Supplemental Materials for "Monetary Policy and the Labor Market: A Quasi-Experiment in Sweden" by Coglianese, Olsson and Patterson

A.1 Data details

A.1.1 Administrative microdata

For the analysis using administrative microdata, we combine several administrative Swedish datasets. Employers and employees are linked via "Register based labor market statistics" (Registerbaserad arbetsmarknadsstatistik, jobbregisteret, RAMS) (Statistics Sweden, 2018d), which is derived from annual tax filings for each employer-employee pair. We use RAMS to link employees to their main employer in the pre-period, to study changes in annual earnings, and to calculate individual indicators of annual employment. From the administrative registers in "Population Register" (Registret över totalbefolkningen, RTB) (Statistics Sweden, 2018e) and "Longitudinal Integrated Database for Health Insurance and Labor Market Studies" (Longitudinell integrationsdatabas för sjukförsäkrings- och arbetsmarknadsstudier, LISA) (Statistics Sweden, 2018b), we extract information on an individual's background characteristics (gender, age, education, immigration status[1]) as well as the number of days they are registered as unemployed over the calendar year. We use the "Wage and salary structure in the private sector" (Lönestrukturstatistik, privat sektor, SLP) to identify blue and white collar occupations (Statistics Sweden, 2018c). We merge in additional information on private-sector firms (sales, number of full-time equivalent employees, sector, juridical form, assets, and debt-measures) from their balance sheets in the dataset "Structural business statistics" (Företagens ekonomi, FEK) (Statistics Sweden, 2018a).[2] Lastly, we combine these data with information on export values and export destinations from the VAT-based trade data for goods "Foreign trade " (Utrikeshandel med varor, UHV) (Statistics Sweden, 2018f). Trade is reported in annual values by the destination country. For within EU-trade, the domestic firm is obliged to report annual trade above a threshold of approximately 0.2 million euros (the average threshold for 2005-07). For all other destinations, all trade is reported. In the analysis exploring exports in Section IV, we therefore restrict the sample to firms that are larger than the average firm that exports to the EU. All registers contain yearly observations from 1997 through 2016. All of the above data is reported at the level of the domestic firm, rather than the local establishment.

Throughout the analysis using individual-level data in Sections V and VI, we restrict our attention to individuals between the ages of 16-68 and consider only private sector firms with non-negative sales and labor costs. We also exclude firms with fewer than 2 full-time equivalent employees in a year and clean the data for firms with negative debt to asset ratios. In order to link workers to firms, we further restrict our attention to the set of workers that were employed for at least 9 months for each year between

[1] Immigration status do not separate between individuals born outside of Sweden and individuals born in Sweden who have emigrated and later returned to Sweden.

[2] Financial firms are excluded. While it is possible to construct a measure of household consumption before 2007, we are not able to do so after 2007, when Sweden abolished the wealth tax and therefore stopped collecting detailed information on household balance sheets.

2006 and 2009, and we assign workers the characteristics of the firm in which they worked in 2009, the year preceding the monetary shock. Specifically, the firm characteristics correspond to the firm where the worker was observed in 2009, conditioning on (a) that the employer-employee link existed for at least 3 months during the calendar year, (b) the employment spell resulted in earnings at least 1.5 times the minimum wage, and (c) the firm accounted for the most earnings that year given (a) and (b). We follow Hensvik et al. (2023) and define the monthly minimum wage as the 10th percentile in the distribution of monthly wage each year. For several pieces of our analysis, in order to isolate the firms most exposed to domestic monetary policy and assuage concerns that shocks outside of Sweden are driving the patterns, we further restrict to only workers who were attached to domestic, non-exporting firms in 2009. Domestic firms are defined from the juridical form (ownership category). We define non-exporting firms as those who report no positive value for exports. In Section IV, Figure 6, right panel, we include firms that are in the domestic sample for the individual-level analysis.

Supplemental Appendix Table A4 shows basic summary statistics for the various samples. We show that the set of workers that we are able to link to a firm between 2006 and 2009 are more attached to the labor force and therefore, have slightly higher earnings, but otherwise, they look similar to the full sample. Additionally, the set of workers at domestically owned non-exporting firms accounts for 40% of the overall linked sample. These firms are substantially smaller both in terms of sales and employment, but the workers have similar ages, education, and wages to those in the general population.

A.1.2 International data

In Section IV, we explored the results of our main estimating equation in several other countries. The lagged controls in Equation 2 include GDP, the vacancy rate, and the layoff rate. Therefore, in order to implement this analysis in other countries, we also need to collect these controls in other countries for as much of the 1996–2019 sample period as possible.[3] The following section documents how we collected this data for each country.

Norway: We collected quarterly GDP, employment, and unemployment from OECD (2020f; 2020g; 2020c). We collected quarterly vacancy data from OECD (2022b). We proxy for layoffs using the number of workers unemployed for 1–4 weeks downloaded from Statistics Norway (2022).

Germany: We collected quarterly GDP, employment, unemployment, and vacancies from OECD (2020f; 2020g; 2020c; 2020h). We fill in historical data on the job-separation rate (i.e. the layoff rate) using data provided from the replication package of Barnichon and Garda (2016a) covering 1987–2012. Specifically, up through 2012, we use the quarterly job separation rate calculated in their replication dataset (Barnichon and Garda, 2016b) and post-2012, we use the fraction of workers placed on short-time work for economic reasons as a proxy for the layoff rate (Federal Employment Agency, 2021). In the period in which the two time series overlap, the correlation of the four-quarter changes is 0.9.

[3] The only exception is the layoffs series for Finland, for which we were unable to find data before 2004.

France: We collected quarterly GDP and unemployment from OECD (2020f; 2020c). We collect quarterly employment information from Eurostat (2022), which gives total quarterly employment using the national concept of total employment. Monthly data on new job openings are from OECD (2022a). We aggregate from monthly to quarterly data by taking the total new vacancies in all months within the quarter. We proxy for layoffs using quarterly data from 2003–2019 on short-term unemployment, which comes from Eurostat (2020). We fill in historical data on the job-separation rate (i.e. the layoff rate) using data provided from the replication package of Barnichon and Garda (2016a) covering 1987–2012. Specifically, before 2003, the layoff rate we use is the quarterly job separation rate calculated in Barnichon and Garda (2016b) and post-2003, we use the layoff rate calculated using employment and counts of those unemployed for less than 1 month. In the period in which the two time series overlap, the correlation of the four-quarter changes is 0.5.

UK: We collected quarterly GDP, employment, unemployment, and vacancies from OECD (2020f; 2020g; 2020c; 2020h). We collected data on layoffs ("redundencies") from Office for National Statistics (2022), which has monthly data on the counts of redundancies from 1995–2019. We aggregate to the quarterly data taking the sum across quarters.

US: We collected quarterly GDP, employment, unemployment, and vacancies from OECD (2020f; 2020g; 2020c; 2020h). We proxy for layoffs using the number of workers unemployed for less than 1 month. This is produced monthly by U.S. Bureau of Labor Statistics (2022). Data from the OECD for job vacancies is based on JOLTS and covers 2001–2019. We combine this with historical data on job vacancies in the US in the Composite Help-Wanted Index developed by Barnichon (2010), with data accessed from (Barnichon and Garda, 2016b). Specifically, before 2001, we construct the vacancy rate using the Help-wanted index (HWI) and post-2001, we construct it using JOLTS vacancies. In order to convert the HWI to vacancy counts, we re-scale using the number of vacancies observed in JOLTS in the first quarter of 2015.

Finland: We collected quarterly GDP, employment, unemployment, and vacancies from OECD (2020f; 2020g; 2020c; 2020h). We were unable to find historical data on layoffs or short-term unemployment in Finland. Instead, we proxy for layoffs using a measure from Statistics Finland (2021), in which respondents are asked what their personal risk of unemployment is at the moment.

A.1.3 Details of monetary shock construction

Data on the Riksbank's internal forecasts are published on their website (Sveriges Riksbank, 2002–2016). We collected and cleaned the forecasts from 2003–2015. In Equation 1, we define the change in the policy rate (i.e. the dependent variable) as the largest absolute change in the repo rate in the month in which the meeting occurs. This definition captures the intended change in the policy rate from the meeting.

In their original specification, Romer and Romer (2004) include the change since the last FOMC meeting in the forecasts for GDP and inflation as additional regressors. This inclusion captures the response of the

FOMC to both current expectations and surprises since the last meeting. In adapting this methodology to the Swedish context, we exclude these variables from our baseline specification to increase our sample size—not only are we working with a shorter time series, but also occasionally, the Riksbank does not publish the forecast for the full set of variables. Therefore, restricting to consecutive observations for GDP and inflation forecasts proved to be too restrictive.

A.2 Alternative monetary policy shocks

A.2.1 Robustness: Monetary policy shocks measured with Consensus Economics forecasts

Our baseline results use monetary policy shocks estimated with the Romer and Romer (2004) methodology applied to Riksbank forecasts. This approach is standard in the literature, but one downside when applied in this context is that the Riksbank did not start consistently releasing forecasts for the variables used in the Romer and Romer (2004) method until 2002, leaving only a short period to estimate the central bank's policy rule before the 2010 deviation.

To address potential concerns about the short estimation period, we repeat the Romer and Romer (2004) estimation using a longer series of forecasts by market participants. We use the set of forecasts for Sweden compiled by Consensus Economics Inc. (2022), which has collected forecasts of economic variables from a variety of different organizations at a monthly frequency dating back to 1990. Using private forecasts in place of central bank forecasts to estimate monetary policy shocks has precedent in Cloyne and Hürtgen (2016) and Holm et al. (2021), the latter of which also use Consensus Economics forecasts (for Norway).

