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

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Abstract

We analyze a quasi-experiment of monetary policy and the labor market in Sweden during 2010–2011, where the central bank raised the interest rate substantially while the economy was still recovering from the Great Recession. We argue that this tightening was a large, credible, and unexpected deviation from the central bank's historical policy rule. Using this shock and administrative unemployment and earnings records, we quantify the overall effect on the labor market, examine which workers and firms are most affected, and explore what these patterns imply for how monetary policy affects the labor market. We show that this shock increased unemployment broadly, but the increase in unemployment varied somewhat across different types of workers, with low-tenure workers in particular being highly affected, and less across different types of firms. Moreover, we find that the structure of the labor market amplified the effects of monetary policy, as workers in sectors with more rigid wage contracts saw larger increases in unemployment. These patterns support models in which monetary policy leads to general equilibrium changes in employment, mediated through the institutions of the labor market.

Keywords: Monetary Policy, Labor Markets, Quasi-Experiment
JEL Classification: E24, E52, E58

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1 Introduction

One of the main goals 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 effects of their actions on the labor market. However, estimating the effects of monetary policy on the labor market from observed changes in interest rates is complicated by endogeneity—policymakers typically only change interest rates in response to changes in economic conditions. Additionally, as central banks including the Federal Reserve have developed the capacity to monitor economic conditions in real time, unexpected deviations of interest rates have become small and infrequent (Ramey, 2016). Indeed, despite the large literature on the topic over the last several decades that have alleviated many of these concerns, Nakamura and Steinsson (2018) report that many prominent economists say that the most compelling evidence for monetary non-neutrality comes from historical case studies, such as the Great Depression, 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 analyze a large monetary policy shock in a modern economy and use it to study the effects of monetary policy on the labor market. 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-target unemployment. We argue that this increase represented a credible, partially unanticipated, and temporary deviation from the historical policy rule, owing to new emphasis that the Riksbank placed on concerns for financial stability (see also Svensson, 2011; 2018; 2019, for a discussion of this episode). Using estimates of the monetary policy shocks created by this deviation, we show that the contractionary shock raised unemployment substantially. Next, we turn to administrative unemployment and earnings records to examine how this shock propagated through the labor market. Although this increase is broad-based, we do find meaningful heterogeneity in this effect, with low-tenure workers and those in firms with high-debt particularly exposed. Lastly, we show that the structure of labor market contracts amplifies the effects of monetary policy in the labor market, highlighting the importance of nominal wage rigidities alongside general equilibrium changes in employment for monetary non-neutrality.

Our paper focuses on a single episode of a steep monetary tightening in a small open economy. While this shock is not the typical monetary shock examined elsewhere in the literature, it is uniquely suited to estimate the effects of raising interest rates from an effective lower bound when the economy is still far from trend. This type of interest rate "liftoff" has become a key subject of study in the monetary policy literature as central banks around the world have experienced interest rates near or at a lower bound following recent recessions (Eggertsson and Woodford, 2003; Werning, 2011). Moreover, while the shock that we study is specific to Sweden, the Swedish economy shares many similarities with other developed economies. Unlike many countries in Europe, Sweden has its own currency and controls its own monetary policy. Although it is much smaller and more reliant on exports than the United States, Sweden has a similarly dynamic labor market with a large domestic manufacturing sector.

To establish this episode as a monetary policy shock, we document four key aspects of the Riksbank tightening. First, we show that this tightening was inconsistent with the historical policy rule of the central bank estimated from their prior policy actions and the bank's own forecasts at the time. This deviation from the previous policy rule was the result of shifting views among some Riksbank board members that placed more weight on concerns about rising house prices and household debt. Second, although the tightening was communicated somewhat in advance, we show that market participants were still surprised by the increase in interest rates during this period. Third, the market believed the change to be credible rather than a temporary mistake that would be quickly reversed. Fourth, this tightening occurred in a low interest rate environment in a recovering economy, conditions in which monetary policy may have larger effects than usual.

We use this shock to answer three questions. First, we identify what effects the monetary policy shock had on the aggregate labor market to test monetary non-neutrality. Second, we examine how the response to monetary policy varied at a micro-level across the labor market and determine which firm and worker characteristics account for this variation. Third, we explore what these patterns reveal about the channels through which monetary policy affects the labor market.

We begin by identifying the aggregate effects of this shock. We use local projections regressions and estimates of the monetary policy shock from the Romer and Romer (2004) method to measure the effect on unemployment. We estimate that a 1 percentage point increase in interest rates leads to a 1–2 percentage point increase in the unemployment rate over the following 2–3 years. These estimates are at the upper end of range of estimates from previous literature, reflecting the fact that our baseline regressions focus solely on a large contraction (Coibion, 2012). We find smaller estimates when using a series of shocks covering a longer time period. Our main result is robust to accounting for differing sets of controls, including export growth, prices, Euro area interest rates, and foreign GDP. It is also robust to excluding multi-national and exporting firms, who were directly exposed to other shocks during this time including the Euro debt crisis. Moreover, we find that the monetary shock lowered output and investment and tempered inflation, in line with predictions of standard New Keynesian models (Galí, 2008).

Next, we turn to administrative micro data to examine how this response to monetary policy varied across workers and firms. We combine employer-employee data on longitudinal earnings histories, records of individuals receiving unemployment benefits, firm characteristics including balance sheets, and export records to create a sample of workers who were attached to domestic, non-exporting firms over 2006–2009. Using the detailed administrative records, we can divide this sample into groups by worker and firm characteristics to estimate the response to monetary policy separately within each group. We examine several splits of our sample, including factors such as firm size and indebtedness that have been proposed in the literature to be important in the transmission of monetary policy to the real economy, and others such as worker age that correlate with worker exposure to recessions (Hoynes et al., 2012).

We find that the effects of the monetary contraction were widespread overall. Almost no segment of the labor market that we consider was insulated from a rise in unemployment and the vast majority experienced increases of 0.5–1 percentage point. While the effects were broad-based, we do find heterogeneity in the response largely by worker characteristics. We find substantially larger effects for young workers and low-tenure workers, relative to older and higher-tenure workers, respectively. The increase in unemployment is also larger for workers at small firms, young firms, and firms with higher levels of short-term debt. These firm-level patterns are consistent with many mechanisms for the transmission of monetary policy proposed in the literature, but the differences across firms are relatively small.

Finally, these patterns shed light on how monetary policy affects the labor market. Several features of the labor market affect not only the incidence of the shock but also amplify the increase in unemployment overall. Specifically, we find that workers with more nominally rigid employment contracts see a larger increase in unemployment, and that even conditional on the individual's own contract, workers at firms and sectors with more rigid contracts see larger increases in unemployment. This market-level effect is consistent with congestion in the labor market, as workers in those more rigid sectors experience depressed job-finding rates and longer unemployment durations compared to workers in more flexible sectors. We also show similar patterns for measures of labor market churn—workers from firms or sectors with low churn are less affected by the shock, even conditional on their own tenure. The findings highlight the important role that the structure of the labor market plays in mediating the effects of monetary policy.

This paper contributes to several strands of literature. First, we contribute to the large and diverse literature on identifying monetary policy shocks and their effects, which has employed several common methods. 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). Romer and Romer (1989) instead used the narrative account to identify natural experiments in which the Federal Reserve intentionally exerted contractionary pressure on the economy. 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., 2021). 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). 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). The analysis in this paper falls into a final category of largely historical papers that focus on monetary shocks during several large episodes including the Great Depression or the Volker Disinflation in the US (Friedman and Schwartz, 1963; Velde, 2009). We add to this literature by providing a case study for a large monetary contraction in a modern economy.

In addition to the literature about the overall effects of monetary policy, there is also a long literature about *how* monetary policy works. Our results point to monetary policy shocks leading in general equilibrium to widespread changes in labor demand across all firms and workers, rather than effects being concentrated among particular interest-rate-sensitive groups. This general equilibrium change in demand is a key channel in Heterogeneous Agent New Keynesian (HANK) models, such as those found in Kaplan et al. (2018), Auclert (2019), and Flodén et al. (2017). Our estimates do indicate that some groups are more exposed to monetary policy than others and this may widen inequality, similar to the results of Coibion et al. (2017) and Holm et al. (2020). In addition, a set of recent papers share our focus on documenting heterogeneity in the labor market effects of monetary policy, generally suggesting that expansionary monetary shocks disproportionately increase the labor income of more fragile workers including minorities, those at the bottom of the income distribution, or those with low labor market attachment (Andersen et al., 2021; Bergman et al., 2020; Bartscher et al., 2021; Amberg et al., 2021; Doniger, 2019).

We also add to a literature highlighting the particular importance of nominal wage rigidities for 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. Our finding that workers with more rigid contracts experience more unemployment as a result of monetary policy shocks complements firm-level evidence on rigidity and employment from Card (1990), Ehrlich and Montes (2014), Kurmann and McEntarfer (2019), Murray (2019), and Olsson (2020). We go further by showing that, in addition to rigidity of a worker's own contract, higher average rigidity at the sectoral level also results in higher unemployment following a monetary policy shock. In this way, wage rigidity does not just determine which workers are laid off following a shock, but also increases the total response to the shock.

Lastly, in using linked worker-firm data, we also build on the literature exploring the role that firms play in transmitting monetary policy. This literature highlights the importance of firm finance and financial frictions in amplifying the effects of monetary policy. In particular, the literature has demonstrated that small and young firms are more responsive to monetary shocks (Gertler and Gilchrist, 1994; Cloyne et al., 2018), as are firms that are more dependent on bank debt (Ippolito et al., 2018) or those with low debt burdens overall (Ottonello and Winberry, 2018). Much of this firm-level literature has focused on the investment channel of monetary policy, and we bring to this evidence an exploration of the ways in which firm heterogeneity affects the transmission of monetary policy to labor income throughout the distribution.

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 describes the data we use in our analysis. Section 4 shows that this shock had large effects on unemployment and other macroeconomics outcomes. Section 5 explores heterogeneity in the incidence of the shock across workers and analyzes the importance of labor market structures for the transmission of the shock to workers. Section 6 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, credible, and unexpected deviation from the historical monetary rule, which occurred in an economy still in the early phase of a recovery.

2.1 The Swedish Economy Pre-2010

Unlike most other countries in the European Union, Sweden does not use the Euro as its official currency, using the Swedish krona instead. This enables the Riksbank to exercise its own monetary policy independently from the European Central Bank (ECB). The Riksbank typically follows a policy of flexible inflation targeting, which involves stabilizing both inflation and the real economy. As is laid out in the Riksbank's 2010 publication *Monetary Policy in Sweden* 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." 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 set both the current repo rate and extensive forward guidance of the likely path of the repo rate going forward.

Sweden has a dynamic labor market, not dissimilar from the United States on many dimensions. Before the Great Recession, about 7 percent of Sweden's workforce was employed in manufacturing, similar to the 10 percent of the US workforce in manufacturing over the same period. Workers in Sweden are unemployed slightly longer than those in the United States, with an average unemployment duration of 26 weeks compared to only 19 weeks in the United States. Annually, about 11 percent of workers in Sweden lost their jobs, a number identical to the United States over the same period, and another 30 percent transitioned employers, which is slightly more than the 23 percent of US workers making job-to-job transitions in any given year.

Like most developed economies, Sweden was deeply affected by the 2008 global financial crisis. That year Swedish exports and GDP contracted sharply, to which the Riksbank responded by dramatically cutting rates from 4.75% to 0.25%, in line with their typical policy. 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 percent and the unemployment rate fell 1 percentage point, indicating a strongly growing economy. Indeed, in an article in 2011, the Washington Post dubbed Sweden "the rockstar of the recovery".[1] However, while the growth rates in this period were impressive, Sweden was still very much recovering from a deep recession and economic activity was well below trend —in fact, 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 impressive growth that the Riksbank, in mid-2010, implemented a dramatic monetary tightening. 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 had to be addressed. While other regulatory bodies within Sweden had official responsibilities for those elements, 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 decision of the Riksbank to raise interest rates through this period was 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 flexible inflation targeting rule (Goodfriend and King, 2015; Svensson, 2011).

Starting in the middle of 2010, the Riksbank decided to raise the policy rate, going from 0.25% in June 2010 up to 2% by July 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 forecast undershoot the 2% inflation target by 0.5% or so for most of the forecast period, and unem-

[1] https://www.washingtonpost.com/business/economy/five-economic-lessons-from-sweden-the-rock-star-of-t 2011/06/21/AGyuJ3iH_story.html

ployment was forecast to remain above the 6 to 7 percent 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." He argued that this ideological shift was a problem, as "widespread research points to the policy rate being an unsuitable instrument for this, as it has small effects on house prices, but sizable effects on production, jobs and unemployment." (Svensson, 2010)

The dramatic shift in monetary policy was the main domestic economic policy enacted during this period. Aside from the automatic fiscal stabilizers that Sweden has, there is no mention of important fiscal responses mentioned either in the Riksbank statements during this time, nor in Goodfriend and King's extensive review of this period.[2] The incumbent political party was re-elected in the 2010 general election. Lastly, there were a couple of policies 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% and (see Annex 3 in (Goodfriend and King, 2015)).

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.

[2] In addition, we find very little effect of the monetary shock on aggregate fiscal spending, confirming these narrative accounts.

2.3 Four Aspects of This Quasi-Experiment

We establish four features of this monetary episode that are essential for understanding how it can be used to inform broader policy. In what follows, we argue that the 2010 monetary tightening created a credible, temporary deviation from the historical policy rule that was at least partially unanticipated by market participants and that occurred in an economy that was recovering from a deep recession.

1. The policy rate deviated from the previous policy rule 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. We follow the methodology of Romer and Romer (2004) to isolate deviations in monetary policy from the Riksbank's typical response to changing economic conditions, drawing on their own forecasts of the Swedish economy. Using the set of monetary policy decisions before the summer of 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. These regressions capture the historical response of the Riksbank to current economic information and their own expectations of future developments. The residuals of this regression reflect movements in the policy rate that were not consistent with the historical relationship between the actions of the Riksbank and the available information at the time.

Specifically, we estimate the following regression on data from the March 2002–February 2010 Riksbank meetings[3]:

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

where the unit of observation is the Riksbank policy meeting m, π is inflation, 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.[4] We construct the predicted change in the policy rate going forward as the fitted values of this regression and define

[3]Our dataset starts in March 2002 since the Riksbank did not consistently release their forecasts at each meeting prior to this point.

[4]See the appendix for alternate specifications and a discussion of how Equation 1 relates to the original Romer and Romer (2004) specification.

the experienced monetary shock as the residuals $\widehat{RR}_m = \epsilon_m$. These shocks capture the movement in the policy rate that was unexplained by the current forecasts of the Riksbank given the historical relationship between those forecasts and monetary policy decisions.

