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Fixed wage contracts and monetary non-neutrality

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Fixed wage contracts and monetary non-neutrality[a]

by

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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 VAR model, we document that monetary policy shocks have a substantially 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. Durant et. al. (2012) and references therein. But up until relatively recently, nominal wage rigidities have not featured as an element in standard New Keynesian macro 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 macro 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] Since then it has become standard in the literature to includes both types of nominal frictions when taking the New Keynesian model to the data. 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 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 strongly related to Olivei and Tenreyro (2007, 2010), who show seasonality patterns that are consistent with larger real impacts of monetary policy shocks during seasons when wage contracts tend not to be renegotiated according to national practices. The key difference here is that we rely on a direct measure of the share of workers in the economy with

[1]See Erceg, et. al (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.

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 also substantial; the preferred point estimates suggest a 0.34 percentage points higher level of industrial production six 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. Using standard bootstrap procedures we find that the effect is statistically significant 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 negotia-tion periods fixed. The results show that the importance of fixed wage contracts for the monetary transmission mechanism is significantly larger than the counterfac-tual 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 monetary policy and wage setting 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

2.1.1 Monetary policy

On January 15, 1993, the independent Swedish central bank, the Riksbank, published a policy statement announcing that it had shifted into a (flexible) inflation target system where the inflation target was set at an annual rate of 2 percent from 1995 and onwards. The policy soon became credible and has remained in place since then. As a consequence, the high inflation rates of around an annual rate of 10 percent preceding the currency crisis in the late 1980s and early years of the 1990s came down relatively quickly and reached levels close to 2 percent in 1994-1995 and has since then varied between -0.3 and 3.4 percent. On October 29, 2014, the Riksbank policy rate (the repo rate) was set to zero to combat low inflation and has stayed at or below zero since then.

2.1.2 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.

2.2 Micro data

To compile our wage contract data, 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 last 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.

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

For example, in 2013, 89 percent of all workers were covered by collective agreements in the Swedish economy according to the National Mediation Office.

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

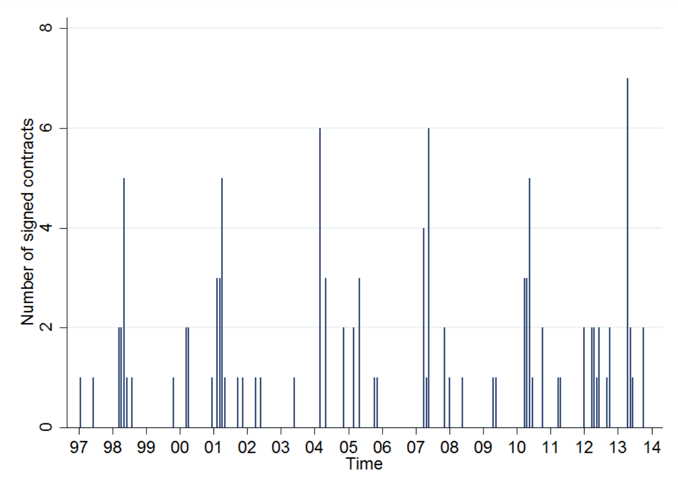
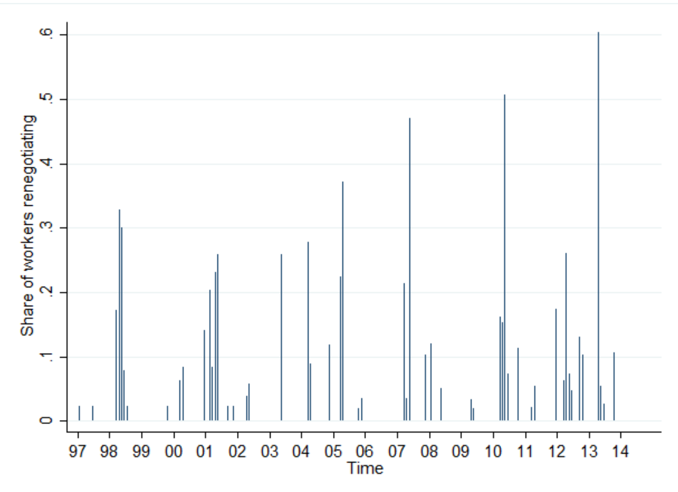