While the Consensus Economics forecast series has a longer history, it also has a few notable drawbacks. First, Consensus Economics has consistently collected forecasts for GDP and inflation in Sweden since 1990, but has not collected forecasts for the unemployment rate. Second, instead of reporting the forecasts for quarterly growth rates, Consensus Economics reports the forecast for annual growth in the current year and the subsequent year. The annual growth rate averages the forecast for periods later in the year yet to come as well as previous periods in which growth has already been determined, with the weights differing over the course of the year. For these reasons, we treat this exercise as a robustness check rather than our preferred specification.

To estimate the Romer and Romer (2004) shocks, we estimate the following regression

$$\Delta r_m = \alpha + \beta r_m + \sum_{\tau=0}^{1} \gamma_\tau GDP_{m,y(m)+\tau} + \sum_{\tau=0}^{1} \phi_\tau \pi_{m,y(m)+\tau} + u_{m-2} + \epsilon_m \tag{A1}$$

where the unit of observation is the Riksbank policy meeting in month m, r_m is the repo rate (Sveriges Riksbank, 2019), $GDP_{m,y}$ and $\pi_{m,y}$ are the Consensus Economics forecasts for the annual rates of GDP growth and inflation respectively, $y(m)$ is the year containing month m, and u_{m-2} is the two-month lag of the published unemployment rate, sourced from the OECD (2020c). We use the two month lag of the unemployment rate to allow for reporting delays in the data release. We estimate the regression using all Riksbank meetings between October 1993 and April 2010 .

Supplemental Appendix Figure A3 shows the shocks estimated from Consensus Economics forecasts compared to our baseline shocks at both monthly and annual levels. Both sets of shocks show that the 2010–2011 period saw repeated positive deviations, while in all other periods, shocks averaged close to zero. The magnitude of the shocks estimated from Consensus Economics forecasts is a bit smaller than in our baseline, but adds up to a total deviation of about 1–2 p.p. in each of 2010 and 2011.

One concern about our methodology is that the deviations over the pre-2010 period are mean zero by construction, while the deviations in 2010–11 are unconstrained. We address this concern with a placebo exercise. For each meeting date from 2000–2008, we estimate Equation A1 using data up to the meeting date, then calculate the implied monetary policy shocks over the subsequent 24 months. As shown in Supplemental Appendix Figure A5, the out-of-sample deviation over the placebo period was centered around zero on average, but the deviation beginning in 2010Q2 was substantially larger than following any earlier meeting, further evidence supporting monetary policy shocks during this period.

We further repeat the estimation of aggregate labor market effects using the shocks estimated from Consensus Economics forecasts. Following our baseline approach, we set the monetary policy shocks to zero outside of the 2010–2011 period and estimate Equation 3 with the same set of controls and sample period as in our baseline specification. Column (5) of Supplemental Appendix Table A1 shows these estimates compared to our baseline estimates. We find slightly larger responses of unemployment using the alternative series than in our baseline, but this difference is not statistically significant.

A.2.2 Robustness: Monetary policy shocks measured with Taylor-style regressions

An alternative method to identify a deviation from a monetary policy rule is to construct a Taylor-style rule for the central bank using observable economic variables. To do so, we estimate the following Taylor rule

$$r_t = \beta_1 r_{t-1} + \beta_2 r_t^f + \beta_3(\pi_t - \pi^*) + \beta_4 \tilde{y}_t + \epsilon_t \tag{A2}$$

where we construct the foreign interest rate (r_t^f) using the weights for the Riksbank's krona index (KIX)[4], $(\pi_t - \pi^*)$ is the inflation gap, defined as the four-quarter percent change in the CPIF net of 2 percent (π^*), and $\tilde{y}_t$ is the output gap, measured using the Riksbank's estimate of potential output constructed from a production function approach (Sveriges Riksbank, 2002–2016). We impose $\beta_1 = 0.85$, matching the value used in the FRB/US inertial Taylor rule (Brayton et al., 2014), and impose $\beta_2 = 0.15$, and estimate β_3 and β_4 using data from 1996–2010Q2. In this exercise, we ask what the policy rate would have been had the Riksbank followed their pre-2010 rule. Therefore, we use our estimated coefficients to construct a dynamic projection starting in 2010Q2 in which we use the lagged projected policy rate ($\hat{r}_{t-1}$) as the input to the current quarter projection. We impose the zero lower bound as a constraint on the predicted policy rate. Supplemental Appendix Figure A4 shows the resulting projected time series for the interest rate in orange and the actual path of the interest rate in blue. This exercise reveal a sizable gap of about 1 p.p. between

[4]We construct the foreign interest rate by multiplying the weights for the Riksbank's Krona index (Sveriges Riksbank, 2023) by the policy rate for each country (OECD, 2020e).

the actual and projected policy rates during 2010–11, similar to the gap implied by the Romer and Romer (2004) shocks (shown by the dashed green line).

Despite the broad similarity of the shocks, we prefer our baseline approach of using the Romer and Romer (2004) method to estimate the monetary shock for two reasons. First, our baseline specification also allows for Riksbank policy to depend on resource utilization through both the output and unemployment margins, rather than just output. Second, the Romer and Romer (2004) method includes central bank forecasts for output, inflation, and unemployment, in addition to contemporaneous and lagged values for each, which accounts for central bank actions that are taken in anticipation of future movements in economic conditions. In our baseline results (shown in Supplemental Appendix Table A5), we estimate coefficients for the leads and lags of inflation that are larger in absolute value than the coefficient on the contemporaneous term, suggesting that the Riksbank is responding to dynamics in inflation rather than just the current value. Importantly, we see that the Taylor rule exercise shown in Supplemental Appendix Figure A4 shows a gap between actual and counterfactual policy rates over 2012–14, while the results following Romer and Romer (2004) show less divergence in this period (shown in Figure 1). During this period, the Riksbank was consistently forecasting inflation to rise between the current quarter and two quarters in the future, which is captured in the Romer and Romer (2004) method but not in the Taylor rule exercise. This highlights the benefits of identifying the monetary deviations while taking forecasts into account.

A.2.3 Robustness: Monetary policy shocks constructed from professional forecast errors

For an additional measure of monetary shocks, we use the unexpected portion of the change in the policy rate. As in Section II.D, we use policy rate expectations collected by TNS Sifo Prospera from private-sector forecasters (TNS SIFO Prospera, 2019). We define the monetary shock as the difference between the actual and forecasted three-month changes in the policy rate (these series are plotted in Supplemental Appendix Figure A1). As in our baseline specification, we interact this monetary shock with an indicator for 2010–11 and estimate the effect on unemployment using Equation 3. In order for the coefficients to be interpretable as the effect of a 1 p.p. unexpected increase in the policy rate, we rescale by dividing the coefficients by the estimated effect of the shock on the policy rate over two quarters.

Using unexpected changes in the policy rate complements our baseline evidence, which uses a different source of variation. As we discuss in Section III, our baseline monetary shock series measures deviations relative to the Riksbank's historical policy rule. Our baseline identification assumption does not require that these deviations were unexpected, only that they were exogenous to labor market developments. We use the narrative record to argue that the Riksbank deviations during 2010–11 were unrelated to labor market development. Using the unexpected change in the policy rate is an alternative approach to exogeneity, relying on the idea that these shocks must be exogenous since they were unanticipated.

Column (6) of Supplemental Appendix Table A1 shows the estimated effect of the unexpected change in the policy rate on unemployment. We find a sizable, positive effect on unemployment twelve quarters after the shock, which is statistically indistinguishable from our baseline estimate.

A.3 Additional results

A.3.1 Robustness: Forecast error methodology for estimating effects of monetary policy shocks

Romer and Romer (1989) provide a key example of using monetary policy episodes as case studies to estimate its effects. Their approach estimated the effect of monetary policy in three steps. First, they estimated an autoregression specification for unemployment to capture typical cyclical dynamics. Second, using these estimates, they iteratively forecast forward a counterfactual path for the unemployment rate following the date of a monetary policy shock. The final step is calculating the forecast error—the difference between the actual unemployment rate and the forecasted counterfactual path—which they interpret as the effect of the monetary policy shock on unemployment.

We conduct an analysis in this spirit, which we refer to as the "forecast error method" for estimating the effects of monetary policy. We estimate a series of local projections regressions:

$$y_{t+k} - y_{t-1} = X'_{t-1}\alpha_k + \varepsilon_{t,k} \tag{A3}$$

where y_t is the unemployment rate, X'_{t-1} includes the same business cycle controls as in our baseline regressions (1st, 5th, 9th and 13th quarter lags of year-over-year changes in GDP, vacancies, and layoffs) and $\varepsilon_{t,k}$ is the forecast error. Compared to Romer and Romer (1989), using local projections allows the forecast errors to be estimated directly instead of through an iterative forward forecast and also is robust to non-invertible IRFs for the cyclical controls.

Supplemental Appendix Figure A7 shows the estimated forecast errors for the quarters leading up to and following 2010Q3. We estimate only small deviations from the usual cyclical dynamics in the quarters before the shock, but the forecast errors jump over the course of 2011 and 2012, reaching a peak of about 1.5 p.p. before gradually easing. These results mirror our baseline event study estimates.

A.3.2 Placebo exercise for event study design

Additionally, to probe potential biases in the event study research design, we implemented a placebo exercise using alternative event dates in Equation 2. Specifically, for each of 100 placebo regressions, we estimated Equation 2 replacing the indicator for 2010Q3 with an indicator for a quarter sampled randomly with replacement from the 1996Q1–2019Q2 period, excluding dates between 2007Q4–2012Q4 as those overlap with our episode of interest. Each regression was estimated on the same sample as in our baseline event study regression and used the same set of control variables. The resulting estimates, which are shown in Supplemental Appendix Figure A6, are symmetrically centered around 0, indicating no systematic upward or downward bias in the event study design.

A.3.3 Aggregate labor market outcomes using individual-level specification

To complement the estimates from public use data, we also estimate a version of this research design using annual administrative microdata. To that end, we construct an annual dataset as described in Section A.1.1.

