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The open economy consequences of U.S. monetary policy

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ABSTRACT

We consider the open economy consequences of U.S. monetary policy, extending the identification approach of Romer and Romer (2004) and adapting it for use with asset prices. Intended policy changes are orthogonalized against the economy's expected future path, which captures any effects from open economy variables. Estimated from a set of bilateral VARs, the dynamic responses of the exchange rate, foreign interest rate, and foreign output are consistent with recent work that identifies U.S. policy via futures market changes and *a priori* impulse response bounds. We compare the two approaches, finding important commonalities. We also outline some advantages of our approach.

1. Introduction

Estimating the impact of unanticipated and exogenous monetary policy shocks on open economy variables such as exchange rates and foreign interest rates is a perennial desideratum in the field of international finance. Important contributions to the open economy empirics of monetary policy include Eichenbaum and Evans (1995), Kim and Roubini (2000), Faust and Rogers (2003), and Faust et al. (2003). The identification of monetary policy shocks from broader movements in monetary instruments poses a particular challenge in the open economy context because of simultaneity amongst asset returns invalidating many of the short-run restrictions used to identify policy innovations in structural vector autoregressions (SVARs). In a partially identified framework, Faust and Rogers (2003) show that the exclusion restrictions associated with the recursive ordering of variables posited by Eichenbaum and Evans (1995) for a set of two-country VARs can be rejected for some country pairs.

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Furthermore, they find that the evidence for delayed exchange rate overshooting is sensitive to relaxing such restrictions.

Our strategy for addressing such simultaneity concerns is to incorporate information from outside of a VAR framework to achieve monetary policy identification.[2] We can then allow the contemporaneous and dynamic responses of open economy variables in the model to be completely unrestricted. The approach builds upon the work of Romer and Romer (2004), who use a two-step procedure to identify U.S. monetary policy shocks. In the first step, they use narrative sources to determine intended changes in the U.S. target federal funds rate. In the second step, these federal funds rate changes are decomposed into two components, one that can be explained by the Federal Reserve's Greenbook (in-house) forecasts for output growth, inflation, and unemployment, and one that cannot be explained by those forecasts. The second component, which captures interventions unrelated to economic forecasts, defines the monetary policy shock. In our implementation, we extend both parts of the identification procedure. In the first stage, we augment the Romers' narratively identified changes in the target federal funds rate at Federal Open Market Committee (FOMC) meetings for the period 1969–1996 with announced changes in the target for the period 1997–2001.[3] In the second stage, the augmented series is orthogonalized against a meeting-based information set that combines the Greenbook forecasts used by the Romers and a measure of capacity utilization constructed by the Federal Reserve.[4] If the controls in the orthogonalization adequately summarize the FOMC's information set regarding its objectives and expectations at the time of its decision, then the subsequent residuals represent the unanticipated and exogenous component of U.S. monetary policy.[5] We convert the resulting identified policy series from a meeting frequency to a monthly frequency via a novel aggregation method which ensures that period average asset price responses are correctly estimated. The identified policy series is then an exogenous variable in the estimation of the dynamic effects of U.S. policy on the exchange rate and other open economy variables.

To facilitate comparisons with previous work, we include our identified policy series in a set of 6 bilateral, monthly VARs, where the U.S. is Home and one of the non-U.S. G7 countries is Foreign. We find that the open economy dynamic responses to a contractionary monetary policy shock which raises the U.S. t-bill rate by 100 basis points (b.p.) are the following:

1. There is always an impact appreciation of the U.S.$ exchange rate.
2. The maximal appreciation of the exchange rate occurs between 1 and 2 months (Canada and Germany) and 20 months (France and Italy). It lies between 0.66% (Canada) and 4.38% (France).
3. There is strong, positive interest rate pass-through from the U.S. to the foreign countries, with the maximum response occurring between 3 months (Canada) and 10 months (Japan and the U.K.). It lies between 0.46 (Japan) and 1.24 (Canada).
4. Foreign output shows a mixed initial response (some positive and some negative) which uniformly becomes negative at horizons of 16 months (earlier for many).

U.S. output responses are generally negative, with the maximum contractions ranging from 1.3 to 2.1 percentage points and occurring between 17 and 27 months after the initial U.S. policy shock. The U.S. price responses in the baseline model are disappointing, showing a price puzzle. We investigated the price responses in detail, finding them to depend critically on the number of lags of the policy shock included in the model. As the number of policy shock lags is increased, the price puzzle disappears while preserving many of the other impulse responses. The response of U.S. non-borrowed reserves exhibits the classic liquidity effect, where a contractionary monetary policy shock leads to a decline in reserves held and a rise in domestic interest rates. We discuss all of these responses in more detail later.

[2] Such a strategy is not unique. Later in the paper, we discuss other strategies which also use outside information to aid identification.

[3] We present evidence for the contiguity of the two series later.

[4] Giordani (2004) has recently emphasized the value of the capacity utilization index as a proxy for the Federal Reserve's output gap perceptions. We discuss the measure in more depth later.

