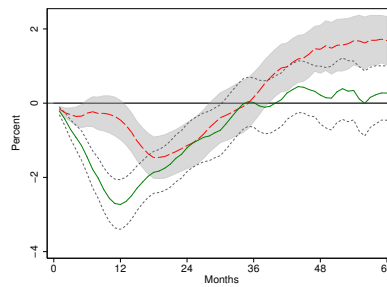
Figure F.6. Monetary Policy Shocks in Rigid and Flexible States: Announcement Shocks with Additional Controls, Unemployment, Standard Transformation



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



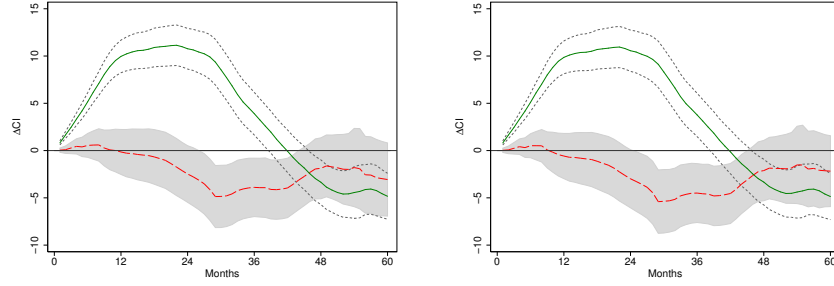
(c) GK (2015), current FFR futures, 1988-2007 (d) GK (2015), 3-month ahead FFR futures, 1990-2007



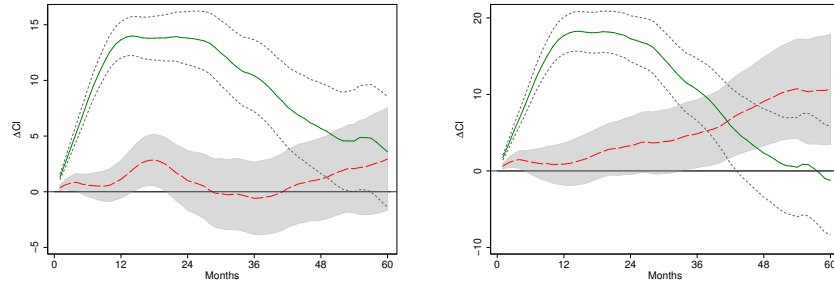
(e) GK (2015), year-ahead futures of Eurodollar deposits, 1984-2007

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 \(2015\)](#).

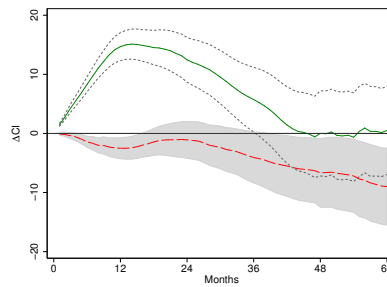
Figure F.7. Monetary Policy Shocks in Rigid and Flexible States: Announcement shocks with additional controls, Coincident, Standard transformation



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



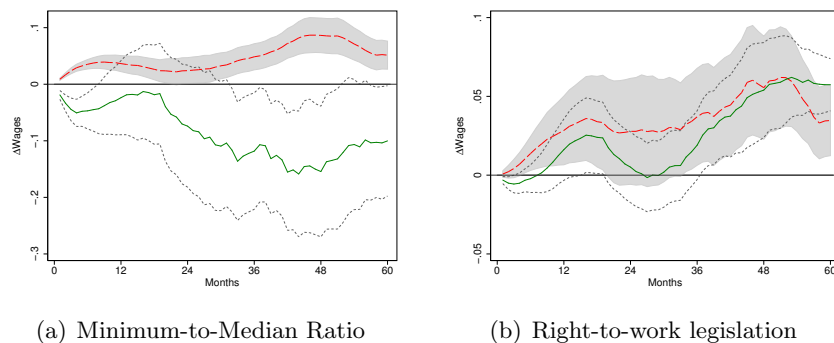
(c) GK (2015), current FFR futures, 1988-2007 (d) GK (2015), 3-month ahead FFR futures, 1990-2007



