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Fixed Wage Contracts and Monetary Non-Neutrality*

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Abstract

We study the importance of wage rigidities for the monetary policy transmission mechanism. Using uniquely rich micro data on Swedish wage negotiations, we isolate periods when the labor market is covered by fixed wage contracts. Importantly, negotiations are coordinated in time but their seasonal patterns are far from deterministic. Using a two-regime VAR model, we document that monetary policy shocks have a larger impact on production during fixed wage episodes as compared to the average response. The results are not driven by the periodic structure, nor the seasonality, of the renegotiation episodes.

JEL Codes: E23; E24; E58; J41
Keywords: Monetary Policy; Wages; Nominal rigidities; Micro-data

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1 Introduction

The idea that nominal wage rigidities can give rise to persistent output movements in response to nominal shocks dates at least back to Keynes. It is also well established that wage contracts tend to be signed at coordinated points in time and last for at least a year; see e.g. Druant et al. (2012) and references therein. But up until relatively recently, nominal wage rigidities have not featured as an element in standard New Keynesian models. Instead, nominal price rigidities were given the key role in connecting the real and the nominal side of the economy. However, Christiano, Eichenbaum and Evans (2005) introduced both types of rigidities in a rich New Keynesian model, arguing that nominal wage rigidity is the key friction to understand the dynamic relationships in the data, whereas nominal price rigidities play a very limited role.[1,2] Importantly, Broer et al. (Forthcoming) shows that when extending the standard New Keynesian macro model to allow for wealth heterogeneity across agents, nominal wage rigidity becomes crucial for generating a transmission mechanism from monetary shocks to real output movements. But despite this evolution on the modelling side, we still have very little direct evidence of the importance of wage rigidities for the monetary policy transmission mechanism. The aim of this paper is to provide such direct evidence.

We study the importance of nominal wage frictions and present direct evidence, derived from a two-regime vector autoregressive (VAR) model, on the extent to which the transmission of monetary policy shocks is different during periods when virtually all nominal wages are predetermined. The approach is closely related to Olivei and Tenreyro (2007, 2010), who show seasonality patterns that are consistent with a larger real impact of monetary policy shocks during seasons when wage contracts tend not to be renegotiated according to national practices. The key

[1]See Erceg, Henderson and Levin (2000) for the seminal paper on how to incorporate nominal wage rigidities in the New Keynesian model.

[2]Smets and Wouters (2007) argues for a more moderate interpretation of the data, but still gives equal importance to the two frictions. Lately, it has become standard in the literature to include both types of nominal frictions when taking the New Keynesian model to the data.

difference here is that we rely on a direct measure of the share of workers in the economy with predetermined wages to define the regimes in the VAR. To this end, we make use of a recently created data set which contains the content of hundreds of Swedish collective agreements over time. Specifically, we use economy-wide micro-level data on the exact dates when collective agreements are signed and the share of the workforce that is affected by the contract. Using these data, we document that contracts are signed during different seasons in different years, which allows us to define periods of predetermined wages with varying seasonal patterns. All in all, we document the share of workers for whom contracts are renegotiated at a monthly frequency during a 17-year period (1997:01-2014:09). These data are then used to estimate the importance of fixed wage contracts for the monetary transmission mechanism.

Our main result, based on a regime-interacted version of a monthly monetary VAR model, shows that output responses to monetary policy shocks are significantly larger when wages are rigid due to fixed contracts as compared to the average response in the data. The magnitude of the difference is of clear economic interest; the point estimates suggest a 0.37 (0.30) percentage points higher level of industrial production 12 (24) months after a policy shock (a reduction of 0.25 percentage points in nominal interest rates) if contracts are fixed relative to the average response in the data. This corresponds to 33 percent of the average response after a year and 16 percent at the peak for the industrial production response at 24 months. Using standard bootstrap procedures we find that the effect is statistically significant at horizons between 3 and 17 months at conventional levels.

Since a wage contract is an agreement on a path of payments to the workers during the contract period, we also provide descriptive evidence from the manufacturing and mining industry (where the contract length is stable over time) showing that the association between monetary policy shocks and actual accumulated nominal wage growth for two years is larger during periods of negotiations. This result is consistent with the presumption that wage contracts, as well as wage outcomes, respond to monetary policy shocks when they can, but also that actual wage outcomes remain more rigid during the duration of fixed contracts.

Importantly, the timing structure of contract signings enables us to separate the importance of wage rigidity from other factors related to the periodic structure or seasonality of the contracts. Specifically, we rely on Fisher-type exact inference and simulate counterfactual negotiation periods. By re-estimating the model on these counterfactual data we can construct a distribution of estimates to compare with our original estimate and to evaluate the probability that our results are confounded with some other seasonal or periodic factor in the data. Here, we first re-estimate the model for all possible alternative permutations with the same periodic structure (moving the sequence ahead one month at a time through the data period). Our second test re-estimates the model on a set of random permutations of dummies corresponding to the contracts, holding the seasonal structure of actual negotiation periods fixed. The results show that the importance of fixed wage contracts for the monetary transmission mechanism is significantly larger than the counterfactual estimates that retain the same periodic structure or the same seasonal patterns.

Overall, our results provide strong support for the notion that the timing of when wage contracts are signed, and hence nominal wage rigidity, is important for the degree of monetary non-neutrality. In particular, we show that the mechanism is fundamental and extends beyond generic seasonal effects and other aspects related to the periodic structure of the renegotiation events.

The rest of the paper is structured as follows. Section 2 describes the wage setting and monetary policy institutions in Sweden, presents the micro-data, and discusses the empirical strategy. The results are presented in section 3. Section 4 describes a number of sensitivity analyses. The last section concludes.

2 Institutions, Data and Empirical Strategy

2.1 Institutions

To identify the effect of nominal wage rigidities on macroeconomic outcomes we rely on the collective wage bargaining system in place in Sweden and for this reason it is important to first discuss this institution. Secondly, to motivate the set up of the monetary VAR and the identification of the monetary policy shock we also outline the Swedish monetary policy framework in this section.

2.1.1 Wage Bargaining

Swedish collective agreements are signed at the industry level with separate agreements for white- and blue-collar workers within these industries. Since 1997 the set-up follows a "pattern bargaining" structure. The bargaining sequence starts with a set of coordinated industry-level agreements for the areas most heavily exposed to international competition (essentially manufacturing and mining). Other sectors follow and sign agreements where wage increases should correspond to the growth rates set by the manufacturing and mining agreements. Notably, however, the agreements contain a host of different elements, including variations in the structure of wages, local implementation procedures, other pecuniary elements such as insurance, work environment and overtime regulations, employment protection procedures and so forth. Thus, the wage-norm set by the leading sector can be traded off against many different alternative elements in different sectors. Different industries may also choose different contractual durations and time paths for wage increases. The contract duration varies but almost always within the 1- to 3-year range (see below).

The procedures for implementing the industry-level agreements at the local level vary substantially between agreements. Procedures range from centrally determined tariffs (mostly transportation agreements) to procedures with varying degrees of guaranteed wage increases at the individual or group level. A universal feature is, however, the local level “peace obligation” which implies that all strikes and lockouts are banned once the industry-level agreements are struck.

