Monetary Policy and the Labor Market: A Quasi-Experiment in Sweden *

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Abstract

We analyze a monetary quasi-experiment in Sweden from 2010–2011, when the Riksbank raised the interest rate substantially. We argue that this increase was unrelated to labor market conditions, driven instead by new concerns at the Riksbank about financial stability. Using a battery of specifications that rule out domestic or international confounders, we show that this monetary tightening led to a substantial economic contraction, raising unemployment by 1–2 percentage points. Using administrative micro data, we find that nominal wage rigidity drove much of the unemployment response and that the monetary contraction was more regressive than the typical business cycle.

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JEL Classification: E24, E52, E58

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1 Introduction

One of the main objectives of monetary policy is to stabilize fluctuations in the labor market. In order to achieve this goal, it is necessary for policymakers to understand the extent to which their actions affect the labor market. However, estimating the effects of monetary policy from changes in interest rates is complicated by endogeneity—policymakers typically change interest rates in response to changes in economic conditions. Additionally, as central banks' capacity to monitor economic conditions has improved over time, unexpected deviations of interest rates have become small and infrequent (Ramey, 2016). Indeed, despite the extensive literature on the topic over the last several decades aimed at addressing these concerns, Nakamura and Steinsson (2018) report that many prominent economists still say that the most compelling evidence for monetary non-neutrality comes from historical case studies, such as the Great Depression of the 1930s, the Volker disinflation in the early 1980s, or the breakdown of Bretton Woods in 1973 (Friedman and Schwartz, 1963; Mussa, 1986).

In this paper, we build on this tradition and introduce a case study of a large monetary policy shock in a modern economy. Our study centers on Sweden, where the central bank (the Riksbank) decided to raise interest rates by nearly 2 percentage points in 2010–2011, despite having below-target inflation and above-average unemployment. Following the narrative approach of Romer and Romer (1989), we argue that this tightening was unrelated to labor market conditions, instead deriving from new concerns at the Riksbank about financial stability (see also Svensson, 2011; 2018; 2019, for a discussion of this episode). We show that unemployment rose following the monetary tightening, and argue that the monetary episode led to this economic deterioration since it cannot be explained by alternative domestic or international shocks. We then use microdata to show that nominal wage rigidity was a key driver of the unemployment response, lending support to a wide class of models that feature this friction. Lastly, we show that this monetary-policy-induced contraction was meaningfully more regressive than a typical business cycle, suggesting that the central bank generally faces a tradeoff between stabilizing employment for some groups while destabilizing employment for others.

The episode we study in this paper is the liftoff of interest rates in Sweden during 2010–11. The Riksbank traditionally exercises monetary policy through flexible inflation targeting, which aims to first stabilize inflation and then the real economy. However, starting in 2010, the Riksbank embarked on a path of monetary tightening, despite being early in the recovery from the Great Recession. In particular, the

Riksbank raised the repo rate, the interest rate governing banks' short-term borrowing and deposits with the central bank, from 0.25% in June 2010 to 2.00% in August 2011.

We argue that this monetary tightening was unrelated to labor market conditions. Drawing on central bank statements, speeches by Riksbank board members, and narrative accounts of the time, we lay out the case that this tightening was instead driven by shifting views within the Riksbank about the merits of using monetary policy to address concerns about household indebtedness. Moreover, we show that this tightening was inconsistent with the historical policy rule of the central bank, as estimated from their prior policy actions and contemporaneous forecasts using the Romer and Romer (2004) method. Although the tightening was communicated somewhat in advance, we show that market participants were still surprised by the speed of the increase in interest rates during this period.

The monetary episode we study is larger and more persistent than typical monetary shocks found in the literature. Monetary policy shocks typically account for only a small portion of the variation in interest rates (Ramey, 2016). However, we estimate Romer and Romer (2004) shocks for this period in Sweden that have an average absolute magnitude of 0.58 percentage point (p.p.) with an autocorrelation of 0.55. In comparison, Romer and Romer (2004) shocks estimated in the US have an average absolute magnitude of 0.11 p.p. and autocorrelation of -0.02. Such shocks are generally underpowered to detect effects on lower frequency outcomes such as output or unemployment (Cochrane and Piazzesi, 2002; Angrist et al., 2018; Nakamura and Steinsson, 2018). Shocks that are large and persistent are relatively better suited for estimating the effects of monetary policy on lower-frequency outcomes like unemployment, which is an important advantage of our setting.

This monetary tightening is also unique because it occurred in a weak economy. While this is not representative of the typical tightening explored in the empirical or theoretical literature, it provides estimates that are particularly relevant for policymakers. Central bankers around the world have faced the decision of when to "liftoff" interest rates from the zero lower bound after recessions in recent decades, which has prompted academic study of optimal monetary policy under these conditions (Eggertsson and Woodford, 2003; Werning, 2011). In 2010–2011, Sweden made the decision to begin to tighten when the economy was still early in the recovery, providing us the opportunity to understand the effects of tightening under these conditions.

We find that this large monetary tightening led to a substantial contraction in economic activity. Our baseline research design uses local projections regressions with Romer and Romer (2004) deviations and controls for lagged business cycle variables. We estimate that unemployment was initially unaffected by the tightening, then rose steadily over the course of two years, eventually reaching a peak increase of about 1–2 p.p. between two and three years after the contraction. This is a large effect, at or above the estimates for a typical monetary shock found in prior studies (Romer and Romer, 2004; Coibion, 2012; Ramey, 2016). We obtain similar estimates from a simple event study specification, and from estimating the forecast errors for unemployment in this period as in Romer and Romer (1989). These results are robust across alternative data sources (including administrative records), alternative definitions of the monetary shock (including those based on private sector forecasts), and alternative controls (including house prices and household debt among others). Beyond unemployment, we estimate that output and investment fell, rates of inflation were lower, and the exchange rate appreciated, all consistent with predictions of a monetary shock in a standard New Keynesian model.

The key identification assumption supporting a causal interpretation of these estimates, meaning that the monetary tightening *caused* the contraction of economic activity, is that there were not other shocks that could have created this contraction in the absence of the monetary tightening. We show several alternative specifications that support this assumption. First, we demonstrate that this pattern is unique to Sweden and unlikely to be driven by contemporaneous international developments such as the Euro crisis. Specifically, we show that international developments throughout Europe did not drive the effects through dampened demand for Swedish exports, as the fall in employment was largest in domestically-owned, non-exporting firms. We also apply our baseline methodology to other advanced economies during this period and estimate fairly precise zero effects for the comparison countries of Norway, Germany, Finland, UK, France, and Iceland. Since our methodology shows an increase in unemployment (net of lagged business cycle controls) for Sweden, and not for any of these other countries, we conclude that we are capturing a shock specific to Sweden.

Second, after having shown that international shocks are unlikely drivers of the estimated effects, we argue that this large monetary tightening was the primary domestic development through this period—we find no significant change in fiscal policy during this period and do not find evidence that housing market developments, independent of the monetary shock, drive our effects. Instead, we find that firms with the most direct exposure to interest rates through their balance sheets saw the largest falls in employment, a pattern consistent with this being a monetary shock rather than a concurrent deleveraging or housing shock. Together, this supplemental analysis supports the causal interpretation of our baseline estimates.

Having established that the monetary tightening caused a large rise in unemployment, we then use administrative microdata to test the extent to which this was due to nominal wage rigidities, which are a key feature of a wide class of monetary models. We exploit an institutional feature of the Swedish labor market to measure nominal wage rigidities following Olsson (2020), namely that certain union contracts specify a path for nominal wages of both new hires and incumbent workers. Using variation across 3-digit sectors in the average degree of rigidity, we find that more rigid sectors see larger increases in unemployment. This effect is especially pronounced among highly-indebted firms, indicating that wage rigidity may interact with other aspects of firms' exposure to monetary policy (Schoefer, 2021). Our estimates imply that wage rigidity of this sort accounts for approximately half of the overall rise in unemployment.

In the final section, we unpack this aggregate effect and examine heterogeneity in the incidence of the monetary contraction in the labor market and compare it to the typical heterogeneity in cyclical exposure across the income distribution. If the groups that are most exposed to business cycles also respond most to monetary policy, central banks can use monetary policy to stabilize employment for these groups without leading to distortions of employment among other groups. We estimate that the monetary policy tightening raised unemployment for workers in the bottom earnings decile by 1 p.p. more than the highest earning workers, consistent with estimates in other recent work showing that contractionary monetary policy increases inequality (Coibion et al., 2017; Holm et al., 2021; Moser et al., 2021; Amberg et al., 2022). However, we find that this monetary contraction was meaningfully more regressive than the typical business cycle, suggesting a tradeoff between stabilizing aggregate employment and distorting relative employment across groups. This finding suggests that labor market heterogeneity is another dimension along which there can be tradeoffs for central banks, adding to the many complexities that heterogeneity introduces for optimal policy, such as those highlighted by Rubbo (2020) and La'O and Tahbaz-Salehi (2022) in multi-sector economies.

Our paper contributes to several streams of literature. First, we introduce a new case study to the large and diverse literature identifying monetary policy shocks and their effects. Some of the most influential work documenting monetary non-neutrality comes from the study of large historical monetary shocks including the Great Depression and the Volker Disinflation in the US (Friedman and Schwartz, 1963; Velde, 2009). The case study approach was applied notably by Romer and Romer (1989), who used the narrative account to identify natural experiments in which the Federal Reserve intentionally exerted contractionary pressure on the economy. This paper answers the call set out by Ramey (2016) in her handbook chapter, who notes "given the important impact of Friedman & Schwartz's (1963) case study of monetary policy during the Great Depression, it is surprising that more case studies have not been conducted."

The case study approach contrasts with other approaches used in the literature to study the effects of monetary policy. One approach focuses on controlling for confounders, using either structural VARs (Stock and Watson, 2001; Christiano et al., 2005; Ramey, 2016) or by controlling directly for central bank internal forecasts (Romer and Romer, 2004). Another approach relies on the presence of currency pegs to identify monetary interventions outside the control of the monetary authority (Jordà et al., 2020; Andersen et al., 2022). A more recent strategy identifies monetary shocks from movements in asset prices in the narrow window around FOMC announcements (Hanson and Stein, 2015; Gertler and Karadi, 2015; Nakamura and Steinsson, 2018; Miranda-Agrippino and Ricco, 2021; Bauer and Swanson, 2022). These high-frequency shocks are well-suited for exploring the effects of policy on relative price movements, but are generally underpowered to detect movements in real variables that occur with long and variable lags (Cochrane and Piazzesi, 2002; Angrist et al., 2018; Ramey, 2016).

Our analysis is also informative about the costs of "leaning against the wind", or using monetary policy tools for macroprudential purposes. As pointed out by Svensson (2017), tightening monetary policy may provide benefits in terms of avoiding financial crises, but these benefits need to be weighed against the cost of lower economic activity even if a crisis is avoided. Our estimates are informative about these latter costs. Other recent work focuses on the benefits side, providing estimates of how contractionary policy may affect the probability of future crises (Gourio et al., 2018; Schularick et al., 2021).

We also contribute to a large literature on how nominal wage rigidities affect monetary policy transmission. This concept dates back to Keynes, but the key role of nominal wage rigidities in New Keynesian models has more recently been emphasized by Christiano et al. (2005), Broer et al. (2019), and Auclert and Rognlie (2018). These rigidities lead to real effects of monetary policy as documented by Olivei and Tenreyro (2007) and, most closely to this paper, by Björklund et al. (2019), who show that output responses to monetary shocks in Sweden are larger when wages are rigid due to fixed contracts. Recent work by Card (1990), Ehrlich and Montes (2014), Kurmann and McEntarfer (2019), Murray (2019), and Olsson (2020) examines the link between wage rigidity and employment at the firm level in response to various types of cyclical shocks. We confirm the importance of nominal wage rigidities using this unique monetary episode and provide further evidence on the mechanisms through which wage rigidities lead to increases in unemployment.

The rest of the paper is organized as follows. Section 2 describes monetary policy in Sweden in 2010 and argues that the contraction in 2010–2011 can be characterized as a large contractionary monetary shock. Section 3 shows that this tightening was followed by large rises in unemployment and a broader economic contraction. Section 4 uses firm-level hetreogeneity and international data to support the claim that it was the monetary tightening that caused these movements. Section 5 provides evidence of the importance of wage rigidity in driving the unemployment effects, and Section 6 documents heterogeneity in the incidence of the shock in the labor market across the income distribution. Finally, Section 7 concludes.

2 The Swedish experiment

In the section below, we combine narrative evidence with estimated policy shocks to argue that Sweden's monetary tightening over 2010–2011 represents a large deviation from the historical monetary rule that was motivated by political shifts within the Riksbank and was unrelated to the conditions in the labor market.

2.1 The Swedish economy pre-2010

Sweden's official currency is the Swedish krona, which enables the Riksbank to exercise its own monetary policy independently from the European Central Bank (ECB). The Riksbank follows a policy of flexible inflation targeting, which involves stabilizing both inflation and the real economy.[1] In order to meet this objective, the Riksbank controls the repo rate, which is the interest rate at which banks can borrow or deposit money with the Riksbank for up to seven days. The Riksbank meets six times per year to give their forecasts of the economy and to both set the current repo rate and provide extensive guidance of the likely future path of the repo rate.

Like most developed economies, Sweden was deeply affected by the 2008 global financial crisis. That

[1] As laid out by Sveriges Riksbank (2010) and discussed at length in Goodfriend and King (2015), "the objective for monetary policy is to maintain price stability" but also "stabilize production and employment around long-term sustainable paths."

year, Swedish exports and GDP contracted sharply, and the Riksbank dramatically cut rates from 4.75% to 0.25%. By 2010, the Swedish economy had begun to recover and exports began to surge, growing by 12 percent that year, even as the exchange rate appreciated back towards pre-crisis levels. Stimulated by surging exports, GDP grew by 6% and the unemployment rate fell by 1%. Indeed, in an article in 2011, the Washington Post dubbed Sweden "the rockstar of the recovery."[2] However, while the growth rate in this period was high, Sweden was still recovering from a deep recession and the level of economic activity was well below trend—in the first quarter of 2010, GDP was 5% below its pre-recession peak and the unemployment rate was 2.5 percentage points above the natural rate (Svensson, 2011).

2.2 The 2010 monetary tightening

It was on the heels of this strong growth that, in mid-2010, the Riksbank began tightening monetary policy. Starting in the middle of 2010, the Riksbank decided to raise the policy rate, going step-wise from 0.25% in June 2010 up to 2% by August 2011. However, the move did not merely reflect a positive assessment of current growth. At that time, members of the Riksbank began to view rising house prices and household debt as a concern that needed to be addressed. While other regulatory bodies within Sweden had official responsibilities for those aspects of the economy, several members of the Riksbank "felt that if no one else was going to do something about it then they should. [...] The Riksbank, therefore, took it upon itself to allow concerns about financial stability to affect decisions on monetary policy" (Goodfriend and King, 2015). Several accounts suggest that the decisions of the Riksbank to raise interest rates through this period were driven by ideological shifts within the Riksbank that led them to "lean against the wind" and tighten more than would have been warranted by the current economic conditions and the historical policy rule (Goodfriend and King, 2015; Svensson, 2011).

In their policy report from the June 2010 meeting, the board justified the decision by saying "developments in the labor market and the high GDP growth indicate that the recovery is on solid ground ... moreover, house prices are rising relatively quickly and household indebtedness has increased substantially in recent years" (Goodfriend and King, 2015). The Riksbank provided similar comments following later rate increases throughout the next year. As Goodfriend and King put it in their 2015 review:

> "The problem for members of the Board [was that] CPIF inflation would according to the fore-

[2] See "Five Economic Lessons from Sweden, the Rock Star of the Recovery," June 24, 2011.

cast undershoot the 2% inflation target by 0.5% or so for most of the forecast period, and unemployment was forecast to remain above the 6 to 7% sustainable rate of unemployment ... Yet, actual CPIF inflation had been running consistently at the 2% inflation target in 2010, and other Board members were all sensitive to the need to balance continued highly expansionary policy against the possibility that exceptionally low interest rates over a long period of time would lead to excessive indebtedness among households, abnormally high house prices, and financial fragility in the future."

