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**GLOBAL MONETARY AND FINANCIAL  
SPILLOVERS: EVIDENCE FROM A NEW  
MEASURE OF BUNDESBANK POLICY  
SHOCKS**

Alan M. Taylor, James S. Cloyne and Patrick Hürtgen

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## Abstract

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JEL Classification: E32, E52, F42, F44

Keywords: Bundesbank

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# Global Monetary and Financial Spillovers: Evidence from a New Measure of Bundesbank Policy Shocks<sup>\*</sup>

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## Abstract

Identifying exogenous variation in monetary policy is crucial for investigating central bank policy transmission. Using newly-collected archival real-time data utilized by the Central Bank Council of the German Bundesbank, we identify unexpected changes in German monetary policy from 580 policy meetings between 1974 and 1998. German monetary policy shocks produce conventional effects on the German domestic economy: activity, prices, and credit decline significantly following a monetary contraction. But given Germany's central role in the European Monetary System (EMS), we can also shed light on debates about the international transmission of monetary policy and the relative importance of the U.S. Federal Reserve for the global cycle during these years. We find that Bundesbank policy spillovers were much stronger in major EMS economies with Deutschmark pegs than in non-EMS economies with floating exchange rates. Furthermore, compared to monetary spillovers from the U.S., German spillovers were comparable or even larger in magnitude for both pegs and floats.

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## 1. INTRODUCTION

How does monetary policy affect the economy at home and spillover abroad? Do spillovers depend on monetary and exchange-rate regimes? Which countries, as Keynes put it, get to be a “conductor of the international orchestra”? These have long been important questions in international macroeconomics (Eichengreen, 1985, 1996; Temin, 1991; Obstfeld, Shambaugh, and Taylor, 2005), but they gained new salience amid rising concerns that U.S. spillovers may play an outsized role in global monetary and financial outcomes in all economies, advanced and emerging, fixed and floating (Rey, 2015; Kalemli-Özcan, 2019; Miranda-Agrippino and Rey, 2020). We contribute to this debate by showing that, in the late 20th century, the “international orchestra” may have had more than one conductor. Germany, as much as the U.S., shaped economic outcomes for a range of major economies, and with spillover effects aligned with the broad predictions of the trilemma, that is, stronger in pegs than in floats.

Germany from the 1970s to the 1990s provides an ideal case study for understanding the domestic and international transmission of monetary policy. During this period, Germany was a large and very open G7 economy with substantial macro-weight in global terms. Importantly, Germany had a large weight especially within Europe, which was already very liberalized in goods markets by the 1970s before going on to remove capital controls in the 1980s. German government bonds provided key benchmark interest rates in global financial markets and the role of safe Deutschmark (DM) assets grew more important as the conservative Bundesbank came to be widely regarded as a highly credible central bank in an era of high inflation. Even in the 1970s, Germany experienced lower inflation relative to, say, the U.S. or the United Kingdom following the collapse of the Bretton Woods System in 1973, the starting point of our study. Finally, the same credibility created an anchor currency role. The Bundesbank therefore played a very special and central part in European monetary policy over this period. Many countries pegged their currencies to the DM, whether *de jure* or *de facto* via the European Monetary System (EMS), hoping to gain access to Germany’s monetary credibility. This process reached its limit with the launch of the euro in 1999, thus marking the end point of our study.

To study the effects of Bundesbank monetary policy, both in Germany and abroad, the standard econometric problem is that we need some identified variation in German policy interest rates. Monetary policy — by its nature — responds to economic conditions and many interest rate changes will be correlated with other economic fluctuations. The construction of a new measure of monetary policy changes that can be used to disentangle cause and effect is our first order of business.

Our approach is in the spirit of Romer and Romer (2004). We rely on previously unused archival evidence from the German Bundesbank, which reveals the precise real-time data available to policymakers at all 580 meetings of the Central Bank Council (*Zentralbankrat*), the body which sets the German policy interest rate in the period of study.

We use this evidence to construct the first series of identified monetary policy shocks for Germany using a quasi-“narrative” approach. Our rich new real-time dataset provides a proxy for the information set of the policymaker just prior to the policy decision.

Specifically, in a first-stage “cleaning” regression, policy rate changes are regressed on this information and additional high-frequency financial information on exchange rates and the yield curve. We treat the residuals of this regression, orthogonalized against that information set, as our newly-constructed series of German monetary policy shocks. The resulting shock series is a research contribution in its own right, a useful resource for future researchers interested in all kinds of questions related to the transmission and impact of German monetary policy. With these shocks in hand, we use local projection (Jordà, 2005) techniques, together with panel data for a set of major European economies to estimate domestic and international impacts of German monetary policy.

This paper has three main sets of results. First, the domestic effects of monetary policy are reassuringly familiar. After an increase in the German policy interest rate, inflation falls with a lag, output falls, unemployment rises, credit contracts and there is a real appreciation — all of the signs of these responses align with conventional models. These results also align with findings from studies in other economies (e.g. Romer and Romer, 2004; Cloyne and Hürtgen, 2016; Champagne and Sekkel, 2018; Holm, Paul, and Tischbirek, 2021). A large battery of checks confirms the robustness of these findings.

Second, we go on to explore whether German monetary policy had important spillover effects in other countries. We find that European countries with DM pegs were much more exposed to German monetary policy shocks and experienced significant contractions following a German monetary tightening. In contrast, we find that DM floats were more insulated from these shocks.

Third, by incorporating Romer and Romer (2004) U.S. monetary shocks into our analysis we show that, compared to monetary spillovers from the U.S., German spillovers were comparable or even larger in magnitude for European economies over these years. This result adds nuance to arguments about the particular role of the Fed in shaping global economic and financial conditions. Yes, the Fed has exerted strong influence, going back many decades into the 20th century, even, as we show, controlling for the influence of other central banks. But, at the same time, the Bundesbank also exerted important influence via spillovers to third countries.

There is, of course, a large literature focused on how to identify exogenous variation in monetary policy. One popular approach uses high frequency financial markets data and extracts the “surprise” movement in interest rates around policy decisions (notable examples include Kuttner (2001), Bernanke and Kuttner (2005), Gürkaynak, Sack, and Swanson (2005), Gertler and Karadi (2015) and Nakamura and Steinsson (2018)). One drawback of this approach is that high frequency data is typically only available for recent years, which limits how far back in history it can be applied. Older structural vector autoregression methods (for example, Christiano, Eichenbaum, and Evans (1999), among

others) rely on timing assumptions and have sometimes produced some puzzling results (such as an increase in prices following a monetary contraction).

In our work, as an alternative route, we follow the approach of [Romer and Romer \(2004\)](#) that constructs a proxy for the information set of the policymaker at each policy meeting and uses this to orthogonalize movements in policy rates around meeting dates. For the U.S., [Romer and Romer \(2004\)](#) use the Fed's Greenbook forecasts that are prepared ahead of each policy meeting. [Cloyne and Hürtgen \(2016\)](#), [Champagne and Sekkel \(2018\)](#), [Holm, Paul, and Tischbirek \(2021\)](#) use similar approaches for the U.K., Canada and Norway.

In the spirit of [Romer and Romer \(2004\)](#) we collected detailed historical data from the Bundesbank archive. Our goal is also to capture the key information presented and discussed at the meeting ahead of the policy decision. We focus on key macroeconomic indicators that were frequently discussed in the meeting itself, as evidenced in the minutes from the policy meetings. But, in the same spirit, we also draw on the high-frequency approach by collecting high-frequency financial information on exchange rates and the yield curve, which was available in real time to the Central Bank Council. Reassuringly, the shocks we construct are unpredictable based on other macroeconomic indicators. Our shocks also generate statistically significant movements in financial markets around the policy decision, but the “predictable” component of the interest rate change does not. These refinements to the original approach provide further reassurance that we are capturing exogenous variation in interest rates. Relative to the existing literature, we therefore construct the first series of monetary shocks for Germany following a [Romer and Romer \(2004\)](#)-type methodology. We also study the effects of these new monetary shocks using panel data on a range of major European economies.

We evidently also relate to the large international macroeconomics literature on monetary policy spillovers. If the trilemma assumptions strictly hold, monetary transmission from the base country to countries that peg should be stronger than to countries that float, but the existence of “dirty” regimes means that the distinctions may not be as sharply binary as in the simplest theoretical setting. An empirical literature continues to search for evidence in favor, or against, this view<sup>1</sup> Two especially relevant papers are by [Degasperis, Hong, and Ricco \(2020\)](#) and [di Giovanni, McCrary, and von Wachter \(2009\)](#). Other closely related works include [Dedola, Rivolta, and Stracca \(2017\)](#), [Iacoviello and Navarro \(2019\)](#) and [Corsetti, Kuester, Müller, and Schmidt \(2021\)](#).<sup>2</sup> An extension to an advanced-and-emerging economy sample ([Obstfeld, Ostry, and Qureshi, 2019](#)) also argues that the evidence still supports the baseline trilemma view.

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<sup>1</sup>Early works that studied Fed spillovers include [Forbes and Chinn \(2004\)](#), [Canova \(2005\)](#), and [Ehrmann and Fratzscher \(2009\)](#). Another recent strand of the literature has looked at spillovers from both the U.S. and the euro area, e.g., [Ehrmann and Fratzscher \(2005\)](#), [Fratzcher, Duca, and Straub \(2016\)](#), [Brusa, Savor, and Wilson \(2020\)](#).

<sup>2</sup>[Corsetti et al. \(2021\)](#) show that after 1999, euro area shocks seem to have similar spillover effects to neighboring countries irrespective of their exchange rate regime.

We make two contributions to this broad literature. First, we provide evidence from a major base country, Germany, which was at the center of the EMS over a key period from the 1970s to the 1990s. We find that EMS pegs were significantly more exposed to this base than floats. Second, we show that, over this period, it was not just the U.S. that exerted important influence over the business cycles of some of the world’s major economies. In fact, for many European economies, German monetary policy was at least as important as the monetary policy of the U.S. Federal Reserve, if not more so.

The rest of the paper is structured as follows. In Section 2 we set out our econometric approach and describe in detail our new data set and the identification strategy. Section 3 examines the domestic effects of German monetary shocks. This section provides a number of results that are interesting in their own right, but also serves as a useful benchmark before moving to consider international spillovers. We also show that our domestic findings are robust to a wide range of checks. Section 4 examines the monetary spillovers from Germany to countries with pegged versus floating exchange rates, and then moves on to consider the relative importance of Germany and the U.S. for a range of European countries over this period. Section 5 concludes.

## 2. EMPIRICAL APPROACH

This section explains how we go about identifying historical variation in German interest rates that can be used to study the effects on the economy at home and abroad. We then discuss our new archival data, the resulting monetary shock series and its properties.

### 2.1. Identification and the First-Stage Regression

Our goal is to construct a proxy for the information set of the policymaker just prior to the policy decision. In the original [Romer and Romer \(2004\)](#) approach for the U.S. this was achieved by collecting the Fed’s Greenbook forecasts on the state of the economy made available to FOMC policymakers just prior to their policy meeting. A regression is then used to orthogonalize changes in the policy instrument against the central bank’s forecasts. This strategy requires careful analysis on policy decision dates, measurement of the change in the policy instrument itself, and collection of real-time and forecast data. It is time-consuming to implement and requires careful historical analysis. As a result, cross-country monetary “shocks” identified using this approach are still few.

For our purposes the Bundesbank Archives contain a rich source of real-time information about what policymakers knew around the policy decision. In the spirit of [Romer and Romer \(2004\)](#) we analyze numerous historical Bundesbank policy reports, briefing materials, and data publications around every policy decision. Unlike the U.S. institutional framework exploited by [Romer and Romer \(2004\)](#), the Bundesbank did not produce macroeconomic forecasts. While this may appear a drawback, it is important to



note that forecasts were also not being analyzed and discussed by policymakers, a fact we have established through a careful reading of the minutes of the Bundesbank policy meetings.

Based on our reading of the Central Bank Council minutes and in line with [Romer and Romer \(2004\)](#) both inflation developments and real activity were closely monitored and discussed. In addition, the committee extensively discussed the money supply, especially since the Bundesbank started monetary targeting in December 1974. Furthermore, the committee also discussed financial market developments, in particular exchange rates and the domestic bond markets. Thus, we have incorporated additional variables such as exchange rates, money supply and bond yields, to capture potentially relevant information for the decision-making process. On balance, to better capture the information set we choose to include a larger set of the most recent data available to the policymakers (which essentially spans the same information set as would be typically summarized in forecasts).<sup>3</sup>

As noted above, the role of forecast data in the [Romer and Romer \(2004\)](#) approach is to capture the information set of the policymaker just prior to the decision. The information set should also be rich enough to capture beliefs about future developments and typical VARs often contain too little information in this regard. While [Romer and Romer \(2004\)](#) achieve this using forecasts, our approach is to collect a rich set of macroeconomic indicators that we find were presented and regularly discussed by policymakers at the policy meeting. By looking directly at the historical documents available to policymakers, we can accurately measure the precise information available to them in real time. In a sense there is a connection between our approach and high frequency identification. Both methods seek to strip out the systematic part of monetary policy by measuring economic conditions as close as possible to the policy decision. We will return to this point later when we examine the properties of our new series of monetary innovations.

Armed with our new real-time data set, we estimate a first-stage “cleaning” regression like [Romer and Romer \(2004\)](#), augmented for open economy aspects following [Cloyne and Hürtgen \(2016\)](#), [Champagne and Sekkel \(2018\)](#), and [Holm, Paul, and Tischbirek \(2021\)](#). We also incorporate insights from [Caldara and Herbst \(2019\)](#) who admit policy responses to financial conditions, as well as to real activity and inflation.

To orthogonalize the change in the policy instrument with respect to this information

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<sup>3</sup>The data presented to the Council in the *Statistical Overviews* (described below) contain further data on public finances, business cycle indicators such as the Ifo Business Climate Index, current account data, industrial production or the number of cars sold. However, most of these data were not provided at every meeting and/or were subject to non-trivial data transformations over the sample (e.g. from non-seasonally-adjusted to seasonally-adjusted data, changes from monthly to quarterly growth rates, the frequency of the data was too low, variable definitions changed, some data was only summarized in charts etc.) restricting us to distill a consistent and complete time series.

set we run the following [Romer and Romer \(2004\)](#) style regression:

$$\begin{aligned}
\Delta i_m &= \alpha + \beta_1 i_{m-1} + \beta_2 \Delta i_{m-1} & (1) \\
&+ \sum_{p=2}^4 \gamma_p \hat{y}_{m,t-p} + \sum_{p=2}^4 \tilde{\gamma}_p (\hat{y}_{m,t-p} - \hat{y}_{m-1,t-p}) \\
&+ \sum_{p=1}^4 \varphi_p \pi_{m,t-p} + \sum_{p=1}^4 \tilde{\varphi}_p (\pi_{m,t-p} - \pi_{m-1,t-p}) \\
&+ \sum_{p=2}^4 \rho_p u_{m,t-p} + \sum_{p=2}^4 \tilde{\rho}_p (u_{m,t-p} - u_{m-1,t-p}) \\
&+ \sum_{p=2}^4 \theta_p \Delta^{12m} MS_{m,t-p} + \sum_{p=2}^4 \tilde{\theta}_p (\Delta^{12m} MS_{m,t-p} - \Delta^{12m} MS_{m-1,t-p}) \\
&+ \sum_{c=1}^C \iota_c \log(XR_{c,m,d-1}) + \sum_{c=1}^C \tilde{\iota}_c (\log(XR_{c,m,d-1}) - \log(XR_{c,m-1,d+1})) \\
&+ \beta_3 \text{AVG\_BOND\_YLD}_{m,d-1} + \sum_{j=2y,4y,6y,8y} \zeta_j \text{BOND\_YLD}(j)_{m,d-1} + \epsilon_m.
\end{aligned}$$

This regression is run using a meeting-by-meeting frequency. The  $m$  subscripts therefore refers to a meeting date.  $i_m$  is the Bundesbank discount rate decided at a particular policy meeting by the Central Bank Council. The subscript  $p$  denotes a month (quarter) of the data release relative to the meeting date and the subscript  $t-1$  refers to information from the previous month (not information at the previous meeting). This more complex set of timing conventions allows us to incorporate a range of relevant real-time economic data that were relevant to meeting  $m$  but were released at different points in time.