2.1.2 Monetary Policy

Sweden has an independent central bank, the Riksbank, which employs a flexible inflation target system since 1995 where the inflation target is set at an annual rate of 2 percent. To achieve this goal the Riksbank sets its policy rate (the repo rate). The term ”flexible” implies that the Riksbank considers real economic outcomes alongside inflation deviations from target when deciding on its repo rate. Since the short nominal interest rate is the policy variable, the nominal exchange rate is floating freely.

This period of standard monetary policy operations ended on October 29, 2014, when the the repo rate was set to zero to combat low inflation and has stayed at or below zero since then. Monetary policy was soon also augmented with significant quantitative easing operations.

2.2 Micro Data

The discussion above identifies the period 1997:01-2014:09 as a period characterized by both a stable collective wage bargaining system and a standard flexible inflation targeting monetary policy framework. To compile the wage contract data for this period, we start by using a recently assembled micro-level data set covering all major private and public sector collective agreements signed between 2001:01 and 2010:12 collected by the Institute for Evaluation of Labour Market and Education Policy (IFAU). These data cover hundreds of bargaining areas and include information on how many workers each contract covers. Importantly, the data also include the date when each agreement was struck. We thus rely on the signing dates in all our analyses.

To focus attention on contracts that cover non-negligible parts of the labor market, we only use information for agreements covering 40,000 workers or more (about one percent of total employment).[3] In a second step we extended our coverage period for 1997:01-2000:12 by collecting data directly from the larger labor unions and the National Mediation Office and for 2010:01-2014:09 by use of the annual reports of the National Mediation Office. The collective agreements in our data cover about 1,800,0000 directly, out of a total of about 4,300,000 workers (mid-sample), but many more workers indirectly via agreements linked to these collective agreements. For example, in 2013, 89 percent of all workers were covered by collective agreements in the Swedish economy according to the National Mediation Office.

2.2.1 Stylized Facts from the Micro Data

In this subsection we highlight a few important facts about the bargaining sequence that we believe are important as a background for our empirical analysis.

Figure 1: Number of contracts signed each month, 1997:01-2014:09

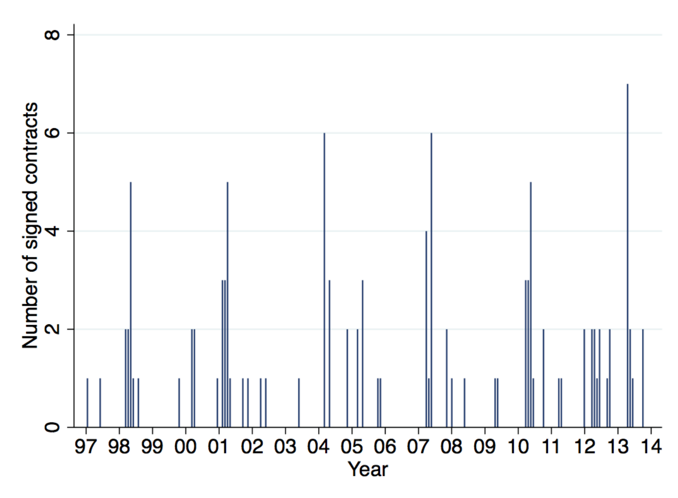


Figure 1 shows the number of newly signed contracts at the monthly frequency. As the bargaining areas vary considerably in terms of size (i.e. the number of workers they cover), we let Figure 2 show the share of total workers covered by contracts

[3] Including all contracts does not change the overall picture presented here.

who renegotiate their wages each month. These data thus provide us with a measure of the share of the economy that re-bargains its wages at each point in time. Both of these figures display a set of notable spikes in the renegotiation activity during a few specific months. This non-smooth frequency of renegotiations suggests that the economy at each point in time is in one of two possible states that are sharply different from each other. The first is a normal and ”rigid” state where few or no agreements are reset. The alternative ”flexible” state occurs during a few specific months characterized by an intensive renegotiation activity.

Figure 2: Share of workers renegotiating

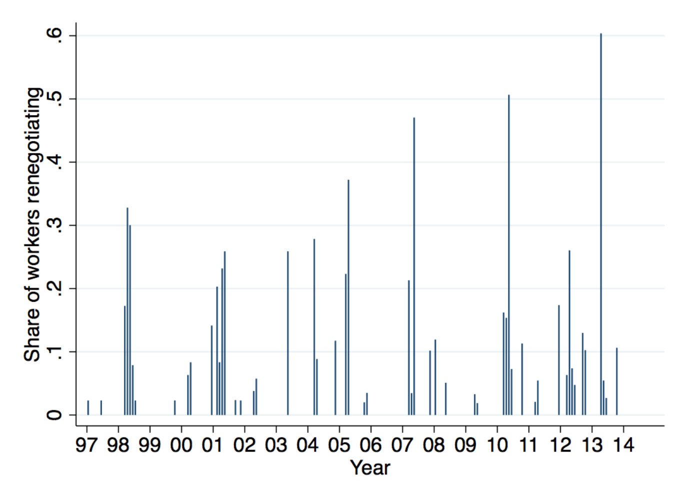
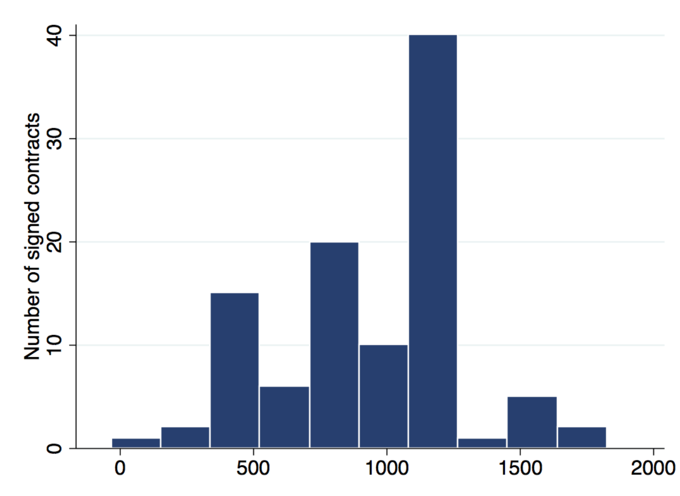
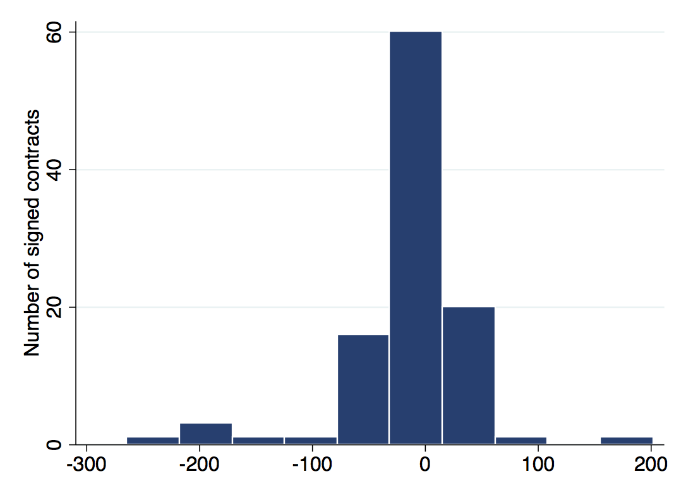


Figure 3: Contract length in days



The next fact we want to highlight is that the contracts are relatively long, but of varying (predetermined) duration. Figure 3 shows the duration distribution. The figure shows that most contracts are between 1 and 3 year long. The mean duration of contracts is 897 days with a standard deviation of 341 days. This varying duration generates variation in seasonality of new contracts. As evident from Figure 2 above, the spikes indicating intense renegotiation periods exhibit substantial variation across seasons and years.