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

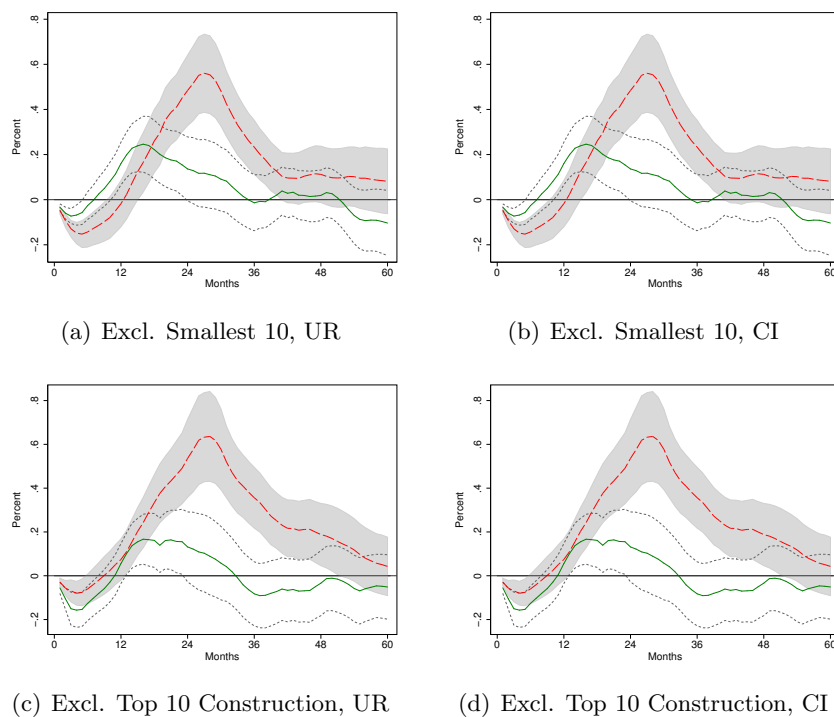
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. GW (2016) stands for [Gorodnichenko and Weber \(2016\)](#); and GK (2015) stands for [Gertler and Karadi \(2015\)](#).

Figure F.8. Monetary policy shocks in Rigid and Flexible States: Median Wages 1980–2007



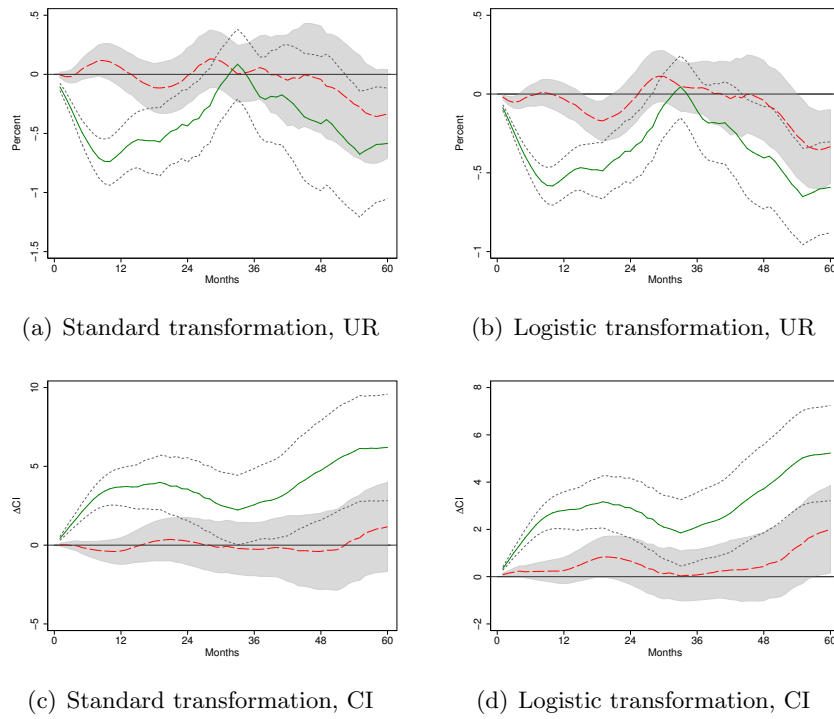
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% confidence intervals calculated using clustered standard errors by state.