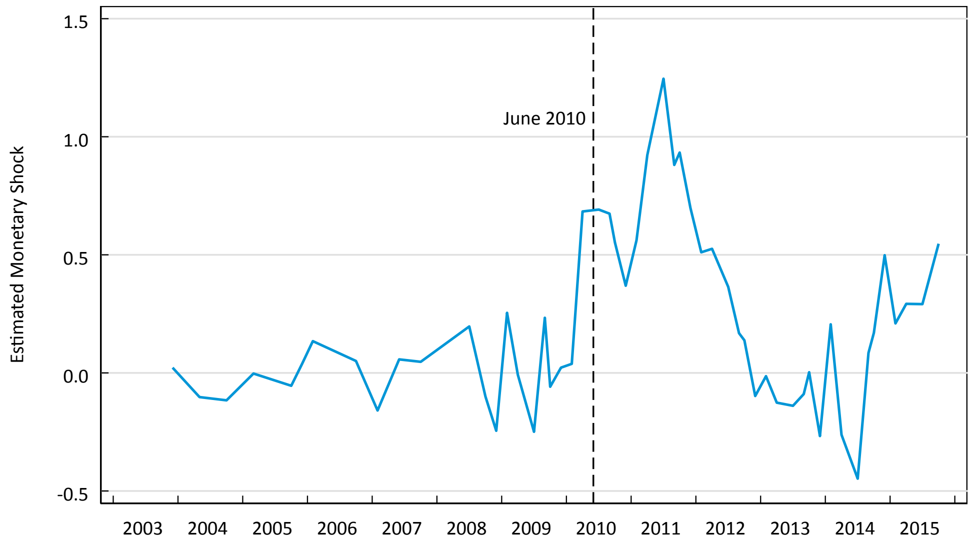
Figure 1 shows the resulting series of monetary shocks.[5] Unsurprisingly, since the monetary shocks are residuals from a linear regression estimated on data through the beginning of 2010, the estimated monetary shocks before mid-2010 are relatively small and centered around 0. However, from June 2010 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 far more than we would have expected given their forecasts at the time. 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.

Since we estimate Equation 1 on the pre-2010 sample, the positive monetary shocks that we uncover in the post-2010 period could either reflect a positive monetary shock under the old policy rule or it could reflect a temporary change in the monetary rule itself. In Appendix Figure A2, we show the resulting shocks from a version where we have included the change in house prices as an input to the policy rule in Equation 1. If the Riksbank had always considered house price growth in making their policy decisions and chose to raise the policy rate in mid-2010 in response to a rapid growth in house prices, then including this variable in Equation 1 should substantially shrink the estimated monetary shocks through 2010 and 2011. However, this is not the case—if anything, the estimated monetary shocks through this period are even larger with this specification.

2. Tightening was largely unexpected by market participants Not only was this tightening out of line with the Riksbank's previous responses to changing economic conditions, but it also appears that it was partially unanticipated at the time. We measure the extent of anticipation using data on Swedish private-sector forecasters' expectations for interest rates from reports published by Prospera Research AB, which is commissioned by the Riksbank to conduct surveys of market participants soliciting their economic forecasts. The solid blue line in Figure 2 shows the 3-month ahead forecast error for the Riksbank's policy rate among this sample of professional forecasters. While on average the forecasters did anticipate that the Riksbank would raise rates over this period, forecasters continually underestimated the speed of the tight-

[5]See Appendix Table A2 for the estimated parameters for Equation 1 as well as alternate specifications.

Figure 1: Estimated Monetary Policy Shocks

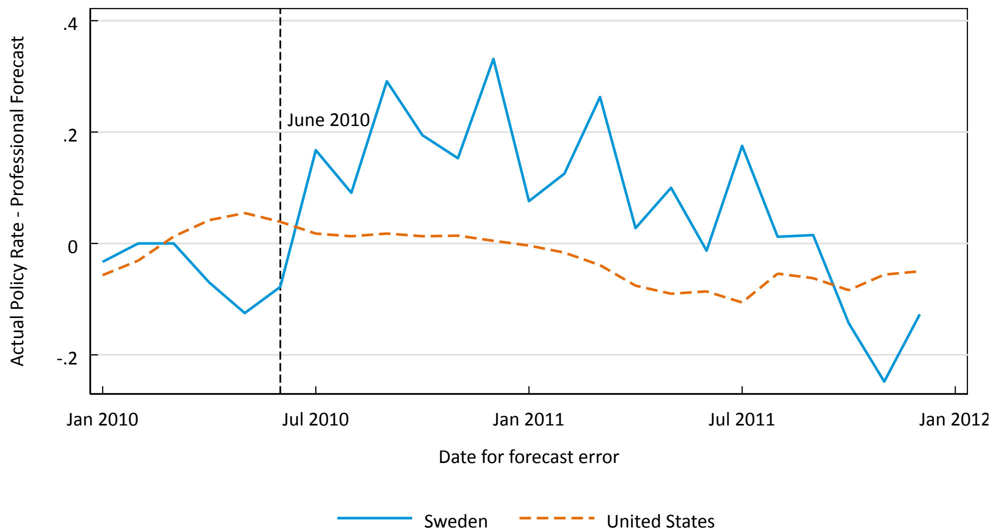


Notes: The blue solid line shows the residuals from Equation 1. The coefficients were estimated from the March 2002–February 2010 Riksbank meetings, and residuals are calculated for the March 2002–October 2015 period using these estimated coefficients.

ening by around 0.15 percentage points. For comparison, the dashed orange line shows the same forecast error in the United States during this period. U.S. forecast errors are consistently small, and even slightly negative in the later part of the sample. Moreover, this difference does not just reflect that forecasters in the U.S. are better on average than forecasters in Sweden—to the contrary, from 2007 to 2010, the absolute value of the average 3-month ahead forecast error was 0.3 in Sweden while it was 0.4 in the U.S.

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). These methods, common in the literature, identify monetary policy shocks using changes in the price of interest rate futures in the short window around the Central Bank's policy announcements (Gertler and Karadi, 2015; Nakamura and Steinsson, 2018). Thus, they isolate the change in the path of interest rates that was entirely unanticipated by financial markets at the time of announcements. The patterns in Figure 2 combined with the small estimated high-frequency shocks likely suggests that the Riksbank effectively communicated very short-run changes in the policy rate but that they had 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.

Figure 2: 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.

3. Higher interest rate policy was credible Another key element determining the impact of this monetary episode on the economy is the extent to which market participants thought this monetary shock would be permanent or transitory. For example, if market participants thought that the Riksbank was making a mistake in implementing this break in the rule, they would anticipate that the change was likely to be reversed quickly. A review of the narrative evidence suggests that this was not the case. 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).[6] 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.

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

4. Tightening occurred during a weak, but improving, labor market As we outlined in Section 2.2, this monetary shock occurred at the start of the recovery from the Great Recession. 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. 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 of 2010 was 8.8%. 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., 2018; Berger et al., 2018).

However, while the estimates from this episode may not equal the effect of an average tightening, 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 example captures the effect of a central bank making a different choice than that of the majority of central banks at the time, giving us the unique opportunity to provide estimates that inform this decision.

3 Data

3.1 Aggregate

We start our analysis with time series data covering the Swedish economy. For the labor market, we combine data on the unemployment rate from Statistics Sweden, the natural rate of unemployment estimated by the National Institute of Economic Research, and measures of new vacancies and layoffs from the Swedish Public Employment Service. We combine this with measures of real GDP, investment, exports, consumer price index (CPI and CPIF)[7], producer prices, and real estate prices from Statistics Sweden; measures of interest rates on consumer and housing loans from the Riksbank; and the average 3-month interbank rate for the Euro area from the OECD. We also calculate the average export-weighted foreign GDP for Sweden by combining data from the bilateral World Trade Flows database and the OECD Main Economic Indicators.

[7] The CPIF is calculated similarly to the CPI, but holds the interest rate for households' mortgage payments fixed.

3.2 Swedish Administrative Data

We combine several administrative Swedish datasets to create our baseline sample. Employers and employees are linked via "Register based labor market statistics" (RAMS), which is an administrative dataset with full coverage of the Swedish working population derived from annual labor earnings records for each employer-employee pair. In contrast to survey data, RAMS are based on tax filings directly reported to the Swedish authorities. 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 also extract information on an individual's background characteristics (gender, age, education, immigration status) 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).[8] Lastly, we combine these data with information on export values and export destinations from the VAT-based trade data for goods "Utrikeshandeln". All registers contain yearly observations from 1997 through 2016. Further, for some robustness analysis, we construct quarterly employment series using monthly employment indicators.[9] All of the above data is reported at the level of the domestic firm, rather than the local establishment.

From the full set of administrative records, we make a number of restrictions. 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. 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 for the firm in which they worked in 2009, the year preceding the monetary shock.[10] For our main analysis sample, we further restrict to only workers who were attached to domestic, non-exporting firms in 2009.[11]

[8] Financial firms from excluded.

[9] Workers are identified as employed in the quarter if registered with positive earnings at any firm for at least 2 months of that quarter, using information in RAMS.

[10] 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. (2017) and define the monthly minimum wage as the 10th percentile in the wage distribution in each year.

[11] Domestic firms are defined from the juridical form (ownership category). We define non-exporting firms as those who report no positive value for exports.

Table 1 shows basic summary statistics for the sample. Panel A shows the full sample of workers in these data. Panel B shows the same statistics for the set of workers that we are able to link to a firm between 2006 and 2009. This sample includes workers that are more attached to the labor force and therefore, have slightly higher earnings, but otherwise, they look similar to the full sample. The average firm in the sample has over 1000 employees and exports about 13% of their sales. Finally, panel C shows the set of workers at domestically owned non-exporting firms, which is the sample that we use for the majority of the analysis. These firms account 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. We make this additional restriction to isolate the firms most exposed to domestic monetary policy and assuage concerns that shocks outside of Sweden are driving the patterns.

Table 1: Summary Statistics for Micro Data Samples

	A. All workers		**B. Sample with Firm Link**		**C. Baseline Sample**	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Worker Characteristics						
Age	41.10	14.77	42.40	11.77	42.33	11.99
Female	0.49	0.50	0.29	0.45	0.31	0.46
Education	2.46	1.14	2.41	1.02	2.33	1.01
Immigrant	0.18	0.38	0.12	0.33	0.11	0.31
Firm Characteristics						
Export/Sales			0.13	7.67	0.00	0.00
Firm First Observed			1998.83	3.24	1999.68	3.80
No. of Employees			1319.69	3185.08	166.89	596.67
Sales Value (Milj. SEK)			4301	13690	381	2094
Labor Market Outcomes						
Frac. of Year in Unemp.	0.04	0.13	0.02	0.09	0.02	0.10
Frac. Unemp. $\geq$91 days	0.06	0.23	0.03	0.17	0.04	0.18
Frac. of Year Employed	0.62	0.47	0.89	0.30	0.87	0.31
Earnings $\geq 6\times$ min. wage	0.63	0.48	0.90	0.30	0.89	0.32
log Earnings	9.95	4.54	12.06	2.34	11.97	2.34
Observations	118,910,994		29,530,510		11,878,234	

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. Sample includes all years from 1997-2016. Education is recorded as 1 for individuals with less than high school, 2 for vocational high school, 3 for academic high school and shorter tertiary education, 4 for higher education.

4 Aggregate Effects of Monetary Policy

Having established the nature of this monetary episode, we next use local projection regressions to estimate the aggregate response to the shock. Using both aggregate data and administrative microdata, we find that the shock led to a 1–2 percentage point increase in unemployment. This result is not driven by exporting firms and is robust to alternative specifications and controls. Non-labor market outcomes also respond in standard ways.

4.1 Response in the Labor Market

We start by examining the response of the aggregate unemployment rate by estimating a set of local projection regressions of the form

$$\tilde{u}_{t+k} - \tilde{u}_{t-1} = \beta_k \widehat{RR}_t + X_t' \alpha + \epsilon_t \tag{2}$$

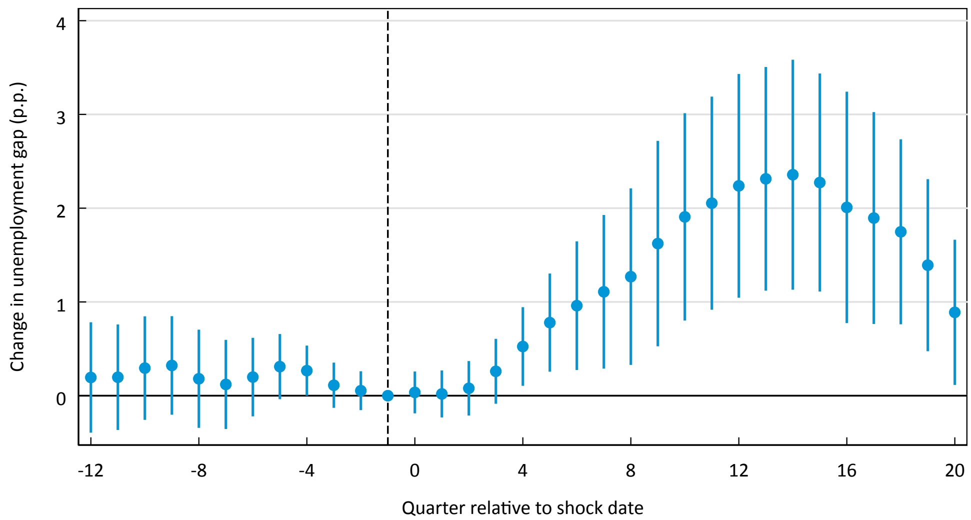
where $\tilde{u}_{t+k}$ is the unemployment gap—the unemployment rate minus the natural rate of unemployment—k quarters in the future, $\widehat{RR}_t$ is the estimated Romer and Romer (2004) shock, and X_t' are time-varying controls for other economic variables that may also affect the evolution of unemployment. In order to focus on this episode, we use only the baseline shocks from Figure 1 for the 2010–2011 episode of monetary tightening, setting the estimated shocks outside of that period to zero.[12] In the baseline specification, we include in X_t' the 1-quarter lagged 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 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.[13] We include in the estimation sample all quarters from 1996Q1–2019Q2.

Figure 3 plots the estimates of β_k. The effects of monetary policy are small initially, but then phase in over time and reach a peak three years after the shock with the unemployment rate increasing by 2 percentage points compared to its pre-shock level. This pattern of a slow increase in effects over 12 quarters

[12] Another motivation for this specification is that we are able to include the full sample in the estimation, rather than having restrict to the years for which we are able to construct the shocks. See Appendix Section A.3 for the effects using the full shock series. Results are similar qualitatively but noisier, likely due to the much diminished sample size and a blending of expansionary and contractionary shocks.

[13] See Appendix Section A.3 for a discussion of the robustness of the analysis to the inclusion of alternate control variables.

Figure 3: Effects of Monetary Policy on Aggregate Unemployment Rate



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 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. Sample includes quarterly data from 1996Q1 to 2019Q2. Bars illustrate the 95% confidence interval with heteroskedasticity-robust standard errors.

is consistent with previous evidence on monetary policy shocks (Ramey, 2016). We find no statistically significant deviations of unemployment before the shock date, consistent with the notion that the Riksbank's policy during this period was not primarily a response to labor market conditions. Appendix Table A3 shows that these patterns are robust to including alternate controls or different time periods and Appendix Figure A3 shows similar effects for other methods for calculating the Romer and Romer shocks as discussed above in Section 2.3.

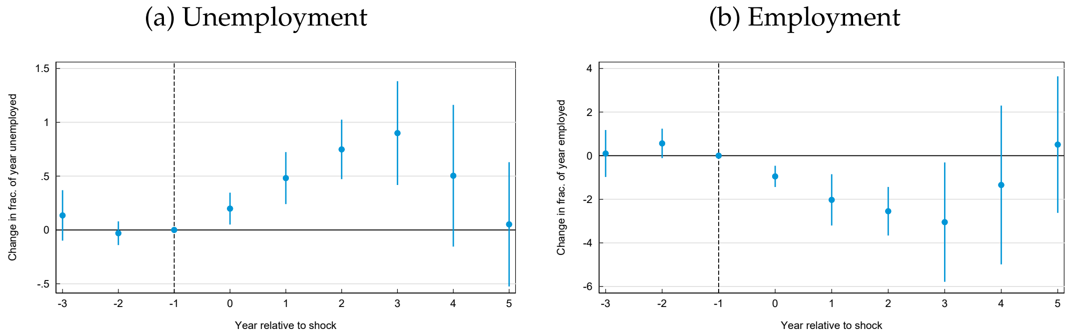
We replicate this aggregate analysis using the annual administrative micro data and run the following regression, which is a slight modification of Equation 2:[14]

$$u_{i,t+k} - u_{i,t-1} = \beta_k \widehat{RR}_t + X_t' \alpha_k + Z_i' \gamma_k + \epsilon_{i,t} \tag{3}$$

where $\widehat{RR}_t$ are estimated Romer and Romer (2004) shocks from Section 2.3, aggregated across quarters within the year, X_t are the same aggregate time-varying controls for other economic variables that capture

[14]While it is not possible to construct quarterly unemployment using our data, we show a specification using a quarterly employment series in the right panel of Appendix Figure A5. The magnitude and dynamics of the estimates are similar to those using annual micro data or aggregate quarterly data.