Figure 4: Length in days between sign-on and start



Finally, we note that the time between the date for signing the contract (sign-on) and the start date of the contract (start) on average is very short with a mean of -13 days (implying a contract with retroactive implications) and a standard deviation of 55 days as illustrated in Figure 4. In our empirical analysis, we rely on sign-on dates throughout.

2.3 Macro Data and Empirical Model

The aim of the paper is to test the hypothesis that nominal wage rigidity affect the monetary policy transmission mechanism. As noted above, Figure 2 shows that the share of workers that negotiate has a very non-smooth distribution with distinct renegotiation periods. For this reason we set up an empirical model with two different regimes.[4] In practice, our empirical approach relies on contrasting two sets of estimates from a VAR-model as in Olivei and Tenreyro (2007, 2010). We first estimate the baseline or average impact of monetary policy shocks across our entire sample period. Secondly, we compare these results to estimates of the impact of the monetary policy shocks after removing the dynamics related to periods of renegotiation.

2.3.1 The Baseline VAR

The baseline specification of the monetary monthly VAR model we use is parsimonious and fairly standard. It includes industrial production, the consumer price index and the Riksbank policy rate, i.e. the repo rate, as endogenous variables. To avoid estimating a negative inflation response to reductions in the policy rate (the "price puzzle", see e.g. Sims, 1992), we follow the literature and include an exogenous forward-looking variable. Here, we use the raw-materials price index specified in dollars. To convert the index into SEK we include the nominal exchange rate between the Swedish krona and the US dollar as an endogenous variable. Note, however, that the nominal exchange rate is intended as a control variable and not as a variable of interest. The aim is not to develop a fully-fledged small-open-economy model since it would be impossible to estimate such a large model with two regimes using the data at hand.[5] The nominal interest rate is expressed in levels and all other variables are in log levels. In Section 4.3, we present a large number of robustness checks to validate that our key results are insensitive to the exact specification of the baseline VAR.

The estimation data spans from January 1997 to September 2014. As noted above, this represents the longest possible period of stable wage setting and monetary

[4]We will later experiment with using a renegotiation index in the robustness exercises presented in Section 4.3.

[5]Interestingly, the nominal exchange rate responded to a monetary policy shock as predicted in Rhee and Song (2013) both on average and across regimes, but given the aim of the paper and the parsimonious baseline model, we leave the analysis of the response of international variables to future research.

policy institutions with non-zero interest rates. Data is adjusted for seasonality. Each variable is included with four lags.[6]

The monetary policy shock is recursively identified in the spirit of Sims (1980) via a Choleski decomposition of the covariance matrix. As standard in the literature, industrial production and the consumer price index are ordered before the interest rate, and the exchange rate is last in the system of equations. The baseline reduced form representation can be written as (omitting constant terms)

$$\mathbf{Y}_t = \sum_{l=1}^{4} \boldsymbol{\rho}_l \mathbf{Y}_{t-l} + \sum_{l=1}^{4} \boldsymbol{\gamma}_l \boldsymbol{Z}_{t-l} + \mathbf{V}_t, \tag{1}$$

where $\mathbf{Y}_t = [\textit{industrial production}_t,\ \textit{cpi}_t,\ \textit{interest rate}_t,\ \textit{exchange rate}_t]'$ is a vector of the four endogenous variables and $\mathbf{Z}_t = [\textit{raw materials price index}_t]$ is the exogenous variable. Bold face letters indicates matrices and vectors. Impulse responses to a monetary policy shock can then be derived from an estimate of equation (1) using standard steps. An alternative, and more flexible, approach would be to estimate the dynamic responses using Jordà's (2005) local projection method. In the robustness Section 4.3 we use this method to show that the functional form assumptions implicit in the VAR impulse responses are reasonable (i.e. close to the results from linear projections), but since the method is much more demanding in terms of statistical power we do not rely on it for our main analysis.

2.3.2 Rigid vs. Flexible States

The average response to a monetary policy shock, derived from the estimate of (1), will be contrasted to the response when we remove the dynamics related to periods of renegotiation using a variation of the VAR-model where the lags are interacted with a lagged indicator for periods of renegotiation of the same lag order. The

[6] Recommended lag-length from AIC, SC, HQ is two. We choose four lags to remove autocorrelation (focusing on LM-tests on shorter horizons), but still keep the model parsimonious. We also test the stability of the model and reject a unit root in the residuals. When plotting impulse responses they are all eventually reverting to zero as the horizon grows.

reduced form representation of this variation of the VAR model is akin to Olivei and Tenreyro (2007, 2010) and can be written as (omitting constant terms)

$$\mathbf{Y}_t = \sum_{l=1}^{4} \boldsymbol{\beta}_l \mathbf{Y}_{t-l} + \sum_{l=1}^{4} \boldsymbol{\delta}_l D_{t-l} \mathbf{Y}_{t-l} + \sum_{l=1}^{4} \boldsymbol{\lambda}_l \boldsymbol{Z}_{t-l} + \mathbf{U}_t, \tag{2}$$

where D_{t-l} is the renegotiation dummy and $\mathbf{U}_t$ is a vector of residuals which are linear combinations of shocks.[7] Note that the time subscript for D_{t-l} implies that we allow for different dynamic effects of outcomes in bargaining versus fixed wage periods. Based on estimates of this model, we calculate the response to monetary policy shocks under fixed wage contracts and contrast it to the average response derived from (1). Or, more specifically, our empirical strategy compares the dynamic responses calculated using the estimated coefficient matrices $\boldsymbol{\rho}$ in equation (1) to the dynamic responses calculated using the estimated coefficient matrices $\boldsymbol{\beta}$ from equation (2). Again, this methodology closely follows Olivei and Tenreyro (2007, 2010) where they compare impulses at different quarters of the year to the standard VAR results.

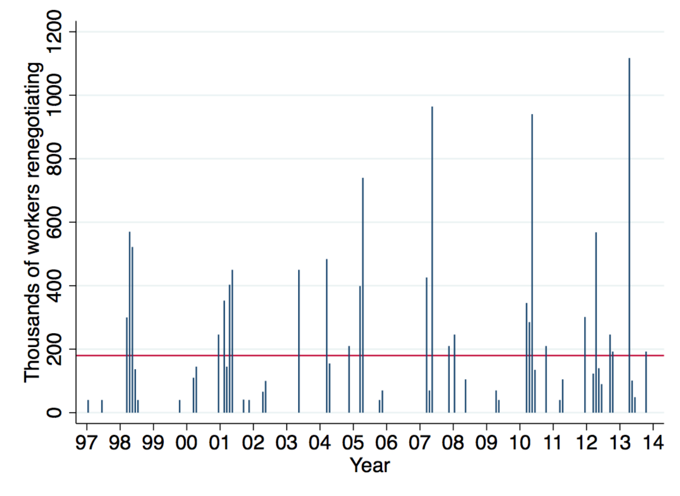
2.3.3 Defining Periods of Renegotiation (D_t)

Our main analysis lets the renegotiation dummy D_t, take on the value of unity in months where more than 180,000 workers have their nominal wages renegotiated as illustrated in Figure 5 (months with bars raising above the red horizontal line).[8] According to this definition, negotiation takes place in 28 months during the period 1997:01-2014:09. Hence, the analysis on fixed wage contracts is restricted to the remaining 185 monthly observations.