The decision by the Riksbank to tighten over this period caused great divisions within the Riksbank and was opposed most vocally by board member Lars Svensson. In a speech in November 2010, Svensson argued forcibly that the Riksbank "conducted a tighter monetary policy than [was] justified". He explained that the Riksbank was motivated by the thought that "growth is good, interest rates are very low and need to be normalized, that [they] need to signal to house-buyers that interest rates will increase, that the rise in house prices and household debts needs to be limited, and that financial imbalances could build up if [they] do not conduct such a policy" (Svensson, 2010).

While the tightening was debated within the Riksbank, the evidence suggests that the market thought this move was credible, at least through the first year of the tightening. It was not until 2012 that tensions within the Riksbank spilled into public disagreements about the objective of monetary policy and the extent to which concerns of household credit and home prices should affect the level of the repo rate (Goodfriend and King, 2015).[3] Additionally, as we show in the following section, longer-term interest rates such as the mortgage rate and consumer loan rate were meaningfully affected by the movements in the repo rate, demonstrating that the market expected the higher interest rates to persist for at least some time.

After the sharp monetary tightening through the end of 2010 and early 2011, the Swedish recovery began to deteriorate: GDP growth slowed substantially, the unemployment rate bottomed out at 7.5% before rising again, the exchange rate weakened, and inflation fell well below the 2% target. The Riksbank eventually reversed course and dropped the interest rate steadily over subsequent years, eventually implementing negative rates in mid-2015.

[3]E.g., from the public debate; https://www.dn.se/ledare/kolumner/riksbanken-maste-bli-tydligare, retrieved 04/28/2022; https://archive.riksbank.se/Documents/Tal/Ekholm/2013/tal_ekholm_131115_eng.pdf, retrieved 04/28/2022; https://ekonomistas.se/2012/09/13/calmfors-om-riksbanksdirektionen/, retrieved 04/28/2022.

2.3 Estimated monetary shocks confirm narrative account

The narrative accounts discussed above suggest that the 2010 monetary tightening was a break from the Riksbank's previous approach to monetary policy. We now turn to examining this more formally by following the methodology of Romer and Romer (2004). Specifically, using the set of monetary policy decisions from March 2002–February 2010, we estimate the relationship between the change in the policy rate at each monetary policy meeting and the Riksbank's internal forecasts for real outcomes and inflation:[4]

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

where the unit of observation is the Riksbank policy meeting m, r_m is the repo rate, π is inflation, GDP is the growth rate of GDP, and u is the unemployment rate. Since economic data is generally released with a lag, the $\tau = -1$ observations capture the new data that became available since the previous meeting and the $\tau = 0$ data is the nowcast. The data at $\tau = 1$ and $\tau = 2$ are the Riksbank's internal forecasts for each variable 1 and 2 quarters ahead.[5] We construct the predicted change in the policy rate as the fitted values of this regression and define the monetary shock as the residuals $\widehat{RR}_m = \min(\epsilon_m, r_m)$, bounded such that the shock cannot be larger than the current interest rate since the zero lower bound would bind.[6] These shocks capture movements in the policy rate that were unexplained by the current Riksbank forecasts, given the historical relationship between those forecasts and policy decisions. Since the narrative suggests a break in the policy rule in mid-2010, we stop the estimation in mid-2010 and explore the implied shocks using the parameters estimated in the period leading up to the monetary episode we are studying.

Figure 1 shows the resulting series of monetary shocks. Unsurprisingly, since the monetary shocks are residuals from a linear regression estimated on data through the beginning of 2010, the estimated monetary shocks before mid-2010 are relatively small and centered around 0. However, from June 2010 through the end of 2012, we see that Sweden experienced a series of large and positive monetary shocks—in those years, the central bank raised interest rates substantially more than we would have expected given their

[4]Our dataset starts in March 2002 since the Riksbank did not consistently release their forecasts at each meeting prior to this point. In Section A.3, we repeat this exercise using a series of private forecasts from Consensus Economics, which have been released consistently since 1990, and find similar results.

[5]See Appendix Figure A2 and A3 for alternate specifications, and Section A.2 for a discussion of how Equation 1 relates to the original Romer and Romer (2004) specification.

[6]Adjusting for the zero lower bound results in slightly smaller shocks in April 2010–July 2010 and December 2014–October 2015, but has virtually no effect on the results we present.

forecasts at the time and the historical relationship between the Riksbank's decisions and their forecast of the state of the economy. Interestingly, these monetary shocks are estimated to be close to 0 beginning in 2013, which is a period when the Riksbank cut rates significantly, suggesting that these subsequent interest rate cuts were in line with economic conditions and the Riksbank's usual policy rule.

It is unsurprising that the out-of-sample errors after 2010 are large compared to the in-sample residuals before 2010. However, the key finding is that the estimates are consistently positive during these two years. Indeed, using a longer estimation sample going back to 1990 that replaces Riksbank internal forecasts in Equation 1 with private sector forecasts from Consensus Economics, we estimate the implied policy deviations over the subsequent 24 months for each meeting between 2000 and 2008.[7] We find that these deviations are centered around zero, with the maximum deviation being a full percentage point smaller than the estimate for 2010 (see Appendix Figure A4). This analysis further confirms the narrative account that the Riksbank's behavior in 2010 reflected an unusual break from the historical policy rule.

While the baseline estimates of Equation 1 use only data for inflation, GDP and unemployment to predict monetary policy decisions, we also find similar estimates when we include either the change in house prices or household debt (see Appendix Figure A2). If the Riksbank had always considered these variables when making their policy decisions and chose to raise the policy rate in mid-2010 in response to rapid growth in house prices or household debt, then including these variables in Equation 1 should substantially shrink the estimated monetary shocks through 2010 and 2011. However, this is not the case, and if anything, the estimated monetary shocks through this period are even larger with this specification.[8]

2.4 Other features of the monetary episode

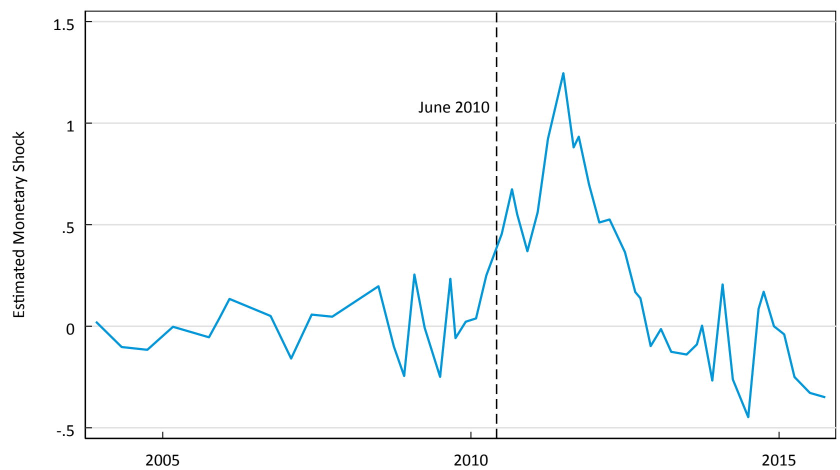
Before moving to our estimate of the effects of this monetary tightening on the economy, we note two features of the Swedish episode that make this quasi-experiment different from the typical monetary shock analyzed elsewhere in the literature.

First, this episode was partially anticipated. We measure the extent of anticipation using data on Swedish private-sector forecasters' expectations for interest rates from reports published by Prospera Re-

[7] The details of this estimation are provided in Section A.3. The 2010–11 shocks using these estimates are slightly smaller than in our baseline, but still add up to a total deviation of 2.5 percentage points over the 2-year period from 2010–2011.

[8] While our interpretation of the narrative evidence is that the policy rule changed discontinuously in 2010 to include concerns of household indebtedness, another possibility is that household indebtedness was always in the policy rule but in a discontinuous manner such that the Riksbank only reacted to these variables when they were sufficiently high. This explanation would also be consistent with the patterns in Figure 1 and such a discontinuous policy rule would also provide quasi-experimental variation.

Figure 1: Estimated monetary policy shocks



Notes: The blue solid line shows the residuals from Equation 1 in percentage points, adjusted for the zero lower bound. The coefficients were estimated from the March 2002–February 2010 Riksbank meetings, and residuals are calculated for the March 2002–October 2015 period using these estimated coefficients. See Appendix Table A5 for the estimated parameters from Equation 1 and see Appendix Figure A2 to Appendix Figure A3 for alternative specifications.

search AB, which is commissioned by the Riksbank to conduct surveys of market participants soliciting their economic forecasts. We find that over this period, the 3-month ahead forecast error for the Riksbank's policy rate among this sample of professional forecasters was around 0.15 percentage points (see Appendix Figure A1). Therefore, forecasters did internalize the Riksbank's extensive forward guidance on the path of interest rates, but still were surprised by the speed with which the Riksbank raised the policy rate.[9]

It is interesting to note that while market participants did not fully anticipate the rise in interest rates through the second half of 2010 and into 2011, the monetary shocks identified using high frequency techniques over this period are small (Sandstrom, 2019). High frequency methods identify monetary policy shocks using changes in the price of interest rate futures in a short window of time around the central bank's policy announcements (Gertler and Karadi, 2015; Nakamura and Steinsson, 2018). In this way, they isolate the change in the path of interest rates that was entirely unanticipated by financial markets before each announcement. The fact that we find meaningful 3-month-ahead forecast errors despite the small estimated high-frequency shocks suggests that the Riksbank was effective in communicating very short-run

[9]In what follows, we do not distinguish between the anticipated and unanticipated component of the monetary tightening because the narrative account supports the required orthogonality assumption, namely that the tightening was unrelated to labor market conditions. In a robustness check, we verify that using only the unexpected component of the monetary tightening produces similar estimates (see Appendix Table A1).

changes in the policy rate, but that they neither convinced the market of their longer run policy nor did the market anticipate a sufficiently rapid economic recovery to support such a monetary tightening.

The second unique feature of our setting is that we analyze one large monetary tightening that occurred early on in an economic recovery. As we outlined in Section 2.2, while Sweden was growing rapidly in the first quarter of 2010, GDP was still 5% below its pre-recession peak and the unemployment rate was 2.5 percentage points above the natural rate (Svensson, 2011). Typically, monetary tightenings occur when the economy is stronger.[10] To the extent that the impact of monetary policy depends on the state of the business cycle, the estimates resulting from this analysis may not reflect those for the typical monetary contraction (Eichenbaum et al., 2022; Berger et al., 2021). Nonetheless, they may be particularly valuable to policymakers. After the Great Recession, many central banks faced the question of when to "liftoff" their interest rates again after a long period close to or at the effective lower bound. This quasi-experiment directly informs these choices.

3 Overall effects of monetary tightening on the economy

Having established the nature of this monetary episode, we next use local projection regressions to show that the Swedish economy contracted substantially following the tightening in excess of what would have been expected given the normal business cycle. Specifically, across an array of specifications, we find that a 1 percentage point increase in the policy rate in this period was associated with an additional 1–2 percentage point increase in unemployment. Later, in Section 4, we will show further evidence that supports the interpretation that the monetary tightening caused this increase in unemployment.

3.1 Estimates for unemployment: Event study specification

We start with a simple event study research design to examine the aggregate effects of the Riksbank's tightening. In the spirit of Romer and Romer (1989), our goal is to estimate the deviation of the unemployment rate following the 2010–2011 tightening from the counterfactual path it would have taken under a typical economic regime.

We implement this event study using local projection regressions of the form:

[10] Specifically, the average level of the unemployment rate when the Riksbank raised the repo rate before 2010 was 5.8%, while the unemployment rate in July 2010 was 8.8%.

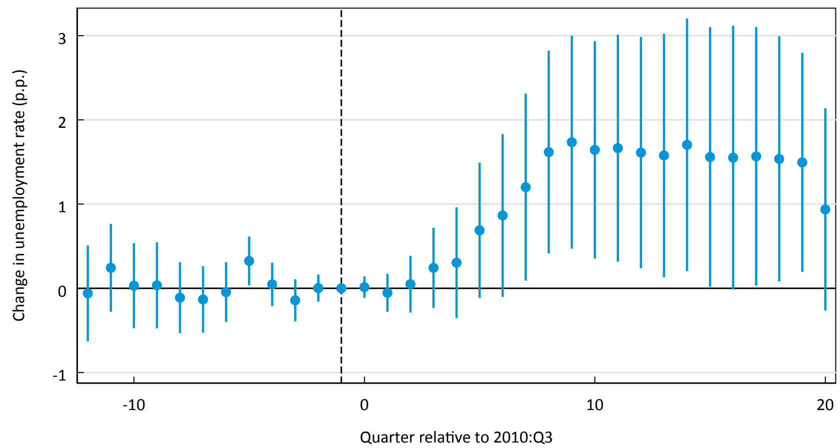
$$u_{t+k} - u_{t-1} = \beta_k \mathbf{1}\left(t = 2010Q3\right) + X'_{t-1}\alpha + \epsilon_{t,k} \tag{2}$$

where $u_{t+k} - u_{t-1}$ is the change in the unemployment rate over the next k quarters, $\mathbf{1}\left(t = 2010Q3\right)$ is an indicator variable for 2010Q3, and X'_{t-1} are lagged time-varying controls for other economic variables that may also affect the evolution of unemployment. In our baseline specification, we include in X'_{t-1} the 1-quarter-lagged year-over-year percent change in GDP and the year-over-year percentage point changes in both the vacancy and the layoff rate, as well as additional lags of each of these variables for each of the three preceding years, intended to control for delayed responses of the unemployment rate to these measures of economic activity. We include in the estimation sample all quarters from 1996Q2–2019Q2. By controlling for time-varying measures of the business cycle in X'_{t-1}, our estimates capture the deviation in unemployment from its counterfactual path under the typical economic regime. Implicitly, including X'_{t-1} not only controls for the direct effect of these variables on the outcome, but also any indirect effects correlated with X'_{t-1} (e.g automatic stabilizers or monetary policy rules that depend on X'_{t-1}).

This event study specification builds on the forecast error approach of Romer and Romer (1989). Their approach estimates the deviation $u_{t+k} - \mathbb{E}\left[u_{t+k}|X'_{t-1}\right]$, where the expectation is computed recursively from the forecasts of an AR(24) regression. Our approach updates this method by using local projections, which both allows us to use a wider set of controls X'_{t-1} beyond autoregressive terms and is robust to cases where the impulse response functions may be non-invertible (Stock and Watson, 2018). We further discuss the connections between our approach and Romer and Romer (1989) in Section A.4, and show the results of forecast error specifications in the following section.