On this sample, we run the following regression, which is a slight modification of Equation 3:

$$y_{i,t+k} - y_{i,t-1} = \beta_k \widehat{RR}_t + X'_{t-1}\alpha_k + Z'_i\gamma_k + \epsilon_{i,t,k} \tag{A4}$$

where i indexes individuals, time t is measured in years (instead of quarters, as in our baseline), $y_{i,t}$ is either the fraction of the year that the individual spends registered with the unemployment agency or the fraction of the year that the worker spends employed, $\widehat{RR}_t$ are the estimated monetary shocks aggregated across quarters within the year, X'_{t-1} are the same aggregate controls as in the baseline, and Z_i are individual-level demographic controls to capture trends in labor market outcomes by demographics.

The results for unemployment are shown in panel (a) of Supplemental Appendix Figure A11. We see that these results look similar, although smaller in magnitude, to those using the aggregate unemployment rate in Figure 4: a 1 p.p. increase in the interest rate caused a 1.5 p.p. increase in the fraction of the year that workers are unemployed 3 years after the shock. These patterns are echoed in panel (b) of Supplemental Appendix Figure A11, where we show the effect on the fraction of the year that workers spend employed, and panel (c), where we show the employment intensity of those who remain employed for at least some part over the year. We find that 3 years after the monetary shock, workers on average spent approximately 4 p.p. less of the year employed. As with the estimates using the aggregate unemployment rate, the large effects on the unemployment rate 2 and 3 years after the shock are robust to various data decisions that define these baseline estimates, such as including the self-employed, the exporting firms, or using individual fixed effects (see Supplemental Appendix Table A7).

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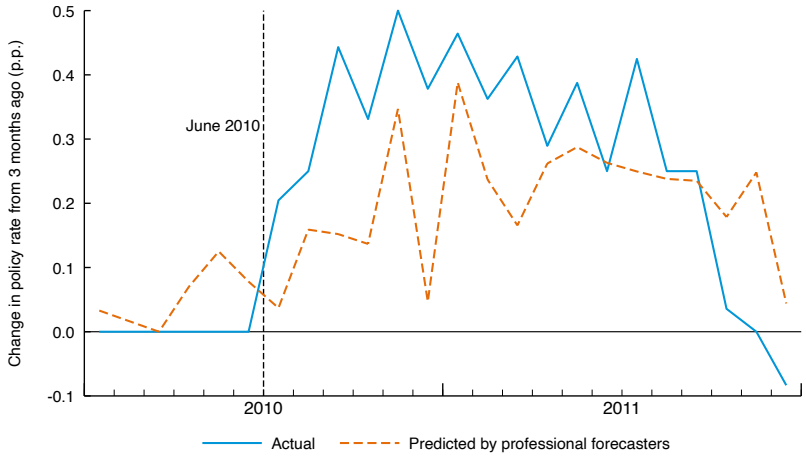
Commissioned by Sveriges Riksbank. Data downloaded from https://www.kantarsifo.se/. Accessed Dec 15, 2019. https://www.origogroup.com/riksbanken/.

U.S. Bureau of Labor Statistics, "Number unemployed for less than 5 weeks [UEMPLT5] [dataset]," FRED, Federal Reserve Bank of St. [distributor] 2022.
Accessed December 7, 2022. https://fred.stlouisfed.org/series/UEMPLT5.

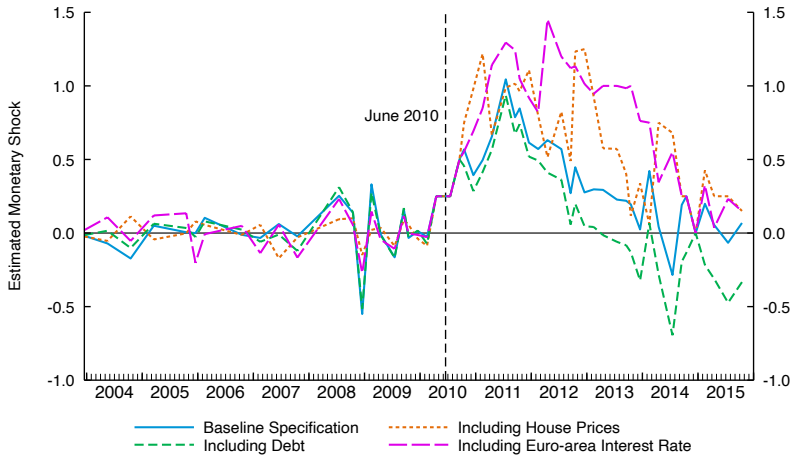
A.4 Appendix Tables and Figures

Supplemental Appendix Figure A1: Professional forecaster expectations for the policy rate



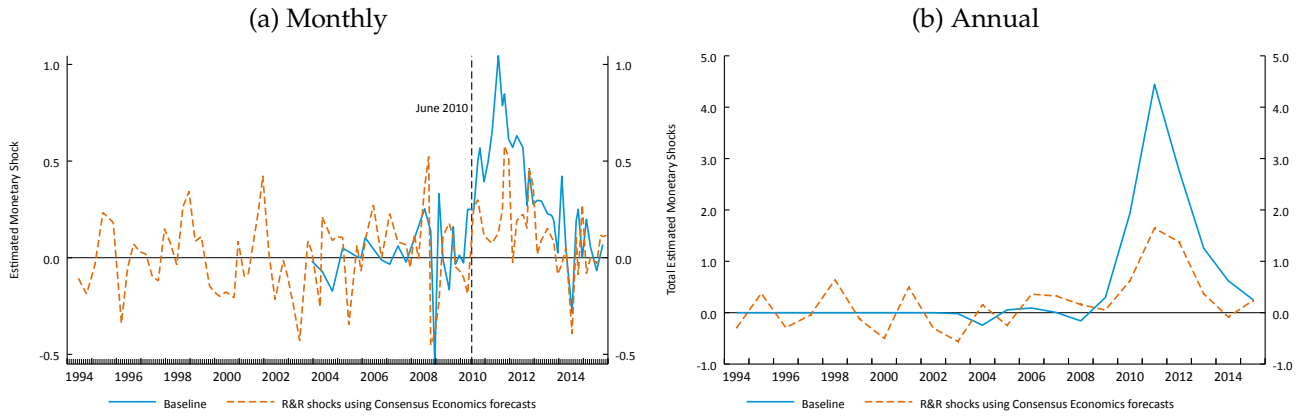
Notes: The blue solid line plots the actual three-month change in the policy rate and the orange dashed line plots the expected three-month change from TNS SIFO Prospera (2019). The black dotted line indicates June 2010, when the Riksbank first raised the repo rate. The units on the y-axis are percentage points.

Supplemental Appendix Figure A2: Alternate monetary policy shocks: adding additional controls



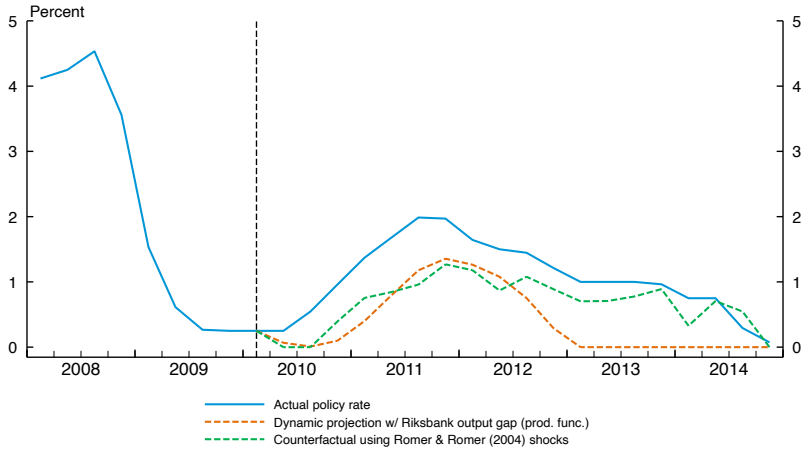
Notes: The blue solid line shows the residuals from Equation 1 estimated on data before April 2010. The orange dotted line shows the residuals from Equation 1 including the change in the house price index in the previous quarter. The green dashed line shows the residuals from Equation 1 including debt per capita. The purple long-dashed line shows the residuals from Equation 1 including the average euro-area 3-month interbank interest rate. The black dashed vertical line marks June 2010, when the Riksbank first increased the repo rate.

Supplemental Appendix Figure A3: Monetary policy shocks using Consensus Economics forecasts



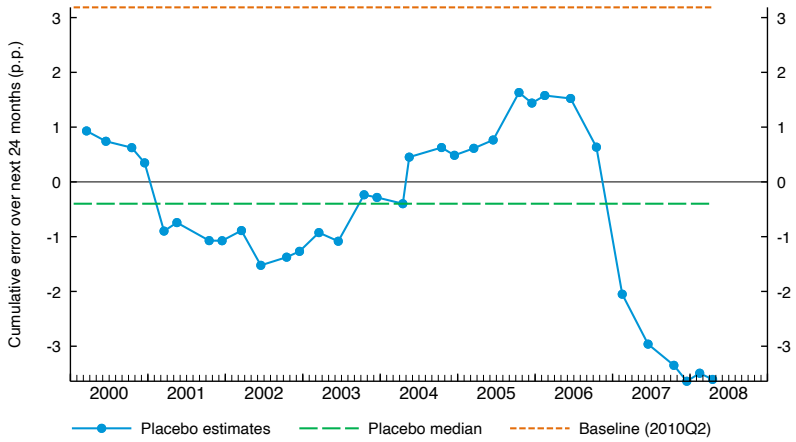
Notes: The left panel shows monetary policy shocks in the months of Riksbank meetings, and the right panel shows the total shock aggregated to the yearly level. In both panels, the blue solid lines show our baseline estimates described in Equation 1, and the orange lines show the alternative shock series estimated from Consensus Economics forecasts. The alternative series is computed as the residuals from Equation A1, with the coefficients estimated from Consensus Economics forecasts from October 1993 to April 2010, and residuals are calculated for the 1993–2015 period using these estimated coefficients. Data source: Consensus Economics, Inc., Consensus Forecasts Subscription, http://www.consensuseconomics.com/.