Figure 4: Effects of Monetary Policy on Worker-level Unemployment and Employment Rates



Notes: 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. Standard errors are twoway clustered at the individual and year level.

the cyclical properties of the labor market, Z_i are individual-level demographic controls to capture trends in labor market outcomes by demographics, and $u_{i,t}$ is the fraction of the year that the individual spends claiming unemployment benefits.[15]

The results are shown in the left panel of Figure 4. We see that these results look similar, although slightly smaller in magnitude, to those using the aggregate unemployment rate in Figure 3: a 1 percentage point increase in the interest rate caused a 0.9 percentage point increase in fraction of the year that workers are unemployed 3 years after the shock. These patterns are echoed in the right panel of Figure 4, where we show the effect on the fraction of the year that workers spend employed. We find that 3 years after the monetary shock, workers on average spent 3 percentage points less of the year employed.[16] 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 excluding the self-employed, include individual fixed effects, or modifying our estimation of the Romer and Romer shocks as discussed in Section 2 (see Appendix Table A4).

[15]Since the microdata are at an annual frequency and therefore have fewer time periods, we include only the contemporaneous value and one annual lag for each of the controls to reduce noise in our estimates. We include the contemporaneous value to capture the unemployment dynamics in the first half of 2010, but this specification also controls for the small contemporaneous effect of the shock seen in Figure 3. However, we find that results are similar when including only 1 and 2 year lags of the controls (See Figure A6). Additionally, in aggregate data collapsed to an annual frequency, we obtain similar estimates using only one lag versus lags up to three years as in our quarterly specification.

[16]Appendix Table A5 shows the results 2 and 3 years after the shock using alternate definitions for unemployment and employment at the individual level, demonstrating that the results are not sensitive to this particular definition of unemployment and employment.

The magnitudes of these estimates are large. Figure 3 demonstrates that a 1 percentage point increase in the policy rate leads to a 2 percentage point increase in the aggregate unemployment rate 3 years later, while Figure 4 shows on a sample of more attached workers that the fraction of the year unemployed increases by 1 percentage point. 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). Our estimates are larger likely because we focus solely on a monetary contraction rather than the typical monetary shock, and recent work has argued that contractionary shocks have larger effects on unemployment than expansionary shocks (Barnichon and Matthes, 2018; Tenreyro and Thwaites, 2016; Angrist et al., 2018). Indeed, when we replicate our analysis using the full set of estimated Romer and Romer shocks from the 2002–2015 period, meaning that we combine contractions and expansions, we recover an estimate for the effect of the monetary shock on unemployment 3 years after the shock of closer to 1 percentage point (see Appendix Figure A3 and Appendix Table A4).

Lastly, in Appendix Figure A7, we explore other dimensions of labor market adjustment in response to the monetary shock. We find that labor force participation fell by just shy of 2 percentage points 3 years following the shock, explaining why the effect on employment in Figure 4 is larger than the effects on unemployment.[17] We also find simultaneous increases in the job separation rate as well as a fall in average weeks employed and labor market earnings for those who remained employed for at least some of the year, suggesting that employment adjusted on both the extensive and intensive margins.

4.2 Robustness of Labor Market Effects

In the following section, we conduct a number of robustness checks to support the patterns in Figure 3 and Figure 4.

4.2.1 Identification and the Euro Crisis

One of the key identification concerns remaining from the analysis in Section 4 is the possible confounding of the monetary shock with the Euro crisis, which occurred within a few years of the monetary tightening episode we consider here. Since Sweden is not part of the Euro, it was not directly affected by changes in

[17] These patterns are consistent with those looking at labor market flows in the United States in White (2019).

the valuation of the Euro. However, as a small open economy within Europe, it could have been exposed to the Euro crisis through effects on export demand.

We first address this concern by including several controls defined to capture this potential confounder. Specifically, columns (4), (5), and (6) of Table A3 include controls for export growth, interbank rates for the Euro area, and the average growth rate of GDP for Sweden's trading partners, respectively. The effect of the shock on unemployment narrows a bit in these specifications, but remains statistically and economically significant.

We further explore this by comparing the response of workers at exporting and non-exporting firms in the administrative microdata. The left panel of Figure 5 shows the estimated effects from Equation 3 using separate regressions for our baseline sample, which include only domestic non-exporting firms, and a sample of all exporting firms in the microdata.[18] We find that exporting firms are, on average, slightly *less* affected by the monetary shock than the domestic firms, the opposite of what we would expect if our results were driven by the Euro crisis instead of the monetary shock. The right panel of Figure 5 examines whether this result comes from differences within sectors. Specifically, we estimate

$$y_{i,t+k} - y_{i,t} = \sum_{f \in \{\text{Exporter,Non-exporter}\}} \left[\beta_{k,f}\widehat{RR}_t + X_t'\alpha_{k,f}\right] \cdot \mathbf{1}(i \in f) + Z_i'\gamma_k + \xi_{s,t} + \epsilon_{i,t} \tag{4}$$

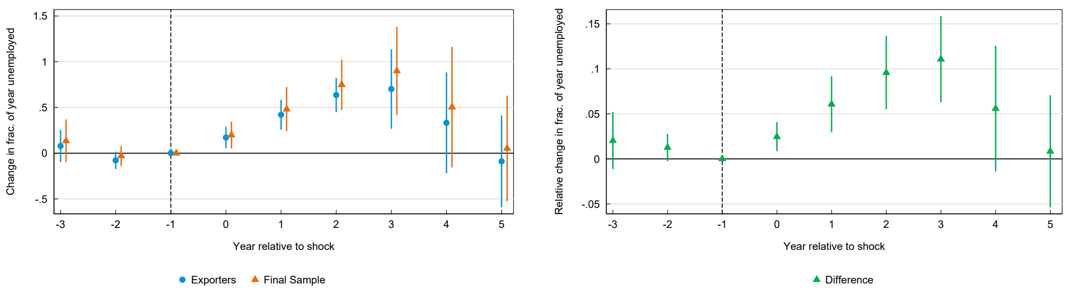
where workers are grouped by the exporting status of their primary firm and we include 3-digit-sector-by-time fixed effects $\xi_{s,t}$. These fixed effects soak up the main effect of the monetary shock, but still allow us to identify the relative effect of the monetary shock on exporting and non-exporting firms. We find similar differences within sectors as overall, indicating that the difference between exporting and non-exporting firms is not due to differences in the response to monetary policy across sectors. These results are consistent with domestic firms being more sensitive to changes in domestic demand or changes in domestic financing that are induced by the monetary shock.

Lastly, in Appendix Figure A8, we use data on the destination of exports to distinguish between those firms that export primarily to Euro-area countries and those that export primarily to the rest of the world. The difference between these two should provide a sense of how important exposure to Euro-area markets were in this period relative to general exchange rate movements that would have affected all exporters. We find that similar increases in unemployment over this period for both Europe-exporting firms and other

[18]We define exporting firms as those who report any positive value for exports.

Figure 5: Effect of Swedish Monetary Shock: Exporters

(a) Level Effects (b) Relative Effects Within Sector



Notes: A firm's exporting status is defined in 2009, although results are similar when defining export status in 2007. Estimates in the right panel include fixed effects for 3-digit sector×time and reflect relative effects for domestic non-exporting firms. 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 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. Standard errors are twoway clustered at the individual and year level.

exporting firms, further suggesting that the Euro crisis had only small effects on the Swedish economy over this period.[19]

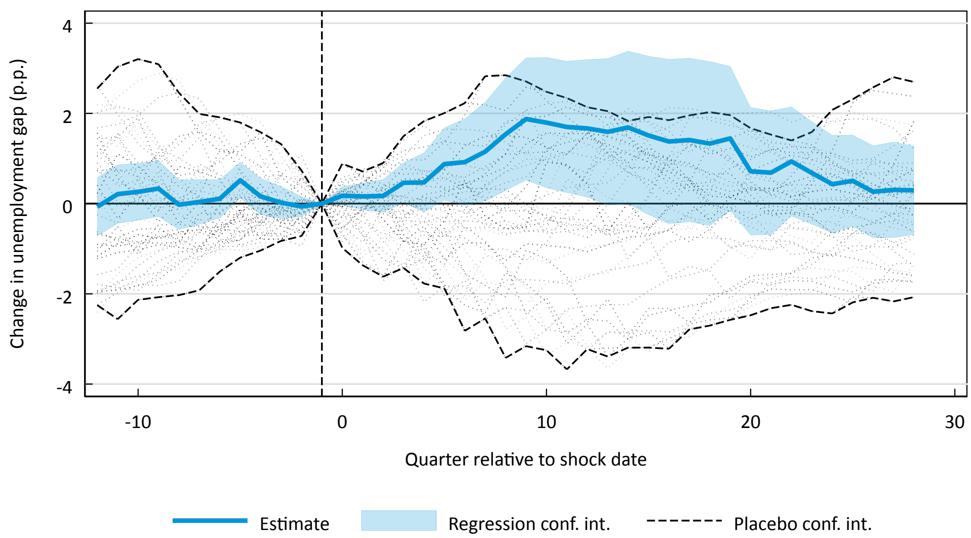
4.2.2 Event Study and Placebo Analysis

A key advantage to our setting is the transparency of the specification and identification. To that end, we also implement a simple event study approach, where we define a dummy variable for the monetary shock, $\mathbf{D}_{t=2010Q3}$, which is equal to 1 in 2010:Q3 and is 0 in all other periods. This approach estimates the deviation of the unemployment rate after July 2010 from its historical cyclical dynamics, which we attribute as the response to the monetary shock.

The blue solid line in Figure 6 shows the results for this event study, with bands indicating a 95% confidence interval. As in Figure 3, we find that there is no deviation from the normal cycle before the third quarter of 2010, but by 3 years after the initial tightening, the unemployment rate has risen by around 2 percentage points relative to what it would have been given the other behavior of cyclical variables. The Riksbank raised the policy rate by 0.5 percentage points in 2010 Q3 and another 0.5 percentage points in

[19]This finding is consistent with comments from Riksbank Governor Stefan Ingves in April 2011, who said in a press conference "Sweden doesn't have particularly high exposure of any kind towards the [PIIGS countries], so, even though they have a great deal of economic problems, we don't think that this will affect Sweden to any great extent." http://archive.riksbank.se/en/Web-archive/Published/Chat/2011/Chat-with-Stefan-Ingves-20-april-2010/index.html

Figure 6: Event Study Approach



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:Q1–2007:Q3 period. Controls include the 1, 5, 9 and 13th 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. Sample includes quarterly data from 1996Q1 to 2019Q2. The darker dashed lines indicate the central 95% range across placebo estimates, while the shaded area illustrates the 95% confidence interval with heteroskedasticity-robust standard errors from our baseline event study specification.

2010 Q4, totaling in a 1 percentage point increase in that 6 month period. Attributing all of them to the change in the policy rate, the event study estimates suggest that the 1 percentage point rise in the interest rate lead to a 2 percentage point increase in the unemployment gap 3 years later, very close to the estimates using the Romer and Romer shocks.

To verify that these results are unique to the monetary shock of 2010–2011, we also conducted a placebo exercise using alternative dates outside of our period of study. For each of 100 placebo regressions, we estimated Equation 2 using a dummy variable for the monetary shock, $\mathbf{D}_t$, where t is a quarter sampled randomly with replacement from the 1996:Q1–2007:Q3 period. Each regression was estimated on the same sample as in our baseline event study regression and used the same set of control variables. Each grey line in Figure 6 shows an estimated impulse response functions from a placebo regression. The placebo results are symmetrically centered around 0, indicating no systematic upward or downward bias in our methodology.[20]

[20]We show this more formally in Table A3, where in column (2), we show that the average estimate from the placebo dates is very close to zero.

4.2.3 Structural vector autoregression specification

Lastly, a complementary analysis to our local projections regressions that relies on a slightly different set of identifying assumptions is provided by structural vector autoregression (SVAR). Under the assumption of invertible impulse response functions (IRFs), the SVAR and local projections approaches consistently estimate the same IRFs (Stock and Watson, 2018; Plagborg-Moller and Wolf, 2018). To verify if this is the case in our setting, we estimate the equivalent SVAR as our local projections regressions and present the estimates using this specification in Appendix Section A.3. We find a similar elevation of unemployment in 2010–2011 relative to the counterfactual without a monetary policy shock, although this effect is slightly smaller than in our baseline results.

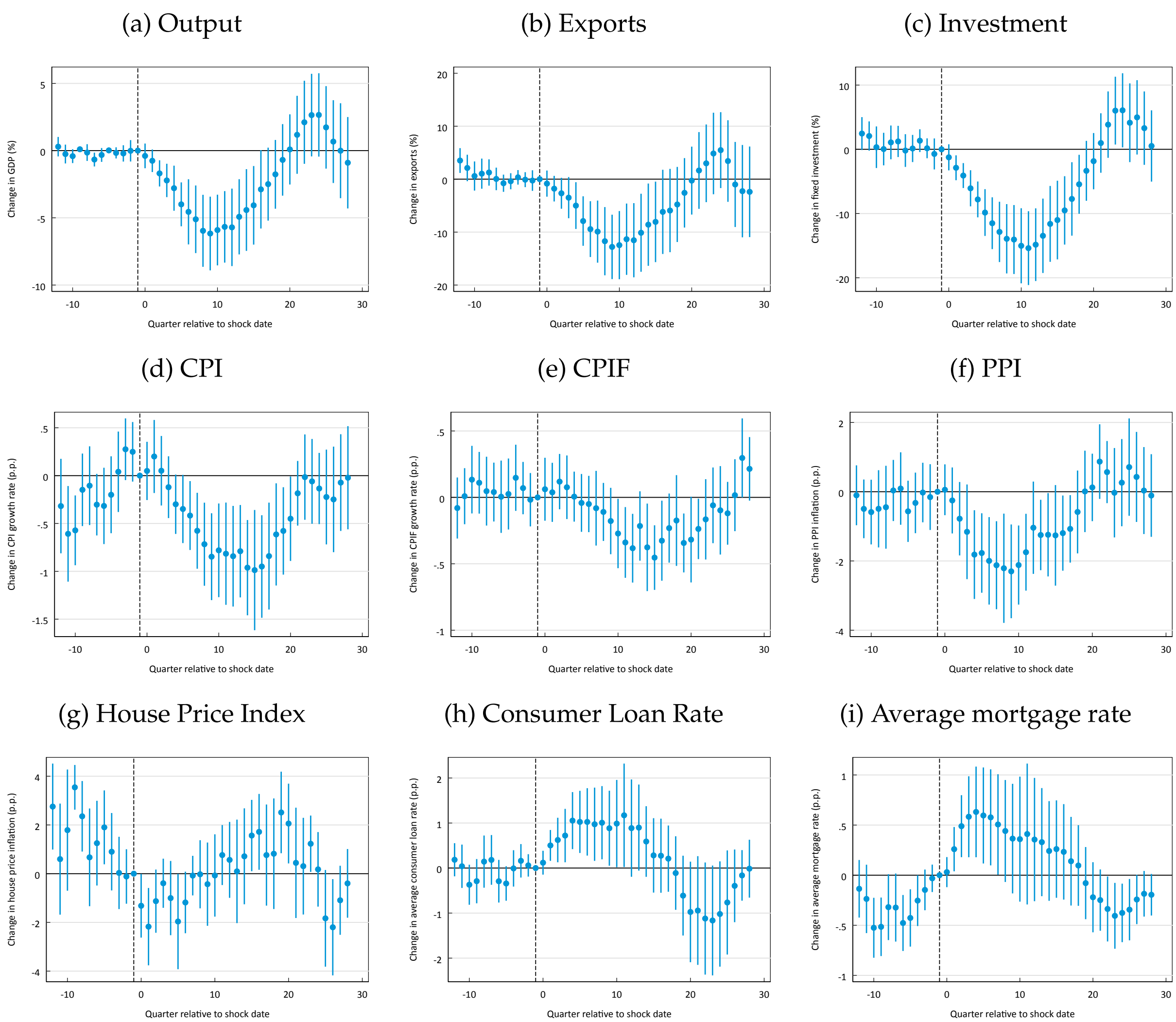
4.3 Response of other economic variables

While we focus in this paper on the effects of monetary policy on the labor market, we can also use this unique shock to explore the effect of the monetary tightening for other economic outcomes. Figure 7 shows the estimates for other macroeconomic outcomes. Overall, we see that 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, 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 both provide reduced-form empirical evidence for the real effects of monetary policy and further bolster our confidence that we have, in fact, identified a monetary shock.