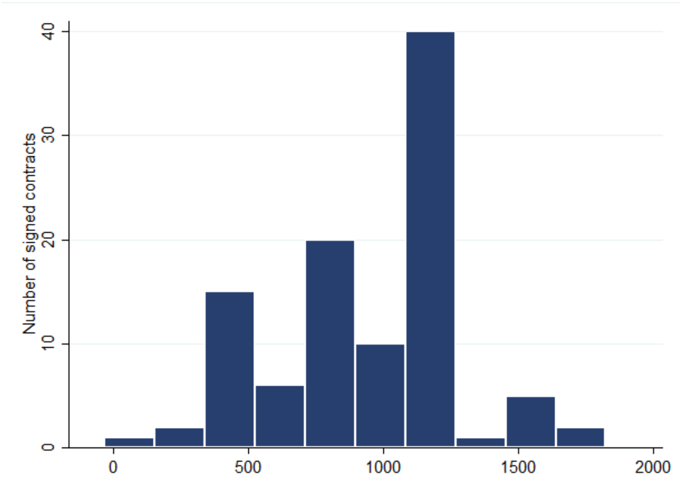
Figure 1 shows the number of newly signed contracts at the monthly frequency. The data show a set of notable spikes in the renegotiation activity, indicating a non-smooth frequency of renegotiations. The position of the spikes also exhibits substantial variation across seasons and years.

Figure 2: Share of workers renegotiating



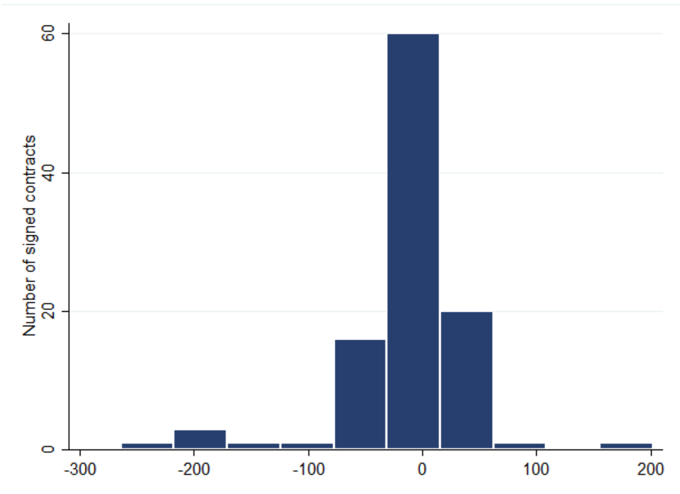
The bargaining areas vary considerably in terms of size (i.e.the number of workers they cover). Figure 2 shows the share of total workers covered by contracts who renegotiate their wages each month. These data thus provide us with a measure of the share of the economy that re-bargains its wages at each point in time.

Figure 3: Contract length in days



The contracts are of varying (predetermined) duration, but mostly in the 1- to 3-year range. Figure 3 shows the distribution. The mean duration of contracts is 897 days with a standard deviation of 341 days.

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



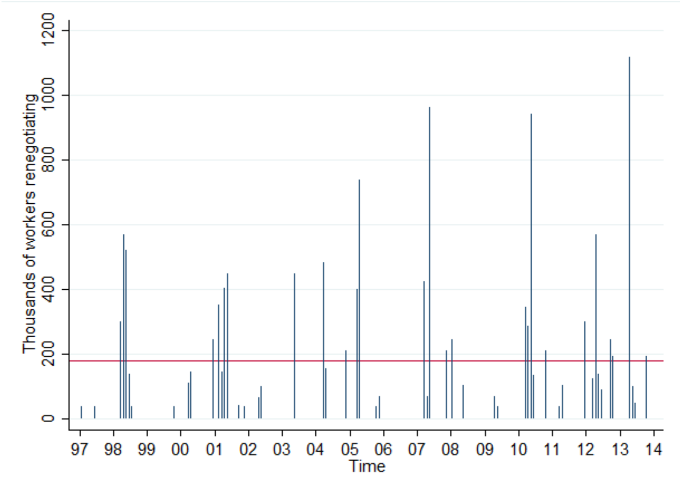
Finally, we note that the time between the date for signing the contract (sign-on) and the start date of the contract (start) is on average very short with a mean of -13 days (implying a contract with retroactive implications) and a standard deviation of 55 days as illustrated in Figure 4. This implies that a large share of the contracts are signed with retroactive effects on wages.

2.2.1 Defining periods of renegotiation

To estimate the importance of nominal wage rigidity for monetary policy effectiveness, we construct a renegotiating dummy indicator, D_n, that takes on the value unity for months when a non-negligible part of the nominal wages is renegotiated. Thus, by construction, the dummy takes on the value of zero in months when al-most all nominal wages are predetermined by contracts in place and thus cannot be affected by contemporaneous shocks. This dummy then allows us to empirically evaluate the importance of nominal wage rigidity for the monetary policy transmission mechanism. Specifically, we let this dummy take on the value of unity in periods 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).[4] 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.