Following the [Romer and Romer \(2004\)](#) specification, we regress the change in the intended policy target ( $\Delta i_m$ ) around the policy decision (in practice, between two meetings) on real-time quarter-on-quarter GDP growth ( $\hat{y}_{m,t-i}$ ), 12-month consumer price inflation ( $\pi_{m,t-i}$ ), the unemployment rate ( $u_{m,t}$ ) and 12-month money supply growth ( $\Delta^{12m} MS_{m,t-i}$ ). Money growth is added given the focus on the money supply in this historical period. In addition, we include the data revision between meetings (the second term in each row of [Equation 1](#)). To account for policy inertia we include not only the pre-meeting policy rate (as in [Romer and Romer, 2004](#)), but also one lag of the policy rate change.

Given the open economy focus of this paper and the fact that Germany is a much more open economy than the U.S., we follow [Champagne and Sekkel \(2018\)](#) and [Holm, Paul, and Tischbirek \(2021\)](#) and include the log-level and log-difference of six bilateral nominal exchange rates (denoted  $XR$  in the equation above) the day before the current meeting and one day after the previous meeting,  $m-1, d+1$ , (i.e. commonly this amounts to the exchange rate change of the last 12-13 days) between the DM and the US dollar, French franc, Italian lira, Spanish peseta, pound sterling and the Japanese yen. In [Equation 1](#) above the index  $c$  denotes country. Furthermore, to bring in additional information from

financial markets, we include the term structure of bond yields from two-year, four-year, six-year and eight-year bonds (variable *BOND\_YLD* in the equation above); and we include the average bond yield (variable *AVG\_BOND\_YLD*).<sup>4</sup> It is worth noting that, although we do not have — or necessarily need — forecast data to measure the information set of the policymaker, these additional financial markets variables are likely to capture some additional forward-looking expectations in our control set.

The regression residuals,  $\epsilon_m$ , form our new measure of monetary policy “shocks.” The shocks are then summed up to monthly frequency for the second-stage regressions below.

## 2.2. Historical Data

Overall we construct and analyze data for 580 Bundesbank Central Bank Council meetings in the December 1974 to December 1998 period. The Bundesbank started monetary targeting in December 1974 and joined the Eurosystem in January 1999. The Council meetings were held every two weeks (more frequently than FOMC meetings) and for each meeting a rich set of statistical overviews were available to Council members.

From the Bundesbank Archives we collect our rich real-time data set primarily from official historical Bundesbank documents available at the time of the policy decisions. These are not digitized and almost all of our new data do not currently exist in electronic form. Our first goal was to examine all the historical publications and ascertain which economic indicators were routinely presented and discussed by policymakers. This required us to assiduously review all the historical publications and examine the minutes of the policy decisions themselves to see what variables were typically discussed and analyzed. The next step was to carefully digitize a range of relevant indicators. When using a set of further Bundesbank sources, such as Monthly Reports, we meticulously matched the publication dates of these data to ensure it was available at the time of the Council meeting. Given the nature of the historical documents, the lack of digitally available data and the large number of policy meetings over this period, this was no small task. We hope the historical data work conducted for this paper is also an important and useful contribution in its own right.

The main source of our real-time data set is direct briefing materials that were provided to the Council.<sup>5</sup> Overall, these documents provide an excellent summary of the Council’s information set. For each Council meeting a rich set of real-time data was made available to Council members; this is the first study that exploits this information. The data comprise numerous tables and charts that were internally dubbed *Statistical Overviews about the Monetary Policy Situation*, or in German, *Statistische Übersichten zur währungspolitischen Lage*.

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<sup>4</sup>The average bond yield is actually available at a higher (daily) frequency, whereas the yield curve information is provided two to four days before the meeting.

<sup>5</sup>We thank the Historical Archive of the Bundesbank for providing access to the data. We are also grateful to the former Bundesbank President Jens Weidmann, who allowed us to include data and minutes, which were not yet available given the official 30-year publication lag.

Based on our reading of the meeting minutes, Council members were engaged in a lively debate about economic developments and the real-time data, which clearly played a key role in the decision-making process.

From our main source — the *Statistical Overviews* — we digitized the consumer price index (CPI), the unemployment rate, the 12-month money growth rate and bond yield curve data. Exchange rates were also available in these briefing materials, but they were readily available in digitized format. Every month an additional set of real-time data was circulated in statistical appendices from the monthly reports (officially referred to as *Statistical Supplements to the Monthly Reports of the Deutsche Bundesbank. Series 4: Seasonally Adjusted Economic Data*). We used these data to occasionally fill some additional data points (e.g., when some backdata were missing in the *Statistical Overviews*). [Table A.1](#) provides a detailed description of the historical data set.

One potential challenge with German data is related to possible adjustments due to the reunification in 1990. The largest part of our sample covers the time before the German reunification and we naturally use data for West Germany. After reunification the economy of East Germany was very small compared to the economy of West Germany.<sup>6</sup> In addition, data availability for East Germany was very scarce and imprecise in the first years after reunification, whereas data for West Germany was regularly reported until the end of our sample. As a consequence, data for unified Germany was often reported with a time lag compared to more readily available data for West Germany. Some real-time variables, such as GDP, are not available for unified Germany before September 1995. On balance, i.e., to maximize the amount data that reflect the largest and most complete real-time data set including the most recent information, we decided to use data for West Germany for GDP, unemployment rate, and CPI. A further, related challenge created by the reunification is that money supply experienced a “money jump”; that is, the Bundesbank increased the money supply by 15 percent in July 1990. The Council also incorporated this adjustment factor into their decision-making process and smoothed money supply; for example, a 12-month money growth of 19.6 percent was effectively treated as 4.6 percent money growth in periods bracketing the jump. We follow this practice to smooth the transition in the same way as the central bank committee did around the meeting time when the jump occurred in our data series.

Another challenge we faced with GDP data — taken from the Bundesbank’s real-time data base — is that the vintages are only matched to a month, but not to a precise publication date.<sup>7</sup> Thus, it is not clear whether the vintage data from a specific month, for example March 1983, were known at the meeting on March 6th, the one on March 20th, or

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<sup>6</sup>East Germany’s GDP in the first quarter of 1991 was less than 8 percent of GDP in unified Germany, according to the September 1995 vintage (which is the first vintage that includes data for unified Germany).

<sup>7</sup>Gerberding et al. (2005) constructed a Bundesbank real-time data base to estimate a Bundesbank reaction function using quarterly data. They stress that using real-time data yields markedly different results compared to ex-post finalized data.

only at the April 10th meeting. Therefore, we use the source information of the real-time database which is based on the *Statistical Supplements of the Bundesbank Monthly Reports* to identify the exact publication date. In this way we can safely match the vintage data to a specific Council meeting. In this particular example the data was published on March 18th, so we can safely assume that this particular GDP vintage was known at the March 20th meeting. By this procedure we refined the real-time database from a specific month to a specific date. In doing so we were also able to fill some gaps in the data set using the *Statistical Supplements to the Monthly Reports*. In reading the minutes of the meetings we confirmed that the most recent data were indeed discussed.

### 2.3. Estimation Results

After carefully constructing our real-time data set for bi-monthly Central Bank Council meetings, we isolate innovations to the policy discount rate that are orthogonal to the real-time information set of policymakers. We include all Council meetings between December 1974 and December 1998. The sample covers 580 meetings.

Table A.4 reports the results from estimating Equation 1. The estimated coefficients have the expected signs. Summing up the coefficients on inflation, real GDP growth, and money growth yields — for each variable — we find a positive effect suggesting that the Bundesbank leaned against these macroeconomic developments. Similarly, higher unemployment rates are associated with an easing of the policy rate. Summing over the coefficients associated with bond yields leads, as expected, to a positive effect. When the DM weakened between two meetings (positive coefficient) against all currencies except the yen, the discount rate was tightened mildly. Finally, the coefficient on the lagged discount rate and the lagged change in the discount rate are negative, reflecting a mean-reversion in the policy rate. The standard set of test statistics is in line with related studies (see, e.g., Romer and Romer, 2004), with  $R^2 = 0.19$ , an  $F$ -statistic of 2.86 indicating that all coefficients jointly explain a significant share of the discount rate changes, and a  $DW$ -statistic of 1.99 confirming no first-order autocorrelation.

### 2.4. The New Monetary Shocks

Before using our new shock measure,  $\epsilon_m$ , to analyze the macroeconomic and spillover effects of monetary policy, it is useful to examine some of the properties of the series.

We start by plotting our new measure of monetary policy shocks in Figure 1. As can be seen from the figure, the new shock series has a mean of zero, by construction, and the shocks are spread out across the sample. Interestingly, unlike in the U.S. or the U.K., there is no obvious regime change around 1980, suggesting that the Bundesbank policy regime was relatively consistent throughout the sample period. Our sample covers — similar to the U.S. and the U.K. — several larger policy changes exceeding 75 basis points in

magnitude, resulting in quite a few larger residualized shocks.

Our sample covers two complete policy rate cycles (see [Figure A.1](#)). Between March 1979 and May 1980 the discount rate was lifted from 3.0 to 7.5 percentage points. During this period several tightening shocks occurred as a result of the heating up of the domestic economy. Over the period from June 1982 to March 1983 the policy rate was reduced from 7.5 to 4.0 percentage points. In this period we identify three large negative monetary policy shocks. For example, in December 1982 the Council members agreed that despite the monetary easing since summer 1982 the German economy still required more stabilizing measures. Though they considered the risks of this decision for the value of the DM in currency markets, in the end that did not outweigh the risks of an economic downturn. Until April 1988 the discount rate was gradually reduced further to 2.5 percentage points — its lowest level in our sample period. Over the next two years the discount rate was raised up to 8.75 percentage points. During this episode we identified a few larger contractionary monetary policy shocks. Amongst these the largest shock we identify materializes in October 1989. At this point in time not only was the German economy booming, but also the economies in many other European economies. Economic indicators showed a substantial risk of rising prices and, consequently, the Bundesbank raised the discount rate by 100 basis points (bps). Between August 1992 and July 1996 the Council reduced the discount rate back to 2.5 percentage points, where the policy rate remained until the Bundesbank joined the Eurosystem and the ECB started to set interest rates for its member countries.

We now conduct a number of sense-checks. The first exercise considers the predictability of our new series of shocks based on other macro and financial markets data. This might be the case if we have omitted important data from the information set in regression [Equation 1](#). To do this we consider whether past economic data, denoted  $x_{t-j}$ , can predict our shock series  $\varepsilon_t$ . We run the following series of Granger causality-type tests:

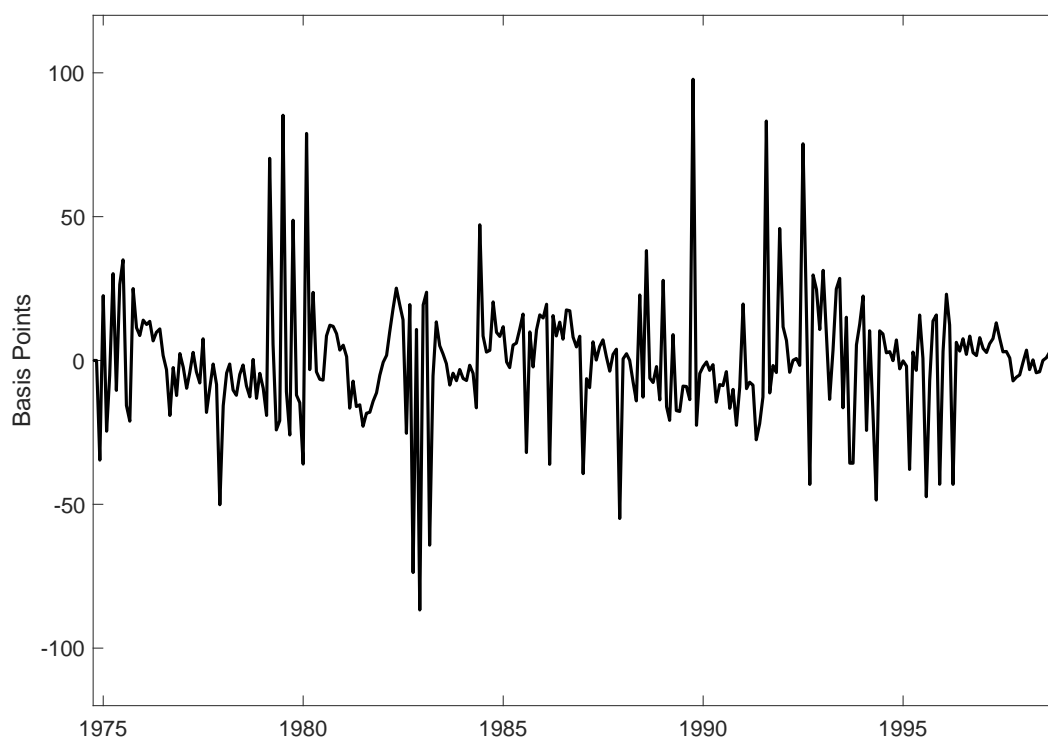
$$\varepsilon_t = \text{constant} + \sum_{j=1}^J \gamma_j x_{t-j} + u_t. \quad (2)$$

We test whether the coefficients  $\gamma_j$  are jointly significant by applying a standard  $F$ -test for lag structures  $J = \{3, 6, 12\}$ . The results are reported in [Table 1](#). For all variables and lag structures the  $F$ -statistics are small. The associated  $p$ -values are all greater than 0.1, and in most cases considerably so. We therefore cannot reject the hypothesis of lack of predictability, giving further statistical reassurance before proceeding with our analysis.