The key identification assumption is that the unemployment rate would have followed its usual cyclical pattern in the absence of the monetary tightening. Formally, using Equation 2, the assumption is that $\mathbb{E}\left[\mathbf{1}(t = 2010Q3)\epsilon_{t,k}|X'_{t-1}\right] = 0$. This assumption would be violated if, for example, the Riksbank's tightening was in response to news about future unemployment, or if a non-monetary negative shock happened to coincide with the monetary tightening and raised unemployment. However, as discussed in Section 2, the Riksbank's motivations for tightening were not related to unemployment (or other labor market developments). Additionally, in Section 4, we provide evidence against the existence of other non-monetary shocks that confound our results. Finally, note that our identification assumption does not require that the monetary tightening was unanticipated, only that it was exogenous with respect to determinants of the

Figure 2: Event study estimates for monetary tightening episode



Notes: This plot shows coefficients estimated from the set of local projections regressions described by Equation 2. Controls include the 1, 5, 9 and 13th quarter lags of year-over-year percent change in GDP and the year-over-year percentage point changes in the vacancy rate and layoff rate. We use harmonized measures of GDP, unemployment, vacancies, and layoffs from the OECD Main Economic Indicators. The sample includes quarterly data from 1996Q1 to 2019Q2. Bars illustrate the 95% confidence interval with heteroskedasticity-robust standard errors.

unemployment rate.[11]

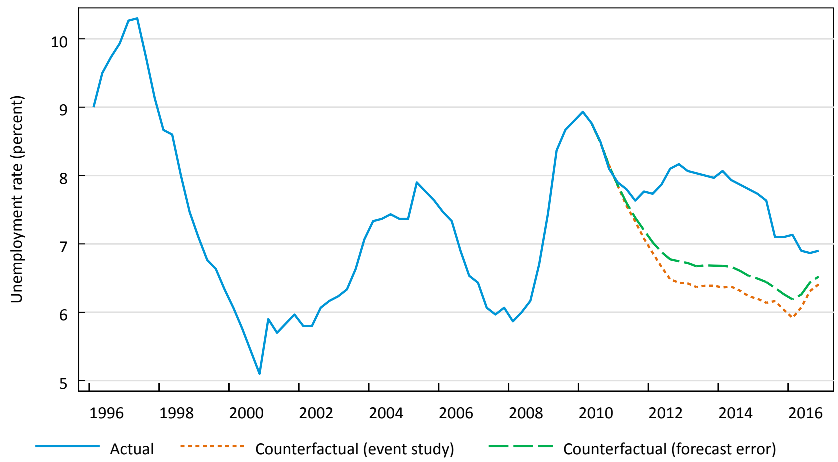
Figure 2 plots the estimates of β_k from Equation 2. The effects of the event study are small initially, but then phase in over time. The effect peaks at a bit less than 2 percentage points three years after the event, statistically significantly different from zero. We estimate that this increase in unemployment largely persists for five years after 2010 Q3. Additionally, the estimates from quarters before the start of the tightening cycle do not show a pre-trend, consistent with the notion that the Riksbank's policy during this period was not primarily a response to labor market conditions.

To understand the magnitude of these event study estimates, we must scale by the magnitude of the interest rate hike associated with this event study. In Appendix Table A3, we report estimates applying our event study research design to several measures of short-term interest rates. We estimate that the overnight interbank rate rose by 0.853 p.p. within two quarters of the event, and combining this with an effect on unemployment of 1.6 p.p. twelve quarters after the event, this implies that a 1 p.p. rise in the interest rate would lead to a 1.9 p.p. increase in the unemployment rate 3 years later.

Another way to visualize these estimates is to back out the implied counterfactual path for the un-

[11]This point was emphasized in the comments on Romer and Romer (2004) by Cochrane (2004), who states in Proposition 1: "To measure the effects of monetary policy on output it is enough that the shock is orthogonal to output forecasts. The shock does not have to be orthogonal to price, exchange rate, or other forecasts. It may be predictable from time t information; it does not have to be a shock to agent's or the Fed's entire information set." Our assumption is essentially the same, replacing output with the unemployment rate and conditioning on lagged controls.

Figure 3: Implied counterfactual paths for the unemployment rate

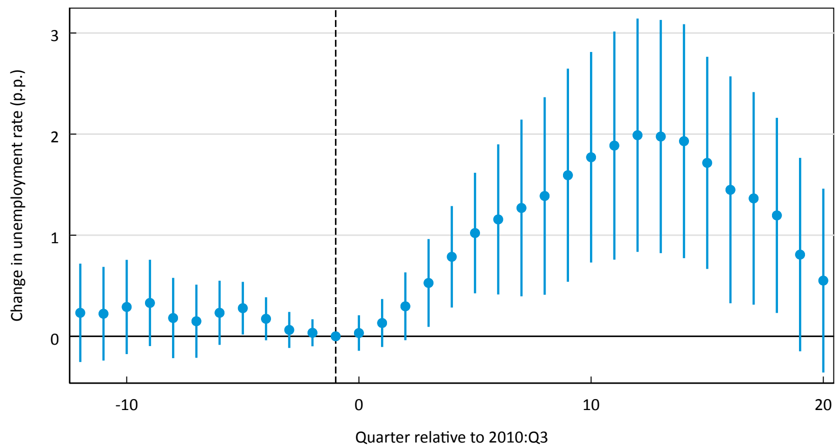


Notes: This plot shows the actual unemployment rate along with two counterfactual paths. The counterfactual labeled "event study" takes estimates of β_k using Equation 2 and subtracts these estimates from the actual unemployment rate. The counterfactual labeled "forecast error" uses predicted values from a regression of the outcome on business cycle controls following Romer and Romer (1989), as described in Section A.4.

employment rate without the monetary tightening episode, which we present in Figure 3. The solid blue line shows the realized path of the unemployment rate, and subtracting our estimates shown in Figure 2 yields the counterfactual path shown by the dashed orange line. Without the monetary tightening shock, we estimate that unemployment would have declined steadily in 2010–11, before flattening out around 6.5 percent, slightly above the troughs reached after the previous two recessions. For comparison, we also show the counterfactual path estimated by the forecast error method of Romer and Romer (1989), which we detail in Section A.4. This path is similar although somewhat higher, implying an effect on unemployment of 1.5 percentage points rather than nearly 2.

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 2010:Q3 with an indicator for a quarter sampled randomly with replacement from the 1996:Q1–2019:Q2 period, excluding dates between 2007:Q4–2012:Q4 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 Appendix Figure A5, are symmetrically centered around 0, indicating no systematic upward or downward bias in the event study design.

Figure 4: Response of unemployment rate to Romer and Romer (2004) shock



Notes: This plot shows coefficients estimated from the set of local projections regressions described by Equation 3. 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. Bars illustrate the 95% confidence interval with heteroskedasticity-robust standard errors. See Appendix Table A2 for Newey-West standard errors.

3.2 Estimates for unemployment: Identified shocks

In this section, we implement a refinement of the event study research design that takes into account the sequential nature of the tightening, replacing the indicator for 2010:Q3 with the identified monetary policy shocks from Section 2.3. Specifically, we estimate a set of local projections regressions:

$$u_{t+k} - u_{t-1} = \beta_k \widehat{RR}_m + X'_{t-1}\alpha + \epsilon_{t,k} \tag{3}$$

where $\widehat{RR}_m$ are estimated Romer and Romer (2004) shocks from Section 2.3. In order to focus on the tightening episode of interest, we use only the baseline shocks for 2010–2011 from Figure 1, setting the estimated shocks outside of that period to zero.[12] This choice aligns this specification with the event study, which focuses only on the monetary shock in 2010–2011. We use the same outcome, controls, and sample period as in the event study estimates. Using identified shocks simplifies the interpretation of the estimates: β_k represents the effect of a 1 percentage point monetary policy hike on the unemployment rate k periods later.

[12]See Appendix Table A1 for estimates using the full shock series and for a version of Equation 3 that separately includes the 2010–2011 shocks and all other estimated monetary shocks. The estimated effect of this quasi-experiment is similar when controlling separately for shocks in other periods (column (3)) and the response to the typical monetary shock is quantitatively smaller although still substantial (column (2)). Additionally, in Section A.3, we estimate the effects using an alternative shock series based on Consensus Economics forecasts, with again similar results.

Figure 4 shows the estimated effects from Equation 3. The unemployment rate is flat before the shock, shows no response immediately on impact, but begins rising steadily for multiple years, eventually reaching a peak effect of about 2 percentage points three years after the shock. This pattern mirrors the estimates from the event study shown in Figure 2, but decays more rapidly, as we would expect given that the specification accounts for the timing of the tightening over this period. As with the event study, we find no evidence of a trend in unemployment before the shock. Appendix Table A2 shows that these patterns are robust to including alternate controls in X'_{t-1}, including lagged controls for household debt, house prices, and both lagged and contemporaneous controls for international variables. Appendix Table A1 also shows similar effects using shocks calculated either with forecasts from Consensus Economics in Equation 1 or using only the unanticipated component of the change in the policy rate.

The magnitudes of these estimates are large. These estimates are at or above the upper end of the range of estimates found in the literature, where estimates using Romer and Romer shocks for the effect of a 1 percentage point increase in the interest rate on unemployment typically range from 0.5 to 1 percentage point (Coibion, 2012; Ramey, 2016; Romer and Romer, 2004). There are many potential explanations for the difference between our estimates in Sweden and others in the literature using Romer and Romer shocks, including but not limited to our focus on a contractionary shock, the fact that the shock occurred in a weak economy, or institutional features of the Swedish labor market. While it is beyond the scope of our analysis to quantify all of the potential differences across settings, we can show that our focus on a single monetary tightening is a primary reason for the relatively large effect.

In our baseline specification, we zero out Romer and Romer shocks outside of 2010–11, while prior work typically uses the full series of Romer and Romer shocks. In Column 2 of Appendix Table A1, we re-estimate our main specification using the full series of Romer and Romer shocks, which shows a similar effect as in our baseline, albeit slightly attenuated. To gauge how much of this effect is driven by our shock versus all others, we show in column 3 estimates from a specification in which we include both the shocks in 2010–11 and the shocks outside 2010–11 separately, allowing for different coefficients on each. We find that Romer and Romer shocks outside of 2010–11 have a point estimate of 0.5 p.p, in line with the rest of the literature, but the estimates are statistically insignificant and unable to rule out a zero effect. In contrast, the coefficient for shocks in 2010–11 is statistically significant and close to our baseline estimates, highlighting that this shock drives identification when all shocks are included as in column 2.

By highlighting the main shock driving identification, our baseline approach of using only the shock in 2010–11 is more transparent than the alternative of including all shocks.

3.3 Estimates for other economic variables

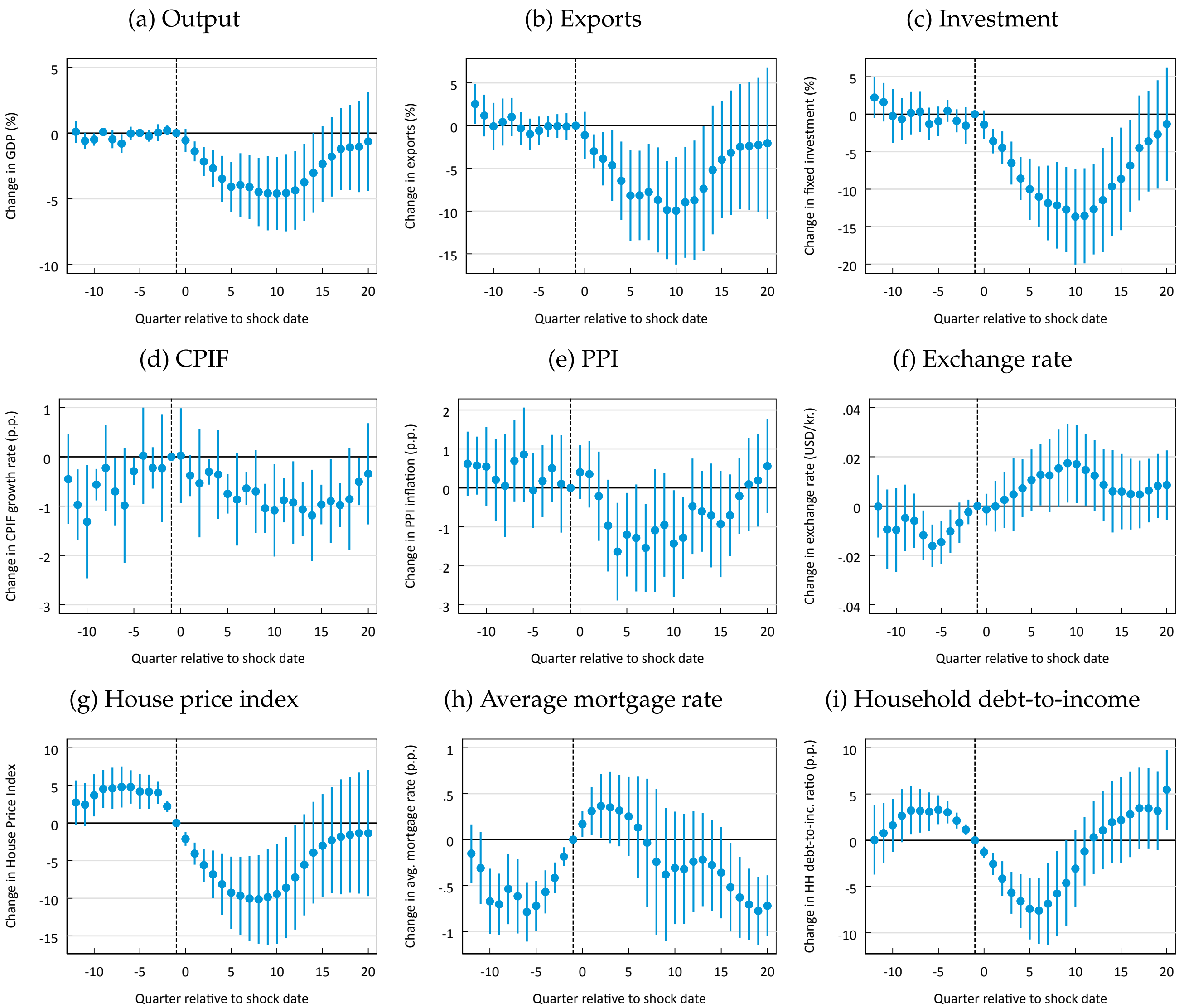
While we focus in this paper primarily on the effects of monetary policy on the labor market, Figure 5 shows the estimates for other macroeconomic outcomes. The results in the first two rows demonstrate that, overall, this monetary shock in Sweden behaved just as would have been predicted by a standard New Keynesian model; in response to an increase in the interest rate, GDP growth slowed, exports cratered, investments slowed, the krona appreciated, and the growth rate of inflation fell for both consumer prices and producer prices. The magnitude of the response of output to the monetary shock is similar to the estimates from Romer and Romer (2004) and larger than those in Coibion (2012), who estimates that industrial production falls by 2-3 percentage points after a 100 basis point increase in the interest rate. These results provide reduced-form empirical evidence for the real effects of monetary policy across the economy.

The bottom row of Figure 5 focuses on several variables that are related to household debt and the housing market, which the narrative analysis reveals was the motivation for the monetary contraction. Here, we see that, unlike the other variables in Figure 5, house prices, mortgage rates, and debt-to-income ratio all exhibit pre-trends – house prices and consumer loans were growing more, and mortgage rates were lower in the run-up to this monetary episode than they usually are at that point in the business cycle. This is in line with the Riksbank's concerns at the time that household debt was unusually high. While the pre-trends slightly complicate the exact interpretation of the estimates, we broadly see evidence that these variables moderated with the monetary episode, with mortgage rates rising, and house prices and household debt to income falling substantially.

4 Testing for potential confounders

Having shown that unemployment rose following the monetary tightening, we now examine whether this was causal. Our key identification assumption is that there were not other concurrent shocks that could have led to the increase in unemployment in the absence of the monetary episode. In the sections that follow, we discuss several possibilities and, through a battery of alternate specifications, additional

Figure 5: Real effects of monetary policy: Alternate outcomes



Notes: These plots show coefficients estimated from the set of local projections regressions described by Equation 3. The outcomes used are (a) log GDP; (b) log exports; (c) log fixed investment; (d) the quarterly growth rate of the CPIF (core consumer price inflation, CPI, net of the direct effect of interest rate expenses); (e) the quarterly growth of the PPI; (f) the quarterly change in the exchange rate; (g) the quarterly growth rate of the house price index; (h) the quarterly growth rate of the average mortgage rate; and (i) the household debt-to-income ratio. We obtain measures of real GDP, investment, exports, inflation, producer prices, and real estate prices from OECD Main Economic Indicators; and measures of interest rates on consumer and housing loans from the Riksbank. 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. Bars illustrate the 95% confidence interval with heteroskedasticity-robust standard errors.

narrative analysis, and informative firm-level heterogeneity, conclude that the large monetary shock was the principal driver of labor market developments in Sweden in this period.