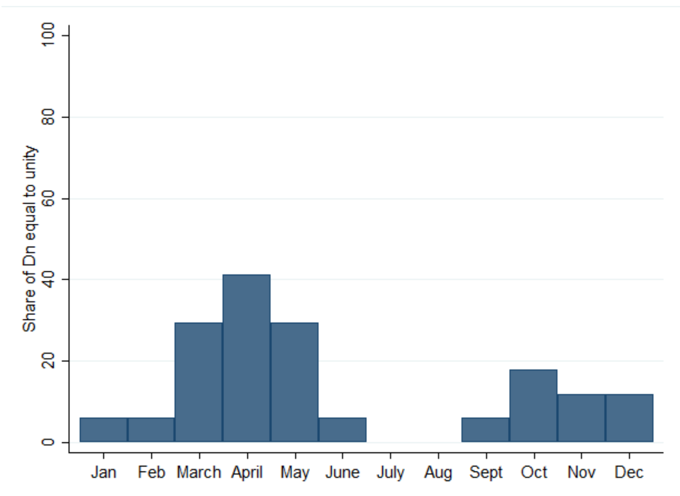
[4]The indicator for June 1998 and April 2000 is set to unity. These months have high negotiation shares and are close to the cut-off of 180,000 even though these monthly observations, collected directly from the labor unions, include fewer contracts on average. In the robustness section we specify the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

Figure 5: Renegotiation indicator, red line at 180,000 workers



In Figure 6 we show, by month, the renegotiation frequency per year according to the definition underlying our renegotiation dummy, D_n. As is evident, renegotiation does not occur uniformly during the year. Moreover, it occurs not only in a specific quarter, but throughout the months of the year with the exception of July and August. The renegotiation frequency peaks in March, April and May, but is not uncommon in October, either. Below 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



2.3 Macro data and empirical model

Our empirical approach relies on contrasting two sets of estimates from a VAR-model as in Olivei and Tenreyro (2007, 2010). To this end, we first estimate the average impact of monetary policy shocks across our sample period. Secondly, we compare this estimate to the estimate of the impact of the monetary policy shocks when we remove the dynamics related to periods of renegotiations (as defined by the renegotiation dummy, D_n), the null hypothesis being that these responses are identical.

The baseline parsimonious specification of the monetary monthly VAR model is 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 estimat-ing 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 exchange the index into SEK we also include the nominal exchange rate between the Swedish krona and the US dollar as an endogenous variable. Note that we are not trying to develop a fully fledged small open economy model here since it would not be possible to estimate such a large model with two regimes with the data at hand. Instead, the nominal exchange rate is included as a control and not as a variable of focus.[5] The nominal interest rate is expressed in levels and all other variables are in log levels. In section 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. This represents

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[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.

the longest possible period of stable wage setting and monetary policy institutions with non-zero interest rates.[6] Data is adjusted for seasonality. Each variable is included with four lags.[7]

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)

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

where $\mathbf{Y}_t = [\textit{industrial production},\ \textit{cpi},\ \textit{interest rate},\ \textit{exchange rate}]'$ is a vector of the four endogenous variables and $\mathbf{Z}_t = [\textit{raw materials price index}]$ is the exogenous variable. Bold face letters indicates matrices and vectors.

The average response to a monetary policy shock, derived from the estimate of (1), will then be contrasted to the response when we remove the dynamics related to periods of renegotiations using a variation of the VAR-model where the lags are interacted with an indicator for periods of renegotiations. The reduced form representation of this variation of the VAR model can be written as (omitting constant terms)

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

where $\mathbf{V}_t$ is a vector of residuals which are linear combinations of shocks.[8] From the

[6] The first industry agreement for wage contracts was signed in 1997. The Riksbank lowered the policy rate to zero in October 2014.

[7] 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.

[8] As a robustness exercise we have also interacted the exogenous matrix with the indicator, adding four additional parameters. The results do not change.

estimate 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 matrix $\boldsymbol{\rho}$ in equation (1) to the dynamic responses calculated using the estimated coefficient matrix $\boldsymbol{\beta}$ from equation (2).[9]