[7]As a robustness exercise we have also interacted the exogenous matrix with the indicator. The results do not change.

[8]The indicator for June 1998 and April 2000 is set to unity. These months have high negotiation shares and are close to the cut-off of 180,000 even though these monthly observations, collected directly from the labor unions, include fewer contracts on average. In the robustness Section 4.3 we specify the dummy at higher levels and validate that the result is not driven by the narrow choice of the indicator.

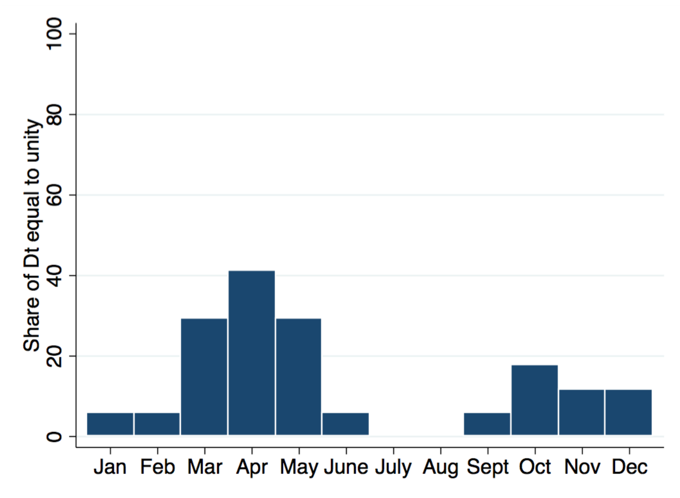
Figure 5: Renegotiation indicator



Note: Red line at 180,000 workers.

In Figure 6 we show the seasonal pattern of D_t. As is evident, renegotiation does not occur uniformly during the year, and it is not restricted to a specific quarter. Instead, negotiations took place during all months except July and August with evident peaks in March, April and May. In Section 4.2 we will use this within-year variation to gauge the possibility of our results being driven by generic seasonal effects.

Figure 6: Seasonal distribution of renegotiation



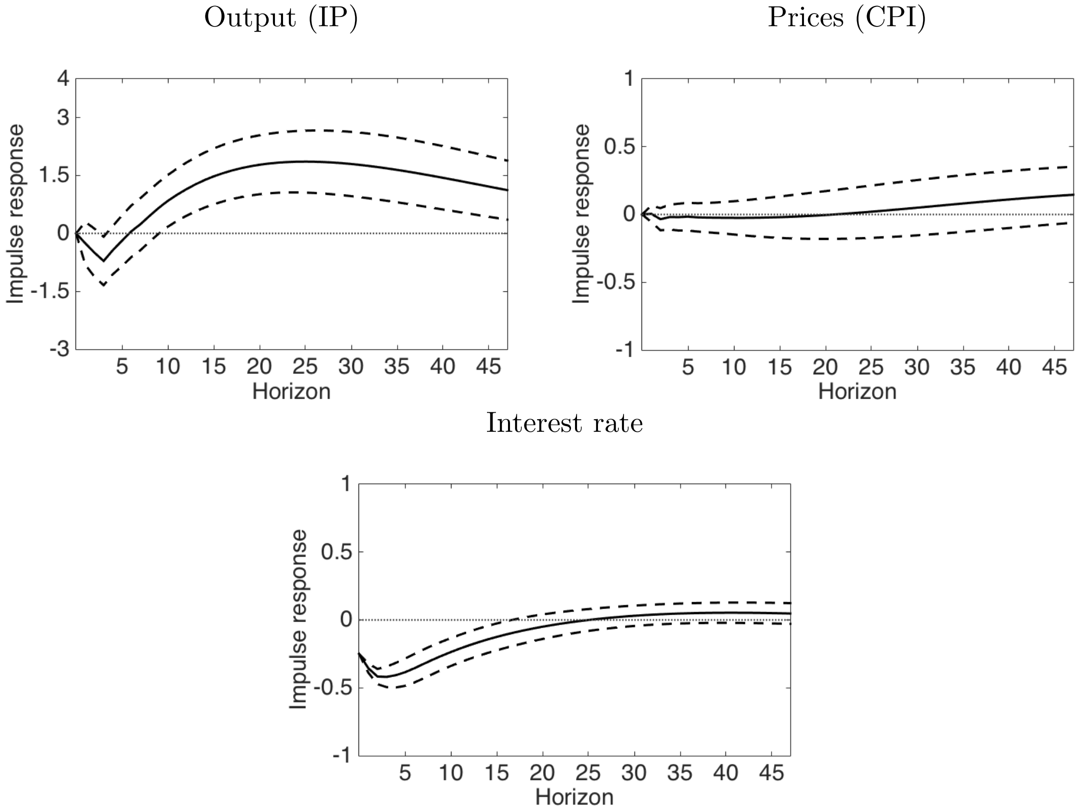
We also experiment with alternative definitions of D_t. First, since theory implies that monetary policy shocks should have less of a real effect as long as wages can react to them, we expand the renegotiation dummy to also take on the value of unity in the two months before the original dummy takes on the value of unity. Secondly, as a parsimonious way of handling the non-zero renegotiation activity that often follow the signing of a large agreements, as is apparent from Figures 2 and 5, we estimate a version where we extend the renegotiation dummy formulation to also take the value of unity in the two months after the original dummy takes on the value of unity. Effectively, this provides a stricter definition of fixed wage periods that we use as a contrast to the estimates from the baseline VAR. Finally, we estimate a version where we extended the renegotiation dummy to also takes on the value of unity both two months before, as well as, two months after the original dummy takes on the value of unity. In the robustness Section 4.3 below we show that using these alternative formulation does not change the overall message of the paper.

3 Results

3.1 Baseline Impulse Responses

To validate the empirical specification we start by estimating the VAR model. We follow standard bootstrap procedures to calculate the confidence interval (Runkle, 1987). Impulse responses following a reduction of 0.25 percentage points in the nominal interest rate are presented in Figure 7. The model displays typical, and well documented, features (see e.g. Christiano, Eichenbaum and Evans, 1999) where output reacts with a positive hump-shaped response, peaking about 24 months after the shock. Inflation is highly persistent and responds with a positive sign after a substantial lag. Overall, we interpret these estimated responses as being well in line with the conventional wisdom regarding the the responses to monetary policy shocks.

Figure 7: Baseline, impulse responses following a lowering of the interest rate by 25 basis points



Note: Dotted lines represents 90 percent bootstrap confidence bands. Variables in levels with a scale denoting percentage units.