[5] We present arguments supporting such an assumption later.

There are of course other approaches to estimating the open economy effects of U.S. monetary policy which incorporate information from outside a VAR framework in order to achieve identification. We contextualize our findings by comparing them to those of Faust et al. (2003), a notable recent example which is a natural comparator for our study.[6] They estimate the high frequency (intra-day or daily) responses of spot exchange rates, Home and Foreign spot interest rates, and three and six month Home and Foreign forward interest rates to U.S. monetary policy shocks. Following Kuttner (2001), the policy shocks are measured from changes in the daily closing price of current-month federal funds futures contracts around FOMC meetings. The maintained assumptions for the validity of their approach are that: (1) market expectations (as captured by the federal funds futures price just prior to the FOMC meeting) account for U.S. policy that is either anticipated and/or endogenous to other macroeconomic shocks; (2) there is no relevant information arrival over the interval for which federal funds futures price changes and asset returns are measured, other than the FOMC announcement; and (3) term premia in the forward interest rate contracts are time-invariant. Faust et al. combine restrictions derived from the high frequency regressions with *a priori* bounds on monetary policy's effects in order to partially identify impulse response functions in two bilateral open economy VARs, where Home is the U.S. and Foreign is either Germany or the U.K. (see Faust (1998) for a full description of the partial identification methodology).

Under our identification scheme, we find that the within-month response of the US$/GRM exchange rate to a monetary policy shock that raises the U.S. t-bill rate by 100 b.p. is an appreciation of 1.70%. The response of the US$/UK£ rate is a 1.30% appreciation. Under Faust et al. (2003)'s identification scheme, the impact estimates are 2.44% and 1.41% respectively, indicating a strong agreement under the two identifications. The maximum exchange rate responses that we estimate occur after 2 months in the case of the US$/GRM exchange rate and 6 months in the case of the US$/UK£ rate. However, the uncertainty surrounding these estimates is considerable. Similar to Faust et al., delays of several years in the maximum exchange rate response cannot be ruled out. The dynamic responses of foreign interest rates are comparable across the two identifications, while the foreign output effects that we estimate are close to the maximum effects associated with the partial identification.

We argue that the robustness of the results for Germany and the United Kingdom across the two monetary policy identifications is an important finding. It casts light on the validity of the different identifying assumptions which underlie the two approaches. In Section 4, we compare the U.S. monetary policy shocks derived from the federal funds futures price (whose effects on asset returns are a key input to Faust et al.'s partial identification) with those derived from the narrative evidence, the Greenbook forecasts, and the capacity utilization index. Although the policy shock magnitudes differ, there is a strong positive correlation between the two identified series. This suggests that the federal funds futures price and the identifying information we employ represent comparable instruments for eliminating anticipated and endogenous movements in U.S. monetary policy.

In addition to validating many of the results in Faust et al. (2003), there are comparative advantages to our approach that we highlight. First, the U.S. monetary policy identification approach that we follow is readily implementable, since it only requires the construction of a single time series for policy shocks which may then be included in a standard reduced-form VAR model or another impulse response model (e.g., local projections). Moreover, estimation can be undertaken for a large sample of countries, whereas Faust et al.'s approach requires liquid markets in a wide range of financial instruments, restricting its application to models featuring either the U.S. and Germany or the U.S. and the U.K. Second, we do not impose any *a priori* bounds on what a reasonable impulse response should be. Under the assumptions of our identification approach, we can allow the estimated responses to be unrestricted. Consequently, any estimated contemporaneous or dynamic effects represent the actual correlations of the endogenous variables with our identified U.S. monetary policy shock measure.

[6] There is a vast literature on alternative approaches to monetary policy identification which we do not attempt to evaluate here (e.g., identification through heteroscedasticity as in Rigobon and Sack, 2004, factor-based models as in Bernanke et al., 2005, sign restrictions as in Uhlig, 2005, etc.). Most of these alternatives have not yet been applied in the open economy context, which is our primary focus.

Another contribution of the paper is an exploration of the robustness of the U.S. domestic results presented in Romer and Romer (2004). They show that their identification scheme which combines narrative and Greenbook information, eliminates the price puzzle associated with many other identifications and increases the magnitude and speed of output's response to monetary policy. In addition to extending the identification approach used by Romer and Romer, we apply a different method for aggregating the shocks from a meeting frequency to a monthly frequency that we argue is more appropriate, given that macroeconomic data are measured on a period average as opposed to an end-of-period basis. It is particularly relevant for the correct estimation of asset price responses. We also estimate models that control for a broader range of macroeconomic variables than do those used by Romer and Romer, and employ lag structures more typical of the literature. Our results indicate that the peak effect of policy on output occurs after 24 months, matching the Romers' estimates, but that the size of the peak is smaller, reflecting both the inclusion of additional controls and differences in the shock aggregation method. As mentioned earlier, evidence of a price puzzle reappears in our results, and is protracted in some cases. The main reason appears to be the inclusion of only 6 monthly policy shock lags in the models that we estimate, compared to the 48 lags in the models estimated by Romer and Romer. The inclusion of additional lags in the models that we estimate eliminates any price puzzle and leads to large and statistically significant price reductions at the four year horizon.