The second exercise we conduct explores how financial markets react to our shock series. If our shocks are genuinely exogenous then they should surprise financial markets. They should therefore produce a significant movement in financial market interest rates. We show that this is the case. In addition, the implied systematic part of monetary policy — in [Equation 1](#) this is  $\Delta i_m - \varepsilon_m$  — should be correlated with market expectations of the



**Figure 1:** *The Monthly Series of German Monetary Policy Shocks*



*Notes:* This figure shows our new monetary policy shock measure. This is constructed using the regression outlined in Section 2.1. These can be seen as “exogenous” innovations to the policy rate as they come from a regression where the change in the policy rate around a policy meeting has been orthogonalized with respect to information available to the policymaker at that meeting. The units of the shocks are in basis points.

policy decision. This idea is closely related to how high-frequency methods construct the surprise change in the policy rate. Below we show that movements in the implied systematic part of policy changes *do not* lead to statistically significant movements in longer-maturity interest rates, which should be the case if the systematic part is indeed expected. To sum up, these two tests confirm that (a) our policy shocks do lead to statistically significant movements in financial market variables; but, (b) the systematic part of policy does not. Although we cannot do high-frequency identification directly, as we lack historical tick data on futures contracts, these results provide further reassurance from financial markets that we are truly isolating monetary surprises.<sup>8</sup>

We note that many policy meetings ended in the afternoon, so markets may not necessarily have heard about a meeting’s outcome before closing. Consequently, the bond yield at the decision day may not contain the policy decision. Thus, to be cautious, we employ the two-day bond yield change. First, we estimate the following regression to test whether the fitted policy change (denoted  $\hat{\beta}X_t$ ) from Equation 1 induces a significant

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<sup>8</sup>We thank Refet Gürkaynak for this excellent suggestion.

**Table 1:** The table reports tests of the null that  $\hat{\gamma}_1 = \dots = \hat{\gamma}_J$ .

Variable	J = 3 lags		J = 6 lags		J = 12 lags	
	F-stat.	p-val.	F-stat.	p-val.	F-Stat.	p-val.
Unemployment	0.41	0.75	0.56	0.77	0.51	0.87
Monthly Inflation	0.59	0.63	0.41	0.87	0.32	0.97
Growth of Ind. Production	1.09	0.35	0.68	0.66	0.60	0.80
GDP Growth	0.14	0.94	0.27	0.95	0.33	0.96
Growth of Money M2	0.63	0.60	0.47	0.83	1.19	0.30
Growth of Money M3	0.35	0.79	0.32	0.93	0.70	0.71
Change in REER	1.14	0.33	1.45	0.20	1.07	0.38

movement in bond yields:

$$\Delta \text{AVG\_BOND\_YLD}_{d+1:d-1} = \gamma_1 \hat{\beta} X_t + \eta_t. \quad (3)$$

We find that  $\hat{\gamma}_1 = 0.0322$  (i.e., 3.22 basis points per 100) and is insignificant with a 95% confidence interval  $[-0.0345, 0.0989]$ . Thus, reassuringly, the implied systematic part of the policy change does not appear to lead to a significant bond yield change. Second, we check whether monetary surprises significantly move bond yields. We replace the fitted policy change with our monetary policy shock:

$$\Delta \text{AVG\_BOND\_YLD}_{d+1:d-1} = \gamma_2 \hat{\epsilon}_m + \eta_t. \quad (4)$$

Reassuringly, we find that  $\hat{\gamma}_2 = 0.0388$  and is significant with a 95% confidence interval  $[0.0007, 0.0669]$ . Thus, the shock leads to a significant bond yield change.

We have records of when the Central Bank Council meetings ended, but we do not have precise records of when their decisions (or non-decisions) were communicated to the public. As a robustness check we treat meetings that ended before noon to be known to the public before bond markets closed the same day. In this case, bond prices should respond within a one-day window around the policy meeting. We confirm that our results also hold for this sample using  $\Delta \text{YIELD}_{d:d-1}$ . Specifically, we estimate  $\gamma_2 = 0.0338$  and find that the monetary policy shock raises the bond yield significantly with a 95% confidence interval  $[0.0011, 0.0665]$ . Results are also robust when classifying 1 p.m. meeting ends as information revealed to financial markets the same day. Starting in 1991, 3-month and 6-month bond returns are available. In both cases the shock predicts a significant move in these returns.  $\gamma_2 = 0.08$  with 95% confidence interval  $[0.02, 0.15]$  and  $\gamma_2 = 0.08$  with 95% confidence interval  $[0.01; 0.15]$ , respectively.



### 3. THE EFFECTS OF MONETARY POLICY IN GERMANY

In this section we use our new measure of monetary policy shocks to examine how interest rate changes affect the macroeconomy in Germany. We first present impulse response analysis, then consider variance decompositions and, finally, discuss a range of robustness checks. In the next section we will consider monetary spillovers to other countries.

#### 3.1. Empirical Specification

To study the effects of a shock to monetary policy we estimate the impulse response function (IRF) for different variables of interest. To do this we estimate the following sequence of local projections:

$$y_{t+h} - y_{t-1} = \alpha_h + \Psi_h(L) \mathbf{x}_t + \beta_h \varepsilon_t + u_{t+h}, \quad (5)$$

where  $y_{t+h}$  is the dependent variable at horizon  $t+h$ ,  $\alpha_h$  is a constant and captures the mean of  $y_{t+h} - y_{t-1}$  for horizon  $h$ ,  $L$  is a lag polynomial for the control variables captured in  $\mathbf{x}_t$ , and  $\varepsilon_t$  is our monetary policy shock. This specification is very close to the local projection specification in [Holm, Paul, and Tischbirek \(2021\)](#) and the robustness section of [Cloyne and Hürtgen \(2016\)](#).

The data are monthly and our sample runs from 1974:12 to 1998:12. The baseline specification includes three lags (one quarter) of all control variables. We also allow for twelve lags (one year) of the monetary policy shock. The rich set of controls and lags of the shock itself are designed to purge any further conditional dependence on lagged information ([Miranda-Agrippino and Ricco, 2019, 2021](#)).  $\mathbf{x}_t$  contains (lags of) all the variables of interest we consider below: (log real) industrial production, the (log) consumer price index, the unemployment rate, (log nominal) loans, and the (log real) effective exchange rate. We also include a commodity price index. Our shocks should be capturing exogenous variation in monetary policy. As such, we do not impose the commonly employed timing restriction that monetary policy affects activity and prices with a lag (the so-called “recursiveness” assumption).<sup>9</sup> All variables are expressed in differences, except the unemployment rate.

In a robustness section below we show that the main results are not materially affected by the choice of controls or the lags. This is to be expected if the shock is genuinely exogenous, but it is commonplace to include such controls to avoid chance correlation in small samples and as “insurance” in case the shock is not fully exogenous with respect to the variables of interest.

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<sup>9</sup>In the robustness section, however, we show that imposing this assumption does not affect our main conclusions. [Miranda-Agrippino and Ricco \(2019\)](#) raise concerns about findings that are sensitive to the imposition of this assumption.

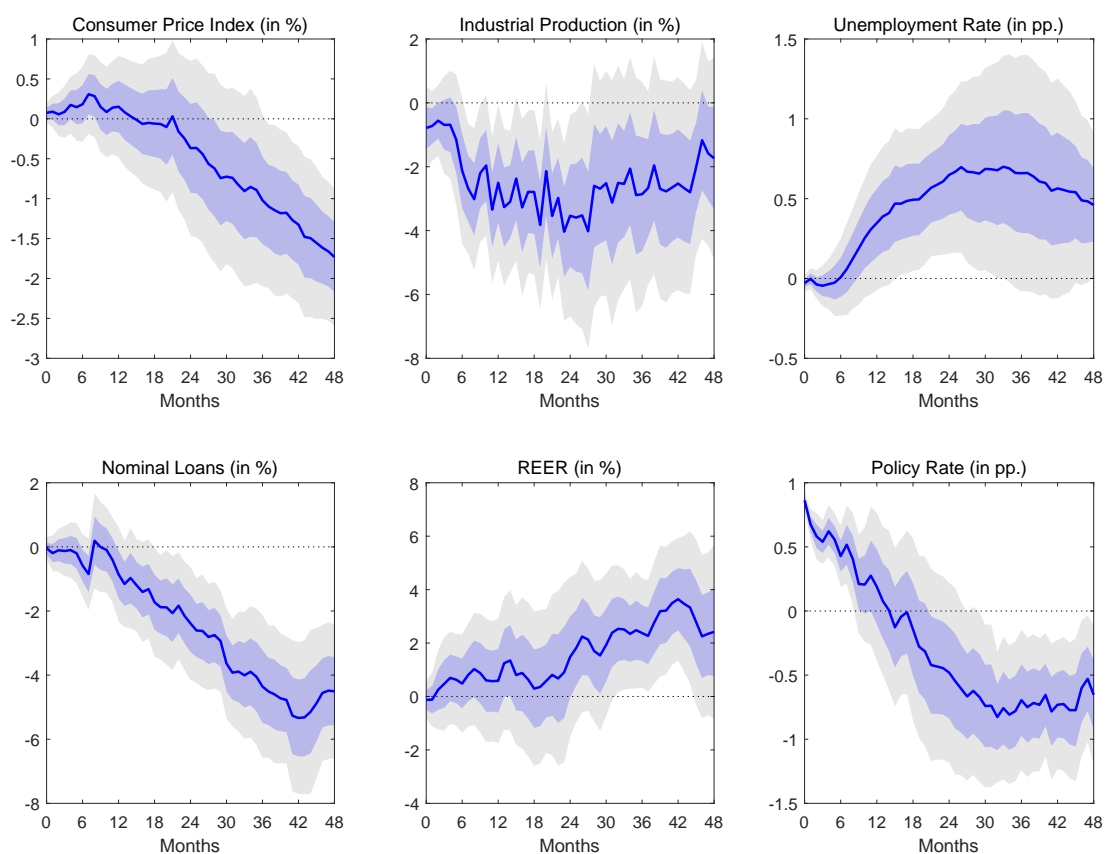
### 3.2. Results

Figure 2 shows our baseline results. The blue lines show the point estimates and the blue and gray shaded areas report the one and two standard error bands. Because the forecast errors in Equation 5 may be serially correlated, standard errors are calculated using the approach of Newey and West (1987).

The results show that a 1 percentage point rise in the policy rate leads to sizable and persistent effects in Germany. The bottom right panel shows the average monetary experiment in the data, that is, the own response of the policy rate itself. The policy rate tightens, remains higher than originally expected for around 18 months and then the policy tightening is reversed. This generates a contractionary effect on the German economy over this period. Industrial production starts to decline relatively quickly, and declines by -3 to -4% over two years. Much of this effect is felt in the first year. The unemployment rate also rises steadily, peaking at around 0.75 percentage points higher after 2 to 3 years. Consumer prices also decline, although similar to results in Romer and Romer (2004) and Cloyne and Hürtgen (2016) prices do not seem to respond much over the first 18 months. The overall decline in the price level is around -1.5%. Using the 12-month change in consumer prices directly, we document that CPI inflation declines significantly by around -0.7 percentage point (see Figure A.2). The inflation rate returns to zero in the medium term, although this persistent decline in inflation leaves the price level permanently lower, at least at the 48 month horizon. Nominal credit also declines sharply, by around 5% after 3 to 4 years. The monetary tightening generates a steady appreciation of the real exchange rate, which is nearly 4% higher after 3 to 4 years.

How do these results compare to other papers in the recent literature? Comparisons are complicated by different empirical specifications (local projections vs. VARs for example) and by different monetary experiments (for example, different papers may have different paths for the interest rate, as discussed in Cloyne and Hürtgen (2016)), but it is still interesting to make a few general comparisons. Our findings for Germany are a bit larger than the effects for the U.K. in Cloyne and Hürtgen (2016). In their local projection specification, a 1 percentage point rise in the policy rate lowers industrial production by around 2%, although the dynamics are similar to those presented here. In terms of the U.S., Romer and Romer (2004) also report a decline in industrial production of more than 4%, although Coibion (2012) compares estimates across a range of specifications and reports a number of results around -2%. Holm, Paul, and Tischbirek (2021), using a similar local projection specification to ours, also report a decline in industrial production of around -4% for Norway (and with similar policy rate dynamics). Champagne and Sekkel (2018) for Canada report a peak decline in real GDP of around -1%. There is clearly variation across countries in the magnitude of the peak effect, which is potentially to be expected, but broadly these papers all tell a similar story regarding the large and persistent real effects of monetary policy changes.

**Figure 2:** *Effect of a Monetary Tightening on Key Variables in Germany*



*Notes:* This figure shows the IRFs following a 100 bps rise in the policy rate on a number of key variables for Germany. A rise in the exchange rate means an appreciation. The regression includes 12 lags of the shock and 3 lags of the controls. The control variables include CPI, industrial production, unemployment rate, loans, and the real effective exchange rate. Shaded areas are 95 and 68 percent confidence intervals calculated using the approach of [Newey and West \(1987\)](#).

Given these large and significant effects, a natural question is: how important were these monetary shocks for the business cycle in Germany over this period? [Figure 3](#) shows the forecast error variance decomposition for the contribution of these monetary policy shocks to each of the variables studied in [Figure 2](#). To calculate the FEVD in a local projections environment, we apply the approach of [Gorodnichenko and Lee \(2020\)](#). [Figure 3](#) shows that monetary policy shocks account for around 40% of the variation in industrial production, 30% of the unemployment rate and 40-50% of the variation in loans and the real exchange rate at the peak (also the contribution is much more modest in the first two years for these two variables). The contribution for consumer prices peaks at around 35%. To help benchmark these numbers, [Gorodnichenko and Lee \(2020\)](#) find that [Romer and Romer \(2004\)](#) monetary policy shocks in the U.S. account for around 25% of the volatility of real GDP and around 50% of the volatility of inflation.

### 3.3. Robustness

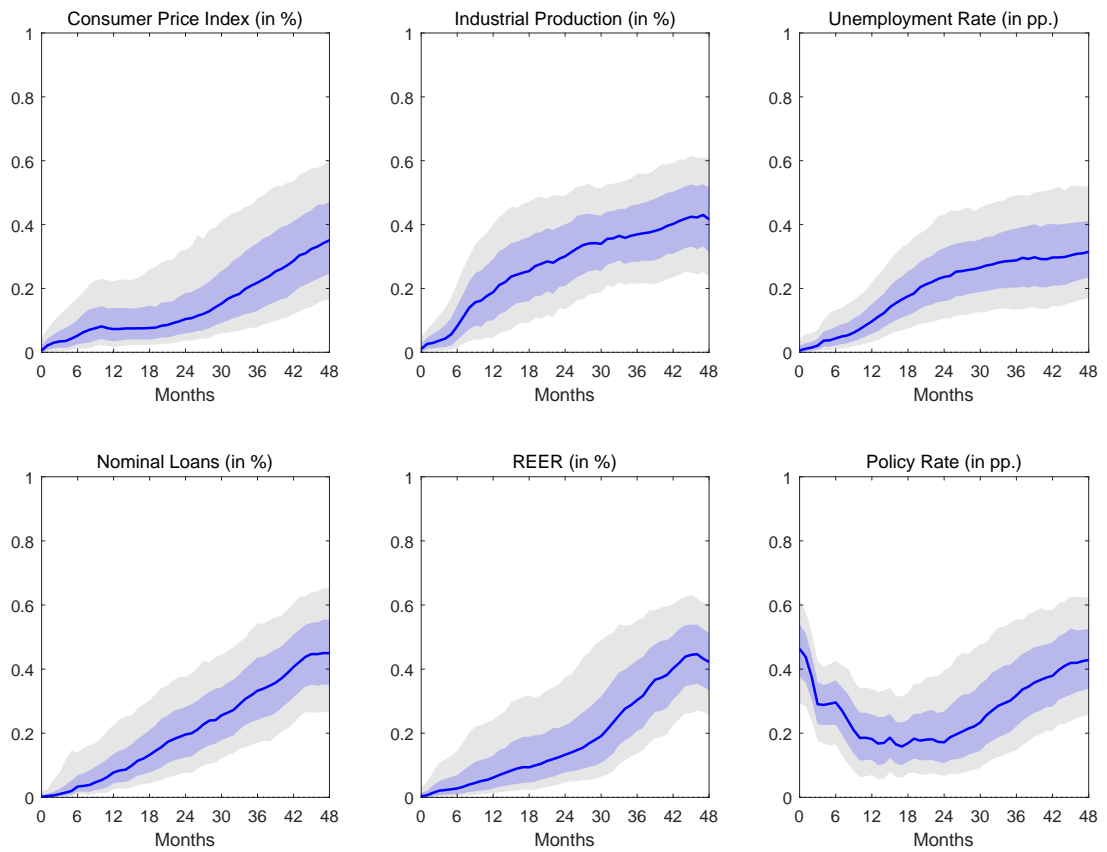
Before moving on to consider monetary spillovers from Germany, we first discuss a number of robustness checks on our results above.