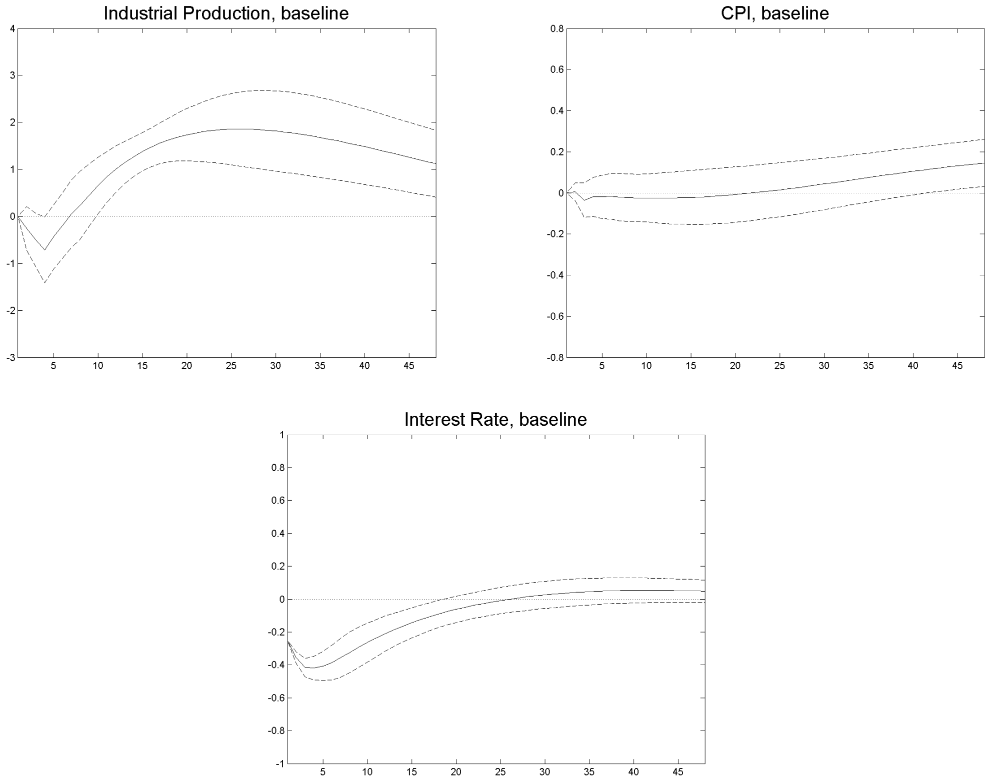
3 Results

3.1 Average 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 et. al., 1999) where output reacts with a positive hump-shaped response, peaking about 20 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, although it should also be noted that, as is common in the VAR-literature, many estimates are fairly imprecise.

[9]The methodology follows closely Olivei and Tenreyro (2007, 2010) where they compare impulses at different quarters of the year to the standard VAR results.

Figure 7: Baseline, impulse responses following a lowering of the interest rate by 0.25 basis points



Note: Dotted lines represents 90 percent bootstrap confidence bands. Interest rate in level and IP and CPI 100*log level.

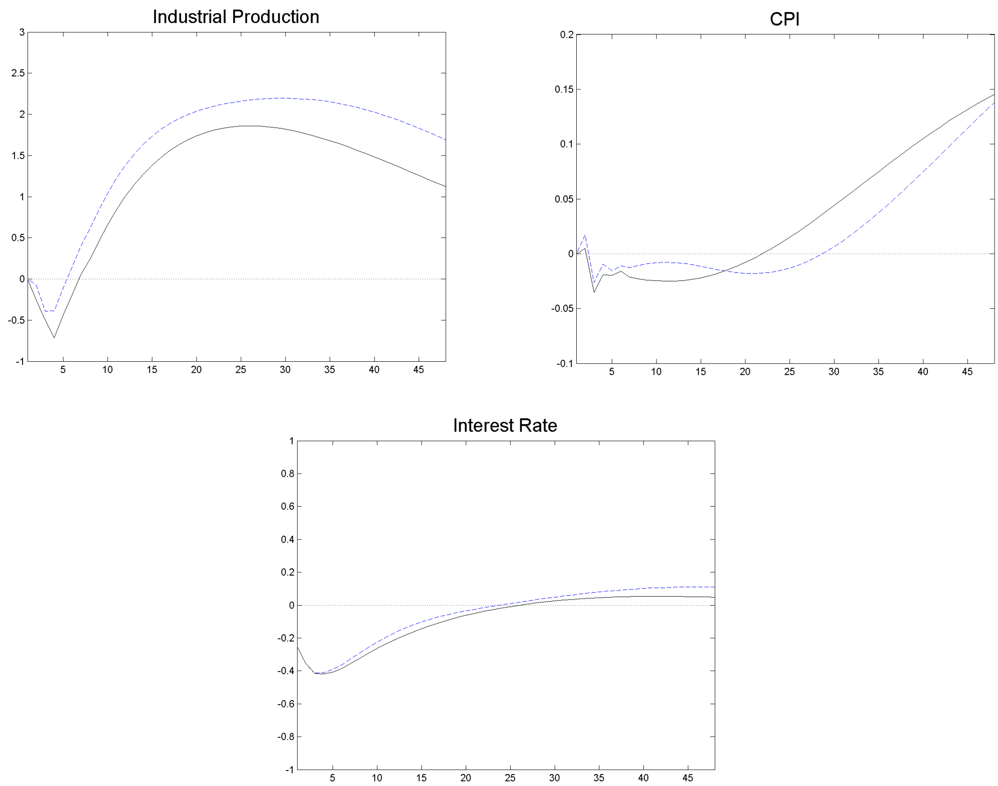
3.2 Impulse Responses when Wage Contracts are Fixed

We now turn to the estimation of the response to monetary policy shocks under fixed wage contracts. The dynamic responses are presented by the dashed lines in figure 8. As with the baseline model, estimates are fairly imprecise.[10] However, our focus is on the differences between the average impulse responses and those under fixed wage contracts. Evaluated 6 (12) months after the policy innovation, the estimated difference for the level of industrial production is 0.34 (0.38) percentage points larger. This difference is also persistent, as shown by the cumulative responses in Figure 9.

