

A Service of



Leibniz-Informationszentrum Wirtschaft Leibniz Information Centre for Economics

Björklund, Maria; Carlsson, Mikael; Nordström Skans, Oskar

# Working Paper Fixed wage contracts and monetary non-neutrality

Sveriges Riksbank Working Paper Series, No. 368

**Provided in Cooperation with:** Central Bank of Sweden, Stockholm

*Suggested Citation:* Björklund, Maria; Carlsson, Mikael; Nordström Skans, Oskar (2019) : Fixed wage contracts and monetary non-neutrality, Sveriges Riksbank Working Paper Series, No. 368, Sveriges Riksbank, Stockholm

This Version is available at: https://hdl.handle.net/10419/215446

#### Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

#### Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



# WWW.ECONSTOR.EU



# Fixed Wage Contracts and Monetary Non-Neutrality

Maria Björklund, Mikael Carlsson and Oskar Nordström Skans

March 2019

#### WORKING PAPERS ARE OBTAINABLE FROM

www.riksbank.se/en/research

Sveriges Riksbank • SE-103 37 Stockholm Fax international: +46 8 21 05 31 Telephone international: +46 8 787 00 00

The Working Paper series presents reports on matters in the sphere of activities of the Riksbank that are considered to be of interest to a wider public. The papers are to be regarded as reports on ongoing studies and the authors will be pleased to receive comments.

The opinions expressed in this article are the sole responsibility of the author(s) and should not be interpreted as reflecting the views of Sveriges Riksbank.

# Fixed Wage Contracts and Monetary Non-Neutrality<sup>\*</sup>

Maria Björklund<sup>†</sup> Mikael Carlsson<sup>‡</sup> Oskar Nordström Skans<sup>§</sup>

Sveriges Riksbank Working Paper Series No. 368 March 2019

#### 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

<sup>\*</sup>We are grateful to Nils Gottfries and seminar participants at Uppsala University, the Uppsala Center for Labor Studies and the EEA 2016 meeting, Geneva, for useful comments. Financial support from the Ragnar Söderberg Foundation and the Handelsbankens Forskningsstiftelser is gratefully acknowledged. We thank the Institute for Evaluation of Labour Market and Education Policy and in particular Alexander Westerberg for providing data on wage contracts. The views expressed in this paper are solely the responsibility of the author and should not be interpreted as reflecting the views of Sveriges Riksbank.

<sup>&</sup>lt;sup>†</sup>Uppsala University. e-mail: maria.olsson@nek.uu.se

 $<sup>^{\</sup>ddagger}\mathrm{Uppsala}$  University and Sveriges Riksbank. e-mail: mikael.carlsson@nek.uu.se

<sup>&</sup>lt;sup>§</sup>Uppsala University, UCLS and IZA. e-mail: oskar.nordstrom\_skans@nek.uu.se

# 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.<sup>1,2</sup> 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

<sup>&</sup>lt;sup>1</sup>See Erceg, Henderson and Levin (2000) for the seminal paper on how to incorporate nominal wage rigidities in the New Keynesian model.

<sup>&</sup>lt;sup>2</sup>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).<sup>3</sup> 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 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

<sup>&</sup>lt;sup>3</sup>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.





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.<sup>4</sup> 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.<sup>5</sup> 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

 $<sup>^4\</sup>mathrm{We}$  will later experiment with using a renegotiation index in the robustness exercises presented in Section 4.3.

<sup>&</sup>lt;sup>5</sup>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.<sup>6</sup>

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)

$$\mathbf{Y}_{t} = \sum_{l=1}^{4} \boldsymbol{\rho}_{l} \mathbf{Y}_{t-l} + \sum_{l=1}^{4} \boldsymbol{\gamma}_{l} \boldsymbol{Z}_{t-l} + \mathbf{V}_{t}, \qquad (1)$$

where  $\mathbf{Y}_t = [industrial \ production_t, \ cpi_t, \ interest \ rate_t, \ exchange \ rate_t]'$  is a vector of the four endogenous variables and  $\mathbf{Z}_t = [raw \ materials \ price \ index_t]$  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

<sup>&</sup>lt;sup>6</sup>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)

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

where  $D_{t-l}$  is the renegotiation dummy and  $\mathbf{U}_t$  is a vector of residuals which are linear combinations of shocks.<sup>7</sup> 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).<sup>8</sup> 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.