First, [Appendix B](#) shows that very similar results to those in [Figure 2](#) are obtained when we use different variations of the empirical specification. In the appendix we show results (i) using inflation and credit growth rate rather than log CPI and log loans ([Figure A.2](#)); (ii) using different lag lengths of the shocks and the controls ([Figure A.3](#)); (iii) restricting the sample period to 1979:4 to 1998:12 when Germany was in the EMS (this is the sample we will focus on in the next section to analyze spillover effects) ([Figure A.4](#)); (iv) using the pre-reunification sample for the second-stage regressions, providing evidence of sub-sample stability in our results ([Figure A.5](#)); (v) a local projection specification in log levels rather than differences ([Figure A.6](#)).

Second, [Appendix Figure A.7](#) imposes a further identification assumption that is common in some applications of the [Romer and Romer \(2004\)](#) approach. In the baseline specification above we do not restrict the impact effect,  $h = 0$ . In [Romer and Romer \(2004\)](#), however, the impact effect is restricted to zero. This is justified by a common assumption that monetary policy takes longer than one month to affect a number of variables. However, an alternative approach is available which allows for contemporaneous effects. This can be implemented in a local projections environment by including *contemporaneous* variables in the control set  $\mathbf{x}_t$  (e.g., [Ramey, 2016](#)).<sup>10</sup> In [Appendix Figure A.7](#) we also implement

<sup>10</sup>More specifically, consider the main regression:  $y_{t+h} - y_{t-1} = \alpha_h + \mathbf{\Psi}_h(L)\mathbf{x}_t + \beta_h \varepsilon_t + u_{t+h}$ . In addition to including lags of all our key variables in  $\mathbf{x}_t$  we now include the contemporaneous values as well, including for the dependent variable  $y_t - y_{t-1}$ . This means that we are then controlling for any variation in the shocks that might still be driven by these additional controls. Current period outcomes can potentially influence  $\varepsilon_t$ . For this to be a valid exercise, we have to assume that the shocks  $\varepsilon_t$  do not affect these variables contemporaneously. By including  $y_t - y_{t-1}$  directly on the right hand side,  $\beta_0$  is then being forced to zero. These are the same assumptions that are usually imposed in a VAR via a Cholesky decomposition: variables can affect policy

**Figure 3: Contributions to Macroeconomic Volatility**



*Notes:* This figure shows the FEVD for the contribution of monetary shocks to a number of key variables for Germany. The regression includes 12 lags of the shock and 3 lags of the controls. The control variables include CPI, industrial production, unemployment rate, loans, and the real effective exchange rate. Shaded areas are 95 and 68 percent confidence intervals calculated using the bootstrap approach of [Gorodnichenko and Lee \(2020\)](#).

this procedure and force the impact effect to be zero. This is similar to the timing restriction often imposed in the VAR literature via a Cholesky decomposition of the variance-covariance matrix of the VAR residuals. Appendix [Figure A.7](#) shows that the results are actually very similar to our baseline, which does not make these additional assumptions. Our baseline results show some movement on impact, but these magnitudes are typically very small. In the robustness check the IRFs are therefore not materially affected by this further restriction. Finally, we show in [Figure A.8](#) that our results are robust when using quarterly data. This specification allows us to include real GDP, which declines markedly by up to  $-2\%$ .

#### 4. MONETARY SPILLOVERS

Thus far we built a new dataset, an identified monetary policy shock series for the German Bundesbank from the 1970s to the 1990s. We then demonstrated how German economic outcomes responded to these shocks using the method of local projections. The results align with standard closed-economy macroeconomic models with nominal rigidities. This empirical evidence from a novel setting buttresses existing work on monetary policy impacts which has often relied on U.S. responses to Federal Reserve policy shocks.<sup>11</sup>

In this section of the paper, we showcase another use of our new data to shed light on a different area of current research. Both Germany and the U.S. are open economies and our data can inform an important ongoing debate in international macroeconomics on monetary policy spillovers. This debate has also understandably had a main focus on the U.S., with keen interest in how Federal Reserve actions may spill over to economic outcomes in the rest of the world, whether via trade or financial mechanisms, and how these impacts are mediated or not by the exchange rate regime (e.g., [Rey, 2015](#); [Obstfeld and Taylor, 2017](#)).

We focus on a range of macroeconomic outcomes across advanced economies in Western Europe for our sample period and our main findings are that: first, German monetary policy spillovers are strong and statistically significant; second, they are at least as strong as U.S. spillovers; and third, German spillovers are stronger still in economies pegged to the DM than in those that float. This is plausible: European economies could never be 100% insulated from outside economies like the U.S. and Germany, with their large roles in global trade and financial markets, but a flexible exchange rate still served as an important shock absorber.

Hence, our evidence on monetary spillovers in this period aligns with the precepts of the classic trilemma, and is consistent with other work based on this period using

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contemporaneously, but policy does not affect the outcome variables in the first period. See [Ramey \(2016\)](#) for further discussion.

<sup>11</sup>Examples of influential U.S. studies among many include [Romer and Romer \(2004\)](#); [Gürkaynak, Sack, and Swanson \(2005\)](#); [Gertler and Karadi \(2015\)](#). For the U.K. see [Cloyne and Hürtgen \(2016\)](#). For the euro area see [Jarociński and Karadi \(2020\)](#).

different approaches as noted below. However, we should emphasize that ours is a study of multi-country spillovers from monetary policy shocks in a particular sample period, the pre-euro Bundesbank era ending in 1999. As we now go on to place our results in the context of the spillover literature, we note that in other, more recent periods the relative strength of various mechanisms of international spillover transmission could well have shifted (e.g., financial and credit channels).

#### 4.1. Spillover Debate in Context

If the trilemma strictly holds, the transmission of rates from a center or “base” currency should be much stronger to a partner that pegs credibly to the base as compared to one that floats (Obstfeld and Taylor, 1998). The differences will be sharpest for hard pegs and clean floats, and weakest for dirty pegs and managed floats. The first long-run evidence was supportive of this view (Obstfeld, Shambaugh, and Taylor, 2004, 2005).

However, whether there are implications beyond the interest rate for real or financial outcomes in the partner will depend on other mechanisms. As is evident from a simple Mundell-Fleming model (see, e.g., Degasperi, Hong, and Ricco, 2020), transmission via the trade channel will depend on elasticities, as well as on the domestic policy regime. For example, assuming a fixed home interest rate, a Fed tightening for a USD peg will entail export demand shrinkage, but an offsetting devaluation, with an ambiguous net demand effect depending on the relative strength of income and substitution effects; beyond that, the home interest rate could vary to achieve domestic goals under a float. But under a peg, the devaluation offset is lost, and contraction is amplified by home tightening. Yet these sharp inferences could be overturned if other mechanisms come into play, e.g., financial channels where additional effects might work through credit conditions and/or risk premia. In this setting, a suitably augmented Mundell-Fleming model can make this intuition clear, too (Degasperi, Hong, and Ricco, 2020). If these other forces are very strong — that is, if dollar financial spillovers are powerful enough — the exchange rate regime may come to matter less (Rey, 2015; Obstfeld and Taylor, 2017; Miranda-Agrippino and Rey, 2020; Kalemli-Özcan, 2019).

Since theory can cut either way, the empirical literature has grown large in search of evidence.<sup>12</sup> Two especially salient papers for us are the recent working paper by Degasperi, Hong, and Ricco (2020), and the older findings for the EMS by di Giovanni, McCrary, and von Wachter (2009). The latter look at contemporaneous real GDP spillovers in the 1973–1998 EMS era from German rates in quarterly data, finding evidence in support for the trilemma, although unlike our study they have no measure of an exogenous component in German rates, and simply use German rates as an IV for local rates in

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<sup>12</sup>Early works that studied Fed spillovers include Forbes and Chinn (2004), Canova (2005), and Ehrmann and Fratzscher (2009). Another recent strand of the literature has looked at spillovers from both the U.S. and the euro area, e.g., Ehrmann and Fratzscher (2005), Fratzscher, Duca, and Straub (2016), Brusa, Savor, and Wilson (2020).



other EMS members. The former paper reports impulse-responses for both advanced and emerging economies and finds mixed results for USD rate spillovers, with little evidence that the exchange rate regimes mattered and a larger role for financial channels.

Other closely related works include [Dedola, Rivolta, and Stracca \(2017\)](#) and [Iacoviello and Navarro \(2019\)](#), where the findings are mixed, the former declaring “no clear-cut systematic relation emerges between country responses and likely relevant country characteristics, such as their income level, dollar exchange rate flexibility, financial openness, trade openness vs. the US, dollar exposure in foreign assets and liabilities, and incidence of commodity exports” and the latter that “trade openness with the U.S. and the exchange rate regime account for a large portion of the contraction in activity.” Another recent study with an extension to a large advanced-and-emerging economy sample ([Obstfeld, Ostry, and Qureshi, 2019](#)) argues that the evidence that a floating exchange rate moderates spillovers from the base to the local economy.

Against the backdrop of this ongoing debate, we turn instead to a slightly different evidentiary framework to help adjudicate the question. Turning away from a singular focus on either U.S. or German monetary policy shocks, by dint of our new data we are the first to be able to examine the spillovers to third countries from both types of identified shocks together for the first time. And for the typical European economy in the 1970s–1990s era, this approach makes sense: any such country was as likely to be buffeted by U.S. policy as by German policy, with both the “global” and “local” hegemon being the source of potentially large monetary, trade, and financial spillovers into the domestic economy of pegs and floats alike. But which mattered more? And for which countries?

## 4.2. Empirical Specification

We estimate spillovers to European partner countries from German and U.S. monetary policy shocks by estimating the following local projection, applied to the EMS sample period from 1979:4 to 1998:12:<sup>13</sup>

$$y_{n,t+h} - y_{n,t-1} = \alpha_{n,h} + (1 - \mathbb{I}_n) \left( \Psi_h^{peg}(L) \mathbf{x}_{n,t} + \beta_h^{peg,DE} \varepsilon_t^{DE} + \beta_h^{peg,US} \varepsilon_t^{US} \right) + \mathbb{I}_n \left( \Psi_h^{float}(L) \mathbf{x}_{n,t} + \beta_h^{float,DE} \varepsilon_t^{DE} + \beta_h^{float,US} \varepsilon_t^{US} \right) + u_{n,t+h},$$

where  $y_{n,t+h}$  is the outcome variable of country  $n \in \{1, \dots, N\}$  at horizon  $h$ ;  $\varepsilon_t^{DE}$  is the exogenous German monetary policy shock;  $\varepsilon_t^{US}$  is the extended [Romer and Romer \(2004\)](#) monetary policy shock for the U.S.;  $\Psi_h^k(L)$  is a lag polynomial for control variables, including lags of the shocks and the endogenous outcome variable, for  $k = peg, float$ ; and lastly an exchange-rate regime indicator for a country which is floating with respect to the DM/Ecu is denoted by  $\mathbb{I}_n$ . The country-fixed effects  $\alpha_{n,h}$  along with  $\Psi_h^k$  and  $\beta_h^{k,DE}, \beta_h^{k,US}$  are the coefficients to be estimated, with  $\beta_h^{k,DE}, \beta_h^{k,US}$  being the IRFs of interest.

<sup>13</sup>The start of the European Monetary System was on March 13th, 1979.



This is a highly-saturated empirical specification by design. We allow for regime-specific effects of the shocks and regime-specific effects of the control variables including lagged outcomes, a more saturated choice of regressors than, say, [Corsetti, Meier, Müller, and Devereux \(2012\)](#). Saturation with a large number of controls assures that a putative shock is purged of any potential conditional dependence on lagged information ([Miranda-Agrippino and Ricco, 2019](#)). Saturation with a large number of lags is important to ensure correct recovery of the IRF at large horizons ([Jorda, Singh, and Taylor, 2020](#); [Olea and Plagborg-Møller, 2021](#)). In our baseline specification we include 3 lags of the endogenous variables, 3 lags of German and U.S. control variables, and 12 lags of the shocks. All controls are included in log-differences except for the unemployment rate, which is in levels.<sup>14</sup>

Finally, as a baseline, the DM (or ECU) exchange-rate regime indicator  $\mathbb{I}_n$  is defined in a time-invariant way, where we separate the sample countries into two bins: those that are predominantly pegged to the DM/ECU in the EMS era and those that are mostly not pegged. Specifically, based on average exchange rate volatility over the sample period, the DM pegs are defined as FRA, NLD, BEL, DEN, ITA, IRL, AUT, PRT and ESP for the entire sample; and the DM/ECU floats are defined as GBR, SWE, CHE, NOR, FIN and GRC for the entire sample. In the robustness subsection below we also report alternative IRF estimates using a time-varying definition of the exchange-rate regime indicator  $\mathbb{I}_n$  using established de-facto regime classification methods, but the results are practically unchanged.

### 4.3. Baseline Results

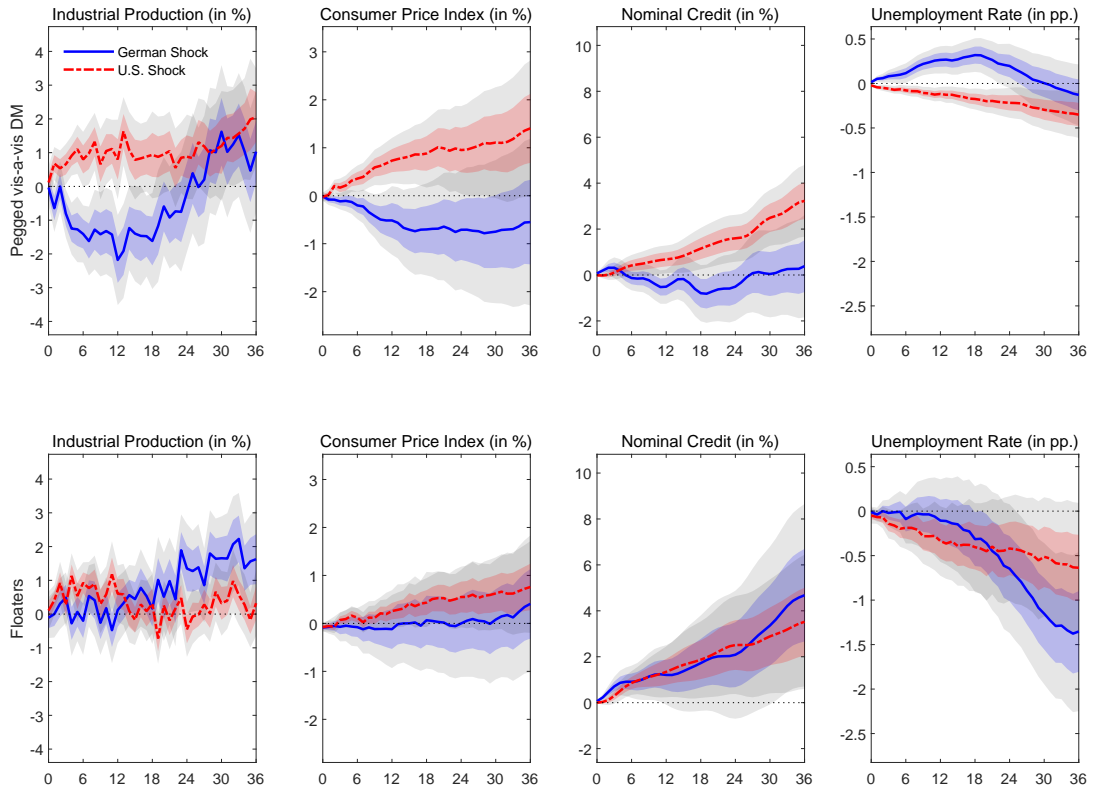
Our baseline spillover results are shown in [Figure 4](#), using the baseline EMS definition as above. The upper row of charts show local projection estimates of the IRFs for the DM/ECU-pegged economies in the EMS system (for short, pegs); the lower row of charts shows the IRFs for the non-EMS economies with a floating exchange rate (for short, floats). The baseline has the full set of regime-specific controls as above, does not employ the recursiveness assumption, includes 12 lags of the shocks, and 3 lags of all other controls.