5 Labor Market Heterogeneity and Mechanisms

In this section, we examine how the effects of monetary policy differ across workers and firms. We break down the change in unemployment from the shock by group based on worker and firm characteristics, both separately and jointly. Overall, the shock led to a broad increase in unemployment across all groups, but some groups saw a larger increase in unemployment than others. While the response differed across

Figure 7: Real Effects of Monetary Policy: Alternate Outcomes



Notes: These plots show coefficients estimated from the set of local projections regressions described by Equation 2. The outcomes used are (a) log GDP, (b) log exports, (c) log fixed investment, (d) the quarterly growth rate of the CPI, (e) the quarterly growth rate of the CPIF (core inflation, CPI net of the direct effect of interest rate expenses), (f) the quarterly growth of the PPI, (g) the quarterly growth rate of the house price index, (h) the quarterly growth rate of the consumer loan rate, (i) the quarterly growth rate of the average mortgage rate. Controls include the 1, 5, 9 and 13th 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. Sample includes quarterly data from 1996Q1 to 2019Q2. Bars illustrate the 95% confidence interval with heteroskedasticity-robust standard errors.

firms somewhat, with patterns that line up with previous literature, most of the heterogeneity in the response can be accounted for by worker characteristics, primarily worker tenure. We also examine how the structure of the labor market affects the response to monetary policy, finding evidence that nominal wage rigidity amplifies the increase in unemployment from a monetary policy shock.

5.1 Estimating Heterogeneous Effects by Group

To estimate heterogeneity in the response to monetary policy, we partition the sample into disjoint groups of roughly equal size for each characteristic (e.g. worker age or firm size) and estimate the equation:

$$y_{i,t+k} - y_{i,t} = \sum_{g} \left[\beta_{k,g}\widehat{RR}_t + X_t'\alpha_{k,g}\right] \cdot \mathbf{1}(i \in g) + Z_i'\gamma_k + \epsilon_{i,t} \tag{5}$$

where g indexes groups, and X_t and Z_i are aggregate- and individual-level controls respectively as in Equation 3. The coefficient $\beta_{k,g}$ represents the k-year-later effect of monetary policy shocks on the outcome for individuals in group g. We allow the coefficients on the aggregate control variables X_t to vary across groups in order to capture differing sensitivities to the business cycle.

Equation 5 estimates heterogeneity in the effects of monetary policy across groups with different levels of a single characteristic, which we refer to as the *unconditional effects*. For example, if g indexes worker age, then the coefficients $\beta_{k,g}^{Age}$ estimate how workers of different ages are differently sensitive to monetary policy shocks. If we run a separate regression using groups g defined by firm size, then we can compare $\beta_{k,g}^{Firmsize}$ to $\beta_{k,g}^{Age}$ to compare the heterogeneity in responses between firm size and worker age. Importantly, though, each of these sets of coefficients have been estimated *without conditioning* on the other characteristic. In a hypothetical scenario in which the response to monetary policy was only due to firm size and not worker age, but worker age was correlated with firm size, we would recover differences in response along both dimensions.

To compare heterogeneity along multiple dimensions, we estimate responses to monetary policy for multiple characteristics jointly, which captures the *conditional effects* for each characteristic. Specifically, for each characteristic w, denote the partition of the sample along w as $\mathcal{G}_w$. We estimate the regression:

$$y_{i,t+k} - y_{i,t} = \sum_{w} \left[\sum_{g \in \mathcal{G}_w} \left[\theta_{k,g}\widehat{RR}_t + X_t'\phi_{k,g}\right] \cdot \mathbf{1}(i \in g)\right] + Z_i'\gamma_k + \nu_{i,t} \tag{6}$$

where the coefficients $\theta_{k,g}$ represent the *conditional* k-year-later response to monetary policy shocks for group g, since they are estimated controlling for the responses of other characteristics.

We examine heterogeneity for both worker and firm characteristics. Worker characteristics include age and education level in 2010, and tenure at their primary firm (defined as of the latest observation in the 2006–2009 period). For firm characteristics, we divide firms into groups based on observations in 2009 and assign workers to the group of their primary firm during the 2006–2009 period. The firm characteristics we use include short-term debt (defined as the kronor value of short-term debt divided by firm assets), size (defined as the average number of full-time employees across all establishments in a year), age (defined based on the year in which the firm ID first appears in the firm accounts database[21]), and labor share (defined as annual payroll divided by annual revenue).

Additionally, for a subsample of the data, we are able to divide workers based on the rigidity of their employment contract. 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 and then, depending on how much flexibility the sector contract allows for, bargaining can take place at the firm level.[22] This variation in stringency of the sector agreements allow us to construct a proxy for cross sectional wage rigidity. We code contracts as "rigid" if they include an individually guaranteed wage growth rate or a piece-wage contract.[23] Contracts without this formulation are coded as "flexible", including workers at sectors that do not have a union contract. Wage contracts are mainly specified in growth rates and apply therefore to a large share of workers, in contrast to a minimum wage floors that typically affect only a limited number of workers. Each contract is specific to blue or white collar workers within a sector. We match data on employment contracts from Olsson (2020) to our sample; we are able to obtain matches for 22% of our sample.[24] Based on the individual contracts we calculate indices of average rigidity at the firm and at the 3-digit sector level.

[21]The overlap between the first observation in the firm accounts data and employer-employee registry is 94 percent.

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

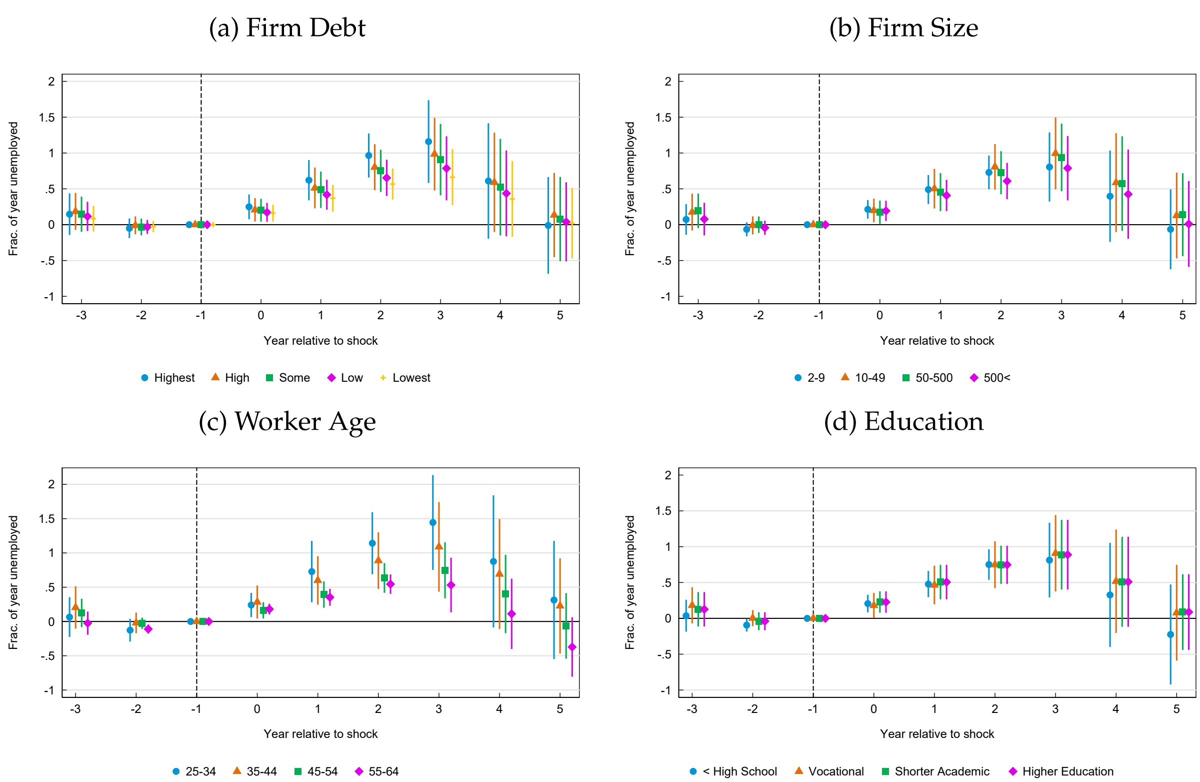
[23]"Tariff agreements" are in this category.

[24]Whether a worker is blue or white collar is registered for a subset of workers, where the large (exporting) firms are over represented. Since the wage contract depends on this distinction, we are only be able to match workers to wage contracts for a subsample.

5.2 Heterogeneous Incidence in the Labor Market

We begin by exploring the effects of the monetary shock for different segments of the economy using Equation 5. While we will explore many dimensions of heterogeneity mentioned above, we begin by showing estimates for a subset of the variables. The estimates presented in Figure 8 capture the unconditional effects of the monetary shock for each variable. The top left panel of Figure 8 splits workers by the debt levels of their primary firm during the pre-period, the top right panel splits by firm size, and the bottom panels split by worker age and education.

Figure 8: Unconditional Heterogeneity of Monetary Policy



Notes: Estimation using Equation 5, where g is firm debt, firm size, worker age, or worker education level. 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 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. Standard errors are twoway clustered at the individual and year level.

There are two clear takeaways from these figures. First, almost all segments of the economy are at least somewhat exposed to the monetary shock. Even the least-affected group across these categories, 55–64 year olds, see a statistically significant increase in unemployment of about 0.5 percentage points 2–3 years after a 1 percentage point increase in the policy rate. This pattern is consistent with large general equilibrium effects generated by the monetary shock, rather than monetary shocks primarily affecting only those workers at firms that are most directly exposed. In Appendix Figure A13, we also show that the effects are similar for workers in manufacturing and services, demonstrating that no one sector is driving the labor market response.

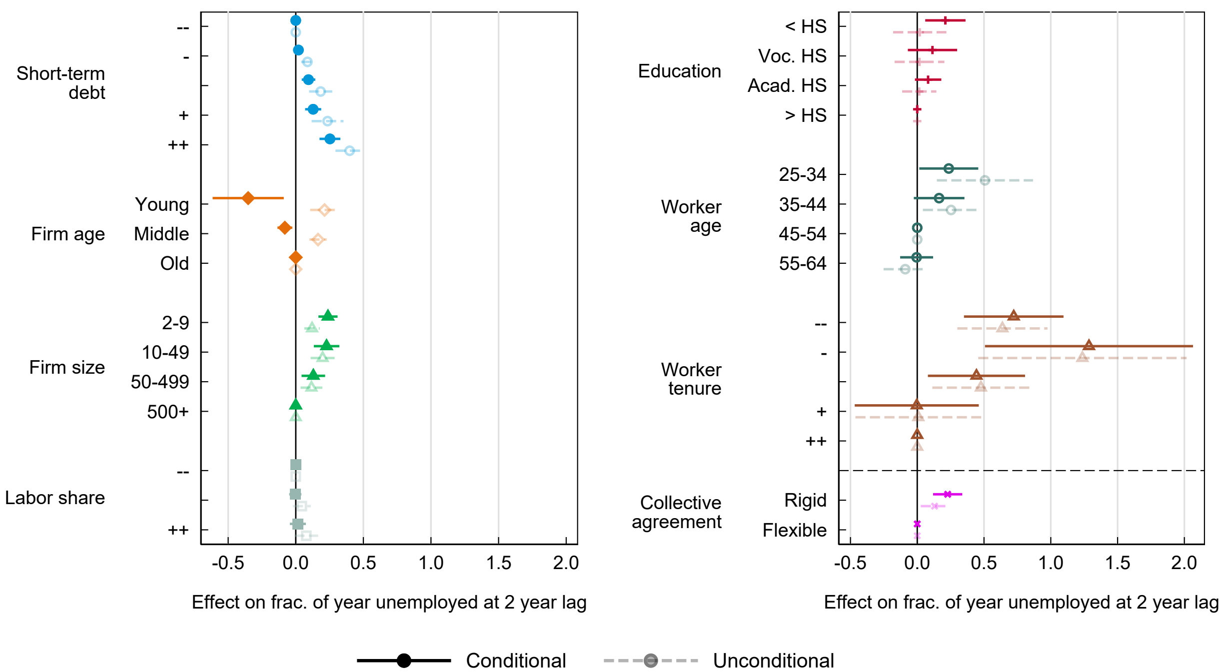
Second, although the differences in magnitude are relatively small compared to the baseline effect on all groups, each dimension exhibits some heterogeneity. For example, the most indebted firms see a 0.5 percentage point greater increase in unemployment 3 years after the shock than the least indebted firms. Among these characteristics, heterogeneity is largest across worker age groups, with 25–34 year olds experiencing an increase in unemployment about 2–3 times larger than that of 55–64 year olds at the peak. Firm size and worker education show smaller differences across groups, with the magnitudes of the effects for these groups all within about 0.2 percentage points of each other.

The estimates in Figure 8 show unconditional effects that do not take into account the correlation across variables. For example, smaller firms may be more likely to hold higher levels of debt, which could lead the estimates for both categories to be correlated. In order to better understand how each aspect of the labor market contributes to heterogeneity in how workers experience monetary policy, we now turn to estimating conditional effects using Equation 6. Figure 9 summarizes the results, reporting the effect of monetary policy shocks on unemployment 2 years later relative to a base category for each characteristic. The left panel of Figure 9 reports estimates for firm characteristics including debt, size, age, and labor share, while the right panel reports estimates for worker characteristics including education, age, and tenure. The right panel also reports estimates for employment contract rigidity, which are estimated on the subsample in which we can construct this measure.[25] The dotted bars show the unconditional effects for each characteristic while the solid bars show the conditional effects, controlling for all other characteristics shown in the figure.