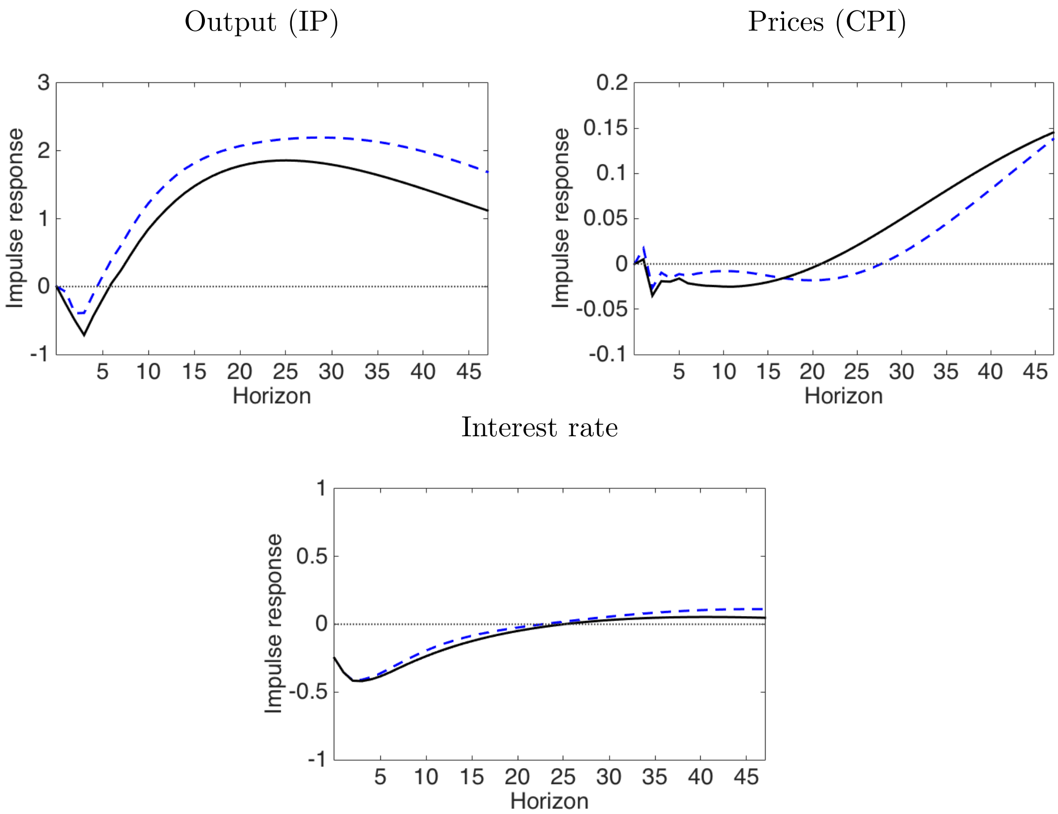
3.2 Impulse Responses when Wage Contracts are Fixed

We now turn to the estimation of the response to monetary policy shocks under fixed wage contracts. We show the dynamic responses (dashed lines) alongside the baseline estimates in Figure 8.[9] However, our focus is on the differences between the average impulse responses and those under fixed wage contracts. These are displayed in Table 1. Evaluated 12 (24) months after the policy innovation, the estimated difference for the level of industrial production is 0.37 (0.30) percentage points larger. The price responses are however similar with essentially flat responses for the first two years, after which they start to rise both on average and under fixed

[9]Note that the average and the fixed wage responses are based on highly correlated estimates. Thus, only comparing whether or not individual error bands for the responses overlap will lead astray in the inference.

nominal wage contracts. Interestingly, the point estimates for the price responses seem to suggest that these responses are slower under fixed wage contracts and starts to rise above zero about two quarters later than the average response.

Figure 8: Impulse responses to a lowering of the interest rate by 25 basis points



Note: Dashed blue lines are impulse responses under fixed wage contracts, and black solid lines are average impulse responses. Variables in levels with a scale denoting percentage units.

Determining the statistical properties of the relative responses requires tests of the dynamics (not on the coefficients) since the impulses are non-linear combinations of the estimated coefficients of the VAR. We construct one-sided bootstrap test of the difference between the impulse responses for each variable under fixed contracts to the standard VAR across different horizons following Olivei and Tenreyro (2007, 2010). The underlying hypotheses is that output should respond more and prices less under nominal wage rigidities (i.e. fixed contracts). The hypothesis for the interest rate response does not have a clear sign and we therefore construct a two-sided test for this variable.[10] We calculate the p-statistics as the fraction of bootstrapped differences that are larger than the estimated differences.

The statistical significance of the differences are denoted by stars in Table 1. The output difference under fixed contracts to the baseline is significant at the at the 5 (10) percent level for the 3-month to 17-month horizons, but the differences become insignificant at the far side of the hump. In contrast, we do not find any significant difference estimates in the responses of prices (second column) or the policy rate (third column).

Table 1: Estimated difference in impulse responses

Horizon	Output (IP)	Prices (CPI)	Interest rate
3	0.3257**	0.0095	0.0097
6	0.3473**	0.0085	0.0281
9	0.3816**	0.0163	0.0392
12	0.3742*	0.0158	0.0435
15	0.3411*	0.0070	0.0393
18	0.3063	-0.0056	0.0307
21	0.2910	-0.0181	0.0230
24	0.3045	-0.0282	0.0192

Note: Difference estimates are fixed wage response − average response. One-sided tests for output (difference > 0) and Prices (difference < 0), two-sided tests for Interest rate. *, **, ***, denote significance at 10, 5 and 1 percent level, respectively.

Overall, the results presented in Figure 8 and Table 1, suggest that the dynamic responses of output as measured by industrial production are larger when wages cannot be reset, whereas there is no statistically significant evidence that prices or the interest rate responds differently.

3.3 Illustration: Shocks and medium-run wage responses

The fact that a signed contract means a commitment to a wage path during the full contract spell of one to three years, with varying time profiles, makes identification

[10]Two-sided tests for all variables reduce the significance levels slightly, but the key results remain significant.

of actual wage responses difficult in a monthly VAR. Here, instead, we analyze the effects of monetary policy shocks on the full contract spell.

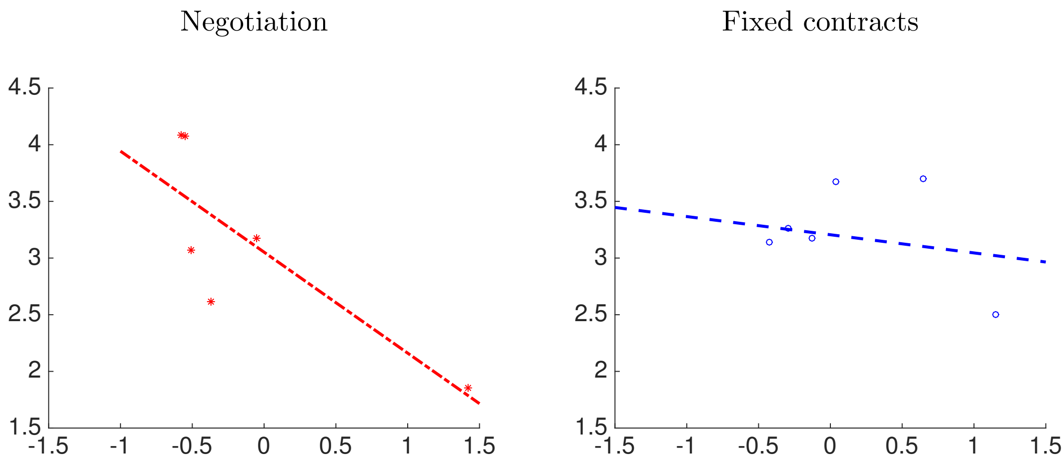
The data we rely on is monthly observations of the annual wage growth in percent. Wages includes total compensation, including compensation for working unfavorable hours, bonuses and so forth.[11] In this section we restrict the analysis to the mining and manufacturing sector, one of the few sectors where we can match actual wages to sectoral contracts. The advantage of using this sector is also the ability to calculate mean wage developments of a series of large contracts that are of almost equal lengths; negotiated in March, April, May in the years 1998, 2001, 2004, 2007, 2010, and 2013. To capture wage developments we calculate the mean wage change for two years starting at the last month before the negotiation period (*negotiation*) and starting twelve months later, i.e. eight periods after the last negotiation month (*fixed contracts*). We then relate these two measures to monetary policy shocks that happened the first month in the two-year spell for which we are calculating average wage growth. The policy shock is backed out from the average VAR outlined in equation (1) above by multiplication of the reduced form errors with the inverse of the Choleski matrix.