Figure F.9. Monetary Policy Shocks in Rigid and Flexible States: Excluding Groups of States, 1980–2007



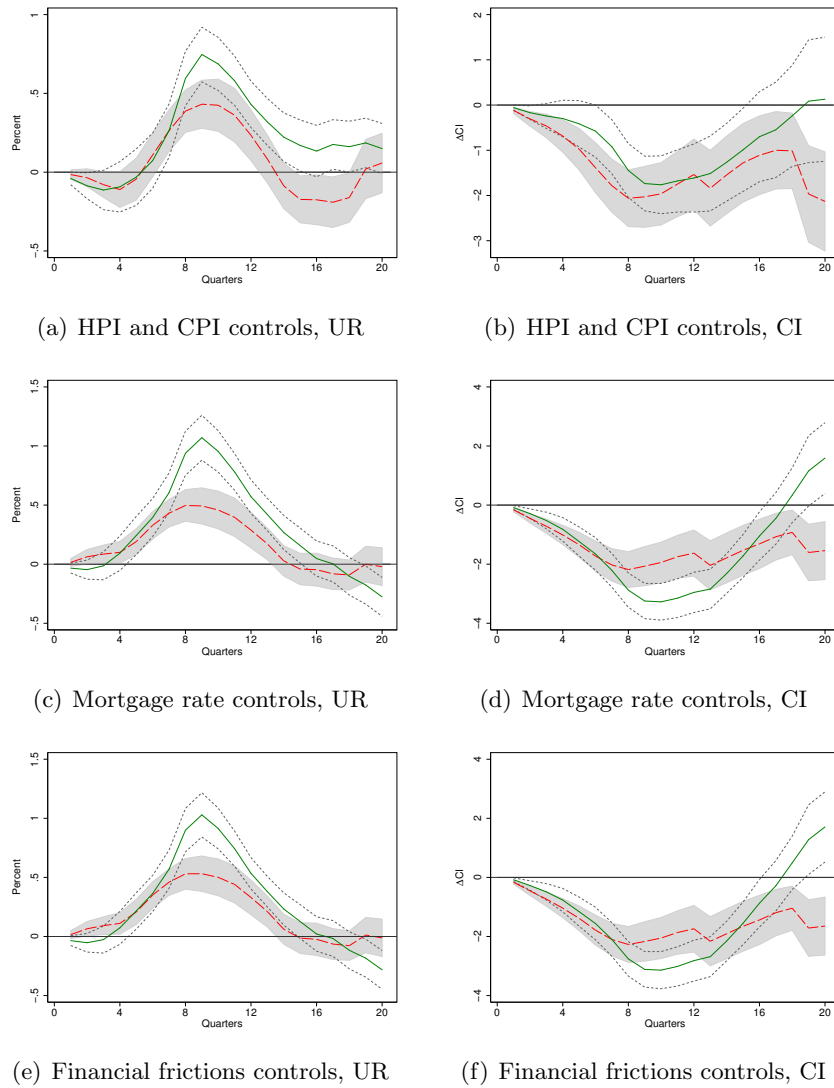
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% confidence intervals calculated using clustered standard errors by state.