We find moderate differences in unconditional effects across groups for several of the firm characteristics. Echoing the patterns in Figure 8, firms with more short-term debt (top quintile) see larger increases in unemployment relative to firms with less debt (bottom quintile), as do younger firms (started in 2005–

[25] To estimate the conditional effect of contract rigidity, we repeat our estimation of Equation 6 using this subsample and controlling for the same set of firm and worker characteristics as our baseline. The estimated conditional effects for these characteristics are close to our baseline estimates.

Figure 9: Firm and Worker Heterogeneity



Notes: Shaded are unconditional effects using Equation 5. Solid are conditional effects using Equation 6. 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 rate, as well as the annual lag of each of these variables, all interacted with group indicators. The estimates for "Collective agreement" come from a separate regression using only a subset of the individuals in our main sample for which contract information is available. At the individual-level, regressions include controls for 10-year age bins, gender, a native/foreign born dummy, and dummies for 4 education levels. Standard errors are twoway clustered at the individual and year level.

2009) relative to older firms (started in 1997 or before) and small firms (2–9 employees) relative to larger firms (500+ employees). These results confirm patterns established in previous work that firms are more sensitive to monetary policy if they are in greater debt (Bernanke and Gertler, 1995; Kashyap and Stein, 2000; Jiménez et al., 2014; Ippolito et al., 2018), are younger (Sedláček and Sterk, 2017), or are smaller (Crouzet and Mehrotra, 2020; Gertler and Gilchrist, 1994). At the same time, we find relatively little heterogeneity in the effects of monetary policy on the labor market across firms with higher or lower labor shares.

However, the differences in unconditional effects across groups partially reflect correlation between firm characteristics. The conditional effect estimates, shown by the solid markers, indicate that the difference in response between high- and low-debt firms is smaller when controlling for response along other dimensions.[26] The difference between unconditional and conditional effect estimates is even starker for firm age, as younger firms actually experience *less* response to monetary policy when controlling for the responses along other dimensions. Young firms are more likely to be smaller and hold higher levels of debt, so much of their unconditional effect stems from these factors rather than a direct effect of firm age. For firm size and labor share, we estimate relatively similar unconditional and conditional effects.

In addition to moderate differences in response across firms, we find differences for worker characteristics that are at least as large and in some cases much larger. Both age and education exhibit gradients similar in magnitude to the firm characteristics, with younger and less educated workers experiencing about a 0.25 percentage point larger increase in unemployment after a shock. Additionally, we find that workers with more rigid employment contracts experience greater increases in unemployment, consistent with downward nominal wage rigidity leading to greater adjustment along the employment margin during downturns (see e.g., Murray, 2019, Olsson, 2020).

Workers with low tenure experience the largest effects of any group we examine. Workers with 1–5 years of tenure see an increase in unemployment of more than 1 percentage point compared to workers with at least 13 years of tenure, a difference between categories roughly 2–5 times larger than for any other worker or firm characteristic. The large effects for low-tenure workers likely reflect a combination of institutional and frictional forces. Employment in Sweden is governed by a last-in-first-out (LIFO) principle, in which the lowest tenured workers at a firm must be laid off prior to laying off similar higher

[26] Figure 9 does not include controls for sector, but we find nearly identical patterns when we also include a set of sector-by-time fixed effects to fully control for differences across sectors.

tenure workers.[27] This policy applies to all firms with more than 10 employees, and it binds the firm's ability to lay off workers of a similar job type, where job type can depend on negotiations between the firm and the union. There may also be non-policy-related forces that lead to larger responses for lower tenure workers, for example if workers and firms learn about the quality of their employment match over time, so higher tenure workers are more positively selected on match quality (Jovanovic, 1979).

We use our conditional effect estimates to quantify the relative importance of firm and worker characteristics in mediating the response of monetary policy. Specifically, for each individual we calculate their predicted employment effect based on their characteristics and our estimates of the conditional effect for each characteristic shown in Figure 9. To gauge the importance of worker and firm characteristics, we conduct this calculation twice: once using only the conditional effects for worker characteristics and once using only the conditional effects for firm characteristics. We find that, overall, the variance of predicted unemployment based only on worker characteristics is about 13 times larger than the variance of predicted unemployment based only on firm characteristics.[28] Within worker characteristics, worker tenure is by far the most important, explaining 75 percent of the variance in predicted unemployment on its own.

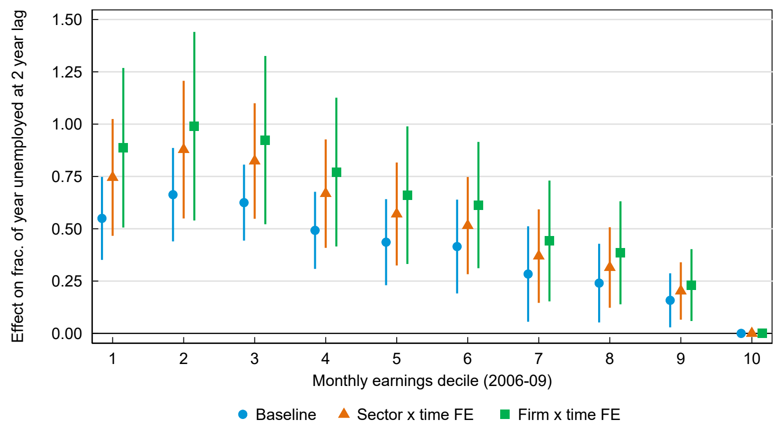
The large increases in unemployment among low-tenure, less-educated, and younger workers suggest that contractionary monetary policy shocks may exacerbate inequality in the labor market. We turn to this more directly in Figure 10, where we divide workers into deciles of their average monthly earnings position during 2006–09 and estimate the unconditional effects across the income distribution using Equation 5. The blue dots labeled "Baseline" show the estimates from this regression, while the orange triangles show estimates from a specification adding in sector-by-year fixed effects as additional controls and the green squares show estimates from a specification adding in firm-by-year fixed effects.

We estimate that lower-earning workers are more sensitive to monetary policy shocks. In our baseline estimates, bottom decile workers experience a 0.5 percentage point larger increase in unemployment compared to top decile workers, indicating that the contractionary monetary policy shock exacerbated inequality. Importantly, this heterogeneity is attributable to differences across workers within firms rather than differences across firms, since the difference between the bottom and top deciles grows to nearly 1

[27] See von Below David and Thoursie (2010), Böckerman et al. (2018) for additional institutional details and evaluations of the effect of the LIFO principle on firm employment.

[28] This calculation is based on our main estimates of Equation 6, which omits employment contract rigidity. Using the estimates that include contract rigidity, which use only a subsample of our main analysis sample, we find that worker characteristics are 7.5 times more important than firm characteristics (treating contract rigidity as a worker characteristic).

Figure 10: Incidence Across the Earnings Distribution



percentage point after controlling for firm-by-year fixed effects. The baseline specification compares workers with different income levels regardless of where they work, while the specification with firm-by-year fixed effects compares workers with different income levels within the same firm. The relative steepness of the two sets of estimates across income groups suggests that the sorting of workers across firms dampens the inequality effects of monetary policy.

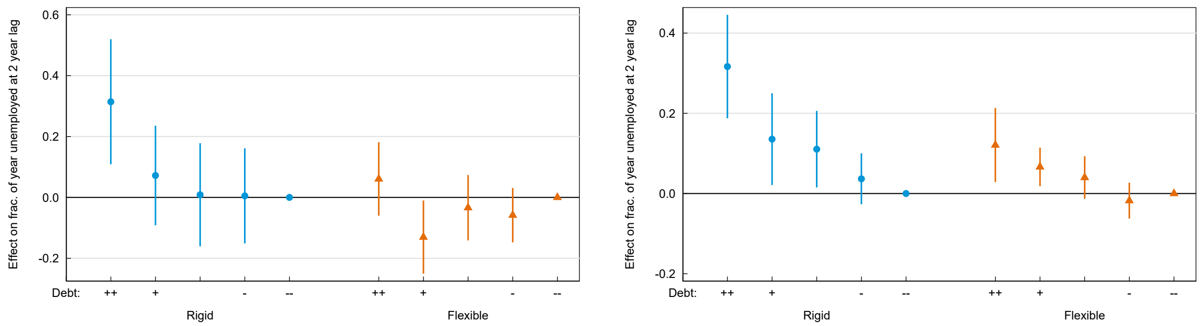
The difference in the magnitude of effects in Figure 10 is large. When controlling for firm-by-year fixed effects, the difference between the lowest earners and the highest earners is about as large as the average effects across the whole sample shown in Figure 4. However, this relative difference underestimates the total effect for the lowest earners, as the highest earners still experience an increase in unemployment in absolute terms (see Figure A9). These patterns overall are consistent with similar estimates in Coibion et al. (2017) and Holm et al. (2020).

In addition to the factors shown above, we also examined heterogeneity using other measures of firm performance (profit share, long-term debt, within sector rank, etc.) and measures of market concentration, all of which we document more fully in Figure A10 in Appendix Section A.4. Overall, we find very limited heterogeneity along these dimensions.

To summarize, we find that exposure to monetary policy shocks is broad-based, largest for low-tenure workers, and exacerbates inequality. Although we find comparable heterogeneity across firms as previous studies, we estimate that most of the heterogeneity in worker exposure is accounted for by worker rather than firm characteristics.

Figure 11: Debt and Labor Market Contracts

(a) Collective Agreements at Firm | (b) Collective Agreements in Sector



5.3 The Role of Labor Market Structure

In this section, we highlight the role that the structure of the labor market plays in mediating the effects of monetary policy. As shown in the previous section, contract rigidity and worker tenure both account for substantial variation in the individual-level effects of monetary policy. These factors reflect a combination of both policy and norms, which we refer to as "labor market structure". In this section, we argue that these structures affect not only individual experiences but also the total labor market response to the shock, indicating that the structure of the labor market amplifies the effects of monetary policy.[29]

We begin by exploring the aggregate importance of nominal wage rigidities. Recall that collective bargaining in Sweden often stipulates the growth rate, rather than the level, of wage.[30] In addition to the direct effect that a worker's contract rigidity has on their own probability of unemployment, it may also have spillover effects on other workers in the same firm or sector. Depending on the magnitude of these spillovers, the aggregate importance of wage rigidity may be greater or lesser than the individual-level effect estimates. Additionally, spillovers may be worse for sectors or firms that are more highly exposed to monetary policy due to other factors, such as firm debt.

We examine whether exposure to rigid contracts at the firm or sector level amplifies heterogeneity in the response to monetary policy. In Figure 11, we show the unemployment response for each quintile of firm debt, separately for firms (panel a) or sectors (panel b) with more rigid and less rigid contracts.[31]

[29]This set of findings are consistent with those documented in Olivei and Tenreyro (2007) and Björklund et al. (2019).

[30]Indeed, Appendix Figure A11 shows that workers with more rigid contracts who stayed at their initial firms saw higher wage growth than similar workers who were under flexible contracts.

[31]We include all firm and individual controls in Figure 9 in Figure 11, notably including both firm debt and worker contract rigidity.

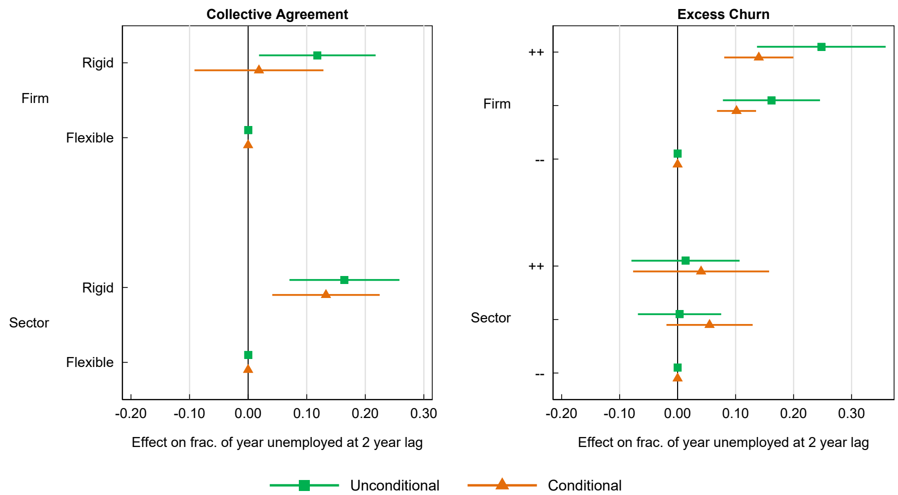
We find that the slope of the unemployment response with respect to initial firm debt is almost zero for the firms with workers who on average have more flexible contracts. This suggests that higher firm debt only translates to firm-level unemployment when the workers face rigid wage contracts and cannot adjust on other margins. Panel (b) shows similar patters at the sector level, demonstrating that the job losses from firms with high debt is about twice as large in sectors with with rigid contracts than it is in sectors with more flexible contracts. This pattern indicates that nominal rigidities not only amplify the effects of monetary policy on the labor market overall, but also amplify the response more for groups that are already more exposed to monetary policy shocks.

In addition to amplifying differences in the response to monetary policy, wage rigidity may affect the total response to monetary policy at the firm or sector level. The left panel of Figure 12 shows the overall effect of the average firm- and sector-level contract rigidity on the fraction of the year that a worker is unemployed 2 years following the shock. The green squares show unconditional effects for workers from firms or sectors with more rigid employment contracts on average. Since wage contracts are signed at the sector-level separately for white and blue-collar workers, variation at the sector-level largely comes from the exact details of union agreements along with the skill composition of the workforce, while variation at the firm-level within sector stems primarily from differences in the skill composition.[32] These estimates largely confirm the worker-level patterns from Figure 9, demonstrating that workers in firms and sectors with more rigid contracts are more likely to experience unemployment. More interestingly, the orange circles show the conditional effects controlling for all firm and individual controls in Figure 9, including the rigidity of an individual's own labor contract. While the conditional effect of firm-level rigidity is close to zero, sector-level rigidity still matters: even conditional on an individual's own contract, being in a sector with more rigid contracts on average increases the probability of unemployment by about 0.15 percentage points.

Why does the average rigidity of the sector matter over and above the rigidity of an individual's contract? We show in Appendix Figure A12 that this is likely, at least in part, the result of congestion in the labor market. Individuals who become unemployed in more rigid sectors are less likely to find new jobs, spend more time unemployed, and, correspondingly, are more likely to switch sectors of employment. Note that this pattern on job finding does *not* extend to workers who become unemployed from more

[32] Some contracts have variation at the 5-digit sector rather than the 3-digit sector. Since sector in Figure 12 refers to the 3-digit sector, some of the firm variation within sector also includes differences in contracts across sub-sectors.

Figure 12: The role of labor market structure



rigid firms, which is consistent with the effects being driven by overall labor market congestion rather than by spillovers within the firm.

In addition to collective agreements, we explore the broader effects of labor market structures by exploring the importance of labor market churn at either the firm- and sector-level in explaining the effects of the shock. We define excess churn at the firm-level as two times the minimum of the job creation rate or the job destruction rate, capturing the job churn that is in excess of the amount needed to change the overall number of workers at the time. The patterns in the orange triangles in the right panel of Figure 12 demonstrate, that even after controlling for individual tenure and the other characteristics of the firm explored above, workers at firms with more churn are more exposed to the shock. The average labor market churn likely reflects some combination of job characteristics (e.g. longer on-the-job training periods) and norms that make firms reticent to layoff workers, or to hire workers on permanent contracts in the first place. These norms dampen the overall employment response of the firm and sector to the shock.