Figure 9 plots the wage development (vertical axis) in response to the interest rate shock (horizontal axis) under negotiation and fixed contracts. The results suggests a negative relationship between wage growth and monetary policy shocks. In addition, the wage growth is larger under negotiation, thus the wage response appears more forceful when contracts are renegotiated. Notably, the agreements have only been renegotiated six times over our sample. Hence, the results are fairly crude and should be taken with a grain of salt. With this caveat in mind, the figure is consistent with the view that actual wage growth is more strongly related to monetary policy shocks occurring just before renegotiations than they are to shocks

[11] Babecký et al. (2012) finds complementarity between nominal wage rigidity and other labor cost adjustments. Our wage data includes total payment but cannot identify postponing a promotion or a new hire. We do not argue that no other adjustments can be used by the firm to adjust when wages are sticky.

occurring about a year after the contracts were signed.

Figure 9: Wage developments and the interest rate shock



Note: Vertical axis: Mean yearly wage growth, Horizontal axis: Structural interest rate shock with standard deviation normalized to unity.

4 Robustness

Next, we turn to a set of exercises designed to assess the robustness of the results. We first study the role of the periodic structure of renegotiations, we then turn to the role of seasonality, and finally we show a number of robustness checks regarding measurement and the design of the VAR.

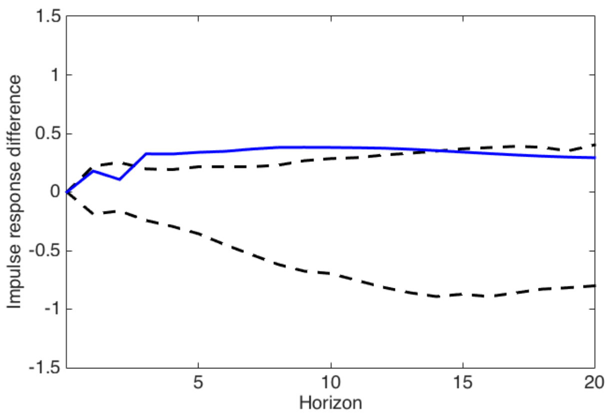
4.1 The Periodic Structure of Renegotiations

As noted in Section 2 above, the duration of contracts varies, but with some notable spikes in the renegotiation activity. This could potentially be important for our estimates if other factors that are related to this non-random periodic structure in turn affect the estimated impulse responses, e.g. if other contracts have a similar structure. To test if the difference we estimate is a random artifact of the periodic structure, we design a permutation-based test in the spirit of exact inference. The test is constructed to determine if the actual renegotiation regimes are significantly different from any other indicator with the same duration distribution. The null hypothesis is that the estimated difference can be obtained by any indicator given the periodic structure of the actual bargaining periods.

We design the test by simultaneously shifting all our 28 dummies for renegotiations forward one month at a time (moving the end points to the start) resulting in 212 potential counterfactual series. We exclude all series where counterfactual dummies overlap with true position of the micro-founded dummies more than 4 times over the 212 data points. Excluding these overlapping series leaves us with 102 alternative ways of specifying the 28 dummies, all with the same period structure. We then re-estimate the model for all of these counterfactual series and let the estimates generate a distribution of differences which we can use to test the null hypothesis that our estimated difference in the response is just a random draw from this distribution.

We construct the counterfactual distribution by ordering the estimated difference in responses (fixed wage response − average response) by size for each horizon, removing the upper and lower five percent to obtain the 90 percent confidence interval. Figure 10 shows the estimated difference in output responses in the actual data and confidence bands drawn from the counterfactual distribution.

Figure 10: Counterfactual keeping periodic structure



Note: Dotted lines at 90 of the counterfactual distribution of the difference test (fixed wage response − average response) of output (IP). Solid line: estimated difference in actual data. Variable in level with a scale denoting percentage units.

The results confirm that our estimates are indeed different from random estimates based on indicators with an identical periodic structure. In particular, the difference of industrial production falls outside of the 90-percent range of the counterfactual distribution for the 3- to 14-months horizons.

4.2 Seasonality

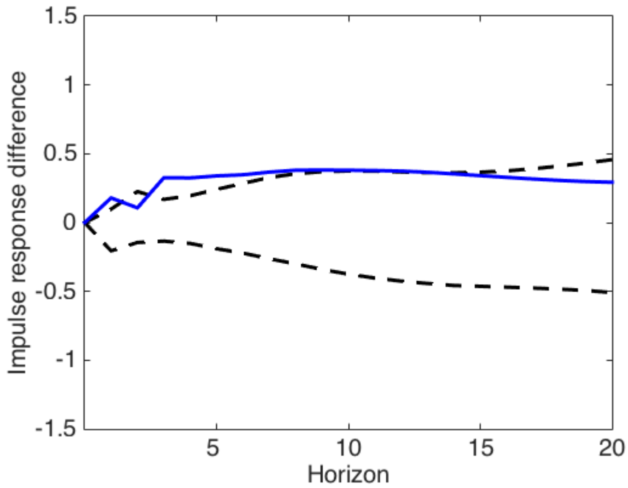
One of the key advantages of our detailed micro data is that we observe that the incidence of bargaining varies within and across seasons. So far, however, we have not fully exploited this aspect of the data. A possible concern is that our results still may be capturing some other seasonal patterns that are unrelated to bargaining, but correlated with the bargaining periods through the non-uniform distribution of the negotiations documented in Figure 6 above.

In order to address this potential concern, we proceed in a spirit similar to the test for the periodic structure we presented above. We generate a number of counterfactual data sets, each of which has 28 counterfactual dummies with a seasonality pattern that exactly matches the true renegotiation pattern documented in Figure 6 above. We then re-estimate the model on each of these counterfactual data sets and calculate a distribution of the difference in responses, just as we did for the periodic structure. This allows us to test the null hypothesis that our estimated difference in responses reflects a random draw of differences from data sets with 28 dummies with the exact seasonal pattern of the true renegotiation dummies.

To be precise, a contract agreement is observed only once in February over the 17 years of data. Hence, there are 16 alternative years where counterfactual contract agreements can be assigned in February. For March the number of positive dummies is 5 out of 17, thus there are 2 unique and non-overlapping ways of placing the 5 counterfactual dummies on the remaining 12 years and so forth. Combining these cases across all months, there are 51 million possible counterfactual data sets that can be created. We randomly selected 51 thousand of these and estimated the

VAR for each of them and keep the 90 percent distribution of differences as when analyzing the periodic structure.