The responses are striking. First, consider the response of domestic conditions in the partner country to a 100 bps contractionary German monetary policy shock, denoted by the blue lines. German monetary policy clearly has much stronger macroeconomic spillover effects on those countries that are pegged to DM/ECU in the upper row. The responses for industrial production, prices, and credit all show declines, while unemployment rises. In contrast, the economies that float with respect to the DM/ECU can insulate better from German monetary policy than pegged countries. The responses for industrial production,

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<sup>14</sup>We use three lags of industrial production, consumer prices, nominal credit, and the unemployment rate. In addition, we include U.S. and German control variables using log differences of consumer price indices, commodity price indices, and industrial production, as well as the level of the unemployment rates.

**Figure 4:** *Monetary Spillovers to Major European Economies: Pegs vs. Floats*



*Notes:* The figure shows the IRFs to 100 bps in the policy rate for the response of key macroeconomic variables depending on whether a country was pegged to the DM (top row) or whether the exchange rate was floating (bottom row). We show responses to the German policy shock (blue solid lines) and the U.S. policy shock (red dashed line). Shaded areas are 95 and 68 percent confidence intervals using the approach of [Driscoll and Kraay \(1998\)](#). Panel estimation is conducted with regime-specific controls and starting the sample in April 1979 to align with the start of the EMS. The main specification includes 12 lags of the shocks and 3 lags of the controls. See text for more detail.

prices, and credit are flat or rising, and unemployment is falling. That is, the two sets of responses in pegs and floats differ in a sharp and statistically significant way.

Second, when we turn to the response of domestic conditions in the partner country to a 100 bps U.S. monetary policy shock, denoted by the red dashes, instead of being different the responses in pegs and floats now have much more in common. The responses for industrial production, prices, and credit are flat or rising, and unemployment is falling.<sup>15</sup>

These findings conform with the idea that a flexible exchange rate can provide insulation. All our sample countries in the two bins float with respect to the US dollar, and their responses to U.S. shocks are comparable. But the two bins differ in having pegs and floats with respect to the DM, and their responses to German shocks are markedly different, with the pegs (floats) suffering large and adverse (small or non-adverse) responses to German shocks.

#### 4.4. Robustness

We perform a number of robustness checks to ensure that the baseline results are not fragile, starting with the large tabulation of IRFs displayed in [Figure 5](#).

**Alternative controls** First, we explore alternative control sets in the local projection panel regression. In the baseline setup the coefficients  $\Psi_h^k(L)$  are allowed to vary across the exchange-rate regime bins with  $k = peg, float$ . We then tried a less flexible alternative with pooled controls with common coefficients  $\Psi_h(L)$  across the bins, shown in [Figure 5a](#); and we also tried a more flexible alternative where the coefficients  $\Psi_h^k(L)$  are allowed to vary across countries with  $k$  indexing each country, shown in [Figure 5b](#). As these figures show, our baseline results are robust to these various specification choices.

**Restricting the impact effect** As noted in [Section 3.3](#), some papers impose a timing assumption that monetary policy cannot affect outcomes within the month. For completeness, we now re-estimate the main results under this additional assumption. We first present the baseline specification with the same set of regime-specific controls with coefficients  $\Psi_h^k(L)$ , shown in [Figure 5c](#). We then present an alternative specification with all U.S. and Germany controls excluded, shown in [Figure 5d](#). Again, as in [Section 3.3](#), our baseline results are robust to using different timing assumptions for identification.

**Other results** We report results for four other experiments in [Figure 5](#). [Figure 5e](#) shows responses calculated with a shorter lag structure consisting of just 2 lags of the shock and regime-specific controls, and with the restriction on the impact effect. [Figure 5f](#) shows

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<sup>15</sup>These results also echo some of the findings in [Ilzetki and Jin \(2021\)](#), who look at U.S. monetary spillovers to a broad range of (European and non-European) countries using [Romer and Romer \(2004\)](#) shocks.

responses calculated with CPI and credit calculated as 12-month (YoY) changes, rather than monthly, and regime-specific controls. [Figure 5g](#) shows the baseline results with 83% confidence intervals, which is more appropriate for a comparison of mean outcomes. [Figure 5h](#) shows the baseline results when Spain and Portugal are re-classified from pegs to floats, these being the two countries previously classified as EMS pegs under the baseline which have the most volatile exchange rates in the data (see [Figure A.9](#)). Again, the baseline results are robust.

**Time-variation in exchange-rate regimes** We report results for time-varying exchange-rate regimes in [Figure 6](#). Instead of assuming a uniform exchange-rate regime over time in each country, we now interact the lagged exchange-rate regime,  $\mathbb{I}_{n,t-1}$ , with the shock and control variables, but in all other respects keep the specification the same as the baseline. To code exchange-rate regime variation, we refer to [Figure A.9](#) which illustrates the exchange-rate regime classifications based on [Ilzetzi, Reinhart, and Rogoff \(2019\)](#) and also plots normalized exchange rate levels against the DM over time. On balance, from our reading of these data and the historical narratives, we employ the following classifications for the EMS period:

- Mainly Pegged to DM: France, Netherlands, Belgium, Denmark, Austria, Ireland, Italy (floating from 1992:9 to 1993:3), Spain (floating from 1979:4 to 1985:12), and Portugal (floating from 1979:4 to 1990:12)
- Mainly Floaters: UK, Switzerland, Norway, Sweden, Finland (pegged from 1997:1 to 1998:12), and Greece (pegged from 1996:1 to 1998:12).

Here again, [Figure 6](#) shows that the message of the baseline specification remains intact in this case too.

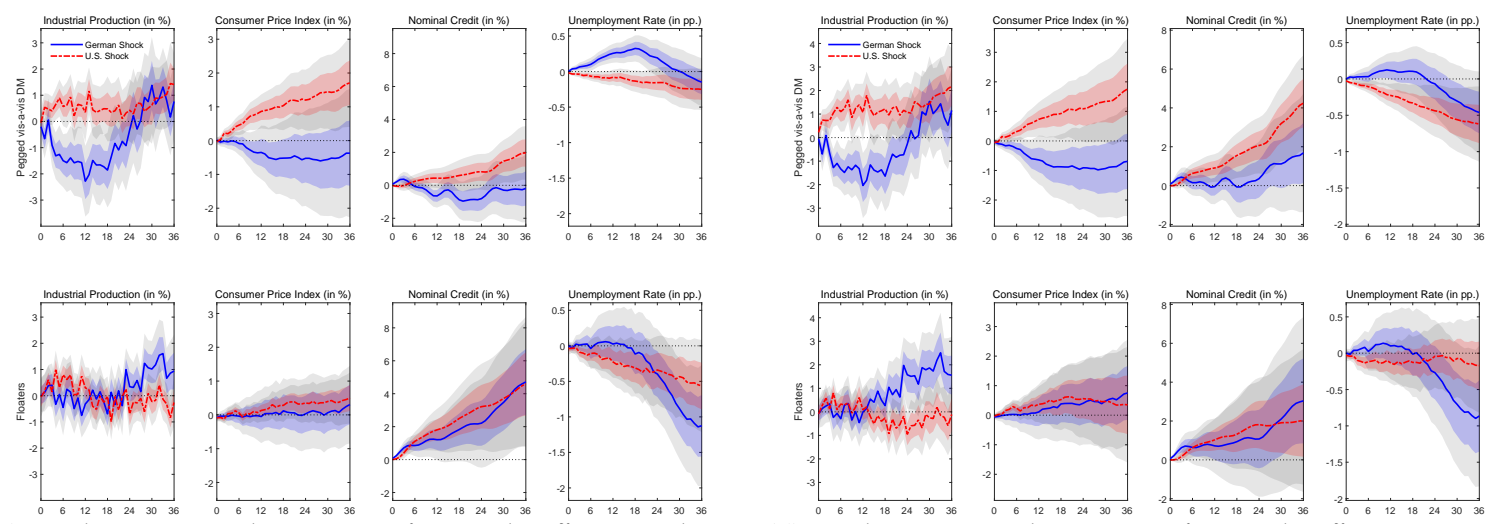
## 4.5. Summary

For the first time we are able to simultaneously look at monetary policy spillovers to third countries from the U.S. and Germany over a long time frame using identified policy shocks for each base country. Across Western Europe, the exposure to U.S. shocks was similar across DM pegs and DM floats, but at the same time the exposure to German shocks radically differed between DM pegs and DM floats: a DM float was insulated from a German shock, while a DM peg was not.

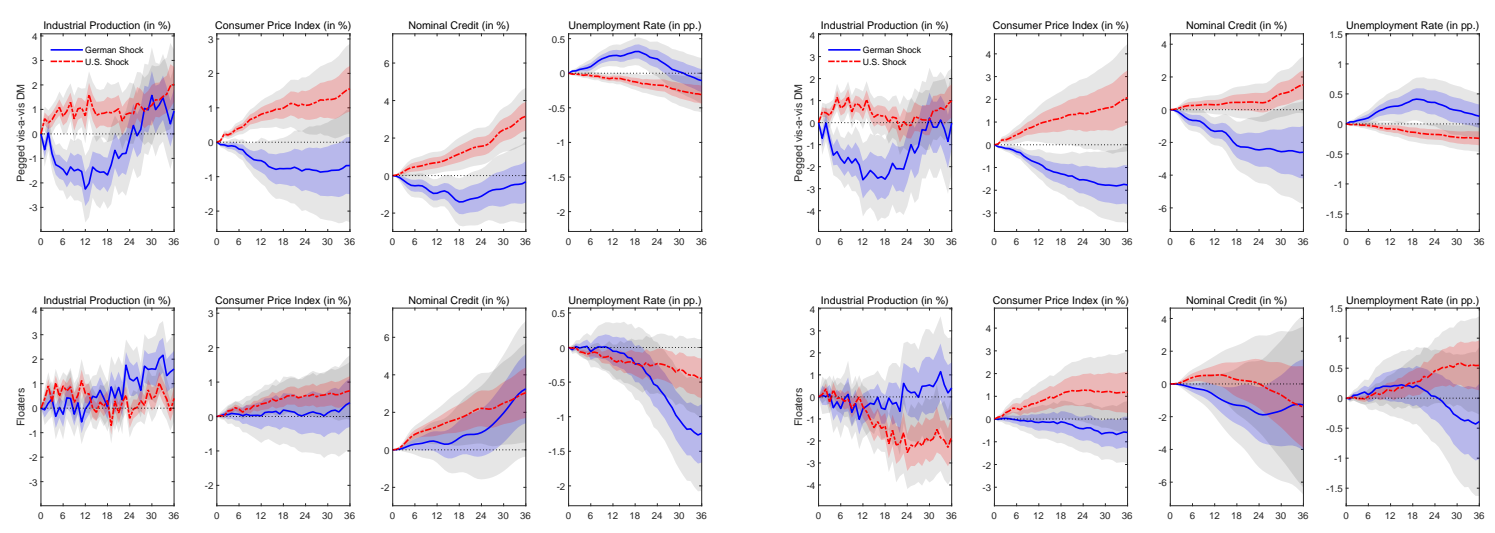
**Figure 5: IRFs: Robustness**

**(a) Panel regression with country-specific control coefficients**

**(b) Panel regression with common control coefficients**

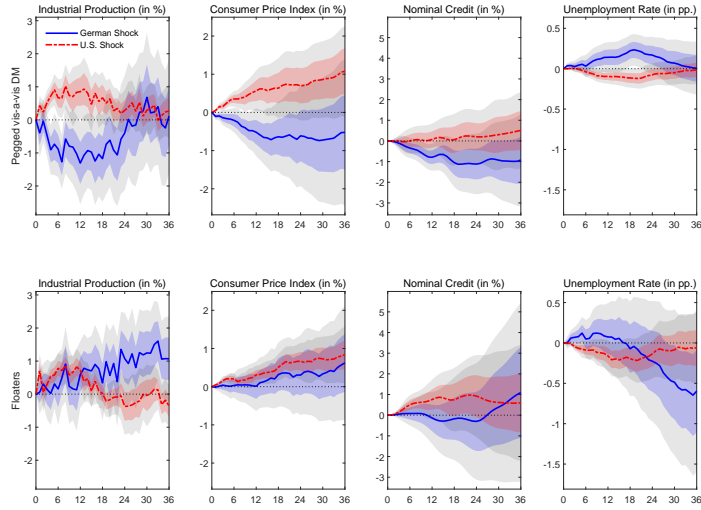


**(c) Panel regression with regime-specific control coefficients and no im-** **(d) Panel regression with regime-specific control coefficients, no U.S. or**  
*pact effect* *pact effect*