Together, the firm- and sector-level results demonstrate that the structure of the labor market affects not only which workers are affected by monetary policy shocks but also the overall effect of monetary policy in the labor market. The findings confirm the importance of nominal wage rigidities in transmitting monetary shocks, and suggest factors related to labor market fluidity are also important considerations.

5.4 Monetary Policy and Other Demand Shocks

Firms and workers that are differently exposed to monetary policy can also be differently exposed to other types of shocks in the labor market. For example, Aaronson et al. (2019) document that younger workers and less educated workers tend to experience larger fluctuations in employment during recessions than other groups, and Giroud and Mueller (2017) show that employment at more-indebted firms is more sensitive to demand shocks. These patterns line up with the patterns we document in Figure 9, raising the question of how monetary responses compare to the responses to other types of demand shocks.

Since our regressions control for business cycle indicators separately by group, we can compare our estimated monetary policy responses to the responses from a generic recessionary shock. In our conditional heterogeneity specification of Equation 6, the coefficients on the control variables $\phi_{k,g}$ capture the average correlations between changes in unemployment and unit changes in GDP, layoffs, and vacancies for each group g. We take our estimates of these coefficients, multiply them by the values of the control variables in 2008, and sum these quantities to get the average change in unemployment k years after a typical business cycle shock for each group g. Since one category in each grouping $\mathcal{G}$ is left out, this estimates the *relative* cyclicality of group g compared to the base category.

Overall, we find substantial similarities between the responses to typical recessionary shocks and the responses to monetary policy shocks. Figure A15 shows our estimates of the response to a typical recessionary shock for the main groups we study. We find that low tenure workers exhibit the largest increase in unemployment, with more moderate increases also found among young workers, high-debt firms, and small firms. All of these patterns are qualitatively in line with the monetary policy responses we find in Figure 9, although the differences in magnitudes across groups are slightly smaller for the typical recession than for monetary policy.

The similarity between responses to monetary policy and responses to other demand shocks has important consequences. Since the groups that respond most to recessions also respond most to monetary policy, central banks can use monetary policy to stabilize employment for these groups without leading to large distortions of employment among other groups. The fact that the responses to both types of shocks line up closely is reminiscent of the “divine coincidence” result of Blanchard and Galí (2007) in which the central bank does not face a tradeoff in simultaneously achieving multiple objectives (unemployment and inflation in Blanchard and Galí (2007), unemployment for different groups in this paper). Additionally, the similarity of the responses to monetary policy and recessionary shocks give support to models of business cycles which feature this similarity, such as Smets and Wouters (2007).

6 Conclusion

This paper used the quasi-experiment of Swedish monetary tightening in 2010–2011 to explore the real effects of monetary policy on the labor market. We argue that this contraction represented a temporary deviation from the monetary rule, supported both by the narrative evidence from central banker statements at the time as well as identified monetary shocks. We use this large, contractionary monetary policy shock—a rare occurrence—to identify the effects of monetary policy on the labor market. We find evidence for monetary non-neutrality, estimating large effects of the shocks on unemployment and other real variables. Using micro data, we decompose the mechanisms through which monetary policy affects labor market outcomes. We show that the shock was broad-based and every demographic group that we consider was at least partially affected. Still, some heterogeneity in exposure exists, most of which is explained by worker rather than firm characteristics. Lastly, we show that labor market structure amplifies the effects of monetary policy, with sectors featuring more rigid union wage contracts and less churn on average experiencing larger increases in unemployment. These findings support models of monetary policy that feature labor market frictions and suggest that structural changes in the strength of unions or a rise in temporary work contracts will have cyclical consequences.

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A Appendix

A.1 The Swedish vs. U.S. Economy

Table A1: Comparing Swedish and U.S. Labor Markets

	Sweden	United States
Manufacturing Fraction	0.07	0.10
U-to-E Annual Flow Rate	0.81	0.75
E-to-U Annual Flow Rate	0.11	0.03
Job-to-Job Annual Flow Rate	0.30	0.24
Avg. Weeks Unemployed	26.1	19.4

Notes: All statistics are reported as the average from 2005-2007. Data for unemployment duration and job-to-job flows for the United States come from the CPS basic monthly survey, and data on U-to-E and E-to-U flows come from the CPS March supplement. For U-to-E rates and E-to-U rates, we calculate annual flows while for job-to-job flows, we annualize the monthly flow. We include data for all workers between the age of 25 and 62. Unemployment duration and manufacturing employment for Sweden comes from the AKU monthly survey, conducted by Statistics Sweden, and Swedish flow rates comes from RAMS annual data. Swedish data include workers between the age of 16 and 64.

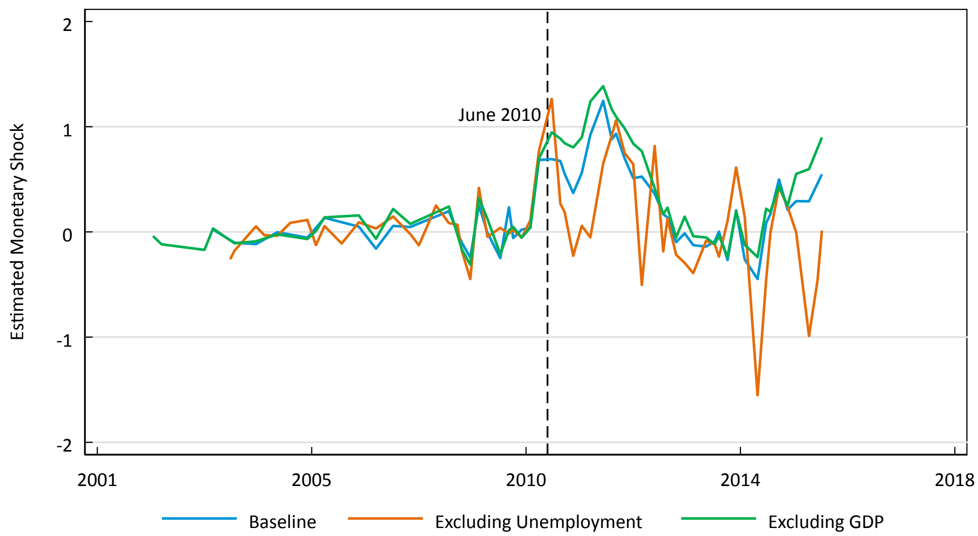
A.2 Details of Monetary Shock Construction

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 meeting in the forecasts for GDP and inflation as additional regressors, capturing any response of the FOMC to not only current expectations but also 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.

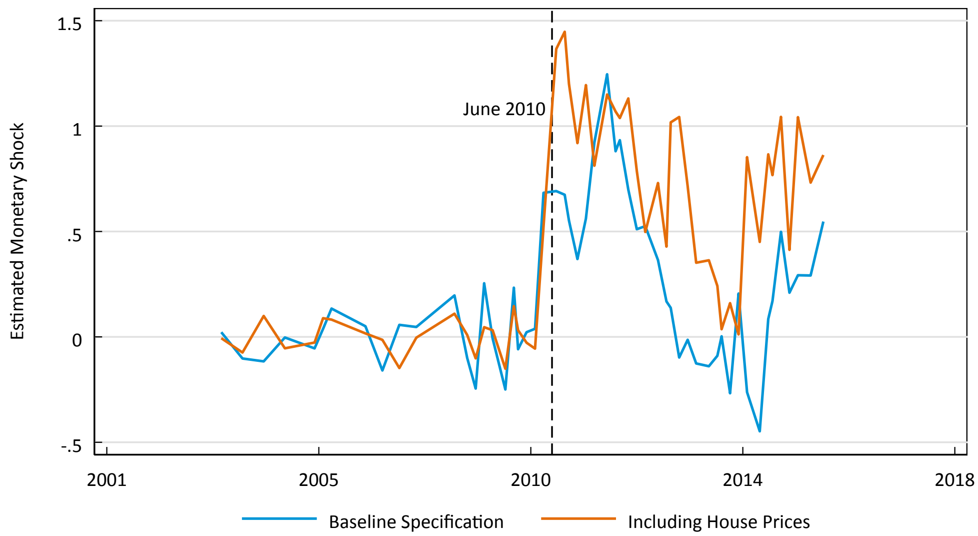
However, we explored two alternative specifications that balance the limited number of observations due to the inclusion of changes in the forecasts over time by including inflation and then either only GDP or unemployment. Figure A1 explores the importance of these modifications for the estimates. Each line reflects the time series for a different specification, each estimated on the 2002–2010 sample. The various specifications produce monetary shocks that are highly correlated and that all feature a large positive shock in July 2010 when the Riksbank first increased the interest rate. The specification that excludes unemployment entirely shows a less persistent positive shock through 2011 than the others, but all are consistent with the finding that the monetary tightening, when it began in 2010, was not a continuation of the historical monetary rule.

Figure A1: Estimated Monetary Shocks: Alternate Specifications



Notes: The blue solid line shows the residuals from Equation 1 estimated on data before June 2010. The orange solid line shows the residuals from Equation 1 excluding the unemployment variables and the green line shows those excluding GDP variables. The black dotted line marks June 2010, when the Riksbank first increased the repo rate.

Figure A2: Alternate Monetary Policy Shocks: Adding House Prices



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 black dotted line marks June 2010, when the Riksbank first increased the repo rate.

A.3 Robustness: Effects of monetary policy on the labor market

A complementary approach to the event-studies and the local projections of identified monetary policy shocks is to use a VAR. To that end, we estimate a VAR with three lags and the following variables: GDP, unemployment gaps, vacancy rates, and layoff rates. We first use historical data through the second quarter of 2010, yielding estimates of the historical co-movement between these variables. We then use these estimates to forecast the unemployment gap beginning in the third quarter of 2010, given the actual evolution of the other variables after mid-2010. Comparing this predicted change in the unemployment gap with the actual evolution of the unemployment gap yields an estimate of the effect of the monetary tightening on the labor market. Figure A4 shows the results of this exercise. As in the local projection specifications, the monetary tightening in July 2010 did not affect the unemployment rate for several quarters; however, by 2013, the actual unemployment rate was about 0.6 percentage points higher than the counterfactual.

Table A2: Regressions for Estimating Romer and Romer (2004) Monetary Policy Shocks

	Baseline	No Unemployment	No GDP
r_m	0.437*	0.122	0.509
	(0.212)	(0.218)	(0.304)
$\pi_{m,i}$	0.125	-0.988	-0.090
	(0.334)	(0.843)	(0.523)
$\pi_{m,i+1}$	-0.440	-0.476	-0.135
	(0.355)	(0.547)	(0.408)
$\pi_{m,i+2}$	0.351	1.359**	0.146
	(0.266)	(0.497)	(0.294)
$\pi_{t,i-1}$	-0.435*	0.111	-0.384
	(0.186)	(0.313)	(0.243)
$u_{m,i}$	-1.794		-1.561
	(1.235)		(1.133)
$u_{m,i+1}$	1.816		2.006
	(1.593)		(1.821)
$u_{m,i+2}$	-0.545		-0.906
	(0.854)		(1.039)
$u_{m,i-1}$	0.630		0.547
	(0.601)		(0.457)
$GDP_{m,i}$	0.087	0.362	
	(0.096)	(0.227)	
$GDP_{m,i+1}$	-0.045	0.018	
	(0.244)	(0.459)	
$GDP_{m,i+2}$	-0.003	-0.518	
	(0.311)	(0.449)	
$GDP_{m,i-1}$	0.019	0.002	
	(0.080)	(0.096)	
$\pi_{m,i} - \pi_{m-1,i}$		0.760*	0.191
		(0.398)	(0.351)
$\pi_{m,i-1} - \pi_{m-1,i-1}$		-0.216	0.070
		(0.230)	(0.145)
$\pi_{m,i+1} - \pi_{m-1,i+1}$		-0.143	-0.190
		(0.350)	(0.276)
$\pi_{m,i+2} - \pi_{m-1,i+2}$		-0.533	0.066
		(0.313)	(0.186)
$GDP_{m,i} - GDP_{m-1,i}$		-0.148	
		(0.121)	
$GDP_{m,i-1} - GDP_{m-1,i-1}$		0.007	
		(0.108)	
$GDP_{m,i+1} - GDP_{m-1,i+1}$		-0.220	
		(0.277)	
$GDP_{m,i+2} - GDP_{m-1,i+2}$		0.660	
		(0.379)	
Constant	-1.414	-0.245	-1.253
	(1.345)	(0.419)	(1.217)
Observations	21	28	24
R-Squared	0.925	0.878	0.897

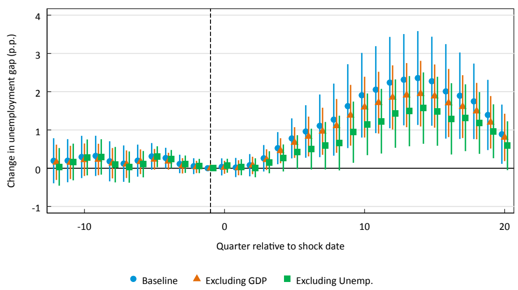
Table A3: The Effect of Monetary Tightening on the Unemployment Rate: Baseline and Alternate Specifications

	(1a) Baseline	(1b) Event Study	(2) Placebo	(3) CPIF	(4) Exports	(5) Interbank Rates	(6) Foreign GDP	(7) Post-Euro Only
Outcome	———	———	———	(Unemp. Gap_{t+12} – Unemp. Gap_t)	———	———	———	———
S_t	2.24	1.67	0.06	0.72	1.87	1.65	1.15	1.53
	(0.61)	(0.78)	(1.68)	(0.34)	(0.41)	(0.45)	(0.46)	(0.51)
	R&R	Event	Event	R&R	R&R	R&R	R&R	R&R
Controls:								
GDP	✓	✓	✓	✓	✓	✓	✓	✓
Vacancy Rate	✓	✓	✓	✓	✓	✓	✓	✓
Layoff Rate	✓	✓	✓	✓	✓	✓	✓	✓
CPIF				✓				
Exports					✓			
Interbank Rates						✓		
Foreign GDP							✓	
Time Period	———	———	———	1996:Q1–2019:Q2	———	———	———	1999:Q1–2019:Q2

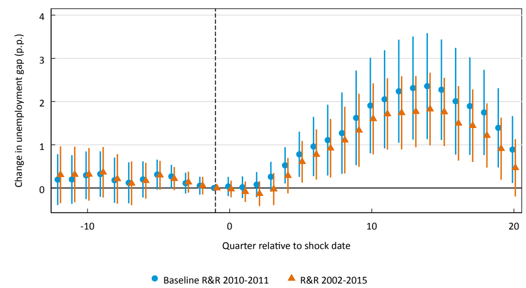
Notes: This table shows estimates of Equation 2 for horizon $k = 12$, varying the shock variable, controls, and sample period. Columns (1a) reports the baseline estimates for the Romer & Romer shocks shown in Figure 3, and (1b) the event-study estimates using 2010Q3 as the shock indicator. Column (2) reports the estimate from the placebo regressions shown in Figure 6. The subsequent columns add additional control variables to the baseline specification using the Romer & Romer shocks. Each added control variable includes its 1, 5, 9, and 13th lags. Column (3) adds the year-over-year percent change in the CPI with fixed interest rate (CPIF), column (4) adds the year-over-year percent change in exports, column (5) adds the year-over-year percentage point change in the average 3-month interbank interest rate for the Euro area, and column (6) adds the year-over-year percent change in GDP of Sweden's trading partners, weighted by the value of Swedish exports to each country in 2007. Column (7) estimates the baseline Romer & Romer specification over the time period following the introduction of the Euro in non-Swedish countries. Asymptotic heteroskedasticity-robust standard errors are reported in parentheses for all specifications, expect for column (3) which reports the empirical standard error computed across 100 placebo regressions.