Supplemental Appendix Figure A4: Robustness: Policy rate counterfactuals



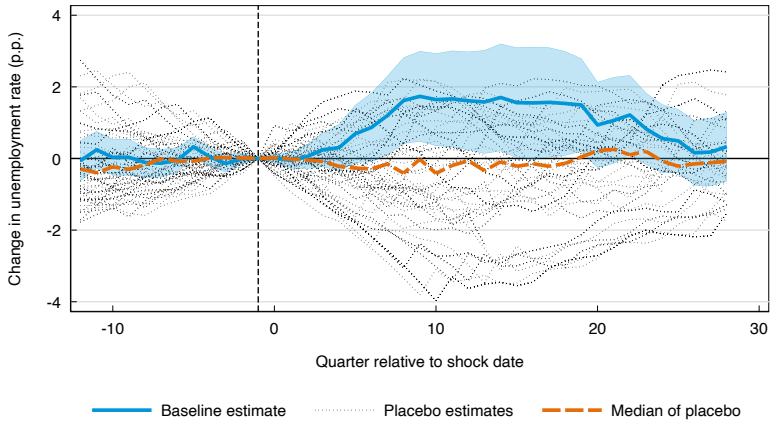
Notes: The solid blue line shows the actual repo rate. The orange dashed line shows the counterfactual policy rate predicted using the Taylor-rule in Equation A2, where the regression is estimated on data from 1996-2010Q2. The dashed orange line shows the counterfactual policy rate implied by the Romer & Romer (2004) shocks estimated by Equation 1. This counterfactual is calculated by assuming that at each meeting from 2010Q2 onwards, the Riksbank would have changed the policy rate from the actual level by the amount predicted from our estimates of Equation 1, i.e. $\widehat{\Delta r_{m_t}}$.

Supplemental Appendix Figure A5: Implied monetary policy shocks in other periods



Notes: The graph shows estimates from a sequence of placebo regressions using Consensus Economics forecasts. For each Riksbank meeting date, we estimate Equation A1 using data up to the meeting date, use the estimated coefficients to construct residuals over the full data period, and then calculate the cumulative residuals over the 24 months following the meeting. The same quantity for our baseline estimates of Equation A1 are shown by the orange dashed line. Data source: Consensus Economics, Inc., Consensus Forecasts Subscription, http://www.consensuseconomics.com/.

Supplemental Appendix Figure A6: Event study placebo

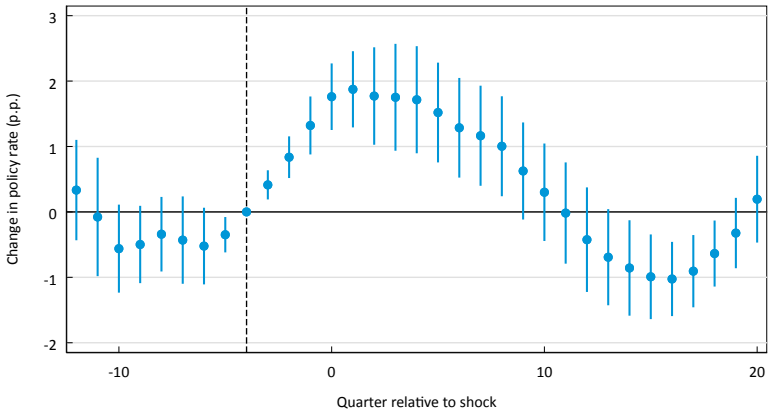


Notes: This plot shows coefficients estimated from the set of local projections regressions described by Equation 2, using placebo dates for the event study shock. 100 placebo dates were randomly drawn with replacement from the 1996Q2–2019Q2 period, excluding dates between 2007Q4 and 2012Q4. Controls include the 1, 5, 9, and 13th quarter lags of year-over-year percent change in GDP, as well as the year-over-year percentage point changes in the vacancy rate and layoff rate. The sample includes quarterly data from 1996Q2 to 2019Q2. The dashed orange line indicates the median of the 100 placebo draws. The thick blue line and blue shaded area show the point estimates and the 95% confidence interval with heteroskedasticity-robust standard errors, respectively, from our baseline event study specification.

Supplemental Appendix Figure A7: Robustness: Forecast error specification

Notes: The blue solid line shows the forecast errors around 2010Q3 estimated from Equation A3, described in Section A.3.1. Controls include the 1, 5, 9, and 13th quarter lags of year-over-year percent change in GDP, as well as the year-over-year percentage point changes in the vacancy rate and layoff rate. The sample includes quarterly data from 1996Q1 to 2019Q2.

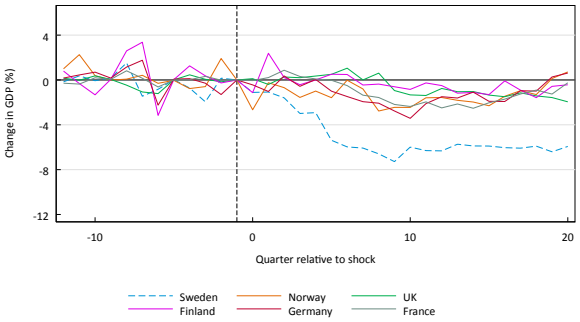
Supplemental Appendix Figure A8: Impulse Response of Policy Rate



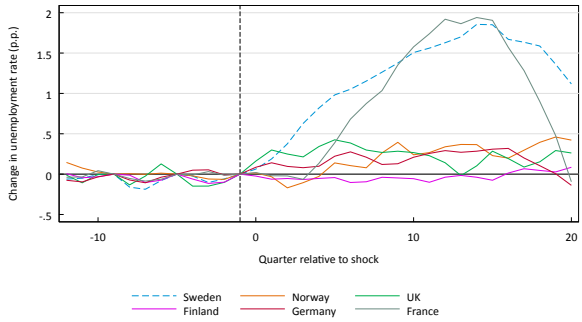
Notes: The blue dots show the estimated response of the policy rate (Sveriges Riksbank, 2020c) to the identified Romer & Romer (2004) shocks using Equation 3. In this specification, we normalize relative to four quarters before the shock to account for serial correlation of the shocks (the average shock falls in the middle of the 2010–11 tightening cycle). Controls include the 1, 5, 9, and 13th quarter lags of year-over-year percent change in GDP, as well as the year-over-year percentage point changes in the vacancy rate and layoff rate. The sample includes quarterly data from 1996Q1 to 2019Q2.

Supplemental Appendix Figure A9: Robustness of international placebo estimates

(a) Effects on GDP



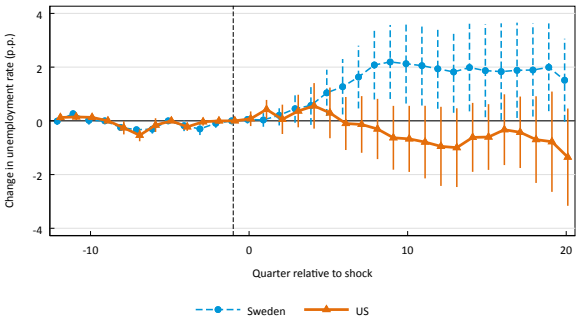
(b) Romer and Romer Refinement



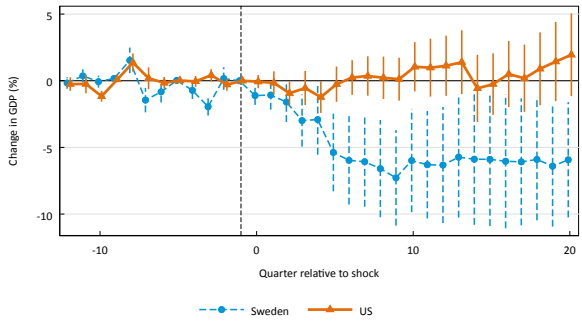
Notes: The left panel shows the coefficients estimated from the set of local projections regressions described by Equation 2, using GDP as the dependent variable. Controls include the 1, 5, 9, and 13th quarter lags of year-over-year percent change in GDP, as well as the year-over-year percentage point changes in the vacancy rate, unemployment rate, and layoff rate. The right panel shows coefficients estimated from the set of local projections regressions described by Equation 3, using unemployment as the dependent variable. Controls include the 1, 5, 9, and 13th quarter lags of year-over-year percent change in GDP, as well as the year-over-year percentage point changes in the vacancy rate, unemployment rate, and layoff rate. See Section A.1.2 for details on data construction for each country.