Figure F.10. Monetary Policy Shocks in Rigid and Flexible States: Unemployment and CI, 1983–2007



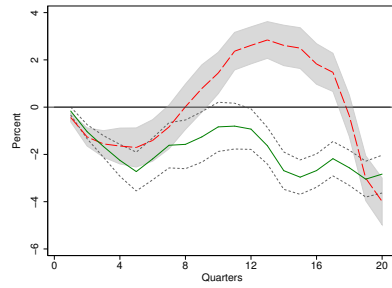
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

Figure F.11. Tax Shocks in Rigid and Flexible States: [Romer and Romer \(2010\)](#), Additional Controls, Logistic Transformation, 1980–2007

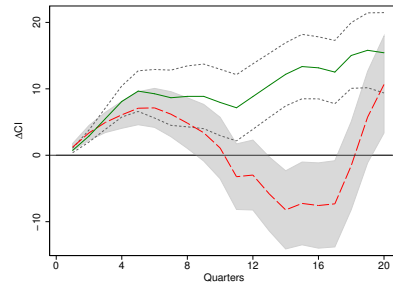


Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

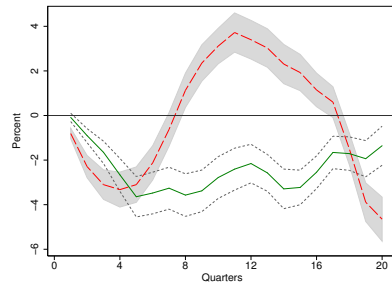
Figure F.12. Tax Shocks in Rigid and Flexible States: [Leeper et al. \(2012\)](#), Additional Controls, Logistic Transformation, 1980–2007