4.1 International spillovers through exports

We start by showing that reduced demand for Swedish exports did not drive our baseline results. One particularly salient potential confounder for our estimates is the European sovereign debt crisis, which occurred within a few years of the monetary tightening episode we consider here. Since Sweden is not part of the eurozone, it was not directly affected by changes in the valuation of the euro. However, as a small open economy within Europe, it could have been exposed to the debt crisis indirectly.

If our results were driven by the Euro crisis (or other international developments) dampening export demand and causing the rise in unemployment, we would expect employment at exporting firms to have fallen substantially more over this period than employment in non-exporting firms, which are less exposed to a depressed demand for exports. To explore this heterogeneity, we construct an annual firm-level dataset from 1997–2016 that includes information on the number of full-time employees in the firm, the dollar value of exports, and several other firm characteristics such as firm age and the composition of the firm's balance sheet. See Section A.1 for additional details on the data.

Since we are particularly interested in understanding the effect of the Euro Crisis, we split firms into three categories based on their exports in 2007: those firms who export primarily to Europe, those who export primarily to countries outside of Europe, and those who do not report any exports.[13] Since exporting and non-exporting firms differ in many dimensions other than their exporting status, we also control for other characteristics of the firm, such as their size (defined in terms of the number of employees), age, and balance sheet composition (defined as the short term debt to asset ratio).

Specifically, for each firm characteristic w (e.g. export status, age, etc.), denote the partition of the sample of firms along w into discrete bins as $\mathcal{G}_w$. We then estimate the regression

$$y_{f,t+k} - y_{f,t-1} = \sum_{w} \left[\sum_{g \in \mathcal{G}_w} \left[\theta_k^g \widehat{RR}_t + X_{t-1}' \phi_k^g \right] \cdot \mathbf{1}(f \in g) \right] + \nu_{f,t} \tag{4}$$

where f indexes firms, time t is measured in years, $y_{f,t+k}$ is the outcome for firm f in year $t+k$ (either the

[13] We use export values in 2007 to define export status because it gives us the best glimpse into European exports. Firms only need to report exports to Europe if they are over a limit, the level of which was raised after 2007.

change in log employment or an indicator for whether the firm exits the market), $\widehat{RR}_t$ are the estimated monetary shocks aggregated across quarters within the year, X'_{t-1} are the same aggregate time-varying controls as in Equation 3, and g indexes the group based on the pre-shock characteristics of the firm. We allow the coefficients on the aggregate control variables X_{t-1} to vary across groups to capture differing baseline sensitivities to the business cycle. The coefficients of interest are θ_k^{Europe} and $\theta_k^{\text{Non-exporter}}$, which represent the differential k-year-later response to monetary policy shocks for firms that either export to Europe or do not export (both relative to firms that export to non-European countries), after controlling for the heterogeneity explained by the other characteristics of the firm.

Panel (a) of Figure 6 shows the result for the effect of firm export status on firm employment (left graph) and firm exit (right graph) 3 years after the shock, plotted relative to firms that export primarily outside Europe. Comparing non-exporters to exporters, we see that in response to the monetary tightening, non-exporting firms saw the biggest slowdowns in employment growth and were most likely to exit the market. This is the opposite of what we would expect if the rise in unemployment was driven by collapsing export demand. Moreover, the magnitude of this heterogeneity is substantial—the average firm in the sample slowed employment growth by 1 percentage point 3 years after the monetary shock, implying that all else equal, non-exporting firms contracted employment growth twice as much as exporting firms.

Furthermore, we find no difference in the outcomes of the firms that export primarily to Europe versus those that export to countries outside Europe. The difference between these two provides a measure of how important exposure to Eurozone markets was for Swedish exporters in this period. Since these two sets of firms experience similar outcomes along these two margins, we conclude that dampened export demand from the Eurozone crisis was not responsible for the increased unemployment we identify in our baseline results.

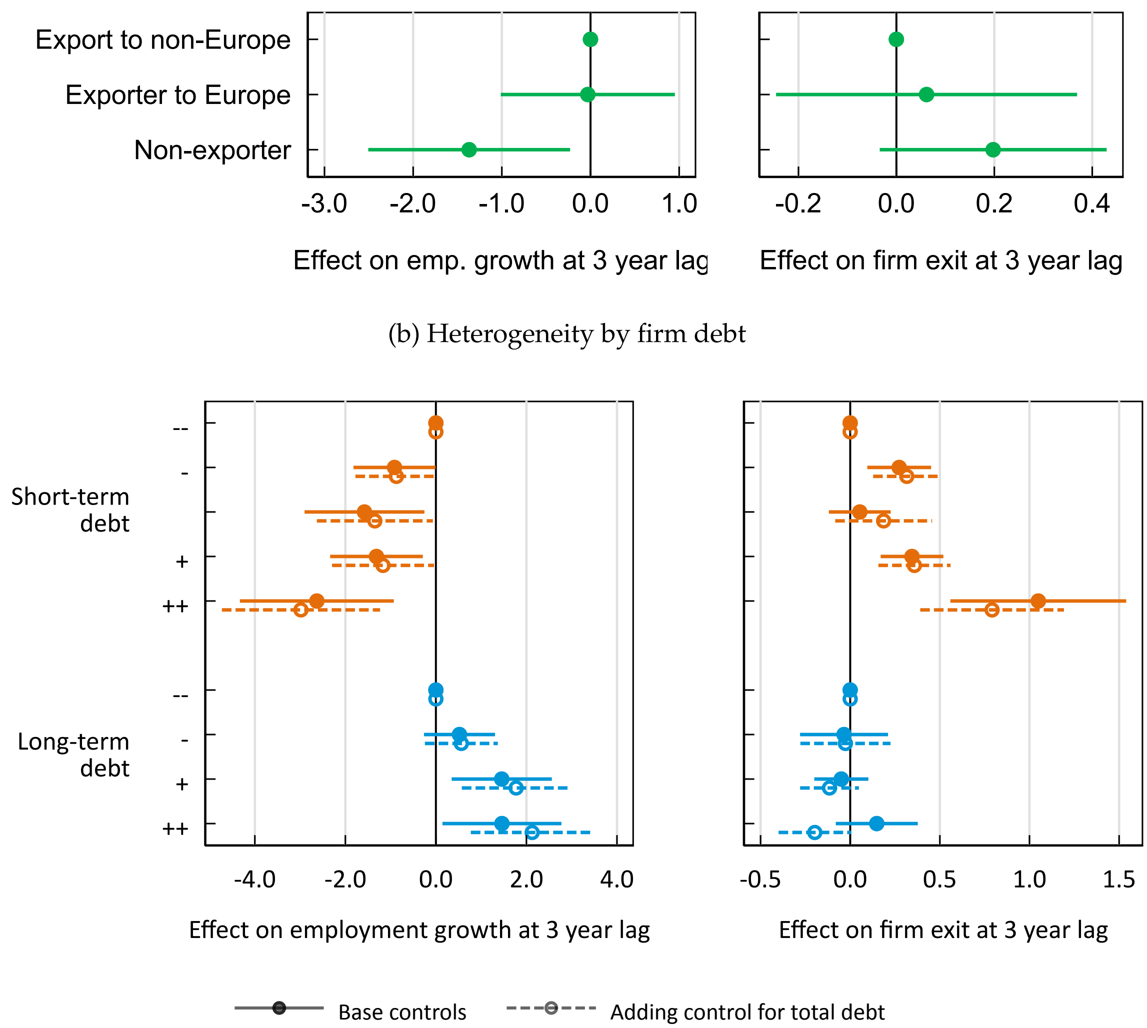
4.2 International spillovers through other channels

International developments could have affected Sweden through channels other than exports. To explore this possibility, we implement placebo analyses in several other countries. If the effects in Sweden were driven by confounding international shocks, such as the European sovereign debt crisis, we would expect to find comparable effects using the same specification in other European countries that were similarly exposed to those international developments but largely unexposed to the Swedish monetary policy.

Figure 6: Firm-level heterogeneity supporting identification of monetary shock

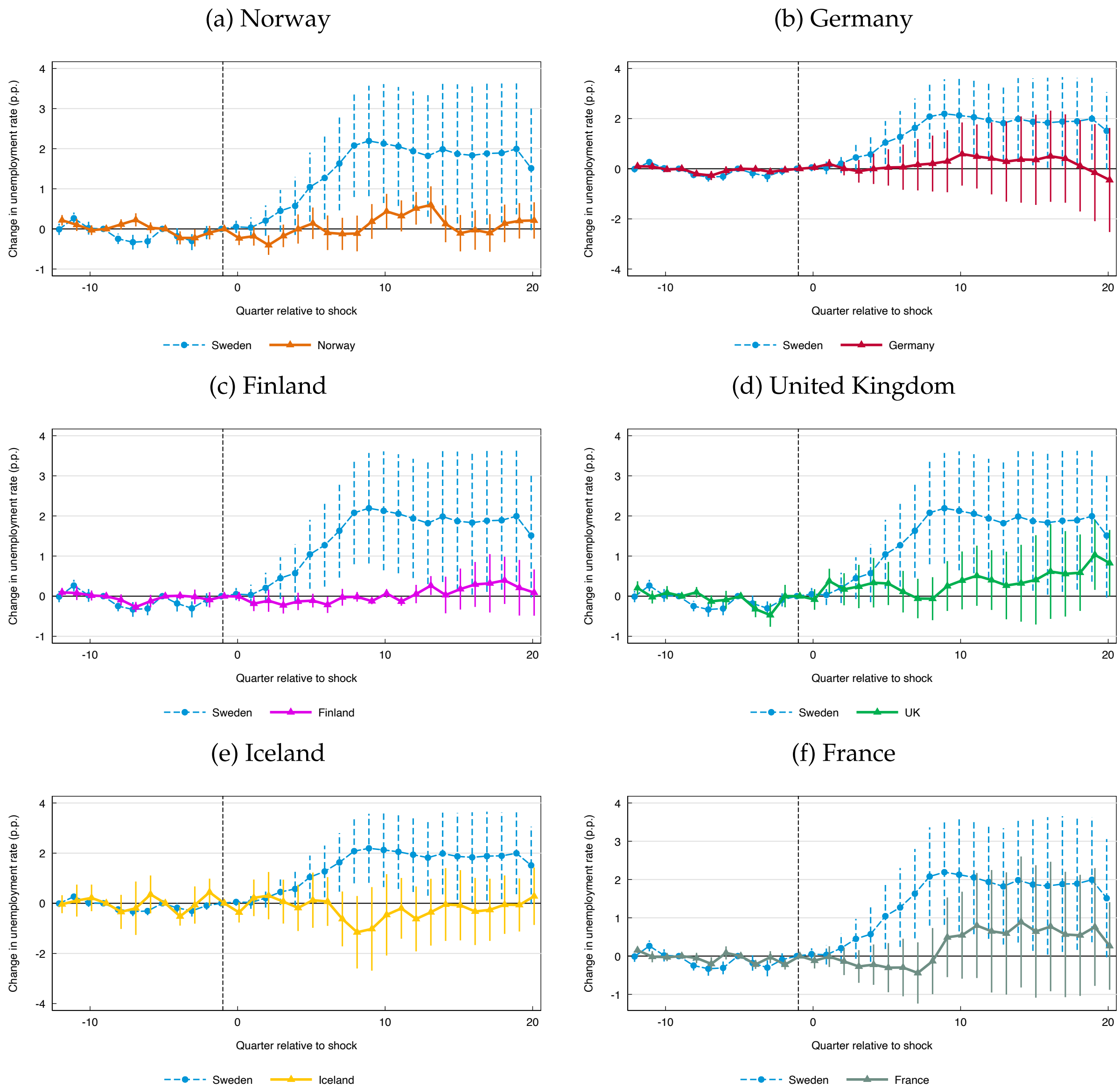
(a) Heterogeneity by exporter status and destination

(b) Heterogeneity by firm debt



Notes: Plots show estimates from regressions described by Equation 4. A firm's exporting status is defined in 2007, although results are similar when defining export status in 2009. We define exporting firms to Europe as those who report at least two-thirds of their annual export value going to countries within member countries of EU or EFTA in 2009 (27 EU countries and Island, Lichtenstein, Norway, Switzerland). Similarly, we define firms exporting to countries outside Europe as those reporting at least two-thirds of their export value going to any other country. Non-exporter are firms that do not report any export value. In panel (a), we further restrict the sample to firms with annual sales of at least the mean size of firms that export to Europe. Panel (b) includes only domestically owned and non-exporting firms. For both panels, to minimize measurement error, the sample requires firms to have at least 10 full time employees on average between 2007–09, and at least one full time employee in every year between 2007–10. In (a), we control for firm size, firm age, and firm short-term debt to asset ratio. In panel (b), unconditional estimates control for firm age and firm size, and the conditional estimates further control for the total debt to asset ratio. All controls are binned, see Appendix Table A7 for details. All regressions define the monetary shock using the estimated Romer and Romer shock from 2010–2011. 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 rates. Since the monetary shock occurred in the middle of the year, for the annual specifications, we define controls from Q2-Q2 for each year and use 2 lags. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the firm and year level.

Figure 7: Real effects of monetary policy: International placebo estimates using event study



Notes: This plot shows coefficients estimated from the set of local projections regressions described by Equation 2, using the unemployment rate 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 a discussion of the data sources and available years for each country. Bars illustrate the 95% confidence interval with heteroskedasticity-robust standard errors.

Figure 7 shows the results of running our baseline specification from Equation 2 using data from Norway, Finland, the UK, Germany, Iceland, and France.[14] In all of these countries, we find estimates of the monetary shock that are centered around zero and fairly tightly estimated. Interestingly, like Sweden, several of these countries did see a rise in unemployment at some point during this sample period. However, these results imply that the rise in unemployment in other countries in this period was either predicted by the control variables or did not line up with the timing of the Swedish monetary episode.[15]

4.3 The (Un)importance of other domestic shocks

Since the previous two sections suggest that international developments did not drive the economic contraction in Sweden following the monetary tightening, the remaining possibility is that domestic shocks other than monetary policy contributed to this deterioration. While it is impossible to fully rule this out, several pieces of evidence suggest this was not the case.

The narrative record of this period indicates that there were no salient domestic policy changes other than the monetary tightening. The incumbent political party was re-elected in the 2010 general election and there is no mention of important fiscal responses either in the Riksbank statements during this time or in Goodfriend and King's extensive review of this period. The most relevant domestic policies are those aimed at stemming the growth rate of mortgages in this period, with a new requirement in October 2010 that the loan-to-value of new loans should not be more than 85% (see Annex 3 in Goodfriend and King, 2015). Additionally, the surge in immigration to Sweden did not come until 2015, at the onset of the Syrian war.

One potential concern is that Sweden could have been experiencing a household deleveraging shock or housing market crash that was independent of the monetary contraction. Indeed, the Riksbank was responding to rising household debt and home prices when it decided to raise rates in mid-2010. To address these concerns, we use firm-level heterogeneity to test whether the employment effects differed sharply based on firms' balance sheets. Prior work has stressed firms' balance sheet compositions as important

[14]We make a small modification to Equation 2 and include lagged changes in unemployment as an additional control. We include this term to better capture labor market dynamics for countries in which vacancies and layoffs may be less well-measured than in Sweden (Section A.1.2 describes the data sources for each country), which helps eliminate pre-trends. For each country, we use the harmonized unemployment rate constructed by the OECD to minimize differences in the concept of unemployment being measured. Including the unemployment rate as an additional control does not diminish the estimated effect in Sweden, as shown in Appendix Table A2.