Supplemental Appendix Figure A10: International placebo estimates: United States

(a) Unemployment



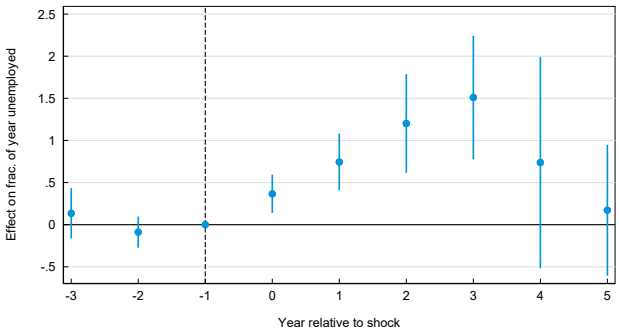
(b) GDP



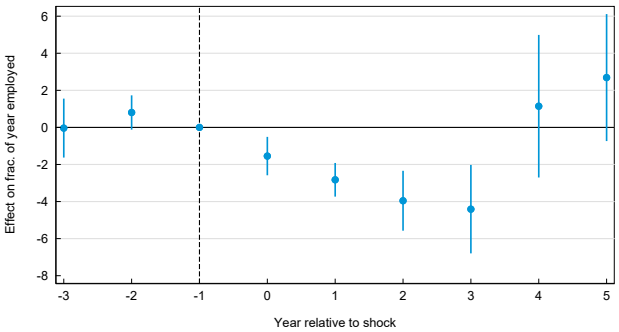
Notes: These plots show the coefficients estimated from the set of local projections regressions described by Equation 2, using data from the United States and using the unemployment rate as the dependent variable in the left panel and GDP as the dependent variable in the right panel. In both panels, controls include the 1, 5, 9, and 13th quarter lags of year-over-year percent change in GDP, as well as the year-over-year percentage point changes in the vacancy rate, unemployment rate, and layoff rate. See Section A.1.2 for the details on the data sources for the United States data. Bars illustrate the 95% confidence interval with heteroskedasticity-robust standard errors.

Supplemental Appendix Figure A11: Effect of monetary policy on individual-level outcomes

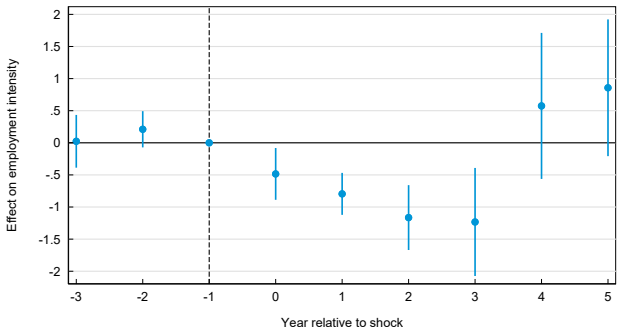
(a) Fraction of year unemployed



(b) Fraction of year employed



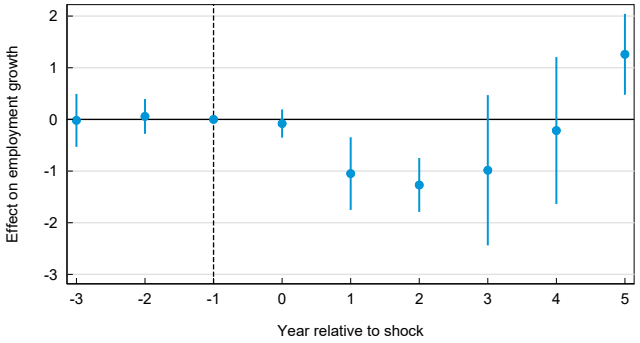
(c) Employment intensity among employed



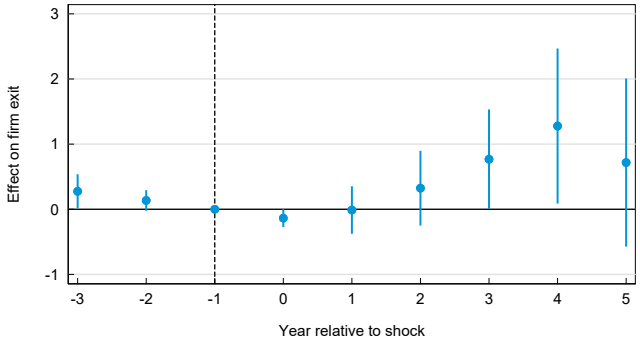
Notes: The left panel shows the effect of the monetary tightening on the fraction of the year that initial employees spend in unemployment, the right panel the effect on the fraction of the year employed, and the lower panel shows the fraction of the year employed for workers who are employed at least one month of the year. For all panels, the sample includes individuals initially employed at domestic non-exporting firms. All regressions include controls for the year-over-year percent change in GDP and the year-over-year percentage point change in both the vacancy and the layoff rate, as well as the annual lag of each of these variables. At the individual level, regressions include controls for 10-year age bins, gender, a native/foreign born dummy, and dummies for 4 education levels. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the individual and year level.

Supplemental Appendix Figure A12: Effect of monetary contraction on firm-level outcomes

(a) Employment Growth

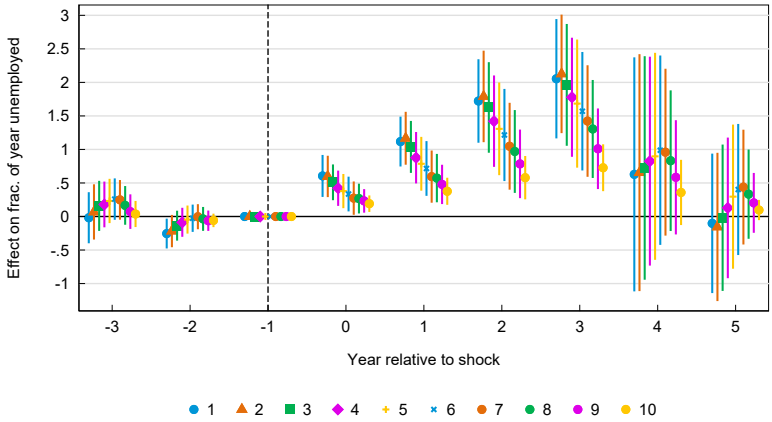


(b) Firm Exit



Notes: The left panel shows the effect of the monetary tightening on the change in the log number of full-time employees at the firm and the right panel shows the effect on the probability of firm exit in year t. In both panels, the sample includes all firms. All regressions include controls for the year-over-year percent change in GDP and the year-over-year percentage point change in both the vacancy and the layoff rate, as well as the annual lag of each of these variables. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the firm and year level.

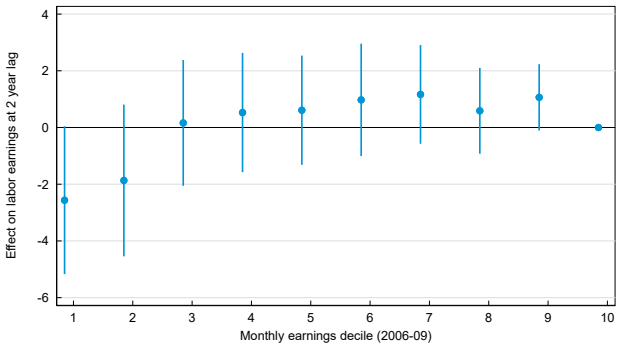
Supplemental Appendix Figure A13: Unemployment across the earnings distribution



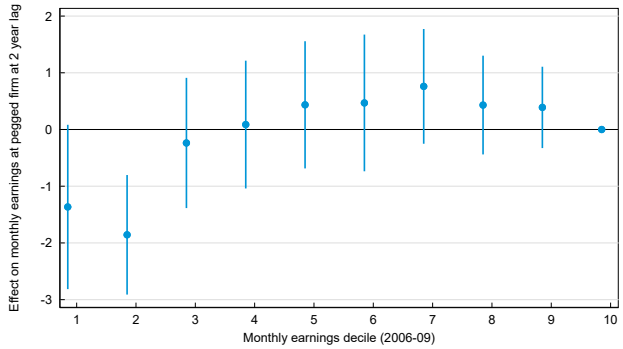
Notes: Regressions are as in Equation 5 estimated separately for each decile of the earnings distribution, with blue circles representing workers in the lowest decile. Workers are pegged to their mode earnings position for 2006–2009. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the individual and year level.

Supplemental Appendix Figure A14: Earnings across the distribution

(a) Labor earnings

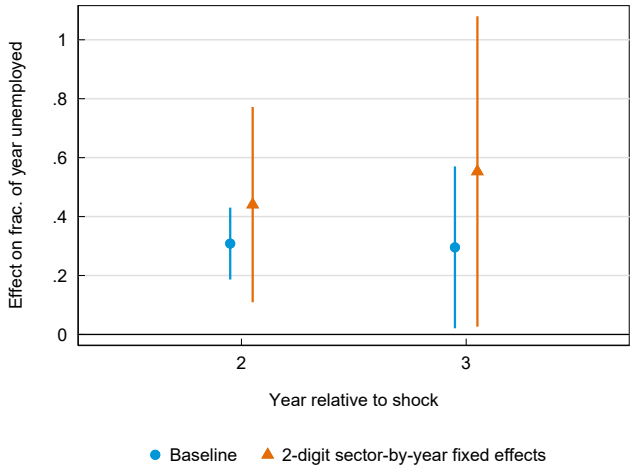


(b) Monthly earnings at initial firm



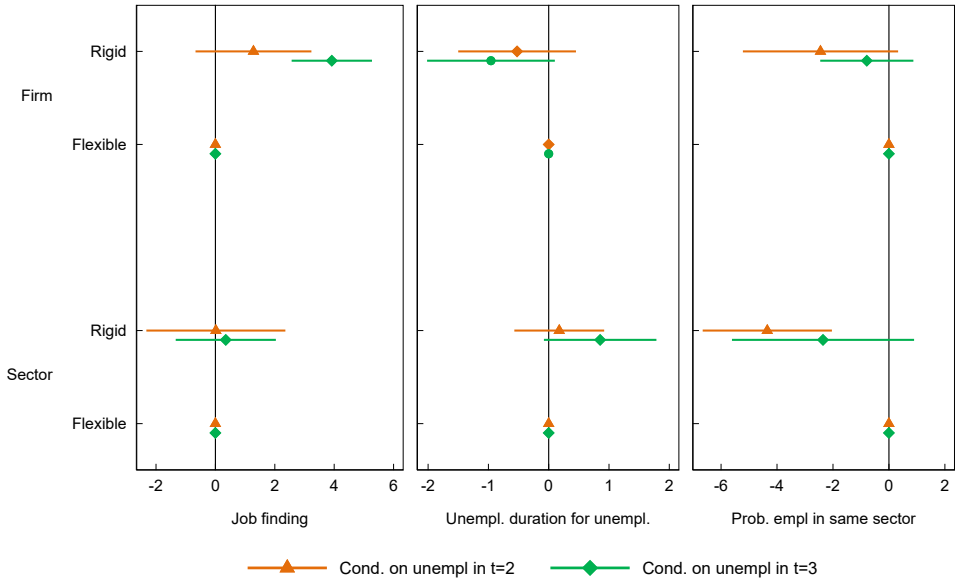
Notes: Regressions are as in Equation 5 estimated separately for each decile of the earnings distribution, with 1 representing workers in the lowest decile. Workers are pegged to their mode earnings position for 2006–2009. Coefficients plotted relative to the highest decile at the 2-year lag. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the individual and year level.