<sup>&</sup>lt;sup>7</sup>As a robustness exercise we have also interacted the exogenous matrix with the indicator. The results do not change.

<sup>&</sup>lt;sup>8</sup>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.<sup>9</sup> 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

<sup>&</sup>lt;sup>9</sup>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.<sup>10</sup> 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).

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

Table 1: Estimated difference in impulse responses

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

 $<sup>^{10}\</sup>mathrm{Two-sided}$  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.<sup>11</sup> 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

<sup>&</sup>lt;sup>11</sup>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.



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.<sup>12</sup>

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.

 $<sup>^{12}{\</sup>rm The}$  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 timevarying 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.

	Output Difference at Horizon			
Model	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
	Unemp	loyment Di	ference at 1	Horizon
Model	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*

Table 2: Alternative specifications I

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.<sup>13</sup> 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

<sup>&</sup>lt;sup>13</sup>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.

	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*	$0.2696^{*}$	$0.3990^{*}$	$0.4864^{*}$

Table 3: Alternative specifications II

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.<sup>14</sup> In the ninth row, the model is adjusted by using the real exchange rate, instead of the nominal exchange rate in the original VAR.<sup>15</sup> 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

<sup>&</sup>lt;sup>14</sup>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.

<sup>&</sup>lt;sup>15</sup>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.

# References

- Babecký, Jan, Philip Du Caju, Theodora Kosma, Martina Lawless, Julián Messina, and Rõõm Tarir. 2012. "How do European firms adjust their labour costs when nominal wages are rigid?" *Labor Economics*, 19(5): 792–801.
- Broer, Tobias, Niels-Jakob Harbo Hansen, Per Krusell, and Erik Oberg. Forthcoming. "The New Keynesian Transmission Mechanism: A Heterogeneous-Agent Perspective." Review of Economic Studies.
- Christiano, Lawrence J., Martin Eichenbaum, and Charles L. Evans. 1999. "Monetary policy shocks: What have we learned and to what end?" In *Handbook* of Macroeconomics. Vol. 1. 1 ed., , ed. John B. Taylor and Michael Woodford, Chapter 2, 65–148. Amsterdam:Elsevier.
- Christiano, Lawrence J., Martin Eichenbaum, and Charles L. Evans. 2005.
  "Nominal rigidities and the dynamic effects of a shock to monetary policy." Journal of Political Economy, 113(1): 1–45.
- Druant, Martine, Silvia Fabiani, Gabor Kezdi, Ana Lamo, Fernando Martins, and Roberto Sabbatini. 2012. "Firms' price and wage adjustment in Europe: Survey evidence on nominal stickiness." *Labor Economics*, 19(5): 772–782.
- Erceg, Christopher J., Dale W. Henderson, and Andrew T. Levin. 2000. "Optimal Monetary Policy With Staggered Wage and Price Contracts." *Journal of Monetary Economics*, 46(2): 281–313.
- Jordà, Oscar. 2005. "Estimation and Inference of Impulse Responses by Local Projections." *American Economic Review*, 95(1): 161–182.

- Newey, Whitney, and Kenneth West. 1987. "A simple, positive semi-definite, heteroskedasticity and autocorrelation consistent covariance matrix." *Econometrica*, 55 (3): 703–708.
- Olivei, Giovanni, and Silvana Tenreyro. 2007. "The Timing of Monetary Policy Shocks." *American Economic Review*, 97(3): 636–663.
- Olivei, Giovanni, and Silvana Tenreyro. 2010. "Wage-setting patterns and monetary policy: International evidence." Journal of Monetary Economics, 57(7): 785–802.
- Ramey, Valerie. 2011. "Identifying government spending shocks: It's all in the timing." Quarterly Journal of Economics, 126(1): 1–50.
- Rhee, Hyuk Jae, and Jeongseok Song. 2013. "Real wage rigidities and optimal monetary policy in a small open economy." *Journal of Macroeconomics*, 37: 110– 127.
- Runkle, David. 1987. "Vector autoregressions and reality." Journal of Business and Economic Statistics, 5(4): 437–442.
- Sims, Christopher A. 1980. "Macroeconomics and reality." *Econometrica*, 48(1): 1–48.
- Sims, Christopher A. 1992. "Interpreting the Macroeconomic Time Series Facts: The Effects of Monetary Policy." *European Economic Review*, 36(5): 975–1000.
- Smets, Frank, and Rafael Wouters. 2007. "Shocks and Frictions in US Business Cycles: A Bayesian DSGE Approach." American Economic Review, 97(3): 586–606.