The paper is structured as follows. In Section 2, we sketch out the nature of the identification problem in an open economy context and explain how narrative evidence, the Greenbook forecasts and the capacity utilization index are used to isolate unanticipated and exogenous U.S. monetary policy shocks. In Section 3, we present our main empirical results for the exchange rate and foreign interest rates, foreign output and U.S. output and prices, and compare them to those in Faust et al. (2003). We also consider the robustness of our findings to excluding data associated with special events in the sample, eliminating units roots from the empirical models and simulating impulse response functions using the method of local projections. In order to probe the reasons for common features across our results and those of Faust et al., in Section 4, we compare federal funds future-identified shocks and our identified shocks. Section 5 concludes the paper with a summary of our main arguments and results, and a discussion of possible future research directions.

2. Empirical approach

2.1. Identification

In order to disentangle policy effects from other movements in observed domestic and foreign macroeconomic data, monetary policy identification requires that we isolate the unanticipated and exogenous component of monetary policy. If policy changes are anticipated, then forward-looking and fast-moving variables such as the exchange rate and foreign interest rate can respond in advance of the policy shift. Empirical work which gauges monetary policy effectiveness by the contemporaneous and lagged impacts of observed policy instruments will neglect such effects. If policy changes are endogenous to variables such as output growth and inflation, their estimated effect on exchange rates and other open economy variables will in part reflect the way in which output and inflation fluctuations affect those variables. For example, a rise in inflation may induce exchange rate depreciation if the real exchange rate is mean-reverting. To the extent that the federal funds rate rises with inflation, any tendency for monetary tightening to appreciate the exchange rate will be obscured. If policy responds directly to open economy variables such as the exchange rate, the estimation results will be subject to simultaneity bias.

2.1.1. FOMC meeting-based monetary policy shocks

To identify unanticipated and exogenous variation in the federal funds rate, we extend the two-step procedure outlined by Romer and Romer (2004). In the first step, a federal funds rate target series is constructed. For the period covering 1969–1996, Romer and Romer use narrative evidence to determine the size of the federal funds rate change targeted by the Federal Open Market Committee (FOMC) at their scheduled meetings. In contrast to the actual federal funds rate, this measure is not affected by transitory shocks to supply and demand in the reserve market and arguably represents a better measure of policy intentions than the actual rate.[7] We extend the original Romer and Romer (2004) target series by appending the FOMC's announced target federal funds rate changes for 1997–2001. Such announcements commenced in 1994, overlapping with the original Romer and Romer series for 2 years. Even though the announced target series does not capture all of the narrative evidence incorporated in the Romer and Romer series, we argue that the pooling of the two is defensible given that policy intentions have been much more transparent during the 1990s. During the overlapping period of 1994–1996, the two series have a correlation that is essentially 1.[8] Moreover, in Section 3.4, we show that our findings do not depend on the data from the post-1996 period.[9]

In the second step, the targeted federal funds rate change is regressed upon the Federal Reserve's Greenbook (in-house) forecasts for real output growth, inflation, and unemployment over horizons of up to two quarters. These represent the central objective variables of the Federal Reserve.[10] The Greenbook forecasts are issued to FOMC members just prior to a meeting, and are thus predetermined with respect to the meeting outcome. We supplement the Greenbook information with final measures of capacity utilization and capacity utilization growth in the month of the FOMC meeting. The capacity utilization index is constructed by the Federal Reserve. However, it is not available to policymakers in real-time because the observations for a particular month are inferred by scaling production indicators with capacity measures interpolated from end-of-year observations – actual capacity is only benchmarked annually. The empirical relevance of capacity utilization is emphasized by Giordani (2004), who shows that controlling for such a proxy for production relative to potential is crucial for accurate policy identification. In the present application, we treat terms in capacity utilization as proxies for latent policymaker perceptions concerning the cyclical position of the economy. Formally, we estimate the following regression:

$$\begin{aligned}\Delta ff_m = {} & \alpha + \beta ff_{m-1} + \sum_{j=-1}^{2} \gamma_j \widehat{\Delta y}_{m,j} + \sum_{j=-1}^{2} \eta_j \left(\widehat{\Delta y}_{m,j} - \widehat{\Delta y}_{m-1,j} \right) \\ & + \sum_{j=-1}^{2} \theta_j \widehat{\pi}_{m,j} + \sum_{j=-1}^{2} \lambda_j \left(\widehat{\pi}_{m,j} - \widehat{\pi}_{m-1,j} \right) \\ & + \sum_{j=-1}^{2} \mu_j \widehat{n}_{m,j} + \sum_{j=-1}^{2} \rho_j \left(\widehat{n}_{m,j} - \widehat{n}_{m-1,j} \right) + \tau CU_m + \phi CUG_m + \varepsilon_m, \end{aligned} \tag{1}$$

where m indexes FOMC meetings, j indexes the forecast quarter relative to the current meeting's quarter, ff is the target federal funds rate level, Δy is real output growth, π is inflation, n is the unemployment rate, CU is the capacity utilization index, CUG is the current monthly growth rate of capacity utilization (both capacity terms are measured in percentage points), ε is a mean-zero error term, and a hat denotes the real-time forecast for a variable. Other lowercase Greek letters denote population parameters. Notice that the specification employs a larger set of unemployment forecasts than do Romer and Romer (2004) (in addition to the capacity utilization terms).