Figure A3: Robustness of Local Projections: Alternate Shock Construction

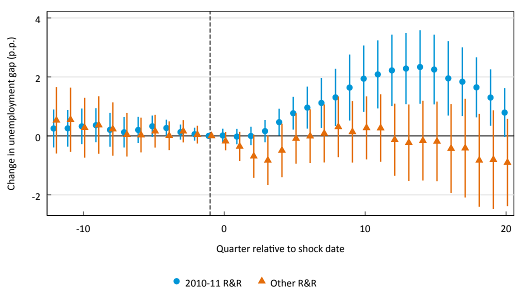
(a) Alternate Controls



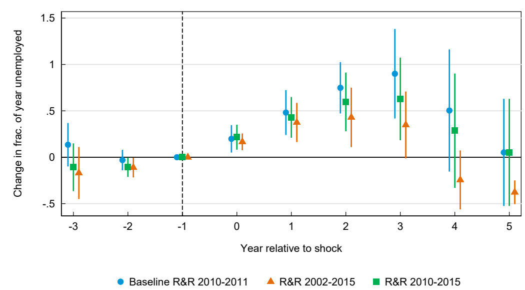
(b) Alternate Time Periods Aggregate Data



(c) Alternate Specification

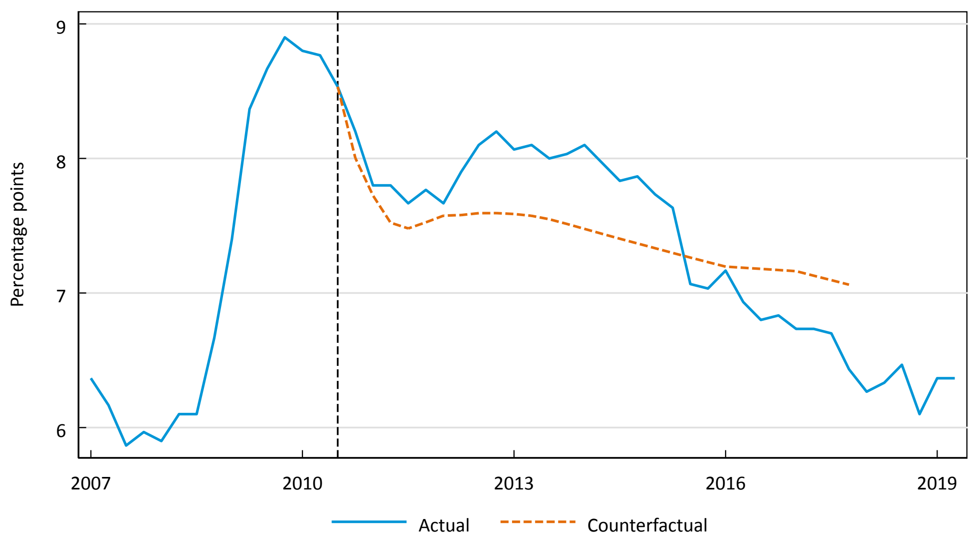


(d) Alternate Time Periods Micro data



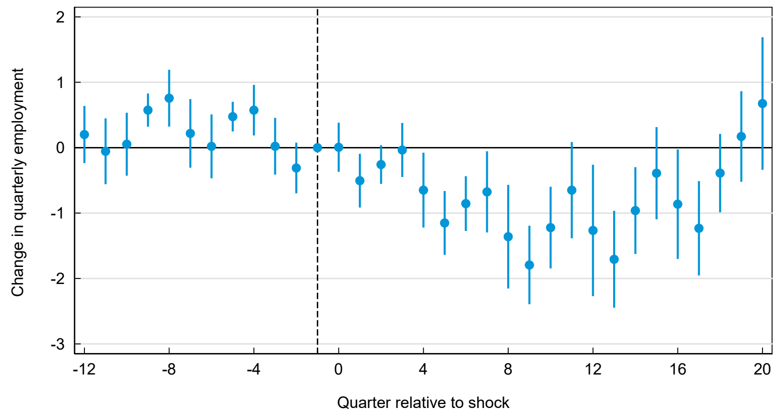
Notes: The panels differ in how the monetary shocks are constructed. Panel (a) and (b) is for the aggregate data, where in all specifications, the regression includes controls the 1, 5, 9 and 13th lags of year of year percent changes in GDP, vacancy rates, and layoff rates. The sample includes quarterly data from 1996Q1 to 2019Q2. In Panel (a), each line corresponds to shocks estimated using Equation 1 with different sets of controls. The blue circles estimate the shocks without changes in the forecasts across meetings (baseline specification), the orange triangles estimate shocks without Riksbank forecasts for GDP and green squares show shocks estimated without Riksbank forecasts for unemployment. Panel (b) shows the Romer & Romer shocks estimated with the baseline controls but using different time periods. The blue circles are the baseline, and the orange triangles use the same shocks as the baseline except without zeroing out shocks outside of 2010-2011. In both specifications, the sample includes quarterly data from 1996Q1 to 2019Q2, and shocks are set to zero in quarters in which data on Riksbank meetings is not available. Panel (c) shows estimates from a specification in which the effects of the Romer & Romer shocks are allowed to differ between 2010–11 and other periods. The “2010–11 R&R” shows the estimated coefficients on the Romer & Romer shock that has been set to zero outside of 2010–11, while the “Other R&R” shows the coefficients on the Romer & Romer shock that has been zeroed out during 2010–11, where these two sets of coefficients have been estimated jointly in a single regression. Panel (d) shows the Romer & Romer shocks estimated in the micro data at the annual frequency with alternatives on periods of active shocks. Blue circles are the baseline estimates, green squares uses the same shock as the baseline but is not zeroing out shocks after 2011, orange triangles does not zero out shocks and uses the same setup as the pre-estimate in panel (b). In all panels, bars illustrate the 95% confidence interval with heteroskedasticity-robust standard errors.

Figure A4: Robustness of Local Projections: Alternate Shock Construction



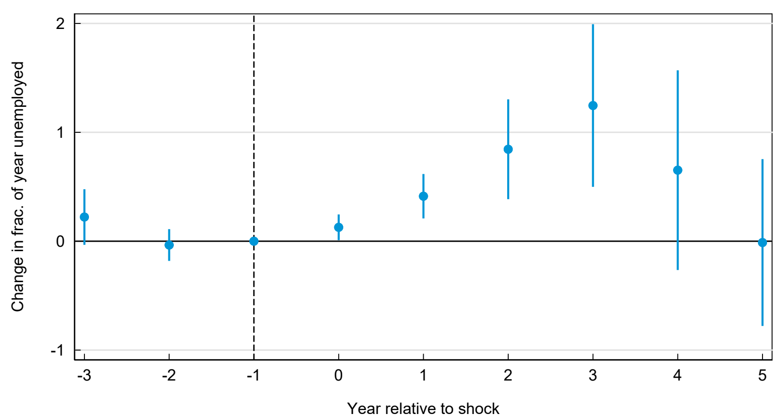
Notes: The blue solid line shows the actual evolution of unemployment. The dotted orange line is the predicted unemployment rate from a VAR(3) with GDP, vacancies, layoffs and the unemployment gap.

Figure A5: Robustness of Micro Estimates: Quarterly Estimates Specifications



Notes: Regressions are as in the main text but for individual-level data at the quarterly frequency. The outcome variable is probability that an individual is employed in the quarter, where we define employment in a quarter as positive labor earnings for at least two months in that quarter.

Figure A6: Robustness of Micro Estimates: Alternate Lagged Controls



Notes: Regressions are as in the main text for the individual-level analysis at the annual frequency but with controls for the 1- and 2-year lags of year of year percent changes in GDP, vacancy rates, and layoff rates.

Table A4: Robustness of estimates to specification within micro data

	(1) Baseline	(2) + Self Empl.	(3) + Exporters	(4) Ind. fe	(5) Post	(6) Full
2-year	0.748***	0.738***	0.691***	0.744***	0.596***	0.430**
	(0.141)	(0.138)	(0.113)	(0.139)	(0.161)	(0.163)
Observations	10,041,575	10,668,449	24,964,937	10,041,570	10,041,575	7,753,509
3-year	0.900***	0.884***	0.797***	0.897***	0.628**	0.348*
	(0.246)	(0.243)	(0.231)	(0.246)	(0.227)	(0.184)
Observations	9,431,335	10,020,030	23,453,793	9,431,311	9,431,335	7,143,086

Notes: All estimates show the effect on the fraction of year unemployed 2 years after the shock (row 1) and three years after the shock (row 2). Regression controls ans 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) includes workers linked to a firm with fewer than 2 fulltime equivalent workers (i.e. the self-employed), column (3) includes exporting and non-domestic firms. Column (4) add individual fixed effects to the baseline specification. Lastly, columns (5) and (6) show specifications using alternative monetary shocks. Estimates in columns 1 through 5 have Romer & Romer shocks estimated for 2010-2011, but column (5) includes Romer & Romer estimates from 2010-2015 along with data from 1997 and column (6) uses 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.

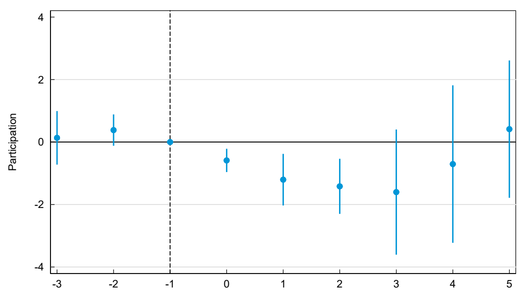
Table A5: Robustness of Estimates to the Definition of Employment and Unemployment

	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployment			Employment		
	Frac. Unempl	Unempl ≥ 1	Unempl ≥ 91	Frac. Empl.	Empl. $\geq$6	Empl. $\geq$9
2-year	0.748***	1.862***	1.262***	-2.548***	-2.237***	-2.699***
	(0.141)	(0.377)	(0.249)	(0.568)	(0.479)	(0.629)
Observations	10,041,575	10,041,575	10,041,575	10,041,575	10,041,575	10,041,575
3-year	0.900***	2.390***	1.517***	-3.048**	-2.610**	-3.341**
	(0.246)	(0.488)	(0.431)	(1.397)	(1.215)	(1.323)
Observations	9,431,335	9,431,335	9,431,335	9,431,335	9,431,335	9,431,335
Mean (2010)	0.016	0.051	0.026	0.942	0.957	0.910
Mean (sample)	0.022	0.080	0.035	0.874	0.887	0.833

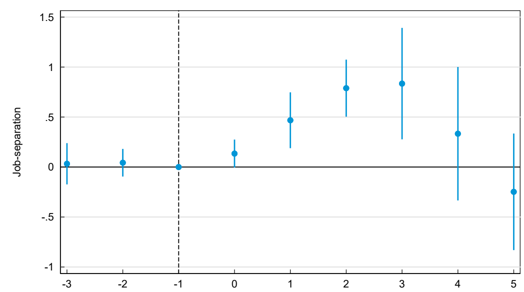
Notes: Estimates in Column 1 are as in the left panel of Figure 4 in the main text and estimates in Column 4 are as in the right panel of Figure 4 in the main text. Column 2 defines an individual as unemployed if they are registered as unemployed job-seeker at least one day in the year; in Column 3 for more than 91 days in the year. In columns 5 and 6, we define an individual as employed if annual labor earning are at least 6 or 9 times the monthly minimum wage in that year, respectively. Estimates in the first and second row are from separate regressions for each outcome and year.

Figure A7: Additional Margins of Labor Market Adjustment

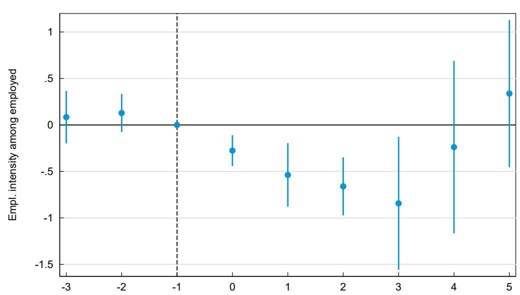
(a) Labor Force Participation



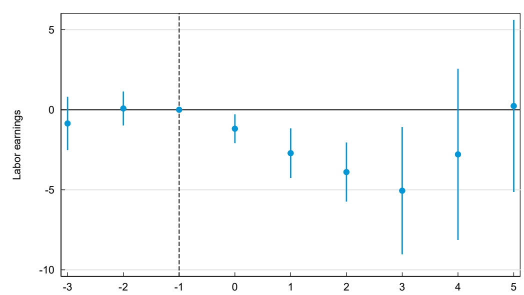
(b) E-to-U Transition Rate



(c) Employment Intensity Among Employed

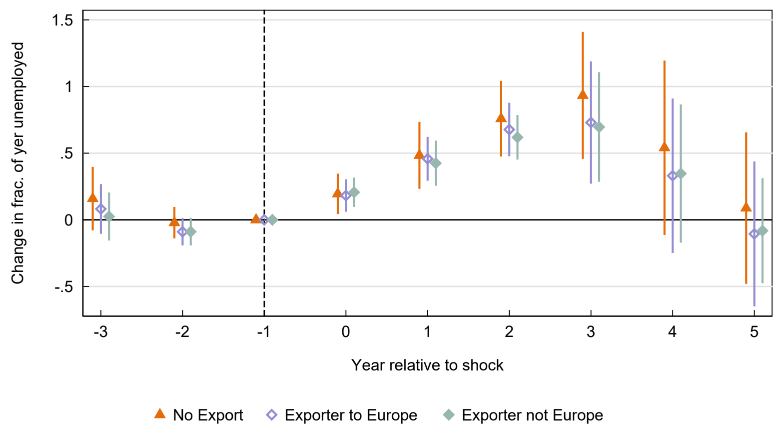


(d) Labor Earnings Among Employed



Notes: In panel (a), we define a worker as a labor market participant if they spend at least 20 percent of the year in either employment or unemployment. In panel (b), we define an individual as separating from a job if they spend at least 91 days in unemployment in the year, and transited from employment with annual earnings that amounted to at least 6-times the monthly minimum wage. In panel (c), we condition the sample to include only those workers who are employed for at least one month in each year. In panel (d), we show the effect on the log of total labor market earnings for the set of workers included in panel (c). 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. Standard errors are twoway clustered at the individual and year level.

Figure A8: Effect of Monetary Shock by Firm Export Destination



Notes: Regressions are as in Figure 5 in the main text but define exporters into those the primarily export to Europe (European Free Trade Association countries) and those that primarily export outside Europe. Export status is defined for the firm in 2009 and assigned based on whether at least two-thirds of annual export value is to countries outside of Europe. Sample is restricted to firms with output value at least as large as the average for all firms with any exports to Europe.