Supplemental Appendix Figure A15: Unemployment by the rigidity of labor market contract: within 2-digit sectors



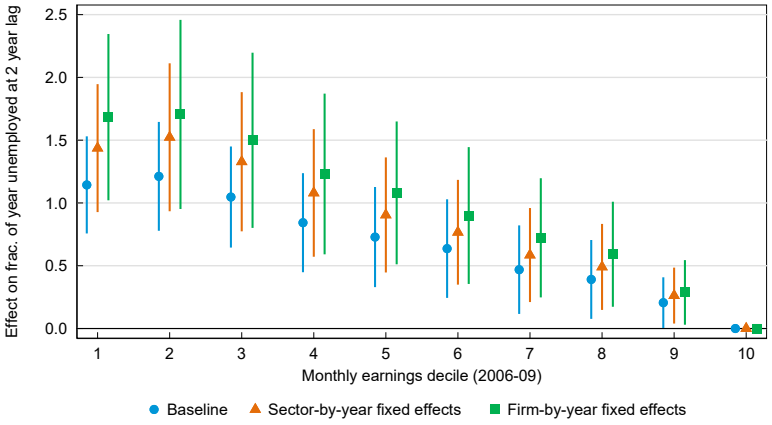
Notes: The coefficient plots the difference in the effect on the fraction of the year unemployed for workers with rigid, relative to flexible contracts. Baseline estimates refer to the conditional estimates shown in Figure 8. Regressions shown in orange triangles include 2-digit sector-by-year fixed effects as well as all the controls in the baseline conditional estimates in Figure 8. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the individual and year level.

Supplemental Appendix Figure A16: Contracts and labor market congestion effects



Notes: Regressions are conditional, including individual contracts, as in Figure 8 but with job-finding (left), unemployment duration (middle), and job-finding in the same sector conditional on job-finding (right), as the outcome variable. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the individual and year level.

Supplemental Appendix Figure A17: Incidence across the earnings distribution



Notes: Estimates display the employment response to monetary tightening for individuals at different positions in the earnings distribution, relative to the highest income bracket. Circles are the baseline unconditional estimates, using aggregate and demographic controls (Equation 5). Triangles show the estimates after adding sector-by-year fixed effects, and square markers firm-by-year fixed effects. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the individual and year level.

Supplemental Appendix Table A1: Robustness of local projections estimates to alternative shocks

	(1)	(2)	(3)	(4)	(5)	(6)
β_{12}						
R&R shock, 2010-11 only	2.287	2.301				
	(0.694)	(0.690)				
R&R shock, excl. 2010-11	-0.447					
	(0.654)					
R&R shock, full sample			1.513			
			(0.475)			
R&R shock using Consensus, 2010-11 only				3.123		
				(0.755)		
Chg. in policy rate, 2010-11 only					2.583	
					(0.598)	
Market forecast error, 2010-11 only						1.334
						(1.335)

Notes: This table shows estimates of Equation 3 for horizon $k = 12$, varying the shock variable. Column (1) reports the baseline estimates using the Romer and Romer (2004) shocks shown in Figure 1 from 2010–11, controlling for the shocks in all other periods. Column (2) omits the control for shocks outside of 2010–11. Column (3) reports estimates using the full Romer and Romer (2004) shocks shown in Figure 1. Column (4) uses shocks estimated using Consensus Economics Inc. (2022) as described in Section A.2.1. Column (5) uses the quarterly change in the policy rate for 2010–11, zeroed out in all other periods (Sveriges Riksbank, 2020c). Column (6) uses the three-month ahead forecast error for the policy rate based on surveys of professional forecasters conducted by TNS SIFO Prospera (2019). All specifications use the same controls and time period as in the baseline specification. Heteroskedasticity-robust standard errors are reported in parentheses for all specifications.

Supplemental Appendix Table A2: Robustness of local projections estimates to alternative controls and standard errors

	β_{12}	
1. Baseline	2.287	(0.694)
2. Newey-West standard errors (lag = 3)	2.287	(0.813)
3. Baseline controls + HPI	2.784	(0.946)
4. Baseline controls + debt per capita	1.721	(0.478)
5. Baseline controls + CPIF inflation	2.106	(0.735)
6. Baseline controls + unemployment rate	2.608	(0.757)
7. Baseline controls + exports	2.374	(0.738)
8. Baseline controls + avg. euro area 3-mth interbank rate	1.701	(0.685)
9. Baseline controls + trade-weighted foreign GDP growth	2.279	(0.637)
10. Baseline controls + Euro-area unemployment rate	2.383	(0.795)
11. Baseline controls + Euro-area unemployment rate (contemporaneous)	1.931	(0.626)
12. Baseline controls + trade-wgt. foreign GDP growth (contemporaneous)	2.292	(0.702)

Notes: This table shows estimates of Equation 3 for horizon $k = 12$ under alternative specifications. Row (1) reports the baseline estimates. Row (2) uses Newey-West standard errors with a bandwidth of 3. Rows (3)–(10) each add one additional lagged control to the baseline business cycle variables. All specifications are estimated using quarterly data from 1996Q1 to 2019Q2. The following variables are drawn from the OECD (2020d; 2020a; 2020c; 2020b): year-over-year percent changes in house prices (Row 3); the four-quarter percentage point change in the CPIF rate (core CPI net of the direct effect of interest rate expenses; Row 5); the four-quarter percentage point change in the unemployment rate (Row 6); the year-over-year percent change in exports (Row 7); and the lagged year-over-year percentage point change in the average euro-area unemployment rate (Row 10). Row (11) includes the same euro-area unemployment rate variable as Row (10), but contemporaneously instead of lagged. Row (4) adds year-over-year changes in debt per capita. Row (8) adds the euro-area three-month interbank rate from OECD (2022c). Rows (9) and (12) include variables constructed using data from the bilateral World Trade Flows database (Feenstra, 2020) and OECD (2020f). Row (9) adds the year-over-year percent change in average GDP of Sweden's trading partners, weighted by exports from Sweden. Row(12) includes trade-weighted foreign GDP growth defined contemporaneous in each local projection. All specifications use the baseline Romer and Romer (2004) shocks and the same time period. Heteroskedasticity-robust standard errors are reported in parentheses for all specifications, except for row (2), which uses Newey-West standard errors.

Supplemental Appendix Table A3: Event study: Effects on interest rates

	(1) Policy rate	(2) Overnight interbank rate	(3) 3-mth. Treasury yield	(4) Avg. consumer loan rate	(5) 2-yr. mortgage bond yield	(6) Avg. new home loan rate
$\mathbf{1}(t = 2010Q3)$	0.701	0.853	0.922	0.869	1.418	0.502
	(0.315)	(0.336)	(0.343)	(0.406)	(0.353)	(0.285)
N	148	148	148	148	148	148

Notes: This table shows estimates of Equation 2 for horizon $k = 2$ for several measures of interest rates. The outcome variables are: the policy rate (Sveriges Riksbank, 2020c) (Column 1), the overnight interbank rate (Sveriges Riksbank, 2020d) (Column 2), the three-month Treasury yield (Sveriges Riksbank, 2020b) (Column 3), the average consumer loan rate (Sveriges Riksbank, 2020e) (Column 4), the two-year mortgage bond yield (Sveriges Riksbank, 2020a) (Column 5), and the average new home loan rate (Sveriges Riksbank, 2020e) (Column 6). All specifications include controls for the 1, 5, 9, and 13th quarter lags of year-over-year percent change in GDP, as well as the year-over-year percentage point changes in the vacancy rate and layoff rate. The sample includes quarterly data from 1996Q1 to 2019Q2. Heteroskedasticity-robust standard errors are reported in parentheses for all specifications.

Supplemental Appendix Table A4: Summary statistics for microdata samples

	A. All workers		**B. Sample with Firm Link**		**C. Domestic Non-Exporting Sample**	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Worker Characteristics						
Age	40.31	13.98	42.39	11.76	42.30	11.97
Female	0.49	0.50	0.29	0.45	0.31	0.46
Education	2.48	1.12	2.41	1.02	2.33	1.01
Immigrant	0.17	0.37	0.12	0.33	0.11	0.31
Firm Characteristics						
Export/Sales			0.12	0.81	0.00	0.00
Firm First Observed			1998.83	3.24	1999.68	3.80
Nr. of Employees			1319.04	3183.32	167.00	596.96
Sales Value (Mil. SEK)			4297.86	13680.45	381.36	2095.81
Labor Market Outcomes						
Frac. of Year in Unemp.	0.04	0.13	0.02	0.09	0.02	0.10
Unemp. ≥ 91 days	0.06	0.23	0.03	0.17	0.03	0.18
Frac. of Year Employed	0.65	0.46	0.89	0.30	0.87	0.31
Empl. ≥ 6 months	0.66	0.47	0.90	0.30	0.89	0.32
log Earnings	10.25	4.32	12.06	2.34	11.97	2.34
Observations	108,058,396		29,456,829		11,846,555	

Notes: Panel A includes all workers with labor earnings at any point between 1997–2016. Panel B includes all workers that were employed in the sample between 2006–2009 for each year. Panel C includes the sample in panel B but further restricts to those workers at a domestically owned and non-exporting in 2009. The sample includes all years from 1997–2016. Education is recorded on a four-point scale as 1 for individuals with less than high school, 2 for vocational high school, 3 for academic high school and shorter tertiary education, and 4 for higher education.