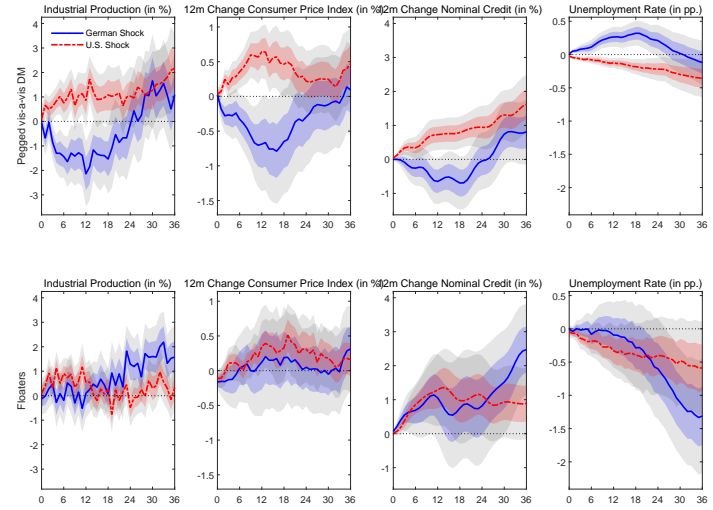


**Figure 5: IRFs: Robustness (continued)**

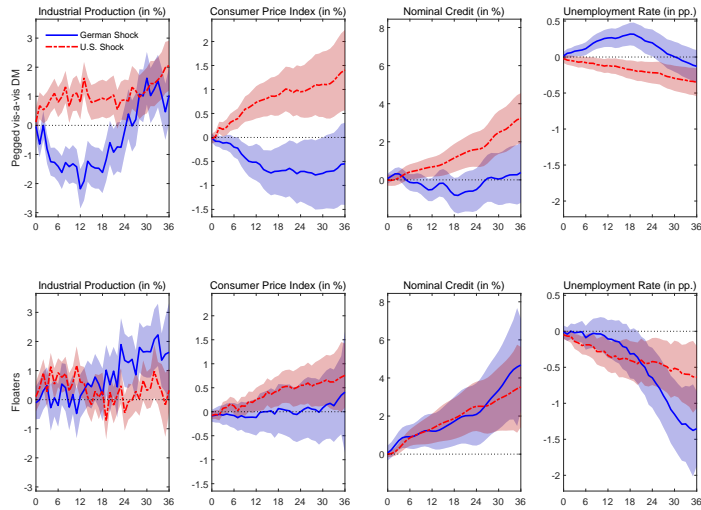
**(e) Panel regression using 2 lags of the shocks and controls**



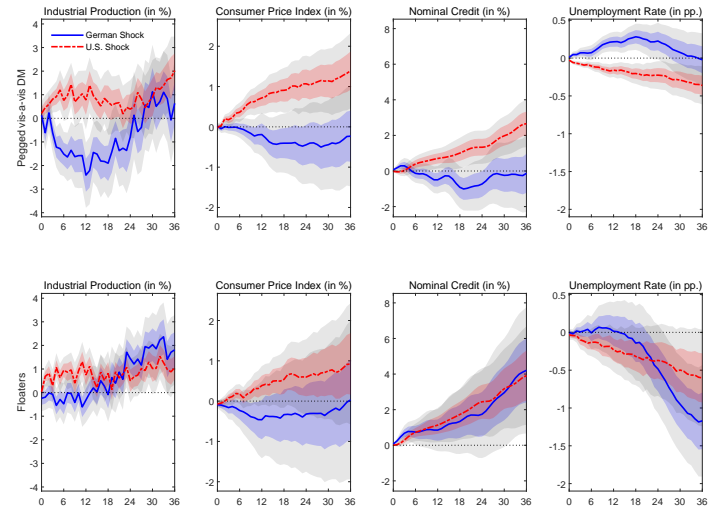
**(f) Panel regression using 12-month growth in CPI and nominal credit**



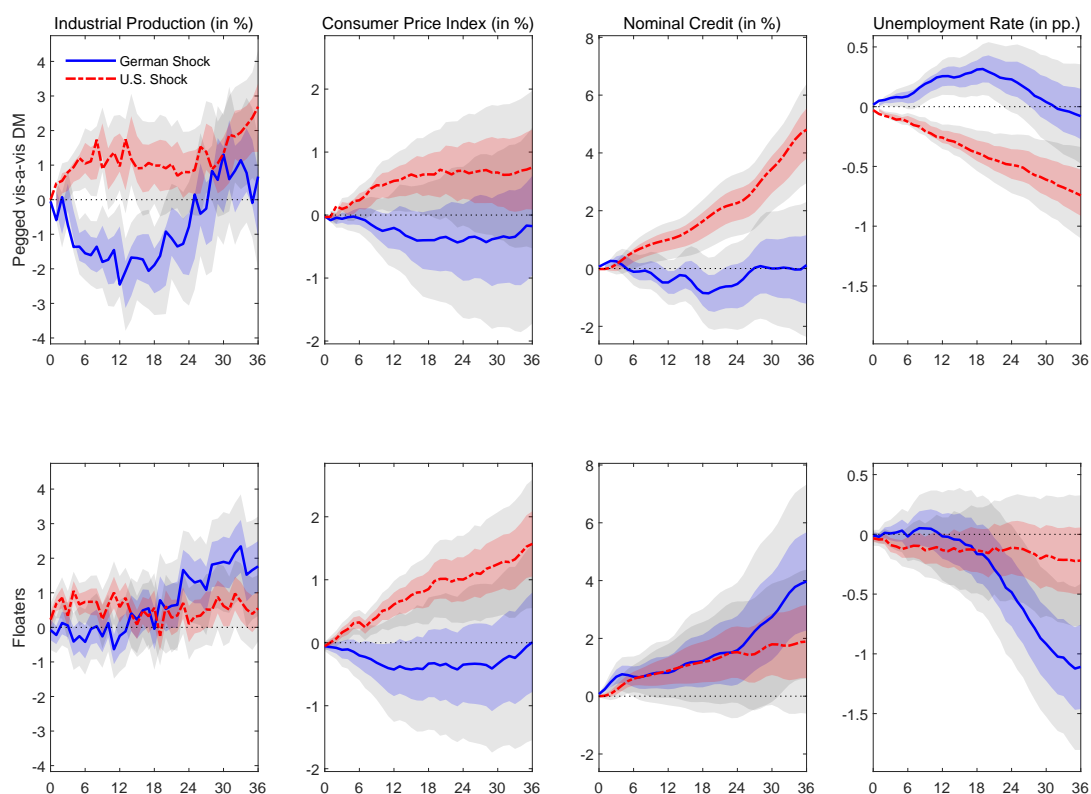
**(g) Panel regression with 83% confidence intervals**



**(h) Panel regression reclassifying Spain and Portugal as floaters**



**Figure 6:** Panel regression with time-varying regimes, 12 lags of the shocks and 3 lags of the controls



*Notes:* The figure shows the IRFs to 100 bps in the policy rate for the response of key macroeconomic variables depending on whether a country was pegged to the DM (top row) or whether the exchange rate was floating (bottom row). We show responses to the German policy shock (blue solid lines) and the U.S. policy shock (red dashed line). Shaded areas are 95 and 68 percent confidence intervals using the approach of [Driscoll and Kraay \(1998\)](#). Panel estimation is conducted with time-varying regime-specific controls and starting the sample in April 1979 to align with the start of the EMS. The main specification includes 12 lags of the shocks and 3 lags of the controls. See text for more detail.

## 5. CONCLUSIONS

This paper revisits a number of classic questions in international monetary economics using a newly constructed series of monetary policy shocks for Germany over the 1970s, 1980s and 1990s. Identification of surprise changes in monetary policy is challenging and we draw on extensive and previously unused archival data from the Bundesbank to help us isolate a new series of monetary shocks. We think that these, and all the historical data work, are important contributions in their own right.

Our work also speaks to a lively ongoing debate on the trilemma and the role of the U.S. as the driver of the global economic and financial cycle. Being a large and important base country for many Western economies in the final decades of the 20th century, Germany provides an excellent case study for examining these questions. By constructing monetary shocks using a similar approach to [Romer and Romer \(2004\)](#) for the U.S., we can also compare the relative effects of U.S. and German monetary policy using a consistent methodology. German monetary policy generated sizable spillovers that were at least as important as those from the U.S., and which impacted DM pegs far more strongly than DM floats.



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# APPENDIX

## A: DATA APPENDIX

**Table A.1:** *Data Description for First-Stage Regression*

<b>Variable</b>	<b>Period</b>	<b>Description</b>	<b>Source</b>
Discount Rate	Jan 1973 - Dec 1998	daily, percentage points	Deutsche Bundesbank
Real GDP (RNGP until 1992); West Germany	Jan 1974 - Dec 1998	monthly, quarter-on-quarter growth rates, in %	Bundesbank Real-time Database; Statistical Supplements to the Monthly Reports of the Deutsche Bundesbank: Series 4
Cost-of-living index for all households; West Germany	Jan 1973 - Dec 1998	monthly, not seasonally adjusted, 12-month change, in %	Central Bank Council: Statistical Overviews
Unemployment rate in percent of dependent labor force; West Germany	Jan 1973 - Dec 1998	monthly, seasonally adjusted, in percentage points	Central Bank Council: Statistical Overviews; Monthly Reports; Statistical Supplements to the Monthly Reports of the Deutsche Bundesbank: Series 4
Central bank money stock; West Germany	Dec 1974 - Mar 1989	monthly, year-on-year, seasonally adjusted	Central Bank Council: Statistical Overviews
Money stock M3; West Germany and from 1991 unified Germany	Dec 1974 - Dec 1998	monthly, year-on-year, seasonally adjusted, in percentage points	Central Bank Council: Statistical Overviews
Bilateral exchange rates of DM against USD, Japanese yen, pound sterling, Italian lira, Spanish peseta, and French franc	Dec 1974 - Dec 1988	daily data	Deutsche Bundesbank, ZIS; Central Bank Council: Statistical Overviews
Average bond yield of central government debt	Dec 1974 - Dec 1998	daily, percentage points	Central Bank Council: Statistical Overviews; Deutsche Bundesbank ZIS
Bond yield curve of central government debt	Dec 1974 - Dec 1998	bi-monthly, percentage points	Central Bank Council: Statistical Overviews

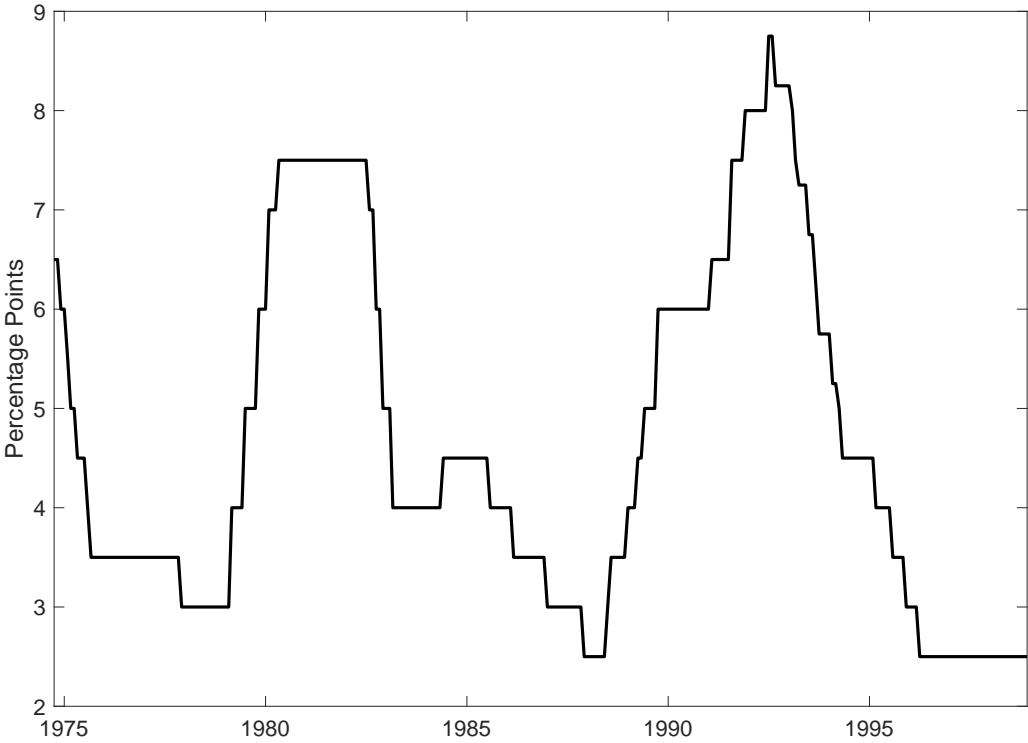
**Table A.2:** *Data Description for Second-Stage Regression: Germany*

<b>Variable</b>	<b>Period</b>	<b>Description</b>	<b>Source</b>
Discount Rate	Dec 1974 - Dec 1998	monthly, percentage points	Deutsche Bundesbank
Industrial production	Dec 1974 - Dec 1998	monthly, calendar and seasonally adjusted	Deutsche Bundesbank
Consumer price index	Dec 1974 - Dec 1998	monthly, calendar and seasonally adjusted	Deutsche Bundesbank
Unemployment rate, West Germany	Dec 1974 - Dec 1998	monthly, calendar and seasonally adjusted, in percentage points	Deutsche Bundesbank
Unemployment rate, unified Germany	Dec 1991 - Dec 1998	monthly, calendar and seasonally adjusted, in percentage points	Deutsche Bundesbank
Nominal credit: bank loans to domestic enterprises and households	Dec 1974 - Dec 1998	monthly	Deutsche Bundesbank
Commodity price index	Dec 1974 - Dec 1998	monthly, seasonally adjusted	Barakchian and Crowe (2013) converted to DM
Exchange rate DM/USD	Dec 1974 - Dec 1998	monthly	Deutsche Bundesbank
Narrow real effective exchange rate: 27 economies	Dec 1974 - Dec 1998	monthly	BIS

**Table A.3:** *Data Description for Second-Stage Regression: Panel data*

<b>Variable</b>	<b>Period</b>	<b>Description</b>	<b>Source</b>
Industrial production	Apr 1979 - Dec 1998	monthly, seasonally adjusted	FRED, OECD Main Economic Indicators
Consumer price index	Apr 1979 - Dec 1998	monthly, seasonally adjusted in Eviews with X-13-ARIMA	BIS
Unemployment rate	Apr 1979 - Dec 1998	cubic interpolation from quarterly to monthly frequency, seasonally adjusted	Deutsche Bundesbank, FRED
Nominal credit: long series on credit to private non-financial sector	Apr 1979 - Dec 1998	cubic interpolation from quarterly to monthly frequency, adjusted for breaks, domestic currency	BIS: <a href="http://www.bis.org/statistics/credtopriv.htm">www.bis.org/statistics/credtopriv.htm</a>
Commodity price index	Apr 1979 - Dec 1998	monthly, seasonally adjusted	<a href="#">Barakchian and Crowe (2013)</a> transformed into national currencies using bilateral exchange rates
Bilateral exchange rates	Apr 1979 - Dec 1998	monthly	Deutsche Bundesbank
U.S. Romer-Romer shocks	Mar 1969 - Dec 2011	monthly	Extended based on original approach using data set from <a href="#">Coibion, Gorodnichenko, Kueng, and Silvia (2017)</a>

**Figure A.1:** *German Monetary Policy Rate*



*Note:* This figure shows the German discount rate from 1974 to 1998. The units are in percentage points.