The results obtained from estimating Eq. (1) for a sample of 298 FOMC meetings from the period 1969–2001 are reported in Table 1. The sample is restricted to meetings through the end of 2001 because there is a lag of at least five years in the publication of Greenbook forecasts. The sums of the coefficients on forecast levels are generally of the same signs as those reported by Romer and Romer (2004), indicating tighter policy in response to stronger economic activity and higher prices. An exception occurs in the case of the sum of the coefficients on the growth forecasts, which is negative

[7] It is possible that large and persistent changes in reserve demand would eventually force a change in the policy intention. Such policy endogeneity is not tackled in the first stage of the identification, but may be addressed in the second stage, as we discuss in more detail below.

[8] There is one instance in which the series differ. For the meeting on September 28, 1994, Romer and Romer (2004) argue that the language associated with the FOMC transcripts amounted to the intention to tighten by 12.5 b.p., even though there was no change in the announced, target federal funds rate.

[9] The importance of an explicit measure of policy intentions is clear. The R-squared from a regression of the target change series upon the associated effective federal funds rate change series is only 0.19 over the FOMC meetings from 1969 to 2001. Over the sub-period 1994–2001 (when intentions are publicly announced), it is 0.13.

[10] See Board of Governors of the Federal Reserve (2005), or the International Banking Act of 1978 (the Humphrey–Hawkins Act).

Table 1
Determinants of changes in the target federal funds rate.

Regressor	Coefficient	Standard error
Intercept	−0.910	1.168
Target from last meeting	−0.024	0.011
Forecasted output growth		
−1	0.006	0.010
0	−0.013	0.020
1	−0.025	0.028
2	0.016	0.031
Total effect	−0.016	0.027
Output growth revision		
−1	0.005	0.025
0	0.134	0.029
1	−0.022	0.041
2	−0.008	0.050
Total effect	0.104	0.068
Forecasted inflation		
−1	0.030	0.023
0	−0.017	0.028
1	0.020	0.043
2	0.014	0.044
Total effect	0.047	0.020
Inflation revision		
−1	0.007	0.030
0	−0.020	0.041
1	−0.016	0.066
2	−0.050	0.074
Total effect	−0.079	0.082
Forecasted unemployment		
−1	−0.137	0.162
0	0.599	0.352
1	−0.290	0.475
2	−0.218	0.319
Total effect	−0.047	0.035
Unemployment revision		
−1	−0.189	0.216
0	−0.515	0.319
1	0.684	0.441
2	−0.444	0.343
Total effect	−0.464	0.204
Capacity utilization	0.015	0.012
Capacity utilization growth	0.136	0.035

$R^2 = 0.36$, $N = 298$. The sample is all scheduled FOMC meetings from the period 1969–2001. See the main text for a description of the regressors. The total effects refer to the sum of the coefficients on sets of forecasts or forecast revisions for the previous, current and next two quarters.

but insignificant. One explanation is that the capacity utilization terms and additional unemployment forecasts capture information contained in the growth forecasts. The inclusion of the capacity utilization terms and additional unemployment forecasts is also reflected in the regression R^2, which is higher than that for the original Romer and Romer (2004) specification (36% as compared to 28%).[11]

The regression residuals are the targeted federal funds rate changes which are orthogonal to the Federal Reserve's information set. How representative is the series of the unanticipated and exogenous component of U.S. monetary policy? Romer and Romer (2000) demonstrate that the Greenbook forecasts encompass alternative private sector forecasts, while Bernanke and Boivin (2003) show that the mean square error for Greenbook forecasts is typically smaller than that for forecasts derived from factor

[11] This may also reflect a reduction in the variation in the target federal funds rate over the years 1997–2001.

models (although pooling across the two yields even greater forecast accuracy). If expectations and their revisions of growth, inflation, and unemployment are the main drivers of anticipated movements in the federal funds rate, the residuals from regression (1) will be unanticipated from the perspective of market participants, assuming that the Greenbook forecasts encompass their forecasts. Of course, there are likely to be many other inputs to the market's target federal funds rate expectations. As such, the real-time predictability of the shocks from the market's perspective is hard to evaluate.[12] We can examine the predictability of the shocks with respect to their own history. A regression of the shocks on the shocks recorded for the last 12 meetings does not yield any slope coefficients that are significant at the 10% level, and a joint test of their significance yields a *p*-value of 67%.[13]