[10]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.

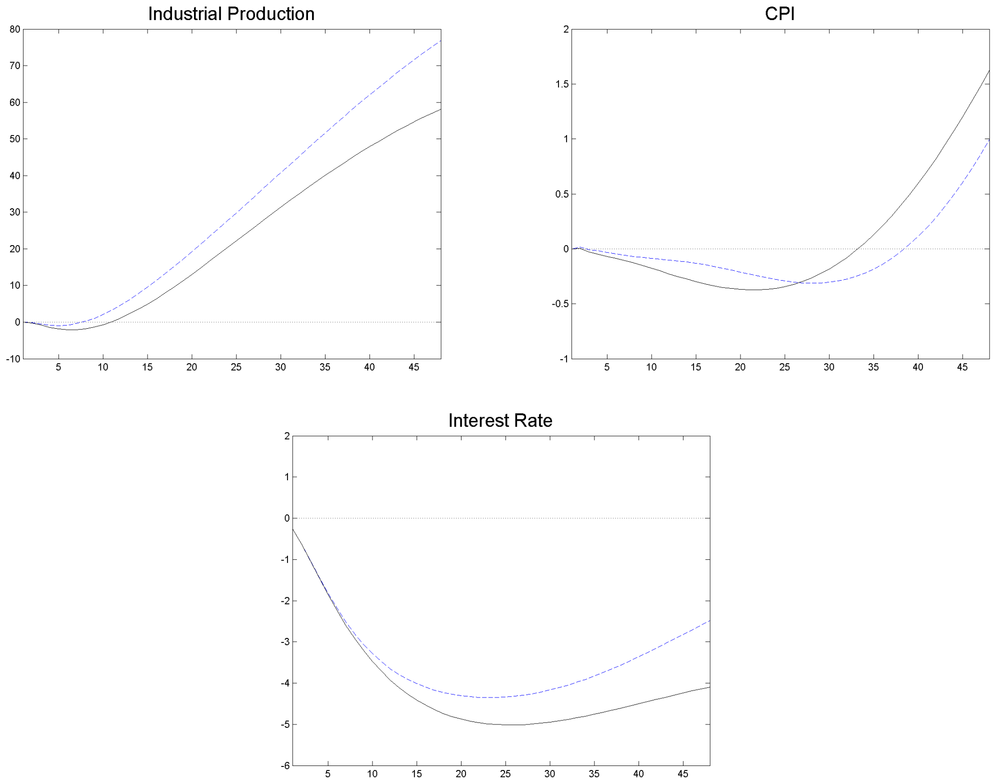
The price inflation responses are similar and respond with substantial lags both on average and under fixed nominal wage contracts. Interestingly, the point estimates for price inflation responses seem to suggest that the inflation response is slower under fixed-wage contracts and starts to rise above zero about two quarters later than the average response.

Figure 8: Impulse responses to a lowering of the interest rate by 0.25 basis points



Note: Dashed blue lines are impulse responses under fixed-wage contracts, and black solid lines are average impulse responses. Interest rate in level and IP and CPI 100*log level.

Figure 9: Cumulative impulse responses to a lowering of the interest rate by 0.25 basis points



Note: Dashed blue lines are impulse responses under fixed-wage contracts, black solid lines are average impulse responses. Interest rate in level and IP and CPI 100*log level.

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 (D) and the cumulative difference (CD) between the impulse responses for each variable under fixed contracts to the standard VAR following Olivei and Tenreyro (2007, 2010). The D-test statistic is the difference between the fixed contract and average impulses at each horizon whereas the CD-test statistic is the corresponding cumulative differences up to the same horizons. The test statistics are constructed from the one-sided hypotheses that fixed contracts should make output respond more and prices less. The hypothesis for the interest rate response does not have a clear sign and we therefore construct a two-sided test for this variable.[11] We calculate the p-statistics as the fraction of bootstrapped differences (and cumulative differences) that are larger than the estimated differences (CDs).

The results are displayed in Table 1. The output difference under fixed contracts to the baseline is significant at the at the 5 (10) percent level for 6- to 9-month (18-month) horizons, but the differences become insignificant at the far side of the hump. The cumulative effect is statistically significant at the 5 percent level at all horizons from 6 to 21. The estimated (D) magnitude has a response of output that is 0.34 percentage points larger six months after the shock under fixed contracts compared to the average response, whereas the cumulative difference (CD) after 12 months is 3.5 percentage points larger under fixed contracts. In contrast, we do not find any significant estimates (D or CD) in the responses of prices (second column) or the policy rate (third column).