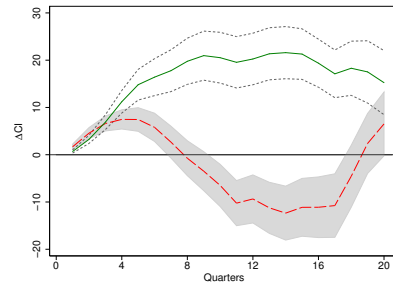
(a) HPI and CPI controls, UR



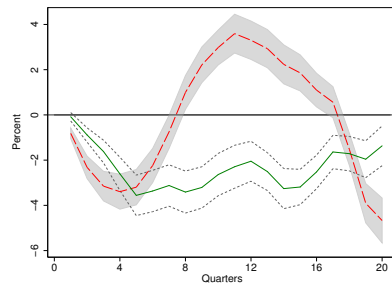
(b) HPI and CPI controls, CI



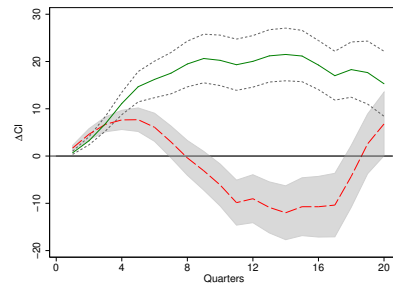
(c) Mortgage rate controls, UR



(d) Mortgage rate controls, CI



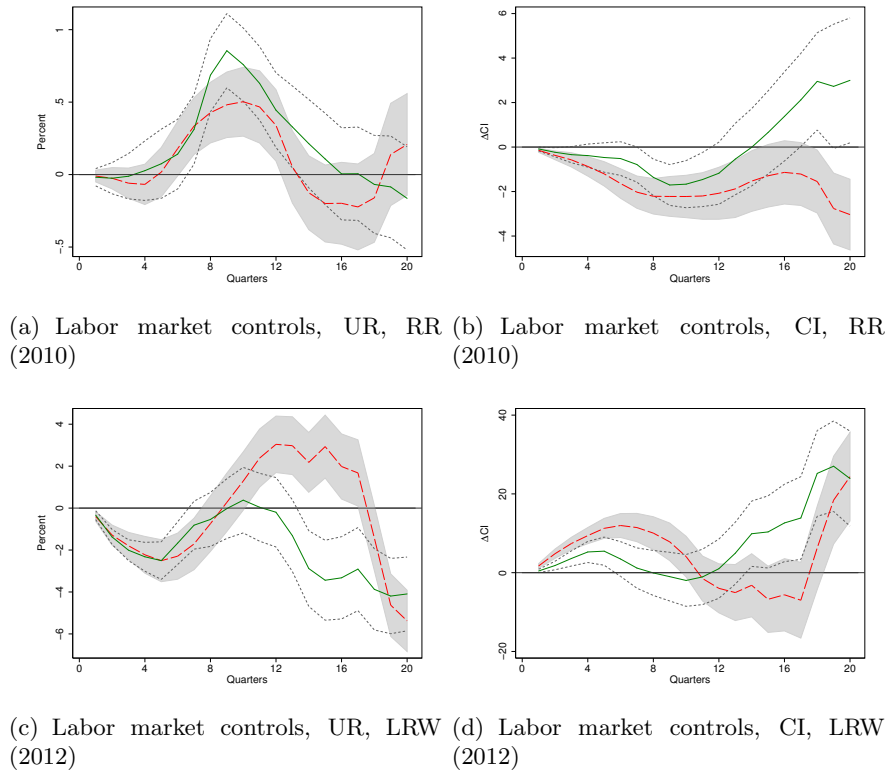
(e) Financial frictions controls, UR



(f) Financial frictions controls, CI

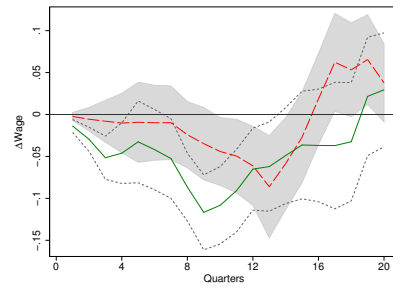
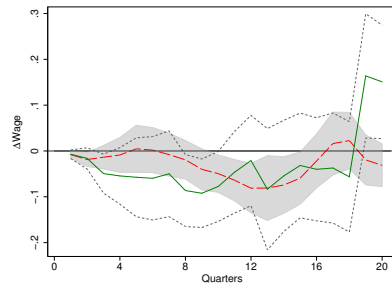
Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals.

Figure F.13. Tax shocks in Rigid and Flexible States: Additional Labor Market Controls, Standard Transformation, 1980–2007

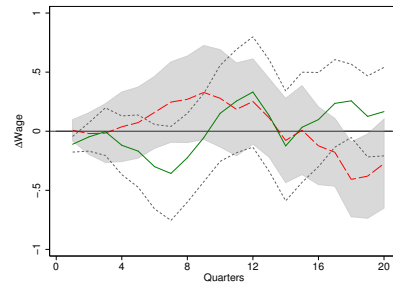
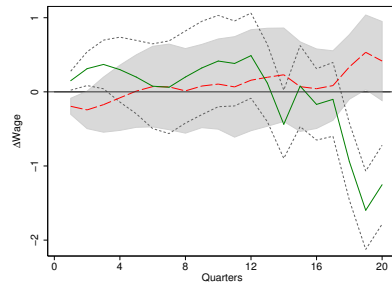


Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. RR (2010) stands for [Romer and Romer \(2010\)](#) and LRW (2012) stands for [Leeper et al. \(2012\)](#).

Figure F.14. Tax shocks in Rigid and Flexible States: Median Wages, 1980–2007



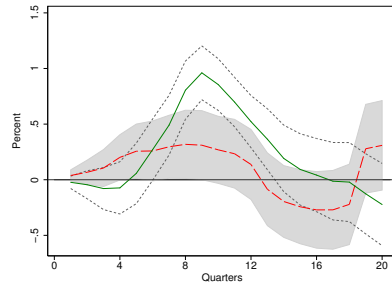
(a) Minimum-to-Median Ratio, RR (2010) (b) Right-to-work legislation, RR (2010)