[15]Appendix Figure A7 repeats this placebo exercise for GDP growth and using alternate specifications and Appendix Figure A8 shows placebo estimates for the United States, all of which continue to support the finding that Sweden's contraction was unique.

predictors of their exposure to interest rate changes (Bernanke and Gertler, 1995; Gertler and Gilchrist, 1994; Kashyap and Stein, 2000; Jiménez et al., 2014; Ippolito et al., 2018). In our administrative sample of firms, we observe the maturity of firms' debt and focus on firms with different levels of either short-term (maturity less than one year) or long-term (maturity longer than one year) debt. A firm with higher short-term debt would have a larger exposure to a monetary tightening through greater rollover risk, while a firm with higher long-term debt would not have the same immediate exposure.[16] We would not expect to see similar firm-level heterogeneity patterns if the economic contraction was driven by household deleveraging.

We test the effects of short-term and long-term debt by using Equation 4 with debt-to-asset ratios for each maturity group in place of export status. Specifically, the set of characteristics that we include in the set $\{w\}$ are the firm's debt-to-asset ratio of a particular maturity (short-term or long-term), firm size (defined using the number of employees), and firm age. Since we are interested in isolating the effect of firm debt positions, we restrict our attention to domestically owned non-exporting firms, where we are more confident that we observe the firm's entire balance sheet. The coefficient $\theta_k^{\text{debt group } g}$ represents the k-year-later response to the monetary policy shock for firms in group g defined by their debt-to-asset ratio, after controlling for heterogeneity explained by the other characteristics of the firm.

Panel (b) of Figure 6 shows the estimated effects for short-term and long-term debt. The solid orange points show the change in either log employment (left graph) or probability of firm exit (right graph) three years after the monetary shock split by the short-term-debt-to-asset ratio of the firm in the pre-period, all plotted relative to the firms with the least short-term debt. We see that there is a clear gradient for both outcomes, with the drop in employment being nearly 3 p.p. larger and the probability of exit being 1 p.p. higher at the most indebted firms compared to the least. However, the estimates split by the long-term-debt-to-asset ratio, shown by the solid blue points, show either the opposite gradient (for log employment) or no gradient (firm exit).

One possibility is that firms with more short-term debt may also have more debt overall, and may then be more sensitive to economic downturns for reasons other than their exposure to interest rates. We explore this possibility in the dashed lines in Figure 6 by adding into the set $\{w\}$ a control for the overall debt-to-asset ratio of the firm. We see that the gradient with respect to short-term debt remains unchanged,

[16] In addition to rollover risk, Ippolito et al. (2018) document that firms with higher short-term debt also tend to have higher levels of floating-rate bank debt, which further exposes these firms to monetary tightening.

demonstrating that it is specifically short-term debt, not overall debt, that is driving this heterogeneity.

Together with the analysis in the previous subsections, these firm-level patterns further support our interpretation that a monetary tightening drove the rise in unemployment in this period.

5 The importance of nominal wage rigidities

Having established in the previous two sections that the monetary contraction caused a substantial rise in unemployment, we now turn to the question of how the rise in interest rates led to increased unemployment. Specifically, we focus on quantifying the importance of one particular channel that has been the subject of many studies: nominal wage rigidities. In a wide class of models, nominal rigidities are the key determinant of the effect of monetary policy on unemployment (Christiano et al., 2005; Broer et al., 2019; Auclert and Rognlie, 2018). While previous studies have documented the pervasiveness of downward nominal wage rigidities throughout labor markets (e.g. Grigsby et al., 2021), the theoretical literature has debated the importance of those measured rigidities for employment outcomes in response to monetary policy (Christiano et al., 2005; Basu and House, 2016). In this setting, we exploit an institutional feature of the Swedish labor market to measure nominal wage rigidities, namely that certain union contracts specify a path for nominal wages of both new hires and incumbent workers. Using this variation combined with the power provided by this unique monetary episode, we provide direct evidence for the importance of nominal wage rigidities, showing that nominal wage rigidities of this sort contributed meaningfully to the large increase in unemployment following the rise in interest rates.

Our measure of nominal wage rigidities uses a unique feature of the Swedish labor market. The Swedish labor market is characterized by a strong norm of collectively bargained wages, negotiated in a two-tiered system where bargaining takes place at the sectoral level separately for blue and white collar workers, and then, depending on how much flexibility the sector contract allows for, also at the firm level.[17] Wage contracts are mainly specified in growth rates and apply therefore to a large share of workers, in contrast to a minimum wage floor that typically affect only a limited number of workers. Each contract is specific to blue or white collar workers within a sector. Since these contracts are signed at the sector level, they generally apply to both existing workers and new hires, potentially creating wage rigidity along both margins. While wage rigidity for new hires is most important in canonical labor search

[17] See Olsson (2020) for a detailed description of union bargaining in Sweden and the employment contract data.

models (Pissarides, 2009), rigidity in wages for incumbent workers matters more in other models of the labor market, stemming from on-the-job search, financial frictions, or endogenous separations (Schoefer, 2021; Carlsson and Westermark, 2022; Bils et al., 2022). Given the nature of wage contracts in this setting, we cannot distinguish between these two margins, but estimate the joint effect of rigidity along both margins using variation across firms and sectors.

We explore the effect of nominal wage rigidities by linking this contract information to an administrative annual dataset from 1997–2016 that links workers to firms and includes information on annual earnings, the number of days that the worker is registered as unemployed, and several characteristics of both the worker and the firms in which they are employed.[18] We are able to match data on employment contracts from Olsson (2020) to 22% of our sample.[19] We refer to a contract as "rigid" if it includes an individually guaranteed wage growth rate or a piece-wage contract. All other workers are subject to "flexible" contracts.[20] We then calculate the average rigidity at the 3-digit sector level across all of the contracts in our sample and divide sectors into "rigid" and "flexible" groups based on the median average rigidity across sectors.

In order to have well-defined measures of firm characteristics, we focus here on a sample of attached workers, defined as those who were employed by domestic non-exporting firms for at least 9 months in each of the three years preceding the monetary episode we are studying. Within this sample, we run the following regression, which is an individual-level version of Equation 4:

$$u_{i,t+k} - u_{i,t-1} = \sum_{g \in \text{rigid, flexible}} \left[\beta_k^{g} \widehat{RR}_t + X'_{t-1} \alpha_k^{g} \right] \cdot \mathbf{1}(i \in \text{sector group } g) + Z'_i \gamma_k + \nu_{i,t} \tag{5}$$

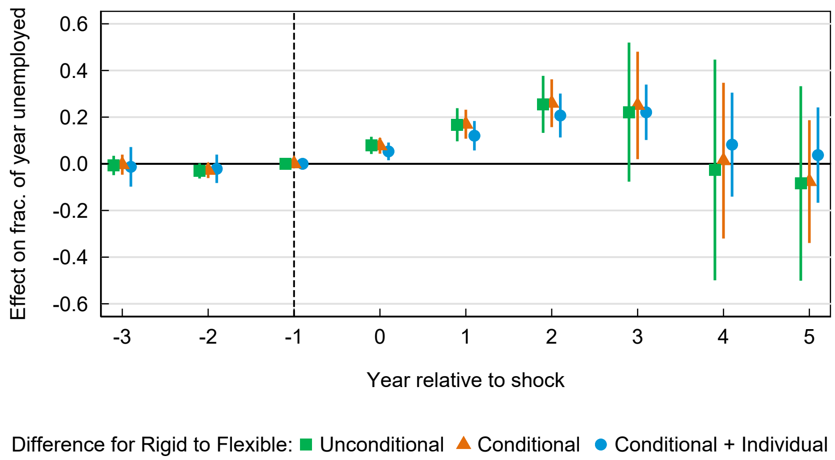
where $u_{i,t}$ is the fraction of the year that the individual spends registered with the unemployment agency, X_{t-1} are aggregate controls as in Equation 4, and Z_i are individual-level demographic controls to capture trends in labor market outcomes by demographics (10-year age bins, gender, an immigration dummy, and dummies for 4 education levels). We focus on an individual-level specification with unemployment as the outcome as we are interested in quantifying the contribution of wage rigidities to overall unemployment,

[18]This data is a combination of several administrative datasets from which we construct a sample of workers attached to the labor market. We describe the cleaning and sample selection process in Section A.1.1.

[19]Whether a worker is blue or white collar is registered for a subset of workers, where large firms are over-represented. Since the wage contract depends on this distinction, we are only able to match workers to wage contracts for a subsample of the domestic non-exporting sample, described in Section A.1.1.

[20]Flexible contracts come in many forms. For example, some do not stipulate wage growth and some stipulate lump sum payments that firms can choose how to allocate across workers.

Figure 8: Unemployment by sectoral nominal wage rigidities



Notes: The coefficient plots the difference in the effect of the monetary contraction after two years on fraction of year unemployed for workers in sectors with rigid, relative to flexible contracts. We use 3-digit NACE codes to classify sectors and use the average rigidity of individual contracts in the sector. Squares are the baseline unconditional estimates, using aggregate and demographic controls following Equation 5. Triangles show the effect adding interacted controls for worker age, education, tenure within firm, firm size, firm age, and firm debt to asset ratio. In circles, we additionally control for the workers' individual contact. All controls are binned, see Appendix Table A7 for details. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the individual and year level.

rather than the effect they have on the employment of an individual firm. Our quantity of interest is $\beta_k^{\text{rigid}} - \beta_k^{\text{flexible}}$, which represents the k-year-later effect of monetary policy shocks on unemployment for individuals in sectors with comparatively more rigid wage contracts, relative to those in sectors with fewer rigid wage contracts.

Descriptively, we find that sectors with more rigid contracts had larger increases in unemployment in response to the monetary tightening. Specifically, the green squares in Figure 8 show the estimated difference in unemployment for workers in sectors with more rigid contracts, relative to those in sectors with less rigid contracts. Two years after the monetary shock, workers in sectors with more rigid contracts were on average 0.3 p.p. more likely to be unemployed than those in sectors with less rigid wage contracts.

Although the rigidity of the contract is not random and could correlate with other features of the sector that may affect their sensitivity to the monetary shock, we find that these correlations are not driving the effects. Appendix Table A9 shows that while rigid and flexible sectors have observably similar workers in terms of their age, education, gender and income, firms in sectors with more rigid nominal wage contracts tend to be larger and more likely to produce goods, rather than services.[21] The orange triangles in

[21] Interestingly, these heterogeneity patterns for wage rigidity stand in contrast to heterogeneity in price setting, where durable

Figure 8 show estimates of Equation 5 where we additionally include controls for several worker and firm characteristics that could both independently affect the worker's exposure to the shock and be correlated with the average wage rigidity of the sector in which the worker is employed. These controls are included in the same manner as sector rigidity, meaning they are included separately, interacted with the control variables, and interacted with the monetary shock. We find that these observable differences do not explain the increased sensitivity of sectors with rigid labor contracts. Indeed, the estimates are remarkably stable, supporting the causal interpretation of the sectoral differences (Altonji et al., 2005). Moreover, we also find in Appendix Figure A13 that the patterns are similar, and even slightly larger when we include 2-digit sector-by-year fixed effects, which sweep out many differences stemming from different demand sensitivities across sectors.

The magnitude of these estimates implies that wage rigidities of this sort meaningfully contributed to the rise in unemployment. Rigid sectors have twice as many workers subject to rigid contracts as flexible sectors (99.7 percent of workers rigid sectors have rigid wage contracts while 46 percent of workers in the less rigid sectors have rigid contracts, see Appendix Table A9). A simple back of the envelope using the estimates in Figure 8 implies that without union wage rigidity of this sort, unemployment among this sample would have risen by 0.6 percentage points, instead of the 1.2 percentage points that it did in reality (see Appendix Figure A9 for the aggregate effects on unemployment in this sample).

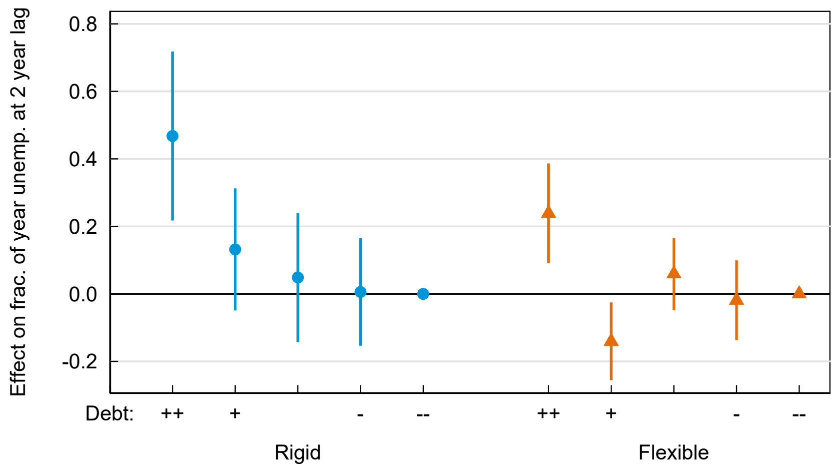
What is the mechanism through which more rigid nominal wage contracts lead to more unemployment following the monetary contraction? These rigid nominal wage contracts stipulated wage growth for workers that was higher than was warranted given economic conditions in 2012 and 2013. Indeed, we find that 2 years after the tightening, monthly earnings growth for workers in sectors with rigid contracts were approximately 0.5 percentage points higher than those of otherwise similar workers in sectors with less rigid contracts (see Appendix Table A8).[22]

We find evidence consistent with two broad mechanisms through which these higher wages led to unemployment. First, we find that nominal wage rigidities amplified firm-level differences in exposure to

goods producing firms reset prices more often (Bils and Klenow, 2004).

[22]Anecdotal evidence strongly suggests that these wage contracts were binding. For example, in the fall of 2012, multiple local employer organizations asked for local adjustments to the sector contract that was signed in the last months of 2011 or early 2012. The employer organizations were mainly asking to be allowed to reduce hours and wages (a so-called "crisis agreement" where hours had to be reduced by more than wages, so the hourly wage was unchanged or increased, but labor costs substantially reduced). These requests were denied (for details, see https://arbetet.se/2012/09/28/inga-lokala-krisavtal-for-if-metall/, retrieved 04/28/2022).

Figure 9: Firm-level wage rigidity and and firm short-term debt exposure



Notes: Coefficients plot the interactive effect of union contracts and short-term debt, relative to the lowest debt quintile by each contract type. Contracts are calculated at the firm level. Estimates are conditional, controlling for firm and individual characteristics as in the orange estimates in Figure 8. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the individual and year levels.

interest rate movements. We can see these patterns in Figure 9, which shows the unemployment response for each quintile of the firm short-term debt to asset distribution, separately for firms with more rigid and less rigid contracts. We find that the slope of the unemployment response with respect to initial firm short-term debt is half as large for firms with workers on more flexible contracts compared to those with more rigid contracts, suggesting that higher firm debt translates to higher unemployment most when wages are rigid. This finding suggests an important interaction between firm balance sheets and the allocative effects of wage rigidity as in Schoefer (2021).

Second, we find suggestive evidence that sector-level nominal wage rigidities affect unemployment by congesting the labor market and depressing hires. Specifically, in Figure 8, the blue circles show the effects of working in a sector with more wage rigidity, after controlling for the rigidity of the worker's own contract. The estimates imply that average sector rigidity predicts unemployment even for workers on flexible contracts in that sector. Additionally, we show in Appendix Figure A14 that individuals who become unemployed in sectors with more rigid nominal wage contracts spend more time unemployed, and are more likely to switch sectors of employment. We do *not* find similar patterns on job finding for workers who separate from firms that on average have more rigid labor market contracts, consistent with the effects being driven by overall market congestion rather than spillovers within the firm.[23]

Overall, these patterns suggest that nominal wage rigidities of this sort amplify the effect of interest rate movements on unemployment. This finding supports a wide class of models, where nominal wage rigidities are a key driver of monetary non-neutrality.