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

Table 1: Estimated difference in impulse responses

	DIFFERENCE		
Horizon	Output	Inflation	Rate
3	0.1069	0.0086	0.0031
6	0.3391**	0.0049	0.0222
9	0.3806**	0.0148	0.0360
12	0.3781**	0.0171	0.0431
15	0.3538*	0.0106	0.0415
18	0.3167*	-0.0012	0.0337
21	0.2929	-0.0141	0.0252
24	0.2968	-0.0252	0.0200
	CUMULATIVE DIFFERENCE		
Horizon	Output	Inflation	Rate
3	0.2861*	0.0208	0.0033
6	1.2754**	0.0392	0.0512
9	2.3699**	0.0748	0.1417
12	3.5100**	0.1253	0.2717
15	4.6034**	0.1653	0.3996
18	5.5898**	0.1742	0.5092
21	6.4872**	0.1445	0.5930
24	7.3672**	0.0793	0.6571

Note: Estimates are fixed-average. One-sided tests for output (IP) and inflation (CPI), two-sided tests for interest rate. *, **, ***, denote significance at 10, 5 and 1 percent level, respectively.

Overall the results presented so far in Figures 8 and 9, and Table 1, suggest that the dynamic responses of output as measured by industrial production are larger when wages cannot be reset. The response of price inflation is not significantly affected by wage rigidities, suggesting that actual and expected real wages, and hence current and expected real marginal costs, respond more when wages are predetermined, thus resulting in a larger output response.

3.3 Illustration: Shocks and medium-run wage responses

The fact that a signed contract means a commitment to a nominal wage path during the full contract spell of one to three years, with varying time profiles, makes identification of actual wage responses difficult in a monthly VAR. Here, instead, we analyze the effects of monetary policy shocks on the full contract spell.

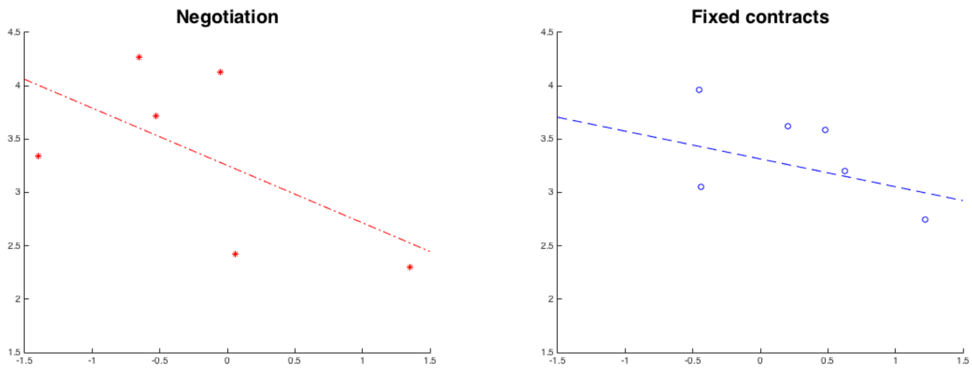
The data we rely on is monthly observations of the annual wage growth in percent. Wages includes total compensation, including compensation for working unfavor-able hours, bonuses and so forth.[12] In this section we restrict the analysis to the mining and manufacturing sector, one of the few sectors 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 10 plots the nominal 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 nominal wage growth and monetary policy shocks. In addition, the nominal 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

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

related to monetary policy shocks occurring just before renegotiations than they are to shocks occurring about a year after the contracts were signed.

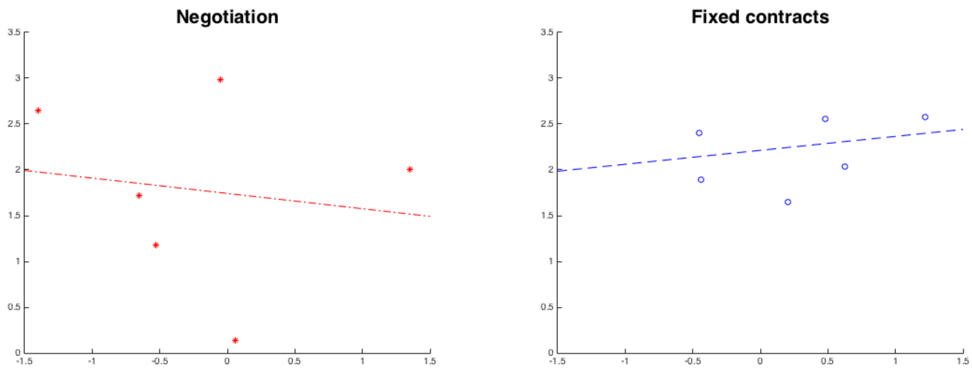
Figure 10: Nominal wage developments and the interest rate shock



Note: Vertical axis: mean (yearly wage growth), Horizontal axis: structural interest rate shock.