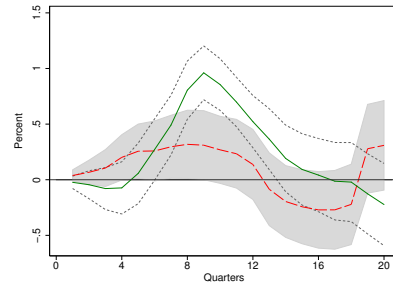
(c) Minimum-to-Median Ratio, LRW (2012) (d) Right-to-work legislation, LRW (2012)

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. RR (2010) stands for [Romer and Romer \(2010\)](#) and LRW (2012) stands for [Leeper et al. \(2012\)](#).

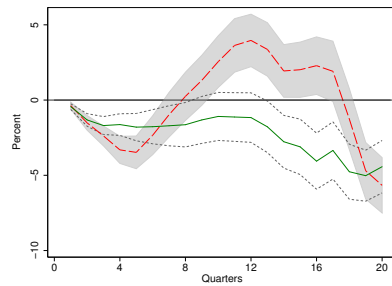
Figure F.15. Tax Shocks in Rigid and Flexible States: Excluding Groups of States, Standard Transformation, 1980–2007



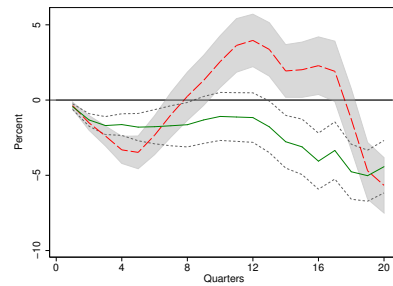
(a) Excl. Smallest 10, UR, RR (2010)



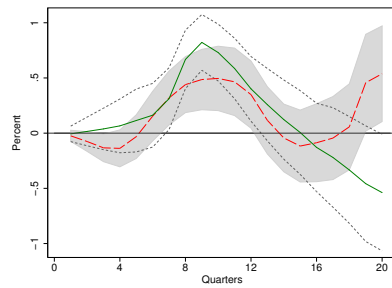
(b) Excl. Smallest 10, CI, RR (2010)



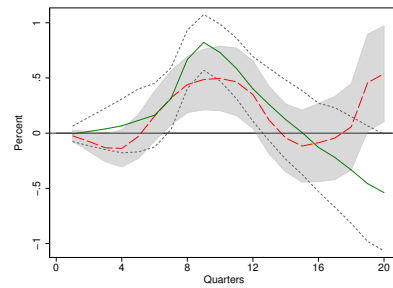
(c) Excl. Smallest 10, UR, LRW (2012)



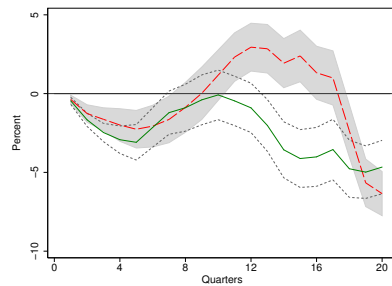
(d) Excl. Smallest 10, CI, LRW (2012)



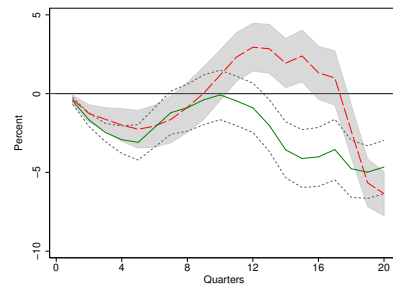
(e) Excl. Top 10 Const., UR, RR (2010)



(f) Excl. Top 10 Const., CI, RR (2010)



(g) Excl. Top 10 Const., UR, LRW (2012)



(h) Excl. Top 10 Const., CI, LRW (2012)

Note: Rigid state in red dashed line; Flexible state in green solid line. 90% intervals. RR (2010) stands for [Romer and Romer \(2010\)](#) and LRW (2012) stands for [Leeper et al. \(2012\)](#).