6 Incidence of monetary tightening in the labor market

In this final section, we turn to documenting heterogeneity in the incidence of the monetary policy shock across workers in the labor market. Differences in the effects of monetary policy are important for determining whether monetary policy can stabilize business cycle shocks that have heterogeneous effects across groups. The incidence of monetary policy in the labor market is also a key element factoring into whether monetary policy narrows or widens inequality across groups (Coibion et al., 2017).

We start by measuring the effect of monetary policy tightening across the income distribution. Specifically, for the points labeled "Monetary Policy" in Figure 10, we divide workers into deciles based on their average monthly earnings position during 2006–09 and estimate the effect of the monetary tightening for each decile using a modified version of Equation 5 that replaces sector rigidity with individual earnings deciles. As in Equation 5, we control for aggregate business cycle factors (interacted with earnings decile) and individual-level demographics.

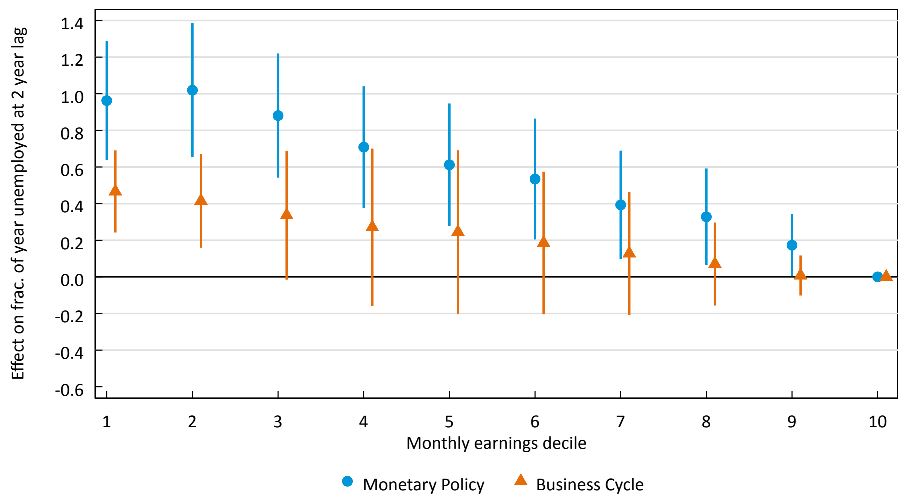
We find that monetary tightening has regressive effects on unemployment. Lower-earning workers see bigger increases in unemployment in response to the monetary tightening. As shown in Figure 10, we estimate that bottom-decile workers experience a 1 percentage point larger increase in unemployment compared to top-decile workers.[24] We find a close-to-linear decreasing gradient across deciles, with each decile experiencing about a 0.1 p.p. lower increase in unemployment than the decile below. These patterns are consistent with similar estimates for earnings inequality in Coibion et al. (2017); Holm et al. (2021); Moser et al. (2021); Amberg et al. (2022).[25]

[23] Variation in contract rigidity at the firm-level within sector stems primarily from differences in the skill composition across firms in a given sector. Additionally, some contracts have variation at the 5-digit sector rather than the 3-digit sector. Since sector in Figure 8 refers to the 3-digit sector, some of the firm variation within sector also includes differences in contracts across sub-sectors.

[24] In addition to heterogeneity on the extensive margin of employment, Appendix Figure A12 shows that even conditional on remaining employed, workers in the lower deciles of the earnings distribution lost more labor earnings than those in the upper part of the earnings distribution.

[25] In Appendix Figure A15, we show that the increase in inequality from monetary tightening stems from differences across workers, rather than differences in exposure across sectors or firms. We add to our specification either sector-by-time or firm-

Figure 10: Incidence across the earnings distribution



Notes: Estimates display the unemployment response for individuals at different positions in the earnings distribution, relative to the highest income bracket. Circles are the baseline unconditional estimates for a monetary tightening, using aggregate and demographic controls (Equation 5). Triangles shows the average unemployment response within each decile to a 1.989 p.p. increase in aggregate unemployment (elasticities computed from Equation 6). For "Monetary Policy", the monthly earnings decile refers to the mode position in 2006-09; for "Business Cycle" the decile refers to the mode of the four preceding years. Bars illustrate the 95% confidence interval with two-way clustered standard errors at the individual and year level.

Beyond inequality, the incidence of monetary policy is also important for understanding central banks' ability to stabilize heterogeneous margins of the labor market. Business cycle shocks generally have different effects on different groups in the labor market, as does monetary policy, and so it is important to determine whether these effects align. If the groups that respond most to business cycles also respond most to monetary policy, central banks can use monetary policy to stabilize employment for these groups without leading to distortions of employment among other groups.[26] Otherwise, the central bank would face a tradeoff between stabilizing employment for some groups while destabilizing employment for others.[27]

To examine whether the incidences of business cycles and monetary policy align, we use our administrative data to measure the heterogeneous response to the business cycle. Specifically, for each earnings decile, we measure the elasticity of unemployment in that decile to the aggregate unemployment rate:

by-time fixed effects, which isolate differences in exposure to monetary policy across the income distribution for workers in the same sector or firm (respectively). We find that the gradient across the income distribution is actually a bit larger within sectors or firms, implying that worker sorting across firms dampens the inequality in the labor market response.

[26]This is reminiscent of the "divine coincidence" result of Blanchard and Galí (2007) in which the central bank's actions are able to simultaneously achieve multiple objectives (unemployment and inflation) without a tradeoff.

[27]This type of tradeoff is similar to other tradeoffs studied in prior work, in particular the tradeoff between stabilizing unemployment versus labor force participation in Erceg and Levin (2014).

$$\Delta u_{i,t} = \sum_{\text{Deciles } d} \left[\beta_d \Delta U_t + \alpha_d\right] \cdot \mathbf{1}(i \in \text{Decile } d) + \nu_{i,t} \tag{6}$$

where $\Delta u_{i,t}$ is the change in the fraction of the year unemployed for individual i between year t and $t-1$, d indexes earnings deciles, and ΔU_t is the change in the aggregate unemployment rate.[28] The coefficients β_d measure the increase in unemployment for decile d in response to a 1 p.p. increase in aggregate unemployment, normalized relative to the top decile. We estimate this regression over the time period 1998–2009. These estimates descriptively capture the average degree of heterogeneity in exposure to fluctuations in the unemployment rate driven by a variety of business cycle shocks.

In Figure 10, the points labeled "Business Cycle" show estimates from Equation 6. For ease of comparison with our estimates of the effects of monetary tightening, we report $\beta_d \times 1.989$, since our baseline estimates of the effects of monetary policy imply a 1.989 p.p. increase in aggregate unemployment (see Appendix Table A1). We find that an increase in aggregate unemployment, scaled appropriately, leads to an 0.5 p.p. greater increase in unemployment for the bottom decile relative to the top decile. Overall, this gradient is less steep than it is in the case of a monetary contraction, and so a monetary tightening will not perfectly offset a positive business cycle shock for each group simultaneously, even if it perfectly offsets on aggregate. This creates tradeoffs for central banks in stabilizing the effects of business cycle shocks across different groups in the labor market, adding to the complexities that heterogeneity adds to optimal policy, such as those highlighted by Rubbo (2020) and La'O and Tahbaz-Salehi (2022) in multi-sector economies. Additionally, this asymmetry might make the central bank hesitant to tighten in a hot labor market, to avoid disproportionately affecting lower-income and marginalized groups of workers (Okun, 1973; Aaronson et al., 2019). Overall, our results indicate that monetary policy has important implications for inequality, and this pattern may complicate the optimal conduct of policy.

7 Conclusion

This paper introduces a case study of a large monetary shock in a modern economy, building on the tradition of historical case studies and narrative analysis. Our setting is Sweden, where the Riksbank raised the interest rate quickly from 2010–2011. We argue that this contraction represented a temporary devia-

[28]Earnings decile in year t is the modal position in year $t-4$ to $t-1$.

tion from the monetary rule that was driven by political shifts within the Riksbank and was unrelated to the state of the labor market. We support the identification of this monetary episode with narrative evidence from central banker statements at the time, identified monetary shocks, international placebos, and informative firm-level heterogeneity. We use this large, contractionary monetary policy episode—a rare occurrence—to identify the effects of rising interest rates on the economy. We find evidence for monetary non-neutrality, estimating large effects of the tightening on unemployment and other real variables.

Using microdata from the labor market and a unique institutional feature of Swedish unions, we find that nominal wage rigidities played a meaningful role in driving the response of unemployment, lending support to a wide class of models featuring this friction. Lastly, we also find that this monetary contraction was not felt evenly throughout the economy, but instead increased unemployment the most at the bottom of the income distribution. These differences across the income distribution are larger than those observed during a typical business cycle, suggesting that the central bank will generally struggle to stabilize employment for all groups simultaneously.

The case study approach is a helpful complement to other approaches empirically studying the effects of monetary policy elsewhere in the literature. Case studies of large monetary shocks are simple to interpret and offer clear identification of the effects of monetary policy. Other methods tend to rely on structural assumptions (e.g. VAR) or extracting relatively small deviations in the policy rate (e.g. high-frequency shocks or typical Romer and Romer (2004) shocks), which may be underpowered to reliably detect effects for real outcomes like unemployment. While the event study approach relies on its own set of assumptions, which we do our best to test and verify, we see value in its simplicity and size. Overall, we think this narrative-based identification, which builds on the tradition set forth in Romer and Romer (1989), complements other estimates in a literature that has sought to find creative ways to identify monetary shocks.

While our focus in this paper was on introducing the Swedish case study and understanding the effects on the labor market, this setting could be used to explore many other questions about the effects of monetary policy, including, for example, the importance of banks, household balance sheets, or expenditure switching. We leave these questions to future research.

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A Appendix

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" (RAMS), 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 "Longitudinal Integrated Database for Health Insurance and Labor Market Studies" (LISA), we extract information on an individual's background characteristics (gender, age, education, immigration status[29]) as well as the number of days they are registered as unemployed over the calendar year. 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 "Företagens ekonomi" (FE).[30] Lastly, we combine these data with information on export values and export destinations from the VAT-based trade data for goods "Utrikeshandeln". 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 4, 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 Section 5 and Section 6, 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 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,

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

[30] 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.

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 4, Figure 6, panel (b), we include firms that are in the domestic sample for the individual-level analysis.

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 4, 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. The following section documents how we collected this data for each country.

Norway: We collected quarterly GDP, employment, unemployment, investment, and inflation from Haver Analytics. We collected quarterly vacancy data from 1955–2019 from OECD, Main Economic Indicators. We proxy for layoffs using the number of workers unemployed for 1–4 weeks. This can be downloaded from Statistics Norway, Table 04552 and is available from 1996 - 2019.

Germany: We collected GDP, employment, unemployment, investment, vacancies, and inflation from 1996–2019 from Haver Analytics. We fill in historical data on the job-separation rate (i.e. the layoff rate) using data provided by Barnichon and Garda (2016) from 1987–2012. Specifically, before 2007, the layoff rate we use is the quarterly job separation rate calculated in Barnichon and Garda (2016) and post-2007, we use the vacancy 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 0.9.

France: We collected GDP, unemployment, investment, and inflation from 1996–2019 from Haver Analytics. We collect quarterly employment information from Eurostat (ESA 2010), which gives total quarterly employment using the national concept of total employment. Monthly data on new job openings are available from 1989–2019 from the OECD, Main Economic Indicators, available for download on FRED. 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 (Table LFSQ_UGAD). We fill in historical data on the job-separation rate (i.e. the layoff rate) using data provided by Barnichon and Garda (2016) from 1978–2012. Specifically, before 2003, the layoff rate we use is the quarterly job separation rate calculated in Barnichon and Garda (2016) and post-2003, we use the vacancy 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, investment, vacancies, and inflation from 1996–2019 from Haver Analytics. We collected data on layoffs ("redundencies") from the Office of National Statistics (Table RED01), which has monthly data on the counts of redundancies from from 1995–2019. We aggregate to the quarterly data taking the sum across quarters.

US: We collected quarterly GDP, employment, unemployment, investment, and inflation from 1996–2019 from Haver Analytics. We proxy for layoffs using the number of workers unemployed for less than 1 month. This is produced monthly by the Bureau of Labor Statistics from 1948–2019 and can be accessed through FRED. Data on job vacancies from 2001–2019 comes from JOLTS, which can also be downloaded from FRED. We combine this with historical data on job vacancies in the US in the help-wanted index, constructed in Barnichon and Garda (2016). 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, vacancies, investment, and inflation from 1996–2019 from Haver Analytics. We were unable to find historical data on layoffs or short-term unemployment in Finland. Instead, we proxy for layoffs using a measure from the Statistics Finland Consumer Confidence Survey, accessed through Haver Analytics, in which respondents are asked what their personal risk of unemployment is at the moment.

Iceland: We collected quarterly GDP, employment, unemployment, investment, and inflation from 1996–2019 from Haver Analytics. We also collected data on the number of short-term unemployed from 2004–2019 from Haver analytics. We collected quarterly vacancy data from 2001–2019 from Statistics Iceland.

A.2 Details of monetary shock construction

Data on the Riksbank's internal forecasts are published on their website. 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.3 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, 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, $GDP_{m,y}$ and $\pi_{m,y}$ and 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. 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.

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 Appendix Figure A4, 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 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.4 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:

$$y_{t+k} - y_{t-1} = X'_{t-1}\alpha_k + \varepsilon_{t,k} \tag{A2}$$

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.

Appendix Figure A6 shows the estimated forecast errors for the quarters leading up to and following 2010:Q3. 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 percentage points, before gradually easing. These results mirror our baseline event study estimates. Additionally, in Figure 3 we plot the counterfactual path formed by the predicted values from Equation A2, which closely aligns with the counterfactual path implied by our event study estimates.

A.5 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:

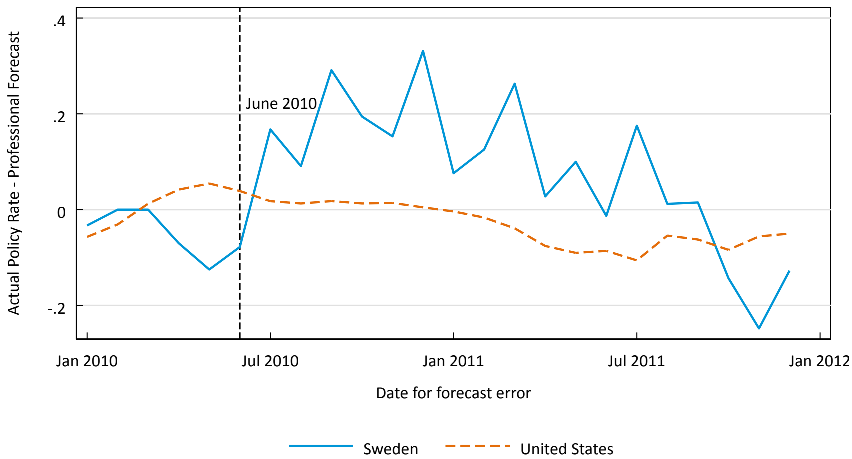
$$y_{i,t+k} - y_{i,t-1} = \beta_k \widehat{RR}_t + X'_{t-1}\alpha_k + Z'_i \gamma_k + \epsilon_{i,t,k} \quad \text{(A3)}$$

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 time-varying 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 Appendix Figure A9. We see that these results look similar, although smaller in magnitude, to those using the aggregate unemployment rate in Figure 4: a 1 percentage point increase in the interest rate caused a 1.3 percentage point increase in the fraction of the year that workers are unemployed 3 years after the shock. These patterns are echoed in panel (b) of Appendix Figure A9, 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 almost 4 percentage points 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, including individual fixed effects, adding controls, or modifying our estimation of the Romer and Romer shocks as discussed in Section 2.3 (see Appendix Table A6).