Our second exercise focuses on real wage growth for the same sample.

Figure 11: Real wage developments and the interest rate shock



Note: Vertical axis: mean (yearly real wage growth), Horizontal axis: structural interest rate shock.

If nothing happens to prices in response to a monetary policy shock during the two-year period we are considering, we should see the same pattern as for the nominal wage growth exercise above. Compared to that exercise, we see that the real wage growth response is much more subdued in the case with negotiation and even changes sign in the case with fixed contracts. These patterns are consistent with prices falling in response to a positive interest rate shock over the two-year period (which is an outcome contained within the error bands of the estimated price responses above), leaving the real wage almost unaffected in the case when nominal contracts can respond and leading to a real wage increase in the case when nominal contracts cannot respond. This set of results is thus also consistent with larger real effects of monetary policy shocks during periods of fixed wage contracts. Again, however, a caveat is in place due to the limited number of observations.

4 Robustness and seasonality

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.

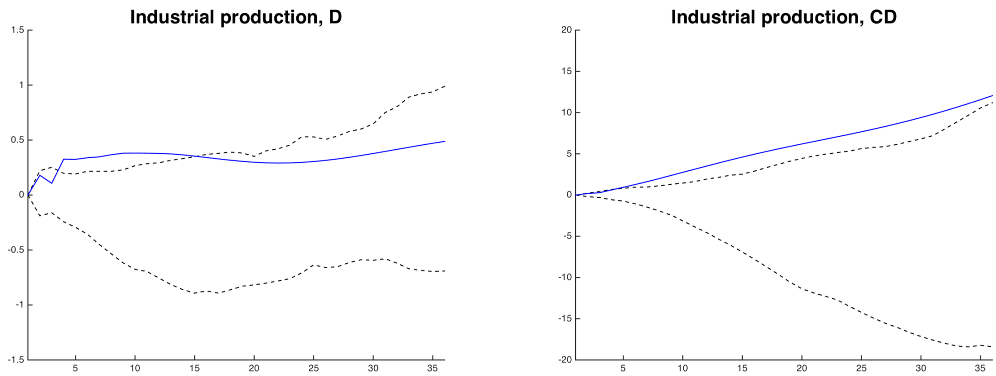
4.1 The periodic structure of renegotiations

As noted in Section 3 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 counter-factual 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 responses are just a random draw from this distribution.

We construct the counter-factual distribution by ordering the estimated difference in responses by size for each horizon, removing the upper and lower five percent to obtain the 90 percent confidence interval. We then repeat this exercise for the cumulative differences. Figure 12 shows the estimated difference and the cumulative difference in output responses in the actual data and confidence bands drawn from the counter-factual distribution for differences and cumulative differences.

Figure 12: Counter-factual keeping periodic structure



Note: Dotted lines at 90 of the counterfactual distribution. IP and CPI 100*log level difference.

The results confirm that our estimates are indeed different from random estimates based on indicators with an identical periodic structure. In particular, industrial production falls outside of the 90-percent range of the counterfactual distribution for the first 12 months and the cumulative difference remain significant throughout all depicted horizons.

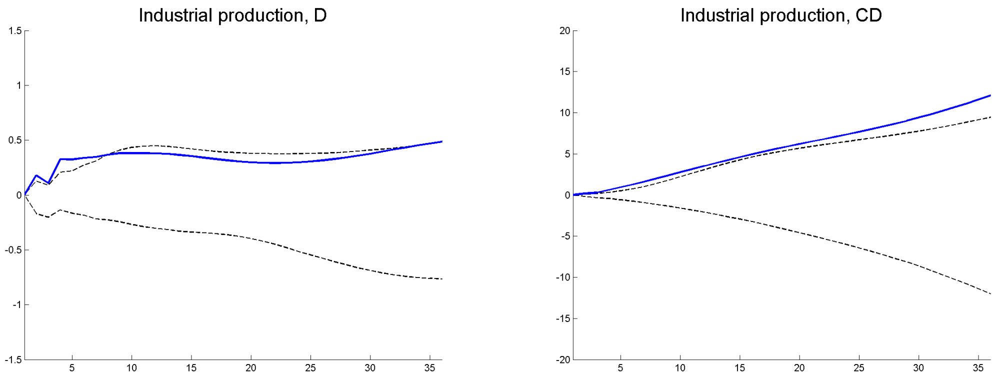
4.2 Seasonality

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 counter-factual data sets, each of which has 28 counter-factual 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 and cumulative differences as when analyzing the periodic structure.