Figure 11: Counterfactual keeping seasons



Note: Dotted lines at 90 of the counterfactual distribution of the difference test (fixed wage response − average response) of output (IP). Solid line: estimated difference in actual data. Variable in level with a scale denoting percentage units.

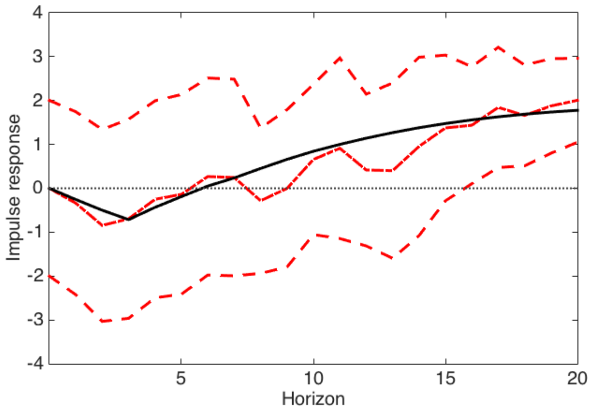
Figure 11 presents the 90 percent confidence bands together with the estimated difference in the responses. The results are very similar to the analysis of the periodic structure. The differences for output at the 3 to 7-months horizons are significant. The results thus show that our estimate of the difference from the observed data is significantly different from estimates based on alternative, counterfactual, data sets with 28 dummies having an identical seasonal structure as the true renegotiation dummies. We interpret this as strong evidence for the notion that our main results are in fact driven by the fixed wage contracts, and not other correlated seasonal patterns.

4.3 Measurement and Specification of the VAR

To evaluate the functional form assumptions implicit in the VAR we next calculate the dynamic response of output using Jordà's (2005) local projection method. This implies calculating the response at each horizon h by regressing output at period

$t+h$ on the structural interest rate shock in t. In Figure 12 we plot both the VAR impulse response and the local projection impulse response from a 25 basis point structural interest rate shock, together with 90 percent confidence bands for the local projection.[12]

Figure 12: Output response: VAR vs. local projection



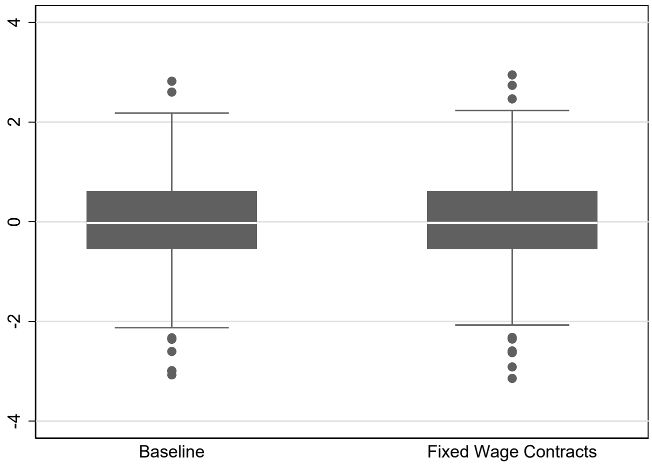
Note: Black line VAR impulse response. Red line: Local projection impulse response. Dotted red lines represents 90 percent Newey West (1987) HAC confidence bands.

Reassuringly the point estimates of the output response are very similar across methods and the VAR impulse response is well covered by the error bands of the local-projection response.

Another concern regards whether the distribution of structural interest rate shocks differ between the base line (full) sample and in the sample of defined by fixed contracts (i.e. months with $D_t = 0$). In Figure 13 we present Box-Whisker plots of the two distributions. Reassuringly, the two distributions are nearly identical.

[12]The confidence bands are computed as in Jordà (2005) using Newey and West (1987) HAC consistent method treating the structural shocks as data.

Figure 13: Structural shock distributions



Note: Vertical axis: Left: (Full) Baseline sample. Right: (Sub) Sample of fixed contracts observations. The standard deviation of the structural interest rate shock series is normalized to unity. The boxes depict the median and the 75/25 percentiles. The whiskers depict the upper and lower adjacent values. Outside observations are depicted by dots.

We next experiment with the definition of the renegotiation dummy D_t and the measurement we use for the real responses. In the second row of the top panel of Table 2 the dummy variable D_t in equation (2) is replaced by an index between zero and one, constructed by dividing the number of workers negotiated each month by the maximum observation in this time series. The output difference is then computed between the response derived from setting this index to zero in the time-varying VAR and the baseline response. As can be seen in the table, the results are similar and statistically significant at the 3- to 6-months horizons. The reduced precision is expected since the underlying data does not provide much information on the full shape of the effect of varying the share of workers bargaining in the economy, but rather on the effects in the end-points of this index, which is better captured by the discrete modeling approach taken in this paper. In the third to fifth rows of the top panel of Table 2 we experiment with the definition of the dummy D_t in equation (2) by also setting the renegotiation dummy to unity the two months before (third row) or after (fourth row) or both before and after (fifth row) a month where D_t takes on a unit value in the original formulation (as discussed in Section

2.3.3). As presented in the Table, the results of these exercises are at par with the original results (top row) or stronger up to the 6-month horizon, but somewhat weaker at higher horizons. All, in all, the experiments with the definition of the renegotiation indicator do not change the overall message of the paper.

Table 2: Alternative specifications I

Model	Output Difference at Horizon 3	6	9	12
1) Original	0.3257**	0.3473**	0.3816**	0.3742*
2) Index	0.2344**	0.2297*	0.2308	0.2055
3) Extended Dummy (B)	0.2989**	0.3152*	0.3312	0.2403
4) Extended Dummy (A)	0.4075**	0.4024*	0.4228*	0.3633
5) Extended Dummy (B/A)	0.4766**	0.3946*	0.3566	0.2122
Model	**Unemployment Difference at Horizon 3**	**6**	**9**	**12**
1) Unemployment	0.0774	-0.1012	-0.1760	-0.1633
2) Unemployment Index	-0.0737	-0.2152	-0.2725	-0.3100
3) Unemployment Extended Dummy (B)	0.0605	-0.2696	-0.4324	-0.4353
4) Unemployment Extended Dummy (A)	-0.1842	-0.6665*	-0.9278*	-0.9721*
5) Unemployment Extended Dummy (B/A)	-0.1294	-0.7022	-0.9995*	-1.0480*

Note: Difference estimates are fixed wage response − average response. One-sided tests for output (difference > 0) and for unemployment (difference < 0). *, **, ***, denote significance at 10, 5 and 1 percent level, respectively. B (A) denotes the case where the renegotiation dummy is extended to also take on the value of unity two months before (after) a unit value for the original renegotiation dummy. B/A denotes the case where the renegotiation dummy is extended in both directions.