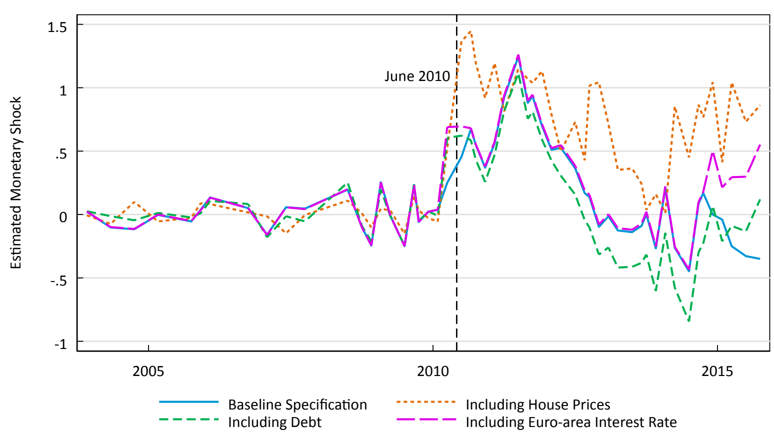
A.6 Appendix Tables and Figures

Appendix Figure A1: Professional forecaster expectations for the policy rate



Notes: Data on expectations for Sweden comes from reports published by Prospera Research AB. Prospera Research AB is commissioned by the Riksbank to conduct surveys that collect information from market participants on their expectations for future wages, prices, and policy rates. Data on expectations for the US come from Bluechip Economic Indicators surveys. Each line plots the mean forecast for the policy rate 3 months in the future less the realized interest rate 3 months in the future. The black dotted line reflects June 2010, when the Riksbank first raised the repo rate. The units on the y-axis are percentage points.

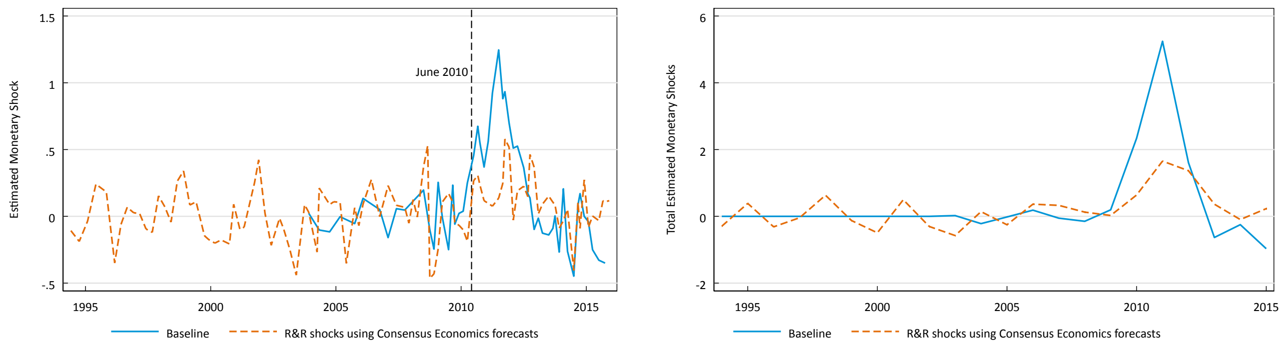
Appendix Figure A2: Alternate monetary policy shocks: adding additional controls



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

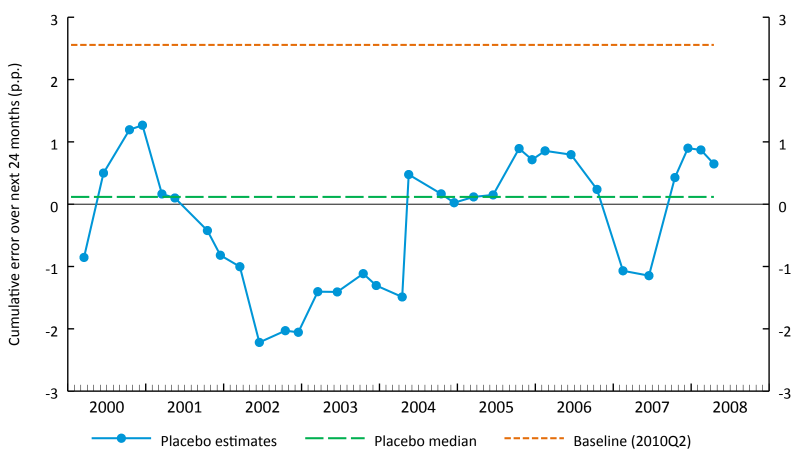
Appendix Figure A3: Monetary policy shocks using Consensus Economics forecasts

(a) Monthly (b) Annual



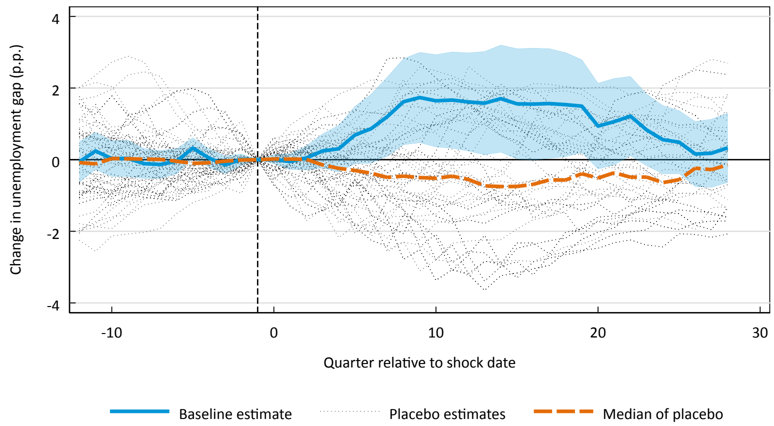
Notes: The left panel shows monetary policy shocks in the months of Riksbank meetings, and the right panel shows the total shock aggregated to the yearly level. In both panels, the blue solid lines show our baseline estimates described in Section 2.3, and the orange lines show the alternative shock series 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/.

Appendix Figure A4: Implied monetary policy shocks in other periods



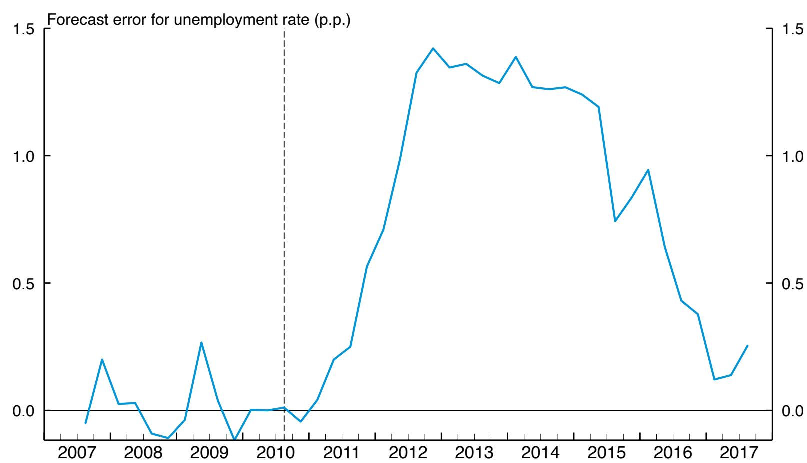
Notes: The graph shows estimates from a sequence of placebo regressions using Consensus Economics forecasts. For each Riksbank meeting date, we estimate Equation A1 using data up to the meeting date, use the estimated coefficients to construct residuals over the full data period, and then calculate the 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/.

Appendix Figure A5: 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 1996:Q2–2019:Q2 period, excluding dates between 2007:Q4 and 2012:Q4. 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.

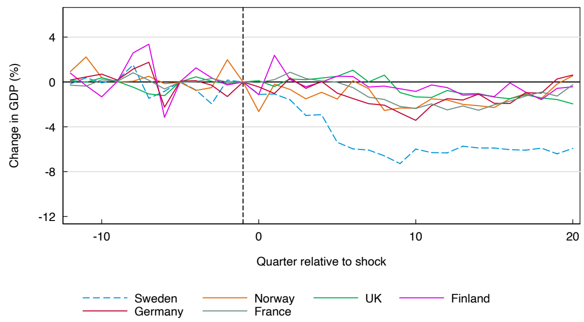
Appendix Figure A6: Robustness: Forecast error specification



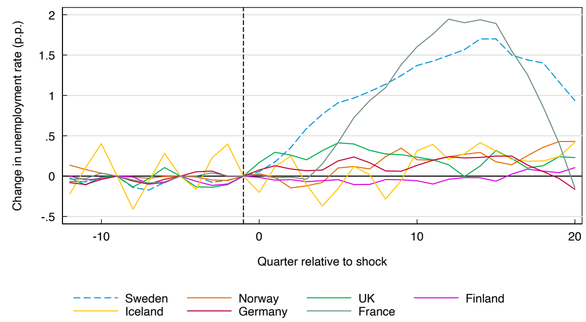
Notes: The blue solid line shows the forecast errors around 2010:Q3 estimated from Equation A2, described in Section A.4. Controls include the 1, 5, 9, and 13th quarter lags of year-over-year percent change in GDP, as well as the year-over-year percentage point changes in the vacancy rate and layoff rate. The sample includes quarterly data from 1996Q1 to 2019Q2.

Appendix Figure A7: Robustness of international placebo estimates

(a) Effects on GDP



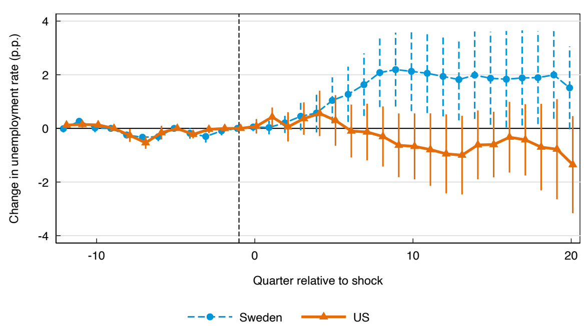
(b) Romer and Romer Refinement



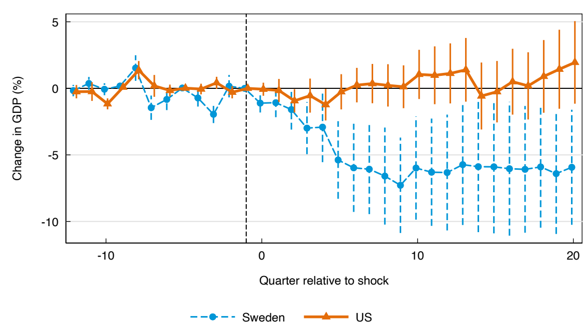
Notes: The left panel shows the coefficients estimated from the set of local projections regressions described by Equation 2, using GDP as the dependent variable. Controls include the 1, 5, 9, and 13th quarter lags of year-over-year percent change in GDP, as well as the year-over-year percentage point 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.

Appendix Figure A8: International placebo estimates: United States

(a) Unemployment



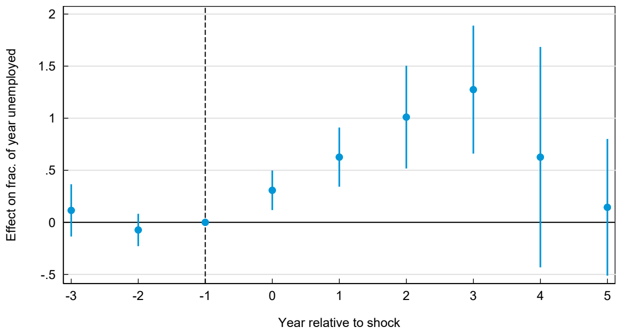
(b) GDP



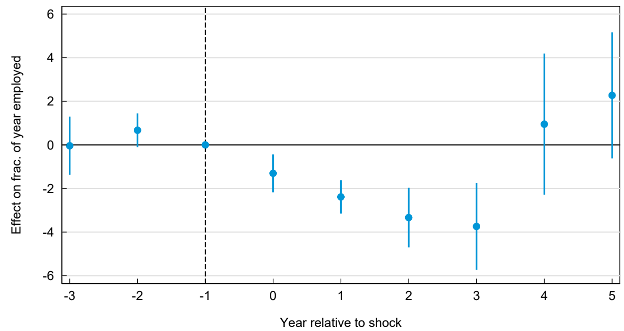
Notes: These plots show the coefficients estimated from the set of local projections regressions described by Equation 2, using data from the United States and using the unemployment rate as the dependent variable in the left panel and GDP as the dependent variable in the right panel. In both panels, 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.

Appendix Figure A9: Effect of monetary policy on individual-level outcomes

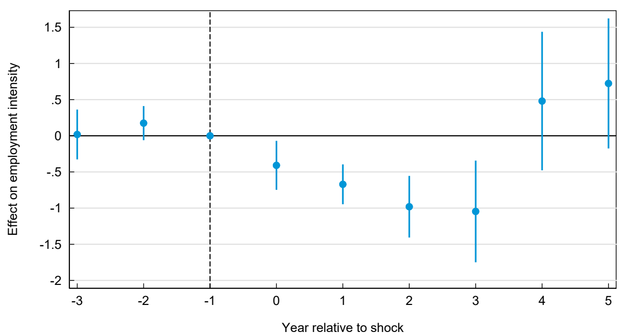
(a) Fraction of year unemployed



(b) Fraction of year employed



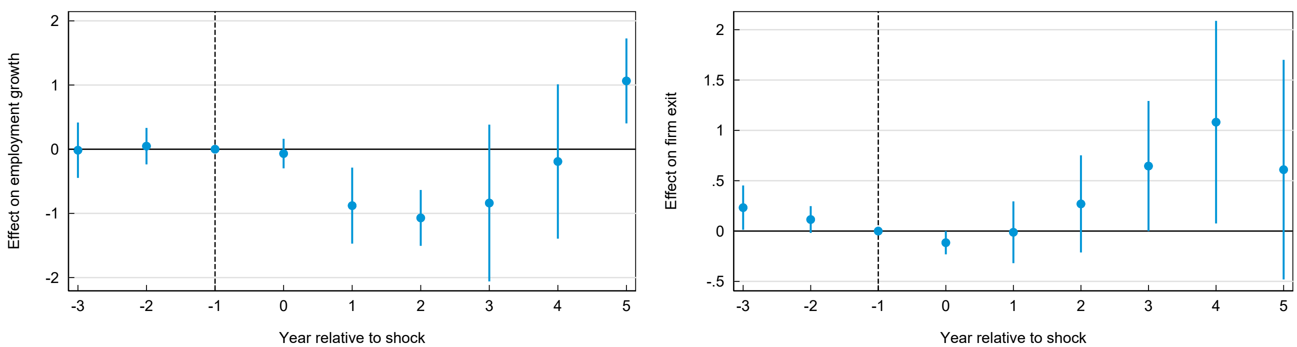
(c) Employment intensity among employed



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.

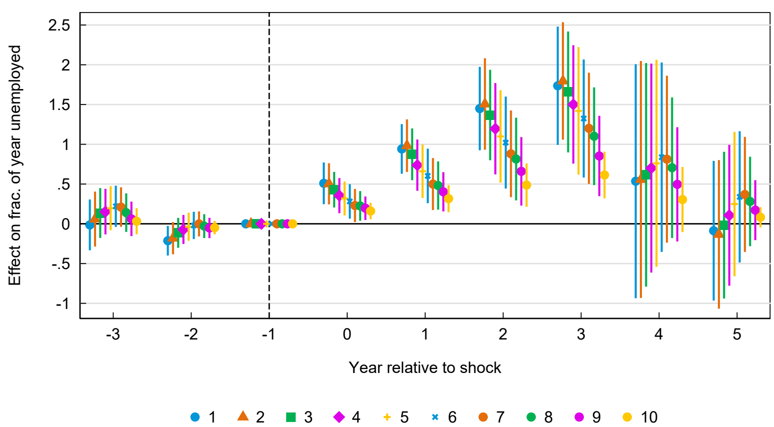
Appendix Figure A10: Effect of monetary contraction on firm-level outcomes

(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.