Figure 13: Counter-factual keeping seasons



Note: Dotted lines at 90 of the counterfactual distribution. IP and CPI 100*log level difference.

Figure 13 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 short-run D and long-run CD estimates for output are significant. The results thus show that our estimate of the difference from the observed data is significantly different from estimates based on alternative, counter-factual, 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

We first investigate 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 2 for the two difference tests of industrial production (the top panel display the difference test and the bottom panel display the cumulative difference test). The first row in each panel shows the original specification as a comparison. Given that the difference test showed significant results for output only for shorter horizons, but the cumulative difference test also for longer horizons for output, we adjust the horizons displayed in the two panels of Table 2 accordingly. The second (US) and third (ROW) row in each panel show the results when foreign industrial production is added to the VAR. The estimated differences for output are slightly larger when including the ROW measure and reduced with the US measure. Overall, the estimates remain close to the original specification and are significant on the same horizons. Adding foreign inflation in rows four (US) and five (ROW) in each panel slightly reduces the estimates and the significance level drops but remains at 10 percent for most horizons. The third variable we add is a 3-month nominal interest rate in rows six (US) and seven (ROW) in each panel. The 6-month difference is smaller

(0.23) while the 12-month difference is larger (0.58), and the cumulative effect is larger at longer horizons. Overall, however, we conclude that the main results are robust to the inclusion of a broad set of variables capturing international variation.

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), 2008:08–2009:01.[13] In the ninth row of each panel, the model is adjusted by using the real exchange rate, instead of the nominal exchange rate in the original VAR.[14] Results in the tenth row of each panel in Table 2 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. Finally, the twelfth row in each panel in Table 2 presents results for a reduced specification with only the industrial production index, inflation and the interest rate. Overall, the differences and cumulative differences are in the same range as the original estimates, and the significance is never above the 10-percent level for the key horizons. Thus, the key results are robust to this set of variations to the original VAR. Finally, in the last row of each panel, we experiment by replacing price inflation with nominal wage growth in the original VAR. The estimated responses grow towards the baseline results with the horizon and turn significant on the 10-percent level at longer horizons. Thus, despite the identification issues discussed in Section 3.3, the basic message remains unchanged from this exercise as well.

[13] 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.

[14] 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.

Table 2: Industrial production. Alternative specifications

	DIFFERENCE at Horizon			
Model	3	6	9	12
Original	0.1069	0.3391**	0.3806**	0.3781*
Output US	0.0569	0.2550**	0.2806**	0.3353**
Output ROW	0.0306	0.2322**	0.2802**	0.2894**
Inflation US	0.0064	0.2636*	0.3335*	0.3268*
Inflation ROW	0.0800	0.2963*	0.2995*	0.2811
Rate US	0.0259	0.2395*	0.3940**	0.5769***
Rate ROW	0.0159	0.2309**	0.4161***	0.6112***
Crisis	0.0726	0.2323**	0.2285*	0.2361*
Real ex rate	0.0830	0.2962**	0.3026*	0.2814*
300 000	0.0903	0.3189**	0.2918*	0.2599*
Trend	0.1066	0.3231*	0.3115	0.2754
3 Variables	0.1249	0.3534**	0.3910**	0.3700**
Wages	-0.0045	0.2067	0.2669	0.3871*
	CUMULATIVE DIFFERENCE at Horizon			
Model	6	12	18	24
Original	1.2754**	3.5100**	5.5898**	7.3672*
Output US	0.9248**	2.6637**	5.0838**	8.1650**
Output ROW	0.8300**	2.4826**	3.9945**	5.1533**
Inflation US	0.9787*	2.8920*	4.6339*	6.1294*
Inflation ROW	1.2203**	2.9632*	4.2473*	4.8639
Rate US	0.8896*	3.4233**	7.8386***	12.5624***
Rate ROW	0.8121*	3.4814**	8.0639***	13.2734***
Crisis	0.9789**	2.3411**	4.0530*	6.5605*
Real ex rate	1.1383**	2.8905**	4.4273*	5.7887*
300 000	1.3983***	2.9481**	4.7050**	6.7819**
Trend	1.2794*	3.0783**	4.3210*	5.1223*
3 Variables	1.2571**	3.5368**	5.3568**	6.3723*
Wages	0.6666	2.3325	4.6458*	6.9794*

Note: Estimates are fixed-baseline. *, **, ***, denote significance at 10, 5 and 1 percent level, respectively. ROW is a weighted measure consisting of Sweden's trade partners.

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 (ex ante) contract duration and seasonality in the signing dates. This variation allows us to isolate the impact of contract du-ration 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 estimated effect implies that industrial production is 0.34 percentage points higher six months after the (-0.25 bp) policy shock in periods of fixed wage contracts as compared to the average response in the data. 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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