## B: ROBUSTNESS AND ADDITIONAL EVIDENCE

### B1: First-Stage Regression Results

**Table A.4: First-Stage Regression Results**

	$\pi_{m,t-p}$	$\Delta\tau_{m,t-p}$	$\hat{y}_{m,t-p}$	$\Delta\hat{y}_{m,t-p}$	$\Delta^{12m}MS_{m,t-p}$	$\Delta\Delta^{12m}MS_{m,t-p}$	$u_{m,t-p}$	$\Delta u_{m,t-p}$
$p = 1$	0.036 (0.29)	-0.052 (0.093)						
$p = 2$	-0.002 (0.039)	0.122 (0.132)	0.009 (0.007)	-0.011 (0.090)	0.396 (0.241)	0.039 (0.075)	-0.164 (0.285)	
$p = 3$	-0.031 (0.038)	-0.179 (0.128)	0.006 (0.007)	0.062 (0.070)	-0.119 (0.261)	-0.100 (0.116)	0.059 (0.311)	
$p = 4$	0.008 (0.027)	-0.051 (0.039)	0.002 (0.008)	0.011 (0.015)	-0.396 (0.196)	0.035 (0.073)	0.002 (0.411)	

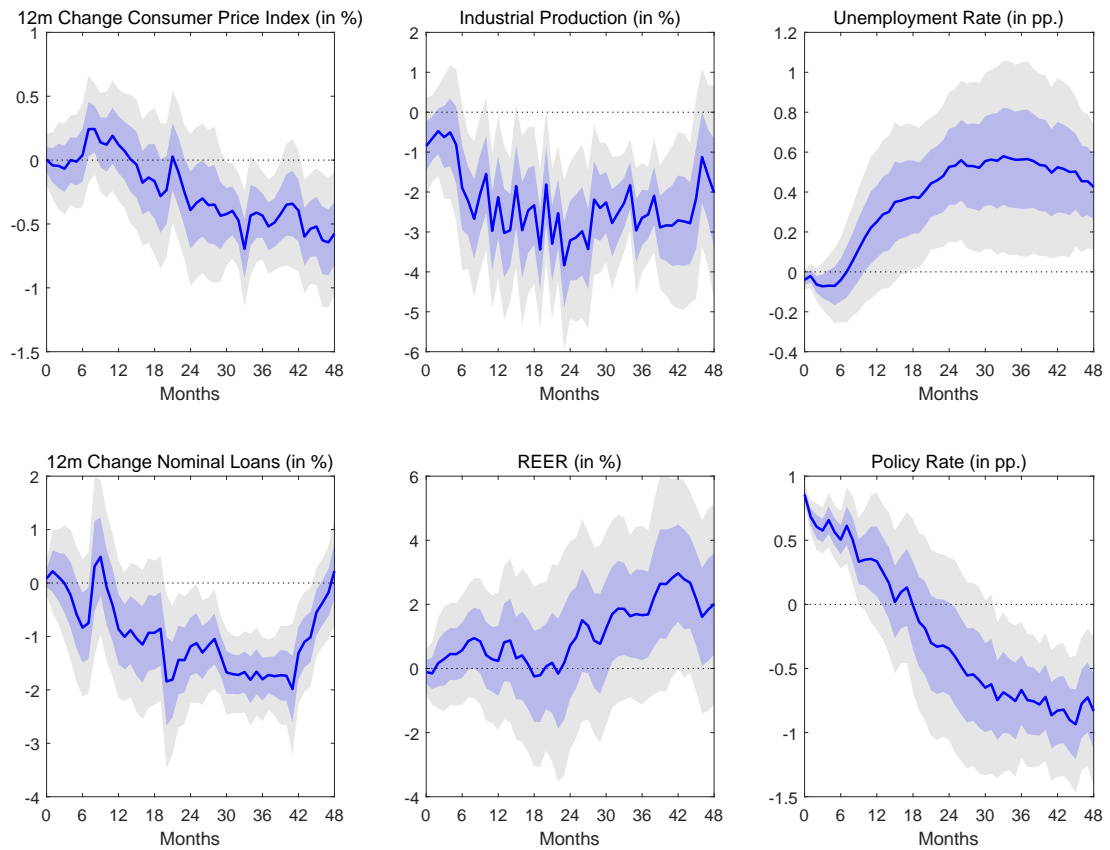
	$XR_{m,d-1}$	$\Delta XR_{m,d-1:m-1,d+1}$	$AVG\_B\_YLD_{m,d-1}$	$B\_YLD(2y)_{m,d-1}$	$B\_YLD(4y)_{m,d-1}$	$B\_YLD(6y)_{m,d-1}$	$B\_YLD(8y)_{m,d-1}$	$i_{m-1}$	$\Delta i_{m-1}$
GER				0.032 (0.120)	0.559 (0.458)	-0.995 (0.650)	0.543 (0.307)	-0.077 (0.014)	-0.112 (0.043)
USA	-0.006 (0.032)	0.006 (0.004)	-0.096 (0.135)						
JPN	0.069 (0.071)	-0.006 (0.004)							
FRA	-0.005 (0.006)	0.004 (0.012)							
ITA	0.076 (0.094)	0.001 (0.007)							
ESP	-0.062 (0.079)	0.005 (0.006)							
GBR	-0.013 (0.037)	0.004 (0.007)							

Notes:  $R^2 = 0.19$ ,  $D.W. = 1.99$ ,  $F - Statistic = 2.86$ ,  $N = 580$ . Sample: covers all Central Bank Council meetings between 1974:12 and 1998:12.

## B2: Specification Choices

### Results using Inflation and Credit Growth

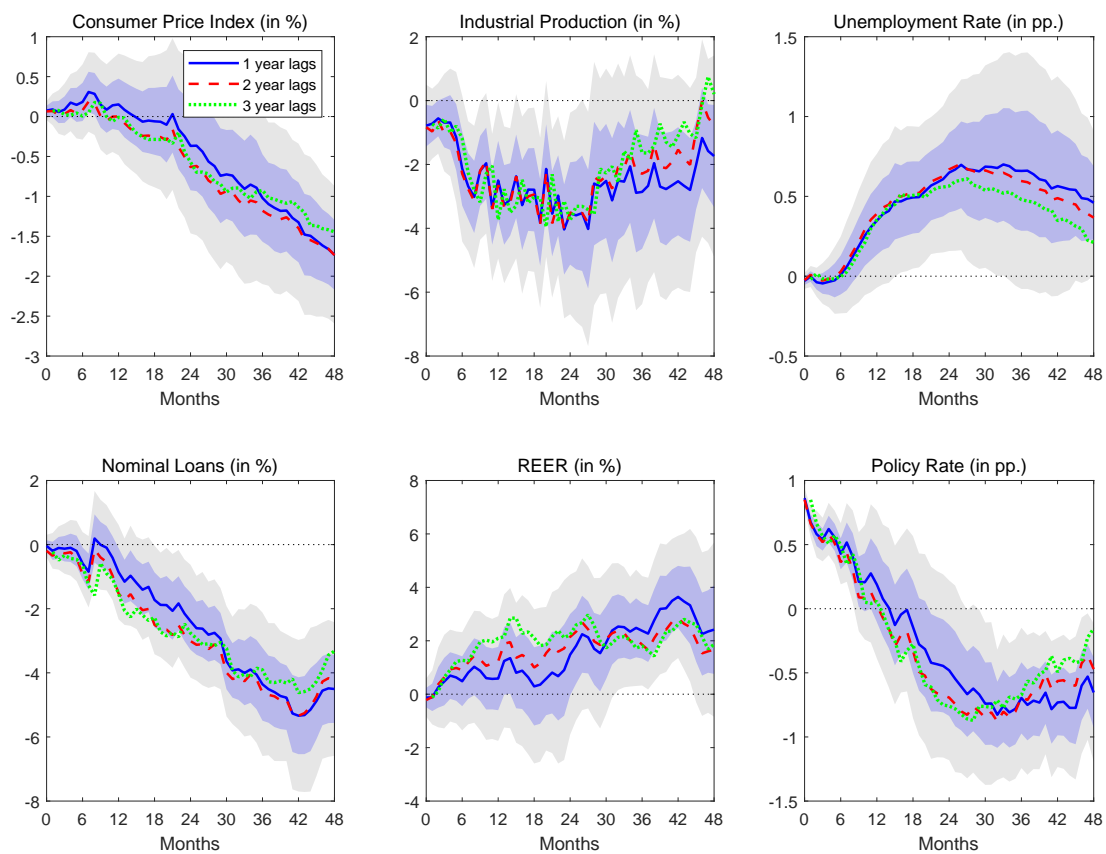
**Figure A.2:** *Baseline Results using Inflation and Credit Growth*



*Notes:* This figure shows the IRFs following a 100 bps rise in the policy rate on a number of key variables for Germany. Relative to the main result in the text, the figure shows the results for CPI inflation and loan growth, rather than the percentage response of the level. A rise in the exchange rate means an appreciation. The regression includes 12 lags of the shock and 3 lags of the controls. The control variables include CPI, industrial production, unemployment rate, loans, and the real effective exchange rate. Shaded areas are 95 and 68 percent confidence intervals calculated using the approach of [Newey and West \(1987\)](#).

## Lag Length Sensitivity

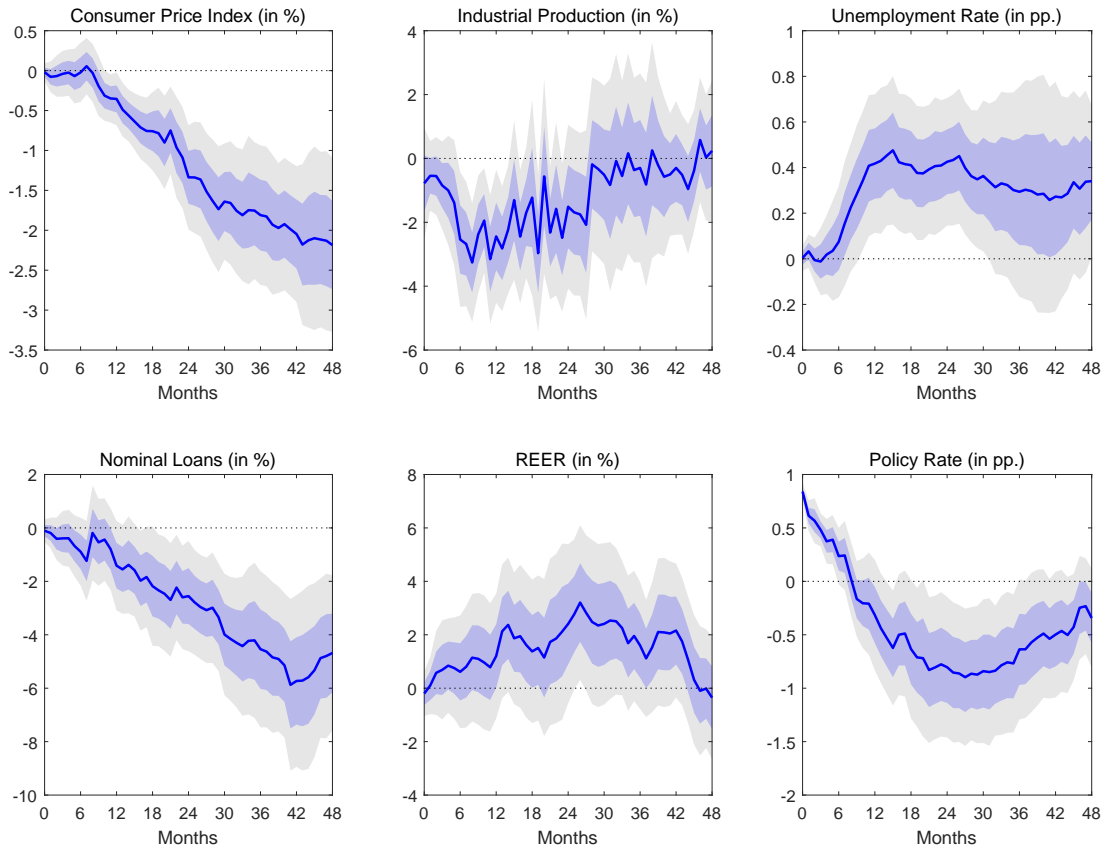
**Figure A.3:** *Effect of a Monetary Tightening on Key Variables in Germany – Lag Length*



*Notes:* This figure shows the IRFs following a 100 bps rise in the policy rate on a number of key variables for Germany. Relative to the main result in the text, the figure shows (in red dashed and blue dotted lines) alternative lag structures. A rise in the exchange rate means an appreciation. The regression includes 12 lags of the shock and 3 lags of the controls. The control variables include CPI, industrial production, unemployment rate, loans, and the real effective exchange rate. Shaded areas are 95 and 68 percent confidence intervals calculated using the approach of Newey and West (1987).

## Results using the EMS Sample: 1979:4 to 1998:12

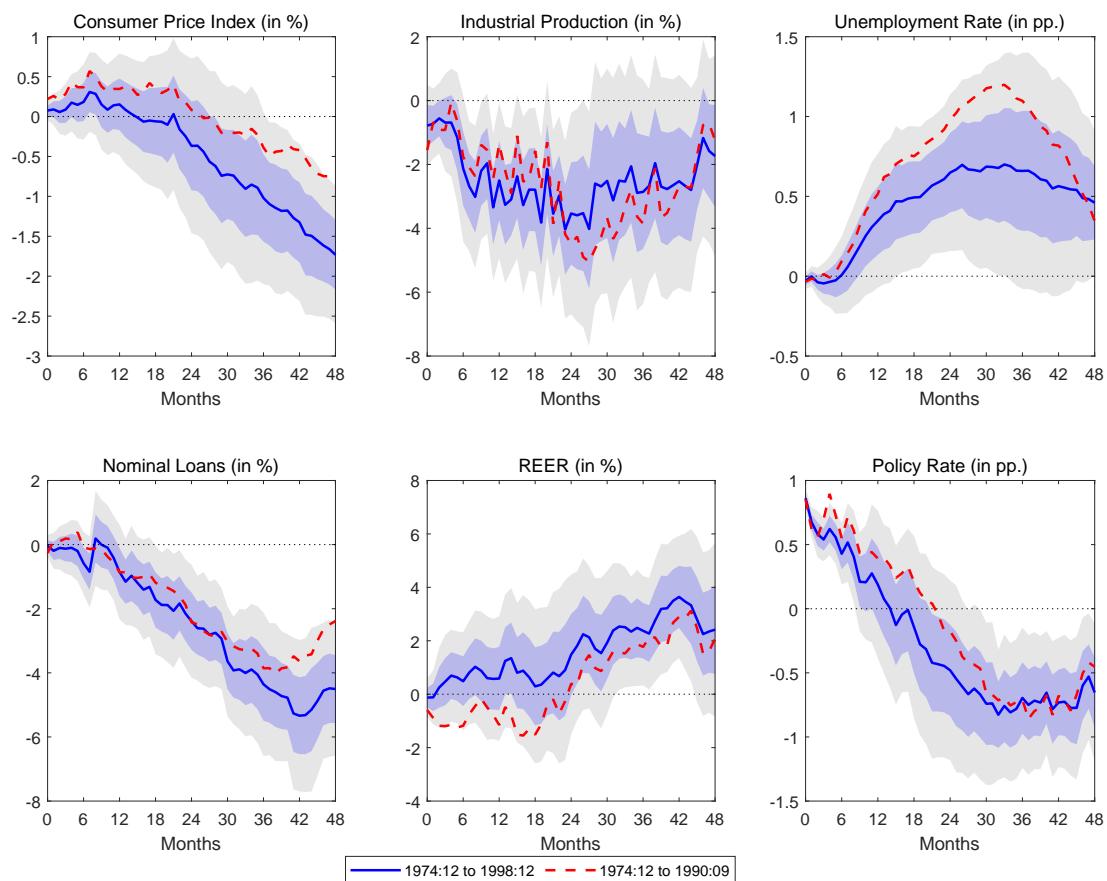
Figure A.4: Baseline Results using the EMS Sample: 1979:4 to 1998:12



*Notes:* This figure shows the IRFs following a 100 bps rise in the policy rate on a number of key variables for Germany. Relative to the main result in the text, the figure uses a slightly shorter sample period when Germany was in the European Monetary System. A rise in the exchange rate means an appreciation. The regression includes 12 lags of the shock and 3 lags of the controls. The control variables include CPI, industrial production, unemployment rate, loans, and the real effective exchange rate. Shaded areas are 95 and 68 percent confidence intervals calculated using the approach of [Newey and West \(1987\)](#).