For our identification approach to be valid, we require that: (1) the Greenbook forecasts and capacity utilization terms are not functions of the change in the federal funds rate target; and, (2) conditional upon the Greenbook forecasts and capacity utilization terms, the open economy variables do not cause target changes. The first assumption rules out reverse causation in Eq. (1). Since the Greenbook forecasts are issued prior to any FOMC meeting, they are predetermined. Moreover, as remarked upon by Romer and Romer (2004), the Greenbook forecasts are generally formulated under the assumption that there is *no* change in policy stance at least until the FOMC meeting after next, ruling out this possibility. One caveat is that Greenbook forecasts may be generated from forward-looking variables that embody market expectations over the policy change at the current meeting. In that case, our identification requires that output, inflation, unemployment and capacity utilization respond to policy with a sufficiently long lag that the controls in Eq. (1) are not subject to reverse causation. The second assumption rules out simultaneity in the regressions to be used in generating open economy responses, which could occur if Federal Reserve policy was responding directly to open economy variables (e.g., exchange rate stabilization). We think this plausible for the period 1969–2001 – see the discussion of FOMC policymaking in Meulendyke (1998). There is at least one episode within the sample period where U.S. monetary policy may have responded directly to the exchange rate. Romer and Romer (2004) cite some narrative evidence which suggests a direct policy response to the exchange rate in the six months spanning the final quarter of 1984 and the first quarter of 1985. Accordingly, in Section 3.4, we investigate the sensitivity of our results to data from that episode.

2.1.2. Mapping meetings to months

Since the macroeconomic data we wish to study are monthly, we need to translate the shock series from a meeting frequency to a monthly frequency. To achieve this end, we use a novel aggregation method where we: (1) define a daily series equal to the residual from regression (1) on FOMC meeting days, and zero otherwise; (2) cumulate the daily series; (3) average the resulting values across days within a month; and (4) take the first difference of the resulting monthly series. The procedure yields a monthly policy shock equal to zero in months in which FOMC meetings do not occur. In the remainder of the paper, we denote the monthly unanticipated and exogenous U.S. monetary policy component by UM.

The method of aggregation outlined here ensures that the structural effects of monetary policy on monthly average values of the exchange rate, foreign interest rates and other macroeconomic series can be consistently estimated. As an illustration of this point, suppose that a 100 b.p. policy shock causes an instantaneous 1% exchange rate appreciation, but no subsequent adjustment. If the shock occurs on the 16th of a 30 day month, then in monthly data the shock will be measured as 50 b.p. and the exchange rate appreciation as 0.5%, yielding the underlying unit contemporaneous relationship. A further 50 b.p. shock and 0.5% appreciation will be recorded the next month, preserving the unit relationship.[14] Alternatives to this approach, such as cumulating the meeting-based shocks that occur within a particular month (the

[12] If the federal funds future's market existed from the start of our sample, one could regress the shock on the future's market-implied target federal funds rate, just prior to a meeting. However, these markets do not exist until October 1988.

[13] Given the current frequency of FOMC meetings, the previous 12 meetings span roughly 18 months. Historically, the meeting frequency has been higher and the time spanned by 12 meetings would be a little over one year.

[14] One implication of this aggregation is that policy shocks at the monthly frequency will be autocorrelated because a within-month shock does not exert a proportionate effect on the interest rate level until the next month, *ceteris paribus*. The strength of the autocorrelation will depend on the day of the month on which FOMC meetings occur – if all meetings occur on the first of the month, there would be no autocorrelation. However, given similar smoothing of the macroeconomic series, they will also be autocorrelated at the monthly frequency and the relationships of interest will be consistently estimated.

method adopted by Romer and Romer, 2004), would not allow consistent estimation of the contemporaneous and dynamic effects of policy. For instance, in the example described, the monthly shock would be measured as 100 b.p. and the estimated impact effect for the monthly exchange rate would be 0.5 rather than 1 (recall that monthly exchange rates and other macroeconomic data used in simulating the effects of policy will be measured on a period average basis). Such considerations are important in the context of this paper because the contemporaneous responses to monetary policy are of interest in the case of fast-moving variables such as the exchange rate (or asset prices in general). They are arguably less crucial in the context of the previous work of Romer and Romer (2004) which focuses on slow-moving goods market variables.