In the bottom panel of Table (2) we replace the output measure in the VAR with the number of prime aged unemployed.[13] The first row of the bottom panel presents the results from this exercise. We see that although the point estimate is in line with the idea that unemployment level is lower when wages cannot adjust following a negative interest rate shock at the 6-months horizon and higher, the differences are not statistically significant. When using an index, as above, instead

[13]We choose to use unemployment as our indicator of labor market responses due to large (partly tax induced) changes in labor supply and retirement patterns over the period. We focus on prime aged unemployment due to revised data collection procedures related to workers at the intersection of education and unemployment which had a large impact on measured youth unemployment from 2005 on-wards.

of the renegotiation dummy in the VAR the effects become slightly larger as shown in the second row of the bottom panel of Table (2). In the final set of exercises, presented in the third to fifth rows of the bottom panel of Table (2) we again experiment with changing the definition of the rebargaining dummy. In the fourth and fifth rows we see that also setting the rebargaining dummy to unity in the two months after (or both before and after) a month with a unit value in the original formulation of D_t has a substantial effect on the results. The unemployment level is significantly lower from the 6- to 9-months horizons and on-wards in the table, with point estimates of around a one percentage point lower unemployment level at the 12-month horizon after a policy shock (a reduction of 0.25 percentage points in the nominal interest rate) if contracts are fixed relative to the average response in the data. The magnitudes of the differences in the last two rows is also of economic interest. Specifically, the difference estimates in row 4 (5) corresponds to 161 (174) percent of the average response at the 12-months horizon and 7 (24) percent close to the peak response of the unemployment level at 24-months horizon. Overall, there is thus evidence that not only output respond more to monetary policy shocks in the short to medium run when wages are fixed, but also that the unemployment level responds more in the same time span.

We continue with investigating the robustness of the results by adding different exogenous variables that can account for international variation. To assess how robust our results are to different ways of accounting for these dependencies, we have included international output, prices and interest rates as exogenous variables to the VAR. We use two alternative measures for each of these: the US and the trade-weighted rest of the world (ROW). The ROW index is based on Sweden's 20th largest trading partners (using imports plus exports) during the sample period using month-specific weights corresponding to the share of total Swedish trade going towards that country.

For both the US and ROW we use industrial production, consumer price index and the nominal 3-month interest rate as exogenous variables in the VAR. To keep the model parsimonious, we rotate across the different variables, adding them to the original VAR one at the time as in Ramey (2011). Results are presented in Table 3 for the difference of industrial production. The first row shows the original specification as a comparison. The second (US) and third (ROW) rows show the results when foreign industrial production is added to the VAR. Overall, the estimates are slightly reduced but remain close to the original specification and are significant on the same horizons. Adding foreign inflation in rows four (US) and five (ROW) slightly reduces the estimates and the significance level drops on longer horizons. The third variable we add is a 3-month nominal interest rate in rows six (US) and seven (ROW). The 6-month difference is smaller (0.26, 0.27) while the 12-month difference is larger (0.64, 0.67). Overall, we conclude that the main results are robust to the inclusion of a broad set of variables capturing international variation.

Table 3: Alternative specifications II

	Output Difference at Horizon			
Model	3	6	9	12
1) Original	0.3257**	0.3473**	0.3816**	0.3742*
2) Output US	0.2299**	0.2570**	0.2907**	0.3600**
3) Output ROW	0.2089**	0.2466**	0.2850**	0.2844**
4) Inflation US	0.3135**	0.2833*	0.3292*	0.3223
5) Inflation ROW	0.3649***	0.2861*	0.2932*	0.2692
6) Rate US	0.2400**	0.2632*	0.4549**	0.6368***
7) Rate ROW	0.2176*	0.2669**	0.4850**	0.6686***
8) Crisis	0.2646**	0.2209*	0.2261*	0.2483*
9) Real ex rate	0.3084**	0.2882**	0.2945*	0.2770*
10) 300 000	0.2840**	0.2579*	0.2631*	0.2706*
11) Trend	0.3432**	0.3112*	0.3008	0.2599
12) 3 Variables	0.3191***	0.3658**	0.3881**	0.3553*
13) Wages	0.2032*	0.2696*	0.3990*	0.4864*

Note: Difference estimates are fixed wage response − average response. One-sided tests for output (difference $>$ 0). *, **, ***, denote significance at 10, 5 and 1 percent level, respectively.

Aside from rotations with added international variables we have also experimented with the basic VAR specification. First, we add a dummy for the worst phase of the 2008 financial crises (eighth row in the top panel), 2008:08–2009:01.[14] In the ninth row, the model is adjusted by using the real exchange rate, instead of the nominal exchange rate in the original VAR.[15] Results in the tenth row in the top panel of Table 3 are estimated for data where we expand the fixed wage case to include all months unless at least 300,000 workers signed a new contract during the month (17 cases). The eleventh row presents results including a linear time trend. The twelfth row in Table 3 presents results for a reduced specification with only the industrial production index, inflation and the interest rate. Overall, the differences and are in the same range as the original estimates, and the significance is never above the 10-percent level for shorter horizons. Thus, the key results are robust to this set of variations of the original VAR. In the thirteenth row, we experiment by replacing price inflation with wage growth in the original VAR. The estimated responses grow towards the baseline results and is significant at the 10-percent level. Thus, the results are reassuring in terms of delivering point estimates that are mostly in line with the base estimates (despite the identification issues discussed in Section 3.3), but we also acknowledge that the lower statistical significance raises some concerns

5 Conclusion

We use a detailed micro-level data set on Swedish collective agreements to study the importance of wage rigidity for the transmission mechanism of monetary policy. In contrast to the previous literature, we have access to detailed micro data covering hundreds of collective agreements, which provides us with an actual measure of the share of the economy that is negotiating its wage contract at each point in time. We document substantial variation in contract duration and seasonality in the signing

[14]Variations on this theme with other dummy constellations, including individual dummies for each month, sometimes gives lower significance levels, but do not change the overall conclusion.

[15]The exchange rate is given by $q = \frac{S_t P_t}{P_t^*}$, where $q = 100 * (s + p_f - p)$, s_t is log of the nominal exchange rate, p_t^* is log of US consumer prices and p_t is log consumer prices in Sweden.

dates. This variation allows us to isolate the impact of contract duration without capturing other seasonal or cyclical components. We use these data to construct indicators of time varying wage rigidities which we use as interaction terms in an monetary VAR model. We then contrast the estimated dynamics during fixed wage contracts with the average responses in the spirit of Olivei and Tenreyro (2007, 2010).

An illustration based on industry-level data from the manufacturing sector suggests that the response of actual wages to a policy shock is different during episodes of fixed wage contracts and negotiation. Our main results based on the VAR show a significantly larger output response when wages cannot adjust. The magnitude of the difference is also of economic interest; the point estimates suggest a 0.37 (0.30) percentage points higher level of industrial production 12 (24) months after a policy shock (a reduction of 0.25 percentage points in the nominal interest rate) if contracts are fixed relative to the average response in the data. This corresponds to 33 percent of the average response after a year and 16 percent at the peak for the industrial production response at 24 months. Using standard bootstrap procedures we find that the effect is statistically significant at horizons between 3 and 17 months at conventional levels. Using permutation-based tests in the spirit of exact inference, we can reject the null hypothesis that our estimates reflect a random draw of estimates from data with the same seasonal pattern as actual negotiation periods as well as the null hypothesis that the estimated differences can be obtained by random indicators given the periodic structure of the actual bargaining periods.

Overall, we conclude by noting that previous studies have documented seasonal patterns consistent with an amplifying role for time-varying wage rigidities in settings where wage contracts are negotiated at the same point in time every year. Our results show that wage rigidities in fact do amplify the monetary policy transmission even conditional on other seasonal patterns and other aspects related to the non-random periodic structure of wage agreements. The results thus suggest that the recent surge of studies on the interaction between wage setting and monetary policy represent a research agenda of first-order importance.

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