# Earlier Working Papers:

For a complete list of Working Papers published by Sveriges Riksbank, see www.riksbank.se

Estimation of an Adaptive Stock Market Model with Heterogeneous Agents by Henrik Amilon	2005:177
Some Further Evidence on Interest-Rate Smoothing: The Role of Measurement Errors in the Output Gap by Mikael Apel and Per Jansson	2005:178
Bayesian Estimation of an Open Economy DSGE Model with Incomplete Pass-Through by Malin Adolfson, Stefan Laséen, Jesper Lindé and Mattias Villani	2005:179
Are Constant Interest Rate Forecasts Modest Interventions? Evidence from an Estimated Open Economy DSGE Model of the Euro Area by Malin Adolfson, Stefan Laséen, Jesper Lindé and Mattias Villani	2005:180
Inference in Vector Autoregressive Models with an Informative Prior on the Steady State by Mattias Villani	2005:181
Bank Mergers, Competition and Liquidity by Elena Carletti, Philipp Hartmann and Giancarlo Spagnolo	2005:182
Testing Near-Rationality using Detailed Survey Data by Michael F. Bryan and Stefan Palmqvist	2005:183
Exploring Interactions between Real Activity and the Financial Stance by Tor Jacobson, Jesper Lindé and Kasper Roszbach	2005:184
Two-Sided Network Effects, Bank Interchange Fees, and the Allocation of Fixed Costs by Mats A. Bergman	2005:185
Trade Deficits in the Baltic States: How Long Will the Party Last? by Rudolfs Bems and Kristian Jönsson	2005:186
Real Exchange Rate and Consumption Fluctuations follwing Trade Liberalization by Kristian Jönsson	2005:187
Modern Forecasting Models in Action: Improving Macroeconomic Analyses at Central Banks by Malin Adolfson, Michael K. Andersson, Jesper Lindé, Mattias Villani and Anders Vredin	2005:188
Bayesian Inference of General Linear Restrictions on the Cointegration Space by Mattias Villani	2005:189
Forecasting Performance of an Open Economy Dynamic Stochastic General Equilibrium Model by Malin Adolfson, Stefan Laséen, Jesper Lindé and Mattias Villani	2005:190
Forecast Combination and Model Averaging using Predictive Measures by Jana Eklund and Sune Karlsson	2005:191
Swedish Intervention and the Krona Float, 1993-2002 by Owen F. Humpage and Javiera Ragnartz	2006:192
A Simultaneous Model of the Swedish Krona, the US Dollar and the Euro by Hans Lindblad and Peter Sellin	2006:193
Testing Theories of Job Creation: Does Supply Create Its Own Demand? by Mikael Carlsson, Stefan Eriksson and Nils Gottfries	2006:194
Down or Out: Assessing The Welfare Costs of Household Investment Mistakes by Laurent E. Calvet, John Y. Campbell and Paolo Sodini	2006:195
Efficient Bayesian Inference for Multiple Change-Point and Mixture Innovation Models by Paolo Giordani and Robert Kohn	2006:196
Derivation and Estimation of a New Keynesian Phillips Curve in a Small Open Economy by Karolina Holmberg	2006:197
Technology Shocks and the Labour-Input Response: Evidence from Firm-Level Data by Mikael Carlsson and Jon Smedsaas	2006:198
Monetary Policy and Staggered Wage Bargaining when Prices are Sticky by Mikael Carlsson and Andreas Westermark	2006:199
The Swedish External Position and the Krona by Philip R. Lane	2006:200

Price Setting Transactions and the Role of Denominating Currency in FX Markets by Richard Friberg and Fredrik Wilander	2007:201
The geography of asset holdings: Evidence from Sweden by Nicolas Coeurdacier and Philippe Martin	2007:202
Evaluating An Estimated New Keynesian Small Open Economy Model by Malin Adolfson, Stefan Laséen, Jesper Lindé and Mattias Villani	2007:203
The Use of Cash and the Size of the Shadow Economy in Sweden by Gabriela Guibourg and Björn Segendorf	2007:204
Bank supervision Russian style: Evidence of conflicts between micro- and macro-prudential concerns by Sophie Claeys and Koen Schoors	2007:205
Optimal Monetary Policy under Downward Nominal Wage Rigidity by Mikael Carlsson and Andreas Westermark	2007:206
Financial Structure, Managerial Compensation and Monitoring by Vittoria Cerasi and Sonja Daltung	2007:207
Financial Frictions, Investment and Tobin's q by Guido Lorenzoni and Karl Walentin	2007:208
Sticky Information vs Sticky Prices: A Horse Race in a DSGE Framework by Mathias Trabandt	2007:209
Acquisition versus greenfield: The impact of the mode of foreign bank entry on information and bank lending rates by Sophie Claeys and Christa Hainz	2007:210
Nonparametric Regression Density Estimation Using Smoothly Varying Normal Mixtures by Mattias Villani, Robert Kohn and Paolo Giordani	2007:211
The Costs of Paying – Private and Social Costs of Cash and Card by Mats Bergman, Gabriella Guibourg and Björn Segendorf	2007:212
Using a New Open Economy Macroeconomics model to make real nominal exchange rate forecasts by Peter Sellin	2007:213
Introducing Financial Frictions and Unemployment into a Small Open Economy Model by Lawrence J. Christiano, Mathias Trabandt and Karl Walentin	2007:214
Earnings Inequality and the Equity Premium by Karl Walentin	2007:215
Bayesian forecast combination for VAR models by Michael K. Andersson and Sune Karlsson	2007:216
Do Central Banks React to House Prices? by Daria Finocchiaro and Virginia Queijo von Heideken	2007:217
The Riksbank's Forecasting Performance by Michael K. Andersson, Gustav Karlsson and Josef Svensson	2007:218
Macroeconomic Impact on Expected Default Freqency by Per Åsberg and Hovick Shahnazarian	2008:219
Monetary Policy Regimes and the Volatility of Long-Term Interest Rates by Virginia Queijo von Heideken	2008:220
Governing the Governors: A Clinical Study of Central Banks by Lars Frisell, Kasper Roszbach and Giancarlo Spagnolo	2008:221
The Monetary Policy Decision-Making Process and the Term Structure of Interest Rates by Hans Dillén	2008:222
How Important are Financial Frictions in the US and the Euro Area by Virginia Queijo von Heideken	2008:223
Block Kalman filtering for large-scale DSGE models by Ingvar Strid and Karl Walentin	2008:224
Optimal Monetary Policy in an Operational Medium-Sized DSGE Model by Malin Adolfson, Stefan Laséen, Jesper Lindé and Lars E. O. Svensson	2008:225
Firm Default and Aggregate Fluctuations by Tor Jacobson, Rikard Kindell, Jesper Lindé and Kasper Roszbach	2008:226
Re-Evaluating Swedish Membership in EMU: Evidence from an Estimated Model by Ulf Söderström	2008:227

The Effect of Cash Flow on Investment: An Empirical Test of the Balance Sheet Channel by Ola Melander	2009:228
Expectation Driven Business Cycles with Limited Enforcement by Karl Walentin	2009:229
Effects of Organizational Change on Firm Productivity by Christina Håkanson	2009:230
Evaluating Microfoundations for Aggregate Price Rigidities: Evidence from Matched Firm-Level Data on Product Prices and Unit Labor Cost by Mikael Carlsson and Oskar Nordström Skans	2009:231
Monetary Policy Trade-Offs in an Estimated Open-Economy DSGE Model by Malin Adolfson, Stefan Laséen, Jesper Lindé and Lars E. O. Svensson	2009:232
Flexible Modeling of Conditional Distributions Using Smooth Mixtures of Asymmetric Student T Densities by Feng Li, Mattias Villani and Robert Kohn	2009:233
Forecasting Macroeconomic Time Series with Locally Adaptive Signal Extraction by Paolo Giordani and Mattias Villani	2009:234
Evaluating Monetary Policy by Lars E. O. Svensson	2009:235
Risk Premiums and Macroeconomic Dynamics in a Heterogeneous Agent Model by Ferre De Graeve, Maarten Dossche, Marina Emiris, Henri Sneessens and Raf Wouters	2010:236
Picking the Brains of MPC Members by Mikael Apel, Carl Andreas Claussen and Petra Lennartsdotter	2010:237
Involuntary Unemployment and the Business Cycle by Lawrence J. Christiano, Mathias Trabandt and Karl Walentin	2010:238
Housing collateral and the monetary transmission mechanism by Karl Walentin and Peter Sellin	2010:239
The Discursive Dilemma in Monetary Policy by Carl Andreas Claussen and Øistein Røisland	2010:240
Monetary Regime Change and Business Cycles by Vasco Cúrdia and Daria Finocchiaro	2010:241
Bayesian Inference in Structural Second-Price common Value Auctions by Bertil Wegmann and Mattias Villani	2010:242
Equilibrium asset prices and the wealth distribution with inattentive consumers by Daria Finocchiaro	2010:243
Identifying VARs through Heterogeneity: An Application to Bank Runs by Ferre De Graeve and Alexei Karas	2010:244
Modeling Conditional Densities Using Finite Smooth Mixtures by Feng Li, Mattias Villani and Robert Kohn	2010:245
The Output Gap, the Labor Wedge, and the Dynamic Behavior of Hours by Luca Sala, Ulf Söderström and Antonella Trigari	2010:246
Density-Conditional Forecasts in Dynamic Multivariate Models by Michael K. Andersson, Stefan Palmqvist and Daniel F. Waggoner	2010:247
Anticipated Alternative Policy-Rate Paths in Policy Simulations by Stefan Laséen and Lars E. O. Svensson	2010:248
MOSES: Model of Swedish Economic Studies by Gunnar Bårdsen, Ard den Reijer, Patrik Jonasson and Ragnar Nymoen	2011:249
The Effects of Endogenuos Firm Exit on Business Cycle Dynamics and Optimal Fiscal Policy by Lauri Vilmi	2011:250
Parameter Identification in a Estimated New Keynesian Open Economy Model by Malin Adolfson and Jesper Lindé	2011:251
Up for count? Central bank words and financial stress by Marianna Blix Grimaldi	2011:252
Wage Adjustment and Productivity Shocks by Mikael Carlsson, Julián Messina and Oskar Nordström Skans	2011:253

Stylized (Arte) Facts on Sectoral Inflation by Ferre De Graeve and Karl Walentin	2011:254
Hedging Labor Income Risk by Sebastien Betermier, Thomas Jansson, Christine A. Parlour and Johan Walden	2011:255
Taking the Twists into Account: Predicting Firm Bankruptcy Risk with Splines of Financial Ratios by Paolo Giordani, Tor Jacobson, Erik von Schedvin and Mattias Villani	2011:256
Collateralization, Bank Loan Rates and Monitoring: Evidence from a Natural Experiment by Geraldo Cerqueiro, Steven Ongena and Kasper Roszbach	2012:257
On the Non-Exclusivity of Loan Contracts: An Empirical Investigation by Hans Degryse, Vasso Ioannidou and Erik von Schedvin	2012:258
Labor-Market Frictions and Optimal Inflation by Mikael Carlsson and Andreas Westermark	2012:259
Output Gaps and Robust Monetary Policy Rules by Roberto M. Billi	2012:260
The Information Content of Central Bank Minutes by Mikael Apel and Marianna Blix Grimaldi	2012:261
The Cost of Consumer Payments in Sweden	2012:262
by Björn Segendorf and Thomas Jansson	
Trade Credit and the Propagation of Corporate Failure: An Empirical Analysis	2012:263
by Tor Jacobson and Erik von Schedvin	
Structural and Cyclical Forces in the Labor Market During the Great Recession: Cross-Country Evidence	2012:264
by Luca Sala, Ulf Söderström and AntonellaTrigari	
Pension Wealth and Household Savings in Europe: Evidence from SHARELIFE	2013:265
by Rob Alessie, Viola Angelini and Peter van Santen	
Long-Term Relationship Bargaining	2013:266
by Andreas Westermark	
Using Financial Markets To Estimate the Macro Effects of Monetary Policy: An Impact-Identified FAVAR*	2013:267
by Stefan Pitschner	
DYNAMIC MIXTURE-OF-EXPERTS MODELS FOR LONGITUDINAL AND DISCRETE-TIME SURVIVAL DATA	2013:268
by Matias Quiroz and Mattias Villani	
Conditional euro area sovereign default risk	2013:269
by André Lucas, Bernd Schwaab and Xin Zhang	
Nominal GDP Targeting and the Zero Lower Bound: Should We Abandon Inflation Targeting?*	2013:270
by Roberto M. Billi	
Un-truncating VARs*	2013:271
by Ferre De Graeve and Andreas Westermark	
Housing Choices and Labor Income Risk	2013:272
by Thomas Jansson	
Identifying Fiscal Inflation*	2013:273
by Ferre De Graeve and Virginia Queijo von Heideken	
On the Redistributive Effects of Inflation: an International Perspective*	2013:274
by Paola Boel	
Business Cycle Implications of Mortgage Spreads*	2013:275
by Karl Walentin	
Approximate dynamic programming with post-decision states as a solution method for dynamic	2013:276
	0040.077
A detrimental reedback loop: deleveraging and adverse selection	2013:277
Distortionany Ficeral Policy and Manatany Policy Coole	2012:270
by Klaus Adam and Roberto M. Billi	2013.218
by Maus Audin and Roberto W. Dilli Dredicting the Spread of Einancial Innovations: An Enidemiclogical Approach	2012-270
hy Isaiah Hull	2013.219
Firm-Level Evidence of Shifts in the Sunnly of Credit	2012.280
by Karolina Holmberg	2010.200

Lines of Credit and Investment: Firm-Level Evidence of Real Effects of the Financial Crisis by Karolina Holmberg	2013:281
A wake-up call: information contagion and strategic uncertainty by Toni Ahnert and Christoph Bertsch	2013:282
Debt Dynamics and Monetary Policy: A Note	2013:283
by Stefan Laséen and Ingvar Strid	
Optimal taxation with home production	2014:284
by Conny Olovsson	
Incompatible European Partners? Cultural Predispositions and Household Financial Behavior	2014:285
by Michael Haliassos, Thomas Jansson and Yigitcan Karabulut	
How Subprime Borrowers and Mortgage Brokers Shared the Piecial Behavior	2014:286
by Antje Berndt, Burton Hollifield and Patrik Sandås	
The Macro-Financial Implications of House Price-Indexed Mortgage Contracts	2014:287
by Isaiah Hull	
Does Trading Anonymously Enhance Liquidity?	2014:288
by Patrick J. Dennis and Patrik Sandås	
Systematic bailout guarantees and tacit coordination	2014:289
by Christoph Bertsch, Claudio Calcagno and Mark Le Quement	
Selection Effects in Producer-Price Setting	2014:290
by Mikael Carlsson	
Dynamic Demand Adjustment and Exchange Rate Volatility	2014:291
by Vesna Corbo	
Forward Guidance and Long Term Interest Rates: Inspecting the Mechanism	2014:292
by Ferre De Graeve, Pelin Ilbas & Raf Wouters	
Firm-Level Shocks and Labor Adjustments	2014:293
by Mikael Carlsson, Julián Messina and Oskar Nordström Skans	
A wake-up call theory of contagion	2015:294
by Toni Ahnert and Christoph Bertsch	
Risks in macroeconomic fundamentals and excess bond returns predictability	2015:295
by Rafael B. De Rezende	
The Importance of Reallocation for Productivity Growth: Evidence from European and US Banking	2015:296
by Jaap W.B. Bos and Peter C. van Santen	
SPEEDING UP MCMC BY EFFICIENT DATA SUBSAMPLING	2015:297
by Matias Quiroz, Mattias Villani and Robert Kohn	
Amortization Requirements and Household Indebtedness: An Application to Swedish-Style Mortgages	2015:298
by Isaiah Hull	
Fuel for Economic Growth?	2015:299
by Johan Gars and Conny Olovsson	
Searching for Information	2015:300
by Jungsuk Han and Francesco Sangiorgi	
What Broke First? Characterizing Sources of Structural Change Prior to the Great Recession	2015:301
by Isaiah Hull	
Price Level Targeting and Risk Management	2015:302
by Roberto Billi	
Central bank policy paths and market forward rates: A simple model	2015:303
by Ferre De Graeve and Jens Iversen	
Jump-Starting the Euro Area Recovery: Would a Rise in Core Fiscal Spending Help the Periphery?	2015:304
by Olivier Blanchard, Christopher J. Erceg and Jesper Lindé	
Bringing Financial Stability into Monetary Policy*	2015:305
by Eric M. Leeper and James M. Nason	
SCALABLE MCMC FOR LARGE DATA PROBLEMS USING DATA SUBSAMPLING AND THE DIFFERENCE ESTIMATOR	2015:306
by MATIAS QUIROZ, MATTIAS VILLANI AND ROBERT KOHN	

SPEEDING UP MCMC BY DELAYED ACCEPTANCE AND DATA SUBSAMPLING by MATIAS QUIROZ	2015:307
Modeling financial sector joint tail risk in the euro area by André Lucas, Bernd Schwaab and Xin Zhang	2015:308
Score Driven Exponentially Weighted Moving Averages and Value-at-Risk Forecasting	2015:309
by André Lucas and Xin Zhang	
On the Theoretical Efficacy of Quantitative Easing at the Zero Lower Bound	2015:310
by Paola Boel and Christopher J. Waller	
Optimal Inflation with Corporate Taxation and Financial Constraints	2015:311
by Daria Finocchiaro, Giovanni Lombardo, Caterina Mendicino and Philippe Weil	2010.011
Fire Sale Bank Recapitalizations	2015:312
by Christoph Bertsch and Mike Mariathasan	2010.012
Since you're so rich, you must be really smart: Talent and the Finance Wage Premium	2015:313
by Michael Böhm, Daniel Metzger and Per Strömberg	2010.010
Debt. equity and the equity price puzzle	2015:314
by Daria Finocchiaro and Caterina Mendicino	2010.011
Trade Credit: Contract-Level Evidence Contradicts Current Theories	2016:315
by Tore Ellingsen, Tor Jacobson and Frik von Schedvin	2010.010
Double Liability in a Branch Banking System: Historical Evidence from Canada	2016:316
by Anna Grodecka and Antonis Kotidis	2010.010
Subprime Borrowers, Securitization and the Transmission of Business Cycles	2016:317
by Anna Grodecka	2010.011
Real-Time Forecasting for Monetary Policy Analysis: The Case of Sveriges Riksbank	2016:318
by Jens Iversen. Stefan Laséen, Henrik Lundvall and Llf Söderström	2010.010
Fed Liftoff and Subprime Loan Interest Rates: Evidence from the Peer-to-Peer Lending	2016:319
by Christoph Bertsch, Isaiah Hull and Xin Zhang	2010.010
Curbing Shocks to Corporate Liquidity: The Role of Trade Credit	2016:320
by Niklas Amberg Tor Jacobson Frik von Schedvin and Robert Townsend	2010.020
Firms' Strategic Choice of Loan Delinguencies	2016:321
by Paola Morales-Acevedo	2010.021
Fiscal Consolidation Under Imperfect Credibility	2016:322
by Matthieu Lemoine and Jesper Lindé	2010.022
Challenges for Central Banks' Macro Models	2016:323
by Jesper Lindé Frank Smets and Rafael Wouters	2010.020
The interest rate effects of government bond purchases away from the lower bound	2016:324
by Rafael B. De Rezende	2010.024
COVENANT-LIGHT CONTRACTS AND CREDITOR COORDINATION	2016:325
by Bo Becker and Victoria Ivashina	2010.020
Endogenous Separations Wage Rigidities and Employment Volatility	2016:326
by Mikael Carlsson and Andreas Westermark	2010.020
Renovatio Monetae: Gesell Taxes in Practice	2016:327
by Roger Svensson and Andreas Westermark	2010.021
Adjusting for Information Content when Comparing Forecast Performance	2016:328
by Michael K. Andersson. Ted Aranki and André Reslow	2010.020
Economic Scarcity and Consumers' Credit Choice	2016:329
by Marieke Bos. Chloé Le Cog and Peter van Santen	
Uncertain pension income and household saving	2016:330
by Peter van Santen	
Money, Credit and Banking and the Cost of Financial Activity	2016:331
by Paola Boel and Gabriele Camera	2010.001
Oil prices in a real-business-cycle model with precautionary demand for oil	2016:332
by Conny Olovsson	2010.002
Financial Literacy Externalities	2016:333
by Michael Haliasso, Thomas Jansson and Yigitcan Karabulut	2010.000

The timing of uncertainty shocks in a small open economy by Hanna Armelius, Isaiah Hull and Hanna Stenbacka Köhler	2016:334
Quantitative easing and the price-liquidity trade-off by Marien Ferdinandusse, Maximilian Freier and Annukka Ristiniemi	2017:335
What Broker Charges Reveal about Mortgage Credit Risk	2017:336
Asymmetric Macro-Financial Shillovers	2017:337
hy Kristina Bluwstein	2017.337
Latency Arbitrage When Markets Become Faster	2017:338
by Burton Hollifield, Patrik Sandås and Andrew Todd	2017.000
How big is the toolbox of a central banker? Managing expectations with policy-rate forecasts: Evidence from Sweden	2017:339
by Magnus Åhl	
International business cycles: quantifying the effects of a world market for oil	2017:340
by Johan Gars and Conny Olovsson I	
Systemic Risk: A New Trade-Off for Monetary Policy?	2017:341
by Stefan Laséen, Andrea Pescatori and Jarkko Turunen	
Household Debt and Monetary Policy: Revealing the Cash-Flow Channel	2017:342
by Martin Flodén, Matilda Kilström, Jósef Sigurdsson and Roine Vestman	
House Prices, Home Equity, and Personal Debt Composition	2017:343
by Jieying Li and Xin Zhang	
Identification and Estimation issues in Exponential Smooth Transition Autoregressive Models	2017:344
by Daniel Buncic	
Domestic and External Sovereign Debt	2017:345
by Paola Di Casola and Spyridon Sichlimiris	
The Role of Trust in Online Lending by Christoph Bertsch, Isaiah Hull, Yingjie Qi and Xin Zhang	2017:346
On the effectiveness of loan-to-value regulation in a multiconstraint framework by Anna Grodecka	2017:347
Shock Propagation and Banking Structure by Mariassunta Giannetti and Farzad Saidi	2017:348
The Granular Origins of House Price Volatility	2017:349
by Isaiah Hull, Conny Olovsson, Karl Walentin and Andreas Westermark	
Should We Use Linearized Models To Calculate Fiscal Multipliers?	2017:350
by Jesper Lindé and Mathias Trabandt	
The impact of monetary policy on household borrowing – a high-frequency IV identification by Maria Sandström	2018:351
Conditional exchange rate pass-through: evidence from Sweden by Vesna Corbo and Paola Di Casola	2018:352
Learning on the Job and the Cost of Business Cycles by Karl Walentin and Andreas Westermark	2018:353
Trade Credit and Pricing: An Empirical Evaluation by Niklas Amberg, Tor Jacobson and Erik von Schedvin	2018:354
A shadow rate without a lower bound constraint by Rafael B. De Rezende and Annukka Ristiniemi	2018:355
Reduced "Border Effects", FTAs and International Trade by Sebastian Franco and Erik Frohm	2018:356
Spread the Word: International Spillovers from Central Bank Communication by Hanna Armelius, Christoph Bertsch, Isaiah Hull and Xin Zhang	2018:357
Predictors of Bank Distress: The 1907 Crisis in Sweden by Anna Grodecka, Seán Kenny and Anders Ögren	2018:358

Diversication Advantages During the Global Financial Crisis by Mats Levander	2018:359
Towards Technology-News-Driven Business Cycles by Paola Di Casola and Spyridon Sichlimiris	2018:360
The Housing Wealth Effect: Quasi-Experimental Evidence by Dany Kessel, Björn Tyrefors and Roine	2018:361
Identification Versus Misspecification in New Keynesian Monetary Policy Models by Malin Adolfson, Stefan Laseén, Jesper Lindé and Marco Ratto	2018:362
The Macroeconomic Effects of Trade Tariffs: Revisiting the Lerner Symmetry Result by Jesper Lindé and Andrea Pescatori	2019:363
Biased Forecasts to Affect Voting Decisions? The Brexit Case by Davide Cipullo and André Reslow	2019:364
The Interaction Between Fiscal and Monetary Policies: Evidence from Sweden by Sebastian Ankargren and Hovick Shahnazarian	2019:365
Designing a Simple Loss Function for Central Banks: Does a Dual Mandate Make Sense? by Davide Debortoli, Jinill Kim and Jesper Lindé	2019:366
Gains from Wage Flexibility and the Zero Lower Bound by Roberto M. Billi and Jordi Galí	2019:367



Sveriges Riksbank Visiting address: Brunkebergs torg 11 Mail address: se-103 37 Stockholm

Website: www.riksbank.se Telephone: +46 8 787 00 00, Fax: +46 8 21 05 31 E-mail: registratorn@riksbank.se