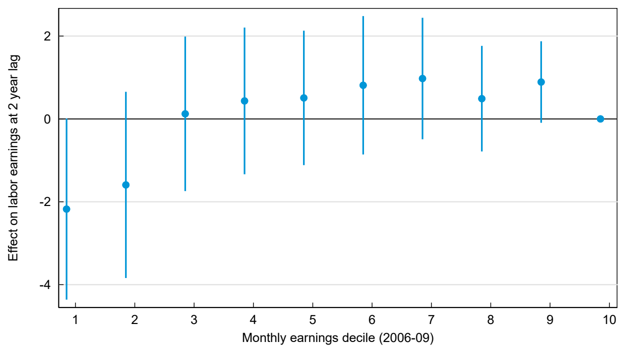
Appendix Figure A11: Unemployment across the earnings distribution



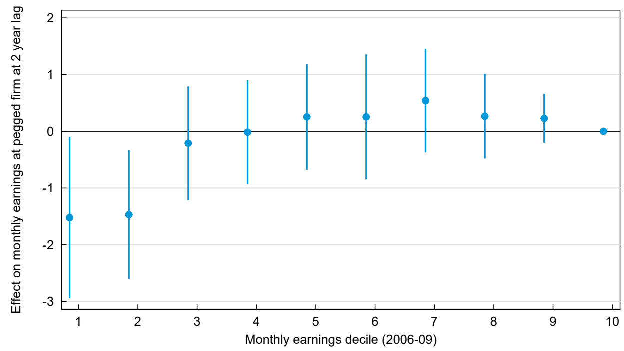
Notes: Regressions are as in Equation 5 estimated separately for each decile of the earnings distribution, with blue circles representing workers in the lowest decile. Workers are pegged to their mode earnings position for 2006–2009. Bars illustrate the 95% confidence interval with two-way clustered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

Appendix Figure A12: Earnings across the distribution

(a) Labor earnings

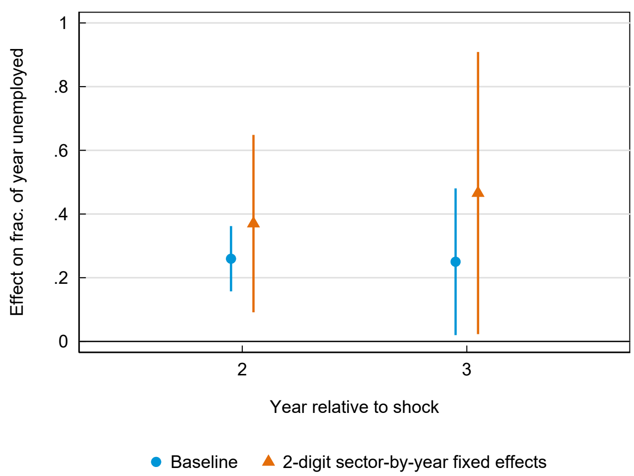


(b) Monthly earnings at initial firm



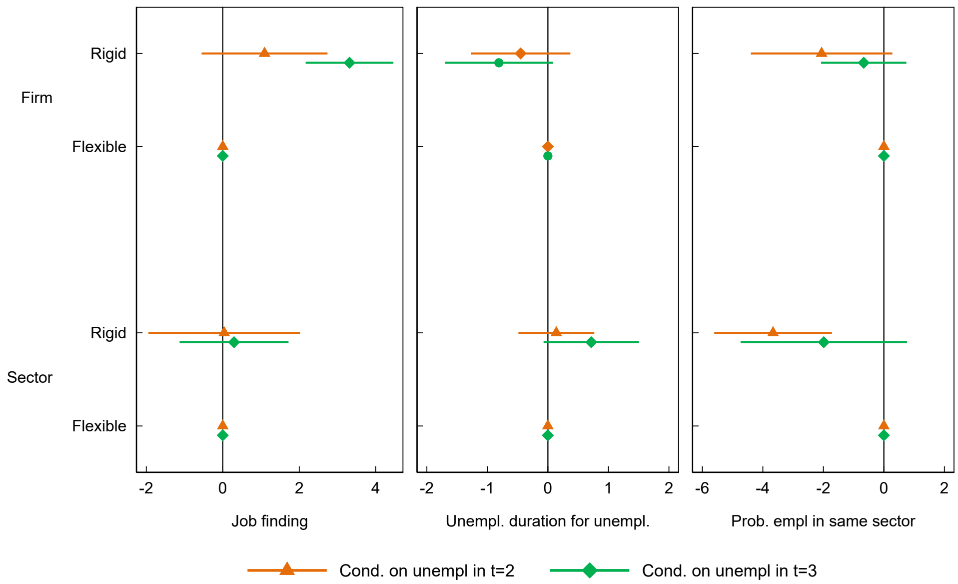
Notes: Regressions are as in Equation 5 estimated separately for each decile of the earnings distribution, with 1 representing workers in the lowest decile. Workers are pegged to their mode earnings position for 2006–2009. Coefficients plotted relative to the highest decile at the 2-year lag. Bars illustrate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

Appendix Figure A13: Unemployment by the rigidity of labor market contract: within 2-digit sectors



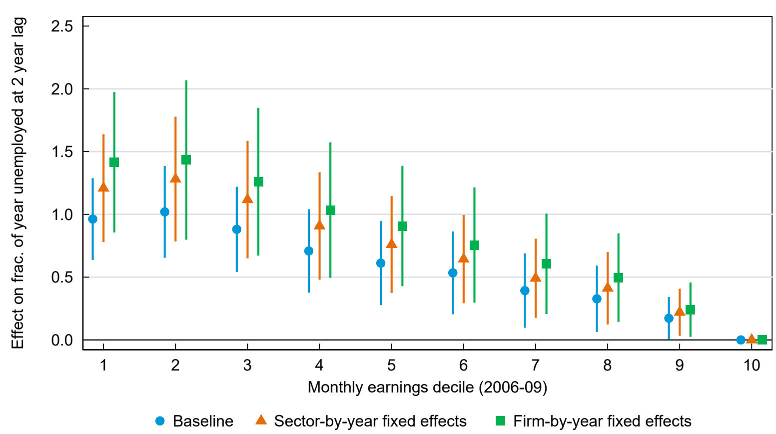
Notes: The coefficient plots the difference in the effect on the fraction of the year unemployed for workers with rigid, relative to flexible contracts. Baseline estimates refer to the conditional estimates shown in Figure 8. Regressions shown in orange triangles include 2-digit sector-by-year fixed 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.

Appendix Figure A14: 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.

Appendix Figure A15: 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.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β_{12}							
R&R shock, 2010-11 only	1.989		2.051				
	(0.589)		(0.602)				
R&R shock, full sample		1.752					
		(0.459)					
R&R shock, excl. 2010-11			0.550				
			(0.786)				
R&R shock, excl. GDP				1.609			
				(0.384)			
R&R shock using Consensus					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 only, zeroing out all other periods. Column (2) reports estimates using the full Romer and Romer (2004) shocks shown in Figure 1, where we impute the shock as 0 outside of the 2002–2015 period for which we have Riksbank forecasts to construct the shocks. Column (3) separates the Romer and Romer (2004) shocks into two terms: one for 2010–11 only, zeroed out in all other periods; and one for all other periods, zeroed out in 2010–11 (as in column 2, shocks are set to zero outside of 2002–2015). Column (4) reports estimates using shocks from the 'No GDP' column of Appendix Table A5. Column (5) uses shocks estimated from Consensus economics forecasts, described in Section A.3. Column (6) uses the quarterly change in the policy rate for 2010–11, zeroed out in all other periods. Column (7) uses the three-month ahead forecast error for the policy rate based on surveys of professional forecasters conducted by Prospera Research AB. 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.

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

	β_{12}	
1. Baseline	1.989	(0.589)
2. Newey-West standard errors (lag = 3)	1.989	(0.663)
3. Baseline controls + HPI	2.421	(0.795)
4. Baseline controls + debt per capita	1.497	(0.417)
5. Baseline controls + CPIF inflation	1.809	(0.624)
6. Baseline controls + unemployment rate	2.258	(0.636)
7. Baseline controls + exports	2.101	(0.631)
8. Baseline controls + avg. euro area 3-mth interbank rate	1.532	(0.580)
9. Baseline controls + trade-weighted foreign GDP	1.986	(0.539)
10. Baseline controls + Euro-area unemployment rate (lagged)	2.061	(0.669)
11. Baseline controls + Euro-area unemployment rate (contemporaneous)	1.671	(0.531)

Notes: This table shows estimates of Equation 3 for horizon $k = 12$ for alternative specifications in each row. Row (1) reports the baseline estimates. Row (2) reports the same estimate using Newey-West standard errors (with a bandwidth of 3). Rows (3)–(10) each adds an additional lagged control on top of the baseline business cycle controls. Row (3) adds the year-over-year percent change in house prices. Row (4) adds the year-over-year change in the debt per capita ratio. Row (5) adds the four-quarter percentage point change in the CPIF rate (core consumer price inflation, CPI, net of the direct effect of interest rate expenses). Row (6) adds the four-quarter percentage point change in the unemployment rate. Row (7) adds the year-over-year percent change in exports. Row (8) adds the year-over-year percentage point change in the average euro-area three-month interbank rate from the OECD. Row (9) adds the year-over-year percent change in average GDP of Sweden's trading partners, weighted by exports from Sweden, calculated by combining data from the bilateral World Trade Flows database and the OECD Main Economic Indicators. Row (10) adds the lagged year-over-year percentage point change in the average euro-area unemployment rate. Row (11) includes this same term, but defined contemporaneously in each local projection regression rather than lagged. 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.

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
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. 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.

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	13.99	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	0.38	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
No. of Employees			1319.04	3183.32	167.00	596.97
Sales Value (Mil. SEK)			4298	13680	382	2096
Labor Market Outcomes						
Frac. of Year in Unemp.	0.04	0.13	0.02	0.09	0.02	0.10
Unemp. $\geq$91 days	0.06	0.23	0.03	0.17	0.04	0.18
Frac. of Year Employed	0.65	0.46	0.89	0.30	0.87	0.31
Empl. $\geq$ 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.

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

	Baseline	House prices	Debt	Euro interest rate
r_m	0.437*	0.106	0.439*	0.424
	(0.212)	(0.167)	(0.215)	(0.402)
$\pi_{m,i}$	0.125	0.321	0.104	0.131
	(0.334)	(0.221)	(0.341)	(0.391)
$\pi_{m,i+1}$	-0.440	0.343	-0.416	-0.445
	(0.355)	(0.325)	(0.362)	(0.404)
$\pi_{m,i+2}$	0.351	-0.086	0.326	0.351
	(0.266)	(0.213)	(0.272)	(0.287)
$\pi_{t,i-1}$	-0.435*	-0.625***	-0.461*	-0.437*
	(0.186)	(0.131)	(0.191)	(0.209)
$u_{m,i}$	-1.794	-1.593*	-1.696	-1.767
	(1.235)	(0.789)	(1.261)	(1.497)
$u_{m,i+1}$	1.816	2.086*	2.111	1.813
	(1.593)	(1.018)	(1.655)	(1.722)
$u_{m,i+2}$	-0.545	-0.965	-0.888	-0.556
	(0.854)	(0.558)	(0.952)	(0.958)
$u_{m,i-1}$	0.630	0.167	0.520	0.616
	(0.601)	(0.407)	(0.624)	(0.745)
$GDP_{m,i}$	0.087	-0.172	0.080	0.087
	(0.096)	(0.099)	(0.098)	(0.104)
$GDP_{m,i+1}$	-0.045	-0.049	-0.074	-0.042
	(0.244)	(0.155)	(0.250)	(0.276)
$GDP_{m,i+2}$	-0.003	0.359	0.026	-0.007
	(0.311)	(0.226)	(0.318)	(0.349)
$GDP_{m,i-1}$	0.019	-0.082	0.024	0.018
	(0.080)	(0.059)	(0.081)	(0.090)
Change in house prices		-0.215**		
		(0.064)		
Debt per capita			0.018	
			(0.020)	
Euro-area interest rate				0.013
				(0.331)
Constant	-1.414	1.704	-1.811	-1.409
	(1.345)	(1.264)	(1.441)	(1.457)
Observations	21	21	21	21
R-Squared	0.925	0.974	0.934	0.925

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.

Appendix Table A6: Robustness of estimates to specification within microdata

	1 Baseline	2 + Self Empl.	3 + Exporters	4 Ind. fe	5 Full	6 Driscoll-Kraay SEs
2-year	1.011	0.996	0.950	1.003	0.787	1.011
	(0.252)	(0.248)	(0.214)	(0.253)	(0.0933)	(0.00548)
Observations	10,013,930	10,666,671	24,905,371	10,013,930	7,735,584	10,013,930
3-year	1.275	1.251	1.155	1.269	0.577	1.275
	(0.313)	(0.310)	(0.285)	(0.315)	(0.225)	(0.00690)
Observations	9,406,303	10,018,361	23,400,183	9,406,293	7,127,778	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. Estimates in columns (1) through (4) and (6) have Romer & Romer shocks estimated for 2010–2011. Column (5) shows a specification using Romer & Romer shocks estimated for 2002–2015 (no zeros) and limits the estimation period to 2002–2015. Estimates in the first and second row are from separate regressions for each outcome and year. Column (1) to (5), two-way clustered standard errors at the individual and year level in parentheses. Column (6) has Driscoll-Kraay (3 lags) standard errors for the baseline estimate in column (1).

Appendix Table A7: Heterogeneity final sample

A. Workers						
Age	(2)	(3)	(4)	(5)	(6)	(7)
Range	16 24	25 34	35 44	45 54	55 64	65 68
Obs.	110884	1963041	3418851	3392967	2640603	320209
No. of ind.	7547	103204	172427	134594	19898	170665
Education	(1)	(2)	(3)	(4)		
Range	(< High S.)	(Voc.)	(Short Ac.)	(Higher)		
Obs.	2313159	5199282	1977663	2356451		
No. 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	
No. of ind.	175412	99605	108704	104239	120375	

B. Firms					
Size (No. FTE)	(Few)	(Small)	(Middle)	(Large)	
Range	2 9	10 49	50 498	501 25832	
Obs.	4342000	4217651	2510251	776653	
No. of firms	87108	18699	1986	59	
First obs. (age)	(Young)	(Middle)	(Old)		
Range	2005 2009	1998 2004	1997 1997		
Obs.	1913400	3209730	6723425		
No. 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
No. of firms	17180	17137	20986	24309	28240

C. Contracts	C1. Individual		C2. Firm		C3. Sector	
	(C1. Rigid)	(C1. Flexible)	(C2. Rigid)	(C2. Flexible)	(C3. Rigid)	(C3. Flexible)
Obs.	1318514	1327257	1036116	1609655	4457151	6654288
No. of ind.	67775	67696				
No. of firms			3923	3884	43265	57978

D. Distributions										
Earnings Dist. (mode)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Obs.	975803	1045837	1036575	1068791	1121465	1178777	1258912	1345666	1394243	1420486
No. 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.

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

	A. Sector contract		B. Firm contract	
	(1)	(2)	(3)	(4)
	Unconditional	Conditional	Unconditional	Conditional
$\beta_k^{rigid} - \beta_k^{flexible}$				
2-year	0.690	0.455	0.797	0.745
	(0.135)	(0.145)	(0.304)	(0.289)
Observations	4,286,811	4,286,811	1,118,253	1,118,253
3-year	1.043	0.692	1.628	1.210
	(0.205)	(0.154)	(0.267)	(0.331)
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.

Appendix Table A9: 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	11.556	0.694	11.694	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
No 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		0.460	
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 5.