## Results for the Pre-Reunification Sample

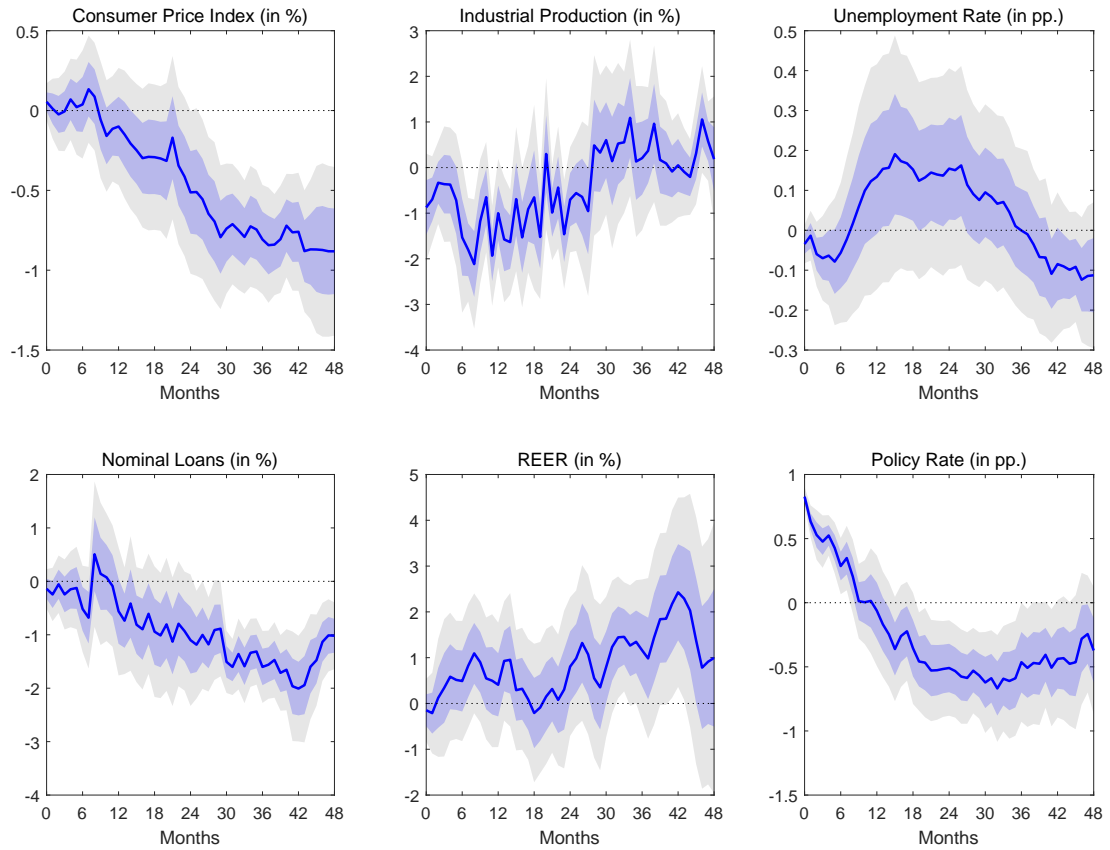
**Figure A.5:** *Effect of a Monetary Tightening on Key Variables in Germany – Pre-Reunification*



*Notes:* This figure shows the IRFs following a 100 bps rise in the policy rate on a number of key variables for Germany. Relative to the main result in the text, the figure shows the results restricting the sample to the pre-reunification period. A rise in the exchange rate means an appreciation. The regression includes 3 lags of the shock and 1 lag of the controls. The control variables include CPI, industrial production, unemployment rate, loans, and the real effective exchange rate. Shaded areas are 95 and 68 percent confidence intervals calculated using the approach of Newey and West (1987).

## Levels Specification

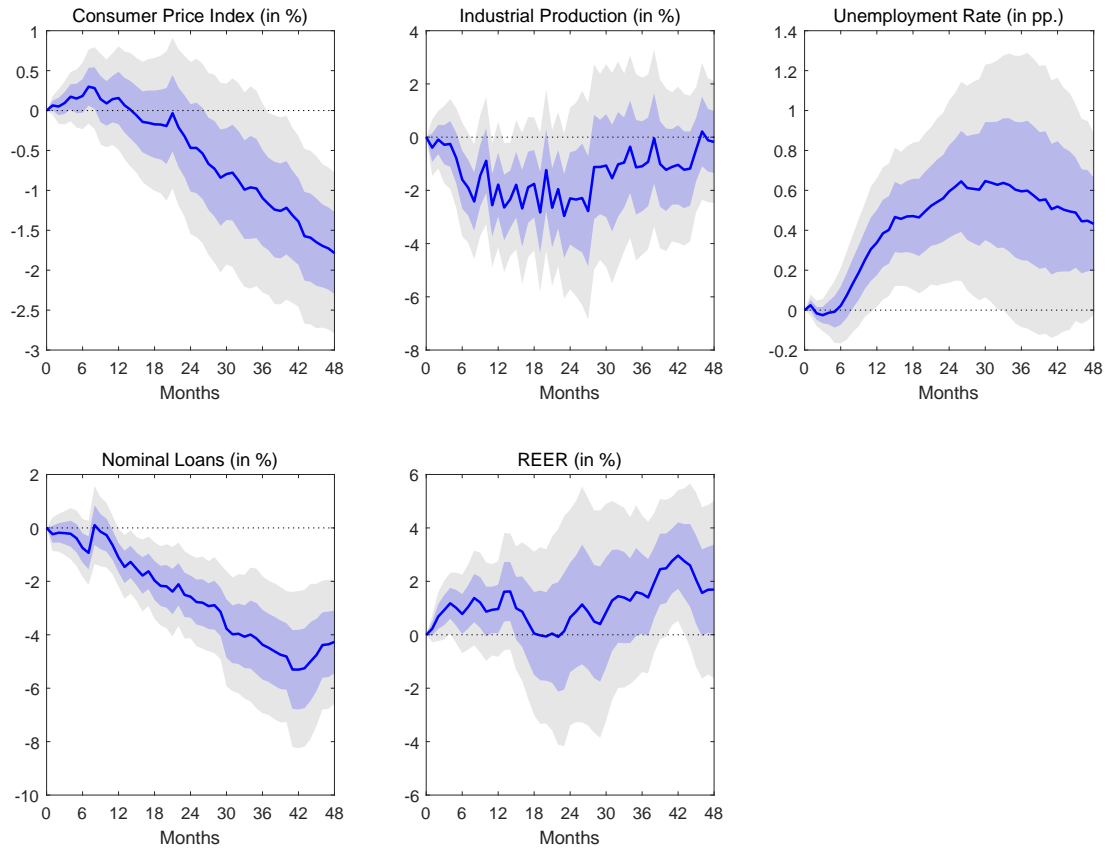
Figure A.6: IRFs Based on Level Specification



Notes: This figure shows the IRFs following a 100 bps rise in the policy rate on a number of key variables for Germany. Relative to the main result in the text, the figure shows the results using a levels-with-trend specification similar to those in Ramey (2016). Specifically:  $y_{t+h} = \alpha_h + \tau_h t + \Psi_h(L)x_t + \beta_h \varepsilon_t + u_{t+h}$ . As usual, a rise in the exchange rate means an appreciation. The regression includes 12 lags of the shock and 3 lags of the controls. The control variables include CPI, industrial production, unemployment rate, loans, and the real effective exchange rate. Shaded areas are 95 and 68 percent confidence intervals calculated using the approach of Newey and West (1987).

## Restricting the Impact Effect

Figure A.7: Baseline Results Forcing the Impact Effect to Zero

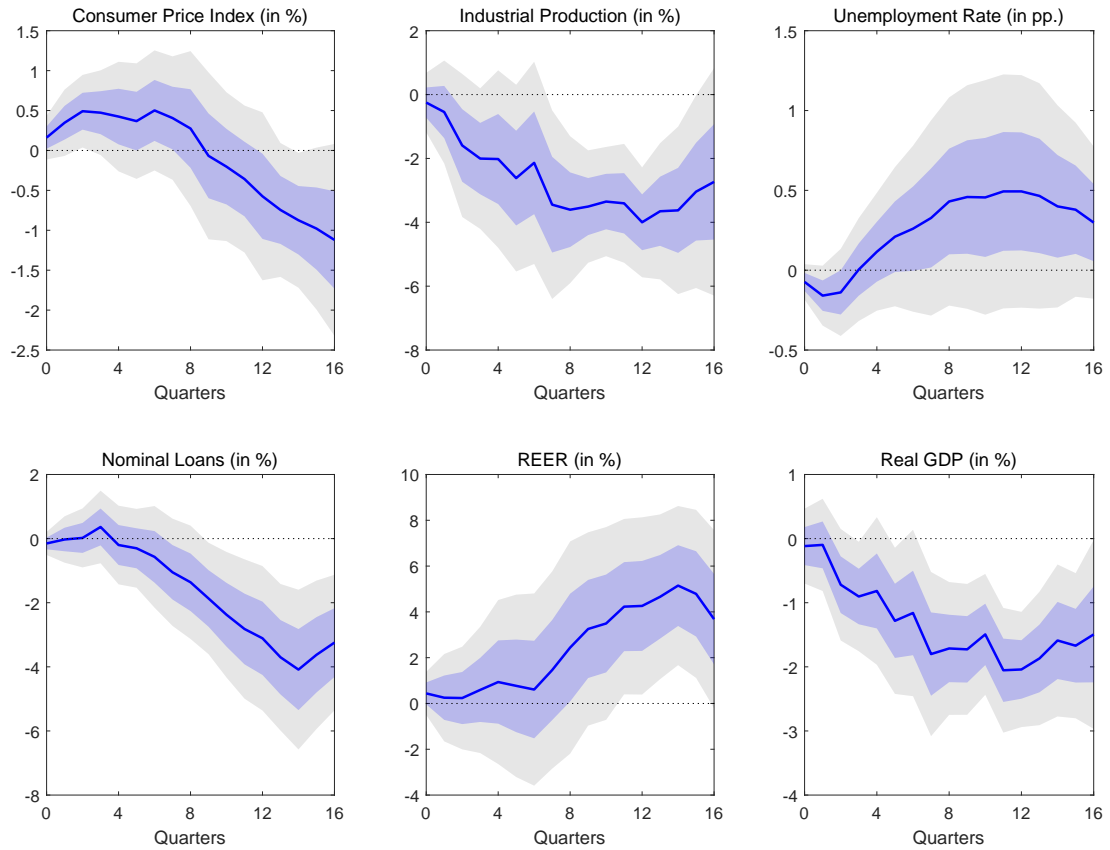


Notes: This figure shows the IRFs following a 100 bps rise in the policy rate on a number of key variables for Germany. Relative to the main result in the text, we include contemporaneous variables in  $x_t$  to force the impact effect to zero. This imposes the typical “Cholesky” identification assumption from the VAR literature, as discussed in Ramey (2016). A rise in the exchange rate means an appreciation. The regression includes 12 lags of the shock and 3 lags of the controls. The control variables include CPI, industrial production, unemployment rate, loans, and the real effective exchange rate. Shaded areas are 95 and 68 percent confidence intervals calculated using the approach of Newey and West (1987).



## Quarterly Data

**Figure A.8:** *Baseline Results using Quarterly Data and Real GDP*



*Notes:* This figure shows the IRFs following a 100 bps rise in the policy rate on a number of key variables for Germany. Relative to the main result in the text, we use quarterly data and real GDP. The regression includes 4 lags of the shock and 1 lags of the controls. The control variables include CPI, industrial production, unemployment rate, loans, and the real effective exchange rate. Shaded areas are 95 and 68 percent confidence intervals calculated using the approach of [Newey and West \(1987\)](#).

## C: EXCHANGE RATE OUTCOMES VS. REGIME CLASSIFICATION

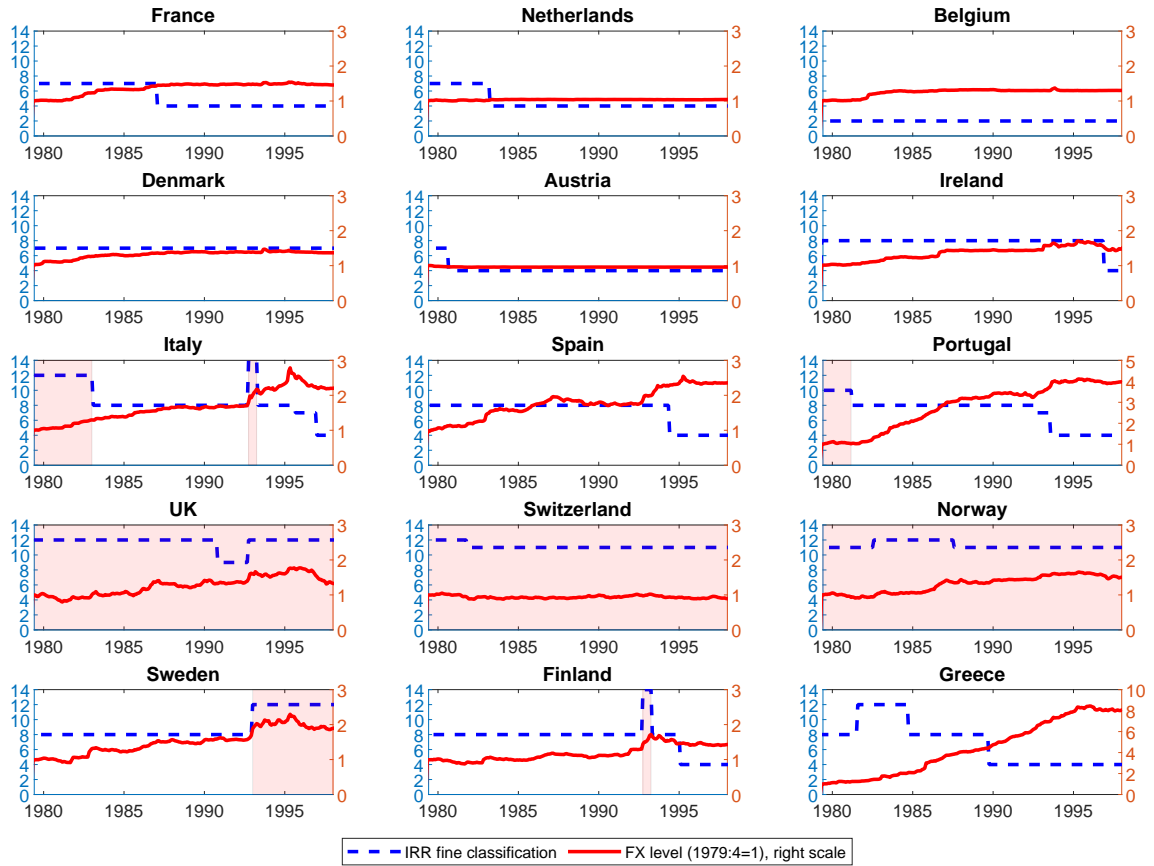


Figure A.9: Exchange Rate against DM (red, right scale) and Exchange Rate Regimes