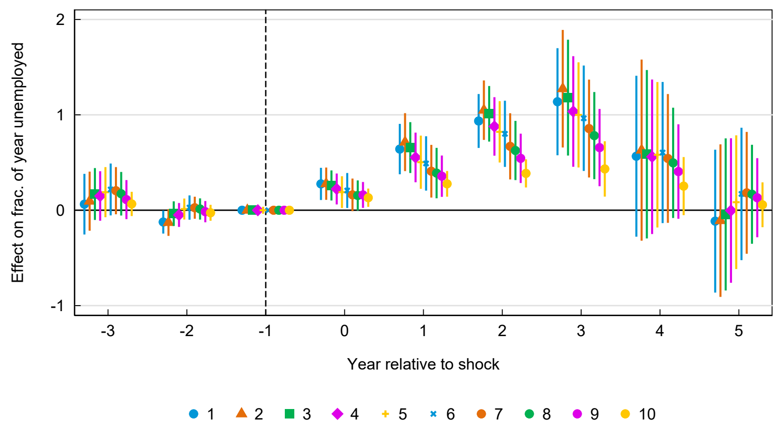
A.4 Additional Heterogeneities

Table A6: Heterogeneity Final Sample

A. Workers						
Age	(2)	(3)	(4)	(5)	(6)	(7)
Range	15 24	25 34	35 44	45 54	55 64	65 74
Obs.	110928	1963987	3420242	3394750	2642309	320419
No. of ind.	7776	106791	181279	182783	145989	21979
Education	(1)	(2)	(3)	(4)		
Range	(< High S.)	(Voc.)	(Short Ac.)	(Higher)		
Obs.	2314758	5201565	1978529	2357783		
No. of ind.	127276	280852	108325	130144		
Tenure	(1)	(2)	(3)	(4)	(5)	
Range	1 2	3 5	6 8	9 12	13 13	
Obs.	2571110	2905234	2065392	1936925	2053515	
No. of ind.	139714	158543	112650	105334	114090	
B. Firms						
Size (No. FTE)	(Few)	(Small)	(Middle)	(Large)		
Range	2 9	10 49	50 498	501 25832		
Obs.	4359477	4228493	2516443	778202		
First obs. (age)	(Young)	(Middle)	(Old)			
Range	2005 2009	1998 2004	1997 1997			
Obs.	1918411	3219242	6744962			
No. of firms	35025	41820	65447			
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.	2375576	2375689	2375643	2371580	2379746	
No. of firms	21088	21334	26714	32267	40636	
Labor Share	(1)	(2)	(3)			
Range	0.355 ≥ 1	0.211 0.355	0.000 0.211			
Obs.	3960757	3960685	3961173			
No. of firms	41019	49098	52175			
C. Contracts						
	C1. Individual		C2. Firm		C3. Sector	
	(C1. Rigid)	(C1. Flexible)	(C2. Rigid)	(C2. Flexible)	(C3. Rigid)	(C3. Flexible)
Obs.	1320923	1329685	1292314	1938106	4469636	6675631
No. of ind.	67961	67874				
No. of firms			4250	4062	43367	58165

D. Distributions										
Earnings Dist. (mode)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Obs.	942133	1011858	999786	1062300	1125612	1201109	1274233	1375803	1438369	1451412
No. of ind.	52601	55431	55332	57700	61523	64986	69325	74683	78365	79221
Earnings Dist. (last)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Obs.	1164136	1201698	1154176	1185685	1189401	1199248	1187524	1198516	1200804	1201427
No. of ind.	64917	64917	64917	64918	64922	64913	64916	64915	65394	64438

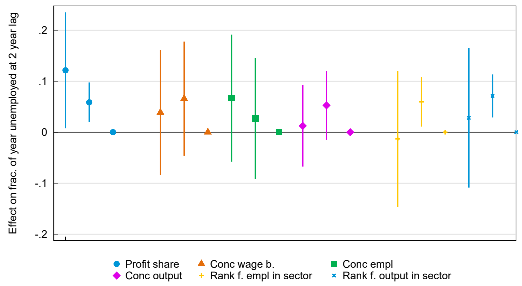
Figure A9: Earnings Distribution



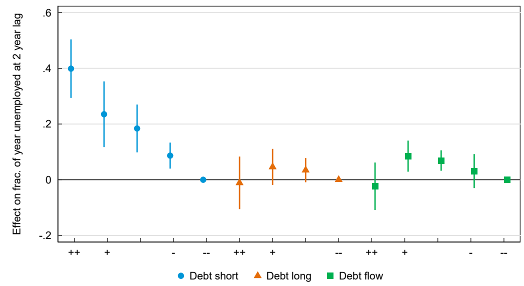
Notes: Regressions are as in Figure 9 estimated separately for each decile of the earnings distribution, with 1 representing workers in the lowest part of the distribution. Workers are pegged to their mode earnings position for 2006-2009.

Figure A10: Additional heterogeneity

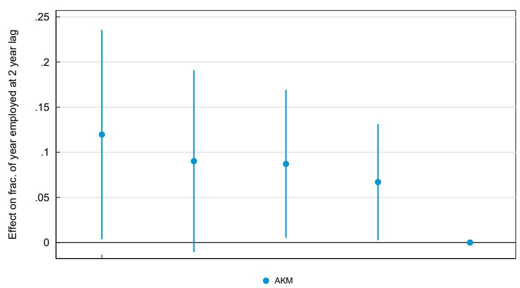
(a) Concentration



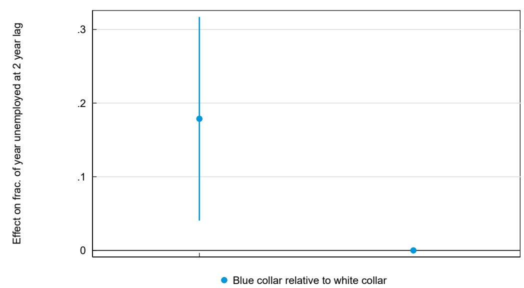
(b) Other measures of debt



(c) AKM-style quintiles

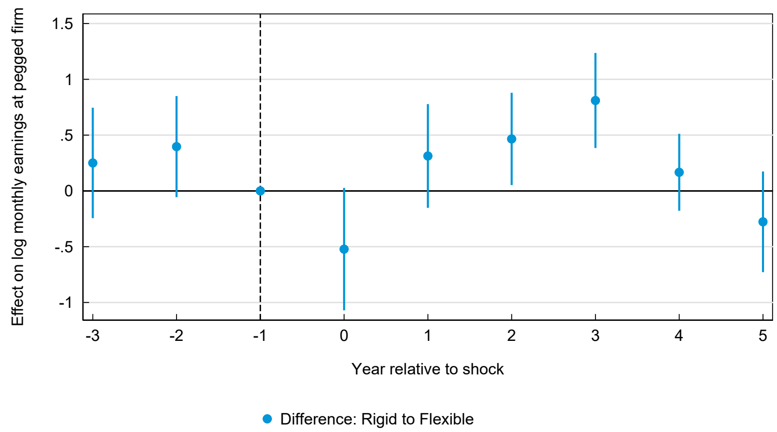


(d) Occupations



Notes: Regressions are as in Figure 9 estimated separately for each heterogeneity at the 2-year lag. (a) shows the effect across different measures of concentration and within sector rank. Profit share (net income over value added) are relative to firms with the highest profit share. Concentration is the share of wage bill or employment or sales that is accounted for by the four largest firms in the sector (across sector variation). The two last heterogeneities capture firm ranking (no of employees, annual sales) within the sector, and plotted relative to the highest ranked firms. (b) shows the effect for short-term debt, long-term debt (total long-term liabilities over assets), and debt-flow (interest rate expenses over revenues). (c) depicts heterogeneity across "AKM-type" of qunitiles. Firm AKM bins are calculated as residuals from the total wage bill regressed on worker characteristics (age, gender, education, immigration), averaged to the firm and binned. (d) shows the relative effect on unemployment for blue versus white collar workers.

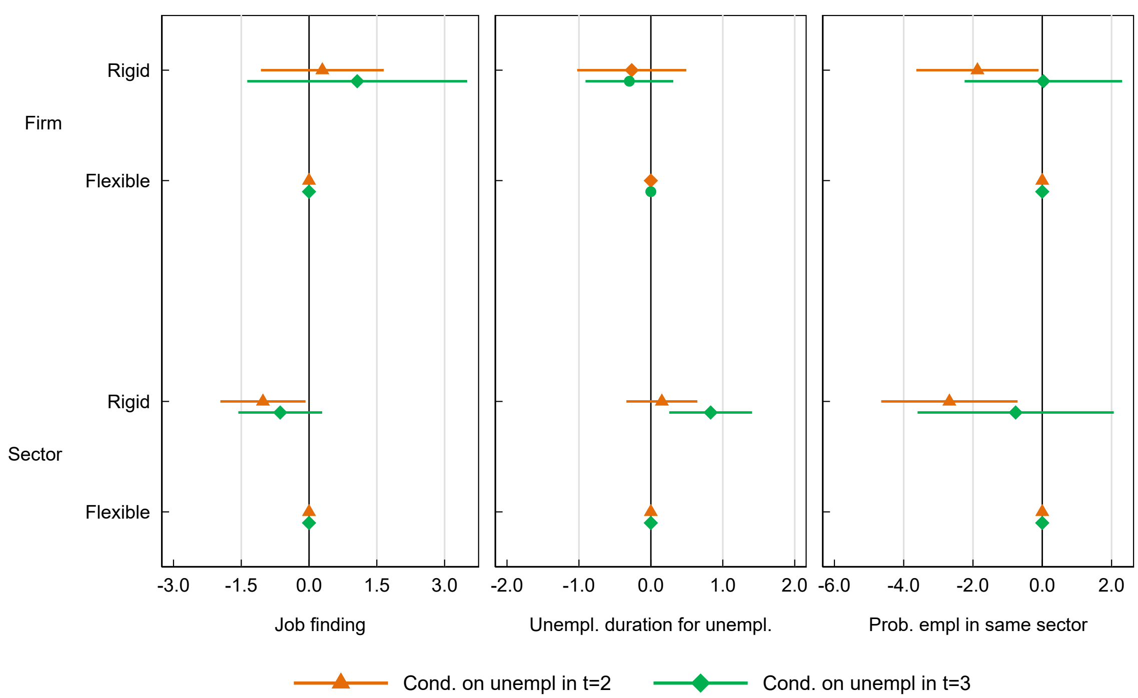
Figure A11: Monthly earnings by the rigidity of labor market contract



Notes: Sample in each year includes the set of workers that remain in their initial firms. The coefficient plots the difference in the effect on monthly earnings for workers with rigid, relative to flexible contracts.

A.5 Congestion

Figure A12: Contracts and Labor Market Congestion Effects

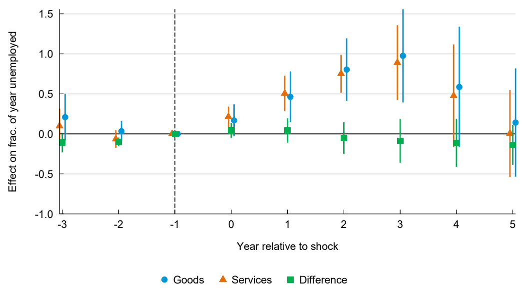


Notes: Regressions are as in Figure 12 but with job-finding (left), unemployment duration (middle), job-finding in the same sector conditional on job-finding (right) and as the outcome variable.

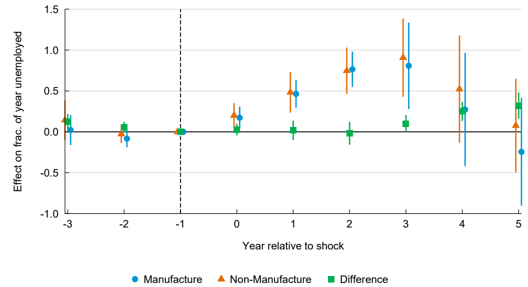
A.6 Sectors

Figure A13: Differences Across Sectors

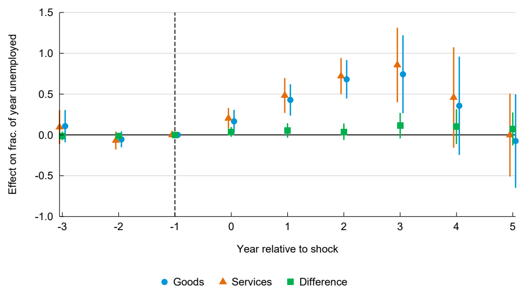
(a) Goods vs Services



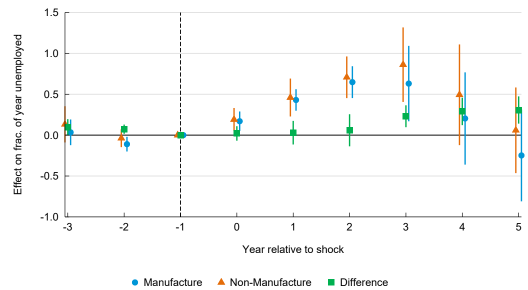
(b) Manufacturing vs Others



(c) Goods vs Services Incl. Exporters



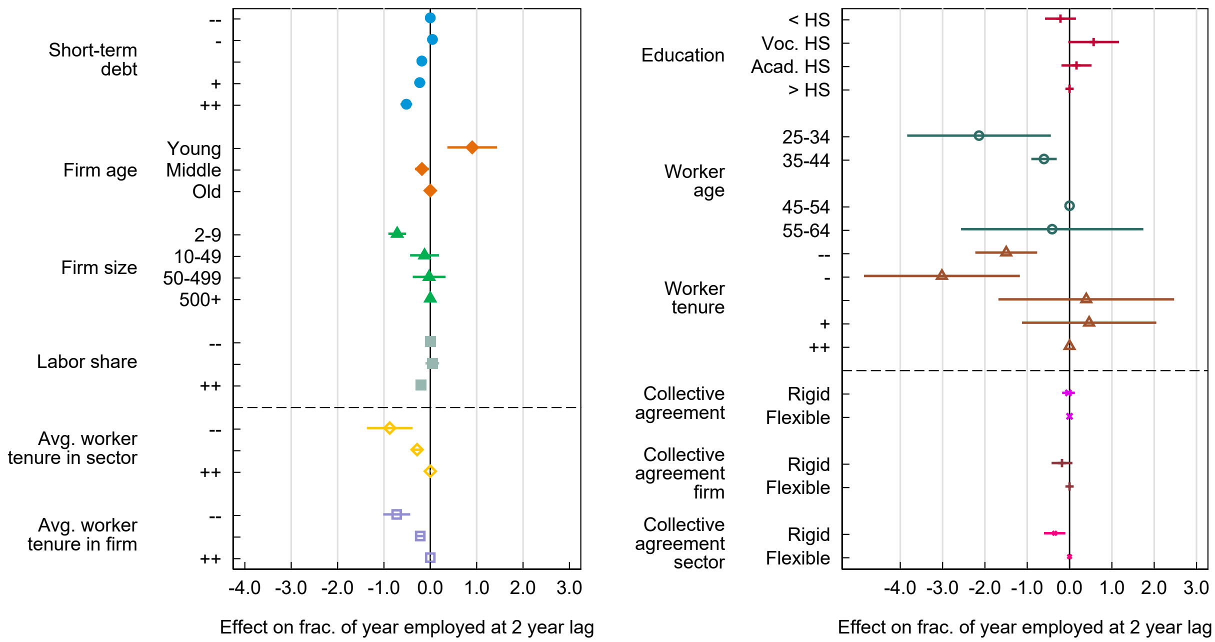
(d) Manufacturing vs Others Incl. Exporters



Notes: Equation 5, where g are different sectors. Goods are defined as the 2-digit NACE (rev. 2) code 10 to 43 , and services from 45-82 and 94-96. Code 10-33 are manufacturing. Panel (a) and (b) is for the final sample, whereas panel (c) and (d) includes multinational and exporting firms.

A.7 Robustness: Heterogeneous Employment Effects

Figure A14: Firm and Worker Heterogeneities



Notes: Regressions are as in Figure 9 and Figure 12 but for the fraction of the year employed as the outcome variable. See main text for description of sample and heterogeneities.

A.8 Heterogeneity in Worker Exposure to the Typical Business Cycle

Figure A15: Heterogeneity in Worker Exposure to the Typical Business Cycle