2.2. Econometric methodology

In order to evaluate the open economy consequences of UM, we estimate the following dynamic model for each U.S.–Foreign pair with monthly data:

$$Z_t = A(L)Z_{t-1} + B(L)UM_t + u_t \tag{2}$$

where $Z = [s, i, i^*, y^*, NBRX, y, p]'$, s is the logarithm of the exchange rate (U.S.$ per foreign currency unit), i is the U.S. 3 month t-bill rate, $i^*$is the foreign 3 month t-bill rate (rates are measured in annual percentage points), y^* is the logarithm of the foreign industrial production index, *NBRX* is the logarithm of the ratio of average non-borrowed reserves plus extended credit to average total reserves in the U.S. banking system, y is the logarithm of the U.S. industrial production index, p is the logarithm of the U.S. CPI, and u is a mean-zero error term. All goods market variables are seasonally adjusted and all interest rates and exchange rates are monthly averages of end-of-day rates – see Appendix Table A.1 for data sources. The elements of Z are identical to those in the bilateral VARs of Eichenbaum and Evans (1995) and Faust et al. (2003). $A(L)$ and $B(L)$ are vector lag operators. Powers of L range from 1 to 6 in the case of $A(L)$ and 0 to 6 in the case of $B(L)$. 6 monthly lags is a common choice in the literature and allow us to maintain comparability with Faust et al. (2003). Notice how the specification includes an unrestricted contemporaneous effect of the identified policy shocks (UM) on each variable in Z. Impulse responses for the endogenous variables can be calculated with the reduced-form estimates of $A(L)$ and $B(L)$. A constant is included in each equation.

We again highlight some important differences between the identification strategy we undertake and a traditional VAR-based identification. Since UM identifies monetary policy shocks, there is no equation capturing feedback from the system of variables to UM. Moreover, unlike a traditional VAR-based identification, we impose no restrictions upon the covariance matrix of the residual vector u_t in order to extract structural shocks, which are represented directly in the system by UM. The VAR is purely employed as an estimation tool for generating dynamic responses.[15] In our application, the prior identification of monetary policy means that restrictions on the VAR are not needed. Of course, the monetary policy identification that we implement is conditional on other assumptions, as we discussed earlier.

We estimate the bilateral VAR model for six country pairs, where the U.S. is Home and one of the non-U.S. G7 nations is Foreign. The foreign countries are: Canada, France, Germany, Italy, Japan, and the United Kingdom. The sample spans the period 1974m1–2001m10 (except for Italy, for whom the interest rate series only begins in 1977m3).[16] For countries which joined the euro in 1999, we append the converted U.S. dollar–euro rate to the exchange rate for the final three years.[17] Estimation is undertaken by means of seemingly unrelated regression, and the results are used to generate impulse

[15] The treatment of UM in the augmented VAR parallels the treatment of the Romer and Romer (1989) dummy variable for monetary contractions in Eichenbaum and Evans (1995)'s investigation of the open economy consequences of U.S. monetary policy.

[16] Recall that our series for UM is only available through the end of 2001 due to lags in the publication of Greenbook forecasts.

[17] The introduction of the euro in 1999 raises the possibility of parameter shifts in the VAR models (and changes to the IRFs generated from them) during the last three years of the sample. In order to check that any such shifts are not important in driving our main results, in Section 3.4 we consider results from a sub-sample that excludes the years following the introduction of the euro.

response functions (IRFs) for monthly horizons 0–48 following an unanticipated and exogenous contraction of the federal funds rate. In order to account for any additional uncertainty arising from UM's character as a generated regressor, we bootstrap 1000 times via pairwise sampling with replacement and then report the observed IRF and bootstrap percentile intervals with 68% coverage.[18] Such a technique is robust to heteroscedasticity, but not serial correlation. Accordingly, in Appendix Table A.2, we report test results indicating that lack of serial correlation cannot be rejected at the 1% level for most of the VAR equations. Exceptions occur for the U.S. t-bill and *NBRX* equations. The evidence for serial correlation in these two cases appears to be due to outliers in the VAR residuals during the 1979–82 period of reserve targeting at the start of Volcker's chairmanship of the FOMC. Estimating the model post-1982 yields residual estimates for which the hypothesis of no serial correlation cannot be rejected at the 1% level for any equation. In order to check the validity of our results in the presence of possible serial correlation, we generated IRFs using the method of local projections and calculated heteroscedasticity and autocorrelation consistent (HAC) standard errors (Jorda, 2005). Pagan (1984) showed that inference using conventional standard errors under a null hypothesis of no effect remains valid in the presence of a generated regressor. As we discuss in Section 3.4, the results from this exercise confirm those obtained using the bootstrap.

The consistency of the IRFs that we report depends upon the stationarity properties of the variables in the VAR. If the variables are nonstationary but not cointegrated, the estimated relationships between them may be spurious, which would affect the estimated IRFs (Yule, 1926; Granger and Newbold, 1974). Accordingly, we conducted a series of Augmented Dickey–Fuller (ADF) tests to determine the time series properties of the variables. The results provide strong evidence that UM is stationary, while each of the other variables are integrated of order one (I(1)) and therefore nonstationary.[19] We then conducted trace tests for the cointegrating rank of a VAR comprising 6 lags in each of the 7 nonstationary variables.[20] The results point to the existence of between one and four cointegrating vectors, depending on which foreign country is included in the model. As is well-known, cointegrating vectors derive stationary relations from I(1) data series. Any part of an IRF derived from the effect of a stationary UM term upon the stationary cointegrating relationship will not be subject to the spurious regression problem. In this way, cointegrating relations allow for valid inference in systems comprising nonstationary variables.