Supplemental Appendix Table A5: Regressions for estimating Romer and Romer (2004) monetary policy shocks

	Baseline	House prices	Debt	Euro interest rate
r_m	0.023	-0.109	0.044	-0.627
	(0.377)	(0.193)	(0.394)	(0.409)
$\pi_{m,i}$	0.245	0.389	0.221	0.481
	(0.425)	(0.217)	(0.444)	(0.349)
$\pi_{m,i+1}$	-0.365	0.484	-0.346	-0.520
	(0.457)	(0.295)	(0.477)	(0.365)
$\pi_{m,i+2}$	0.386	-0.146	0.362	0.327
	(0.337)	(0.206)	(0.353)	(0.266)
$\pi_{t,i-1}$	-0.361	-0.599***	-0.394	-0.325
	(0.269)	(0.145)	(0.284)	(0.212)
$u_{m,i}$	-0.768	-1.113	-0.725	0.775
	(1.755)	(0.891)	(1.830)	(1.532)
$u_{m,i+1}$	0.581	1.556	0.943	-0.148
	(2.271)	(1.167)	(2.428)	(1.811)
$u_{m,i+2}$	0.065	-0.750	-0.308	-0.127
	(1.181)	(0.623)	(1.351)	(0.932)
$u_{m,i-1}$	-0.020	-0.148	-0.102	-0.699
	(0.834)	(0.423)	(0.878)	(0.718)
$GDP_{m,i}$	0.085	-0.214*	0.079	0.041
	(0.124)	(0.090)	(0.130)	(0.099)
$GDP_{m,i+1}$	-0.031	-0.031	-0.063	0.185
	(0.315)	(0.159)	(0.331)	(0.264)
$GDP_{m,i+2}$	0.038	0.412	0.066	-0.167
	(0.394)	(0.215)	(0.412)	(0.322)
$GDP_{m,i-1}$	-0.013	-0.109	-0.006	-0.066
	(0.104)	(0.057)	(0.109)	(0.085)
Change in house prices		-0.244***		
		(0.053)		
Debt per capita			0.018	
			(0.026)	
Euro-area interest rate				0.553*
				(0.239)
Constant	0.556	2.897**	0.074	1.014
	(2.034)	(1.146)	(2.238)	(1.610)
Observations	21	21	21	21
R-Squared	0.880	0.974	0.888	0.937

Notes: This table shows estimates from Equation 1 estimated on data before April 2010. The first column shows estimates from our baseline specification, including controls for lags and forecasts of GDP, unemployment, and inflation following Romer and Romer (2004). The second column additionally controls for the change in the house price index in the previous quarter. The third column adds to the baseline specification a control for debt per capita. The fourth column adds to the baseline specification a control for the average euro-area 3-month interbank interest rate. The sample for all columns consists of Riksbank meetings from March 2002 to February 2010.

Supplemental Appendix Table A6: Estimated Shock Series and Economic Outlook

Meeting	RR	policy	CPI				GDP				Unemployment			
			t-1	t	t+1	t+2	t-1	t	t+1	t+2	t-1	t	t+1	t+2
2003m12	-0.02	2.75	1.70	1.53	0.53	1.05	1.39	1.68	1.74	2.00	4.70	5.09	5.12	4.80
2004m5	-0.07	2.00	0.14	0.47	0.54	0.64	2.80	2.90	2.90	3.00	5.61	5.56	5.43	5.38
2004m10	-0.17	2.00	0.62	0.85	0.83	1.21	3.55	3.43	3.09	2.95	5.40	5.36	5.26	5.13
2005m3	0.05	2.00	0.48	0.16	-0.11	0.09	2.65	2.78	2.92	3.19	5.30	5.30	5.20	5.10
2005m10	0.01	1.50	0.50	0.80	1.27	1.53	2.22	2.78	3.18	3.40	6.20	5.53	5.28	5.06
2005m12	-0.00	1.50	0.50	0.67	1.03	1.43	2.34	3.00	3.41	3.70	6.04	5.61	5.33	5.06
2006m2	0.10	1.75	0.73	0.60	1.07	1.20	3.96	3.88	3.84	3.68	5.70	5.32	5.05	4.85
2006m10	-0.01	2.50	1.60	1.50	1.90	1.60	3.64	3.20	3.04	2.88	5.16	5.11	5.37	5.48
2007m2	-0.03	3.00	1.53	1.77	1.17	1.40	4.80	4.19	3.70	3.39	5.01	5.01	5.12	5.14
2007m6	0.06	3.25	1.95	1.74	2.00	2.60	3.35	3.35	3.18	3.29	4.85	4.65	4.59	4.57
2007m10	-0.02	3.75	1.94	2.66	2.88	2.92	3.42	3.19	3.17	2.81	4.45	4.29	4.27	4.27
2008m7	0.25	4.25	3.84	4.46	4.16	4.40	2.09	1.83	1.40	1.29	5.82	5.86	6.09	6.27
2008m10	0.14	4.75	4.28	3.33	3.07	2.16	0.75	0.22	-0.01	0.07	6.18	6.42	6.57	6.81
2008m12	-0.55	3.75	4.28	2.81	2.10	1.34	0.25	-0.49	-0.75	-0.71	6.18	6.50	6.83	7.18
2009m2	0.33	2.00	2.45	0.47	-0.50	-1.34	-1.13	-1.71	-1.85	-1.72	6.66	7.30	7.78	8.13
2009m4	-0.01	1.00	0.80	-0.38	-1.21	-0.46	-5.45	-5.28	-4.58	-2.12	7.44	8.35	9.11	9.75
2009m7	-0.16	0.50	-0.34	-0.91	-0.28	1.16	-6.60	-6.22	-1.20	0.01	8.37	9.24	10.13	10.57
2009m9	0.16	0.25	-0.36	-1.09	-0.53	0.75	-6.25	-5.65	-0.35	0.94	8.16	9.05	9.86	10.38
2009m10	-0.03	0.25	-1.10	-0.74	0.34	0.67	-5.09	0.32	1.79	2.21	8.83	9.47	9.96	10.30
2009m12	0.01	0.25	-1.10	-0.52	0.45	0.71	-5.16	0.31	1.82	2.25	8.88	9.38	9.74	10.05
2010m2	-0.03	0.25	-0.41	1.24	1.35	1.56	0.09	1.57	2.00	2.58	9.09	9.32	9.41	9.46
2010m4	0.25	0.25	1.01	1.02	1.05	1.39	0.23	1.15	2.21	3.72	9.03	9.03	8.98	8.93
2010m7	0.25	0.25	1.05	1.17	1.49	1.75	3.18	3.76	4.26	3.70	8.92	8.88	8.78	8.65
2010m9	0.50	0.50	1.04	1.05	1.30	1.49	3.62	4.24	4.60	3.89	8.61	8.39	8.18	8.05
2010m10	0.57	0.75	1.13	1.53	1.57	1.71	5.23	5.66	5.03	3.84	8.35	8.10	7.83	7.61
2010m12	0.39	1.00	1.13	1.82	2.24	2.33	6.83	7.05	6.28	4.91	8.30	8.03	7.75	7.51
2011m2	0.50	1.25	1.88	2.51	2.70	2.73	7.02	6.25	4.88	3.44	7.89	7.59	7.35	7.18
2011m4	0.66	1.50	2.61	3.36	3.61	3.35	6.61	5.12	3.66	3.09	7.67	7.34	7.12	6.97
2011m7	1.04	1.75	3.33	3.45	3.18	2.76	5.03	3.65	2.68	2.54	7.52	7.23	6.99	6.87
2011m9	0.79	2.00	3.25	3.27	2.83	2.22	5.33	3.79	2.57	2.25	7.52	7.43	7.38	7.32
2011m10	0.85	2.00	3.32	2.89	2.07	1.76	3.33	2.46	2.08	1.67	7.32	7.28	7.27	7.25
2011m12	0.61	2.00	3.32	2.67	1.92	1.49	4.57	3.39	2.79	1.94	7.34	7.42	7.48	7.51
2012m2	0.57	1.75	2.67	2.01	1.30	1.09	2.90	2.20	1.26	-0.06	7.47	7.55	7.65	7.76
2012m4	0.63	1.50	1.76	1.21	0.96	1.04	1.21	0.51	-0.02	1.46	7.55	7.68	7.79	7.86
2012m7	0.57	1.50	1.10	0.68	0.82	1.06	0.64	0.14	1.47	1.13	7.57	7.67	7.74	7.79
2012m9	0.27	1.50	1.12	0.86	0.97	1.06	2.18	1.17	2.45	2.03	7.56	7.64	7.70	7.75
2012m10	0.45	1.25	0.62	0.21	0.27	0.41	0.46	1.83	1.64	1.39	7.74	7.89	7.97	7.96
2012m12	0.28	1.25	0.62	0.01	-0.11	-0.12	0.66	1.61	1.26	0.92	7.73	7.89	8.06	8.14
2013m2	0.30	1.00	0.07	0.07	0.04	0.32	1.62	1.31	0.93	1.01	7.84	8.03	8.12	8.19
2013m4	0.29	1.00	-0.06	-0.08	0.08	0.54	1.47	1.07	1.35	1.85	8.10	8.17	8.19	8.15
2013m7	0.23	1.00	-0.27	0.00	0.60	0.90	1.16	1.44	1.94	2.15	8.20	8.23	8.25	8.24
2013m9	0.22	1.00	-0.28	0.12	0.60	0.89	0.60	0.94	1.53	1.81	8.02	8.02	8.02	8.01
2013m10	0.19	1.00	0.09	0.36	0.60	0.99	0.38	1.18	1.64	2.74	7.91	7.90	7.88	7.83
2013m12	0.02	1.00	0.09	-0.00	-0.03	0.44	0.27	1.19	1.36	2.41	7.92	7.93	7.91	7.83
2014m2	0.42	0.75	0.07	0.02	0.42	0.50	1.05	1.31	2.28	3.05	7.95	7.89	7.85	7.74
2014m4	0.04	0.75	-0.23	0.06	0.16	0.90	2.46	3.14	3.40	2.44	8.04	8.00	7.89	7.72
2014m7	-0.28	0.75	-0.04	-0.30	0.22	0.83	2.42	2.85	2.07	2.98	8.01	7.94	7.81	7.63
2014m9	0.19	0.25	0.01	-0.03	0.36	0.93	1.91	2.22	1.39	2.29	7.95	7.81	7.73	7.59
2014m10	0.25	0.25	-0.16	-0.25	0.16	0.10	2.31	1.66	2.17	2.16	7.81	7.73	7.63	7.52
2014m12	0.00	0.00	-0.16	-0.27	-0.07	-0.09	2.05	1.46	1.85	2.13	7.83	7.85	7.76	7.60
2015m2	0.20	0.00	-0.21	-0.19	-0.33	-0.04	1.51	1.90	2.20	2.67	7.81	7.76	7.68	7.55
2015m4	0.05	-0.25	0.01	-0.02	0.21	1.19	2.71	2.90	3.22	2.87	7.77	7.70	7.56	7.43
2015m7	-0.07	-0.25	-0.19	0.04	0.87	1.54	2.51	2.75	2.73	3.25	7.73	7.66	7.54	7.44
2015m10	0.07	-0.35	-0.06	0.37	1.01	1.22	3.39	2.83	2.91	2.49	7.19	7.17	7.16	7.12