In many applied studies, researchers attempt to explicitly identify the cointegrating vectors to permit their interpretation as long-run macroeconomic relationships. We do not pursue that option in this paper for two reasons. First, our interest centers upon the open economy IRFs derived from the policy identification captured in UM. Cointegration is necessary for the validity of IRFs from the VAR in levels when some of the response variables are I(1), but the precise form of that cointegration is not crucial. Second, in a seven variable VAR there are several potential sources of long-run stationary relationships, including an interest parity relation, an exchange rate effect in the pricing or IS curve relations, and various closed economy relationships for individual countries. The explicit identification of the cointegrating vectors requires that we take a stand on the relative importance of these potential long-run relationships amongst different country pairs. In order to avoid such restrictions and to facilitate comparisons with previous studies of the open economy consequences of U.S. monetary policy, we concentrate on the IRFs generated from VAR models in which cointegrating relations exist but are not explicitly identified. A disadvantage to such an approach is that it leaves open the possibility that some nonstationary variables from the VAR lie outside the cointegration space, and as such are a potential source of spurious regression. In order to address this concern, we report results in Section 3.4 from a version of the baseline VAR in which each of the nonstationary series enter as stationary first-differences. Such specifications are immune to the spurious regression problem because nonstationary variables are excluded from the system. Furthermore, Ashley and Verbrugge (2009)

[18] Similar to Faust et al. (2003), we do not implement the bootstrap correction for finite sample bias in estimates of autoregressive models described by Kilian (1998). However, the observed IRFs that we report are very close to the median IRFs obtained from the bootstrap replications, suggesting that any finite sample bias may be limited.

[19] These results are available upon request.

[20] The UM term was excluded from the VAR used to determine cointegrating rank because the standard critical values used in the trace test procedure do not allow for non-modeled generated regressors.

provide evidence that inferences concerning IRFs from VARs in first-differences tend to be reliable even when there is cointegration in the VAR in levels, but those cointegrating relations are not incorporated in the first-differences specification. As we discuss in Section 3.4, the results from a model in first-differences broadly support our findings from the baseline analysis.

3. Results

We now present the estimated impulse responses for each of the six country pairs, considering: (1) the exchange rate and foreign interest rates; (2) foreign output; and (3) U.S. output, prices and bank reserves. We close the section with a discussion of relevant robustness tests. For all of the IRFs, the experiment is an unanticipated and exogenous monetary contraction, normalized to exert a 100 b.p. effect on the U.S. t-bill rate.[21]

3.1. Exchange rate and interest rate responses

In Fig. 1, we present IRFs for bilateral exchange rates following a 100 b.p. increase in UM. In all six cases, the U.S.$ appreciates on impact, consistent with the predictions of the traditional Mundell–Fleming–Dornbusch model. The contemporaneous U.S.$ appreciation versus the German mark is 1.70%, and that versus the British pound is 1.30%. These estimates are close to the impact exchange rate responses obtained by Faust et al. (2003), which are 2.44% and 1.41% respectively. It is interesting that the impact effects estimated by Faust et al. for the period 1994–2001 using intra-day exchange rate data are similar to those that we estimate using data from a much longer period, and a different approach to identification. However, the uncertainty associated with our estimates is slightly larger than that associated with Faust et al.'s estimates. Our results also highlight the importance of the method by which policy shocks are aggregated from a meeting frequency to a monthly frequency. If our identified shocks are simply cumulated within months, which we contend is inconsistent with the use of period average monthly data, the contemporaneous exchange rate effects are much smaller, in the range 0.5%–1% (results not reported).

The maximum U.S.$ appreciation versus the German mark occurs at a two month horizon, while the maximum appreciation against the British pound occurs at a six month horizon. The timings for these maximum effects are towards the lower end of the range of horizons for which maxima may occur within the bounds of the partially identified IRFs reported in Figs. 3 and 4 of Faust et al. (2003). However, there is a substantial degree of uncertainty associated with the IRFs that we report in Fig. 1. For example, in the case of the US$/GRM exchange rate, the estimated maximum effect that occurs after 2 months is encompassed by the confidence intervals at 48 months. As such, although our exact identification of the effect of monetary policy on the exchange rate points to estimates of delayed overshooting at the lower end of the range of estimates documented by Faust et al., the evidence is not precise enough to narrow the range established via their partial identification of monetary policy's effects.

The sizes of the maximum U.S.$ appreciations that we estimate are 3.3% for both the German mark and the British pound. In Faust et al. (2003), the midpoints of the 68% confidence interval at the maximum bounds are roughly 4% for the German mark and 2.7% for the British pound (the maximums are 7.6% and 4.6% respectively). Taken alongside the estimated contemporaneous exchange rate responses and the approximate timing of the maximum responses, these results indicate a strong concordance of the exchange rate responses to U.S. monetary policy estimated under two extremely different identification schemes.

The concordance is important because debate often centers on the validity of the identifying assumptions used for monetary policy. As discussed by Faust et al. (2003), one of the threats to their approach is that changes in the futures price around FOMC announcements may reflect the revelation of private, central bank macroeconomic information. If so, then the futures price change would contain an unanticipated and *endogenous* component of monetary policy. Faust et al. (2004) provide

[21] Such an interpretation is facilitated by scaling the impulse responses by the reciprocal of UM's impact effect on the U.S. t-bill rate.

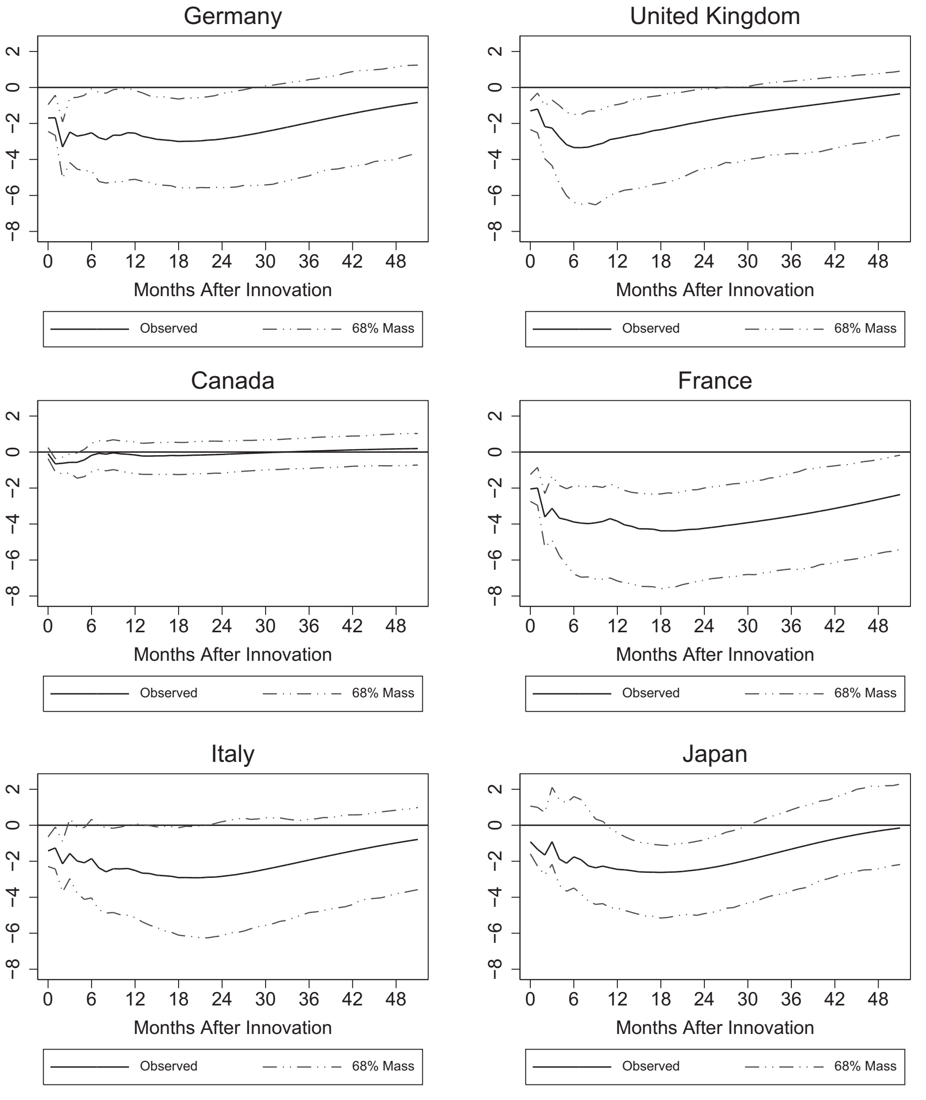

Fig. 1. Bilateral exchange rate (US$/) impulse response. Note: Experiment is a 1 percentage point US interest rate innovation. All responses are in percentage points. US interest rate measure is the US 3 month t-bill rate and its innovation is derived from the monthly difference of the average, daily-cumulated UM. 1000 bootstrap replications.

evidence that this problem is unlikely to affect their results. In our application, note that the Greenbook forecasts are a potential source of private, central bank information (their release is subject to a five year lag). By construction, our U.S. monetary policy shock measure is orthogonal to this information. The fact that our estimated exchange rate responses for Germany and the UK are consistent with those reported by Faust et al. (2003) supports the assertion that any endogeneity of futures price changes is likely of limited relevance for the estimation of U.S. monetary policy's open economy effects.

On the other hand, one of the threats to our identification approach is that public information which is useful in predicting target changes may arrive in the interval between the first circulation of the Greenbook forecasts and