Notes: Table display estimated monetary policy shocks, policy rate and current economic outlook across monetary policy meetings. All variables are from the Riksbank's monetary policy report for each meeting.

Supplemental Appendix Table A7: Robustness of estimates to specification within microdata

	(1) Baseline	(2) + Self Empl.	(3) + Exporters	(4) Ind. fe	(5) Driscoll-Kraay SEs
2-year	1.202	1.184	1.129	1.193	1.202
	(0.299)	(0.294)	(0.254)	(0.300)	(0.00651)
Observations	10,013,930	10,666,671	24,905,371	10,013,930	10,013,930
3-year	1.510	1.482	1.367	1.503	1.510
	(0.374)	(0.369)	(0.340)	(0.376)	(0.00819)
Observations	9,406,303	10,018,361	23,400,183	9,406,293	9,406,303

Notes: All estimates show the effect on the fraction of year unemployed 2 years after the shock (row 1) and 3 years after the shock (row 2). Regression controls and specifications are as described in the main text for the individual level. Column (1) to (3) show alternative samples. Column (1) is the baseline, column (2) adds to the sample workers linked to a firm with fewer than 2 full-time equivalent workers (i.e., the self-employed), column (3) is the baseline sample plus exporting firms. Column (4) adds individual fixed effects to the baseline specification. All columns uses the Romer & Romer shocks for 2010–2011. Estimates in the first and second row are from separate regressions for each outcome and year. Column (1) to (4), two-way clustered standard errors at the individual and year level in parentheses. Column (5) has Driscoll and Kraay (1998) (3 lags) standard errors for the baseline estimate in column (1).

Supplemental Appendix Table A8: Relative earnings growth of job-stayers by labor market contract

	A. Sector contract		**B. Firm contract**	
	(1) Unconditional	(2) Conditional	(3) Unconditional	(4) Conditional
$\beta_k^{rigid} - \beta_k^{flexible}$				
2-year	0.822	0.541	0.950	0.887
	(0.161)	(0.173)	(0.361)	(0.344)
Observations	4,286,811	4,286,811	1,118,253	1,118,253
3-year	1.238	0.822	1.934	1.437
	(0.246)	(0.184)	(0.319)	(0.395)
Observations	3,789,346	3,789,346	994,671	994,671

Notes: Regressions are as in Equation 5, showing the relative effect on the monthly earnings growth of job-stayers in rigid relative to flexible sectors (columns 1–2) and firms (columns 3–4). Row 1 shows the effect 2 years after the shock and row 2 shows the effect 3 years after the shock. Sectors are defined using 3-digit NACE codes and rigidity is calculated as the average worker rigidity within that sector, where rigidity is coded as 1 if the union contract stipulates an individually guaranteed growth rate of nominal wages and 0 for all others. Similarly, firm rigidity is the average rigidity of the workers within the firm, and firms are grouped into two bins based on their average rigidity. Columns (2) and (4) control for worker age, education, tenure within firm, firm size, firm age, and firm debt. All regressions include aggregate and demographic controls. Employees with zero earnings at the initial firm are coded as missing. Two-way clustered standard errors at the individual and year levels are in parentheses.

Supplemental Appendix Table A9: Heterogeneity final sample

A. Workers						
Age	(2)	(3)	(4)	(5)	(6)	(7)
Range	20 24	25 34	35 44	45 54	55 64	65 68
Obs.	110884	1963041	3418851	3392967	2640603	320209
Nr. of ind.	7547	103204	172427	170665	134594	19898
Education	(1)	(2)	(3)	(4)		
Range	(< High S.)	(Voc.)	(Shorter Ac.)	(Higher)		
Obs.	2313159	5199282	1977663	2356451		
Nr. of ind.	119741	266434	101458	120702		
Tenure	(1)	(2)	(3)	(4)	(5)	
Range	1 3	4 5	6 8	9 12	13 13	
Obs.	3412821	1917642	2110968	2049466	2355658	
Nr. of ind.	175412	99605	108704	104239	120375	
B. Firms						
Size (Nr. of FTE)	(Few)	(Small)	(Middle)	(Large)		
Range	2 9	10 49	50 498	501 25832		
Obs.	4342000	4217651	2510251	776653		
Nr. of firms	87108	18699	1986	59		
First obs. (age)	(Young)	(Middle)	(Old)			
Range	2005 2009	1998 2004	1997 1997			
Obs.	1913400	3209730	6723425			
Nr. of firms	25619	30615	51618			
Debt	(1)	(2)	(3)	(4)	(5)	
Range	0.719 > 1	0.548 0.719	0.405 0.548	0.259 0.405	0.000 0.259	
Obs.	2369688	2370342	2369329	2364998	2372198	
Nr. of firms	17180	17137	20986	24309	28240	
C. Contracts						
	(C1. Rigid)	(C1. Flexible)	(C2. Rigid)	(C2. Flexible)	(C3. Rigid)	(C3. Flexible)
Obs.	1318514	1327257	1036116	1609655	4457151	6654288
Nr. of ind.	67775	67696				
Nr. of firms			3923	3884	43265	57978

D. Distribution										
Earnings Dist. (mode)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Obs.	975803	1045837	1036575	1068791	1121465	1178777	1258912	1345666	1394243	1420486
Nr. of ind.	51111	54469	53506	54957	57542	60391	64386	68691	70959	72323

Notes: The table displays the different categories and quintiles of heteroegeneity. Observations refer to individual and year. Worker age and education refer to the 2010 age and highest completed education level, defined as less than high school, vocational high school, shorter tertiary education or academic high school, or higher education. In A. Tenure and panel B., bins are defined in 2009 and the range refers to the 2009 value. Debt is defined as the short-term debt to asset ratio. In panel C., contract type is defined as the mode type in the pre-period and workers take on the characteristic of their pre-period pegged firm. In panel D., the position in the earnings distribution refers to the mode monthly earnings position in 2006–2009, using the final sample of workers (i.e., initially employed workers at domestic and non-exporting firms). Each column represents a decile of the earnings distribution.

Supplemental Appendix Table A10: Characteristics of sectors by rigidity of labor market contract

	Rigid Sectors		Flexible Sectors	
	mean	sd	mean	sd
A.				
Age	41.232	10.927	42.568	10.594
Education (4 bins)	2.040	0.829	2.363	1.010
Female share	0.312	0.463	0.241	0.428
Immigrant share	0.101	0.302	0.089	0.285
Frac. of year unempl.	0.011	0.061	0.008	0.054
log Real earnings	12.254	0.694	12.392	0.714
Firm age (3 bins)	2.713	0.518	2.702	0.520
Goods vs services	0.402	0.490	0.265	0.441
Manufacture vs other	0.191	0.393	0.025	0.158
Debt (short)	0.493	0.390	0.446	2.974
Debt (long)	0.133	0.279	0.190	0.297
Labor share	0.225	0.147	0.297	0.154
Nr. of employees	247.513	923.428	88.315	264.959
Observations	1,013,911		1,139,173	
B.				
Share of workers on rigid contracts	0.997		0.360	
Observations	4,457,151		6,654,288	

Notes: Sectors are defined using 3-digit NACE codes and rigidity is calculated as the average worker rigidity within that sector, where rigidity is coded as 1 if the union contract stipulate an individually guaranteed growth rate of nominal wages, and 0 for all others. Goods are defined as NACE codes 100–439, and services 450–829 and 940–969. Manufacturing is code 100–339. Labor share above one is normalized to unity. Earnings are at the pegged firm. For all columns, we begin with the sample including the set of workers with a firm link in 2009 who are in non-exporting domestically-owned firms (i.e., the baseline sample). In panel A, we further restrict to the set of workers in their pegged firm at some point in 2000–2007 and report the average values for worker-level variables for those workers from 2000–2007. Firm-level characteristics refer to the value in 2009. Panel B shows the share of workers on rigid contracts within the sector, with observations corresponding to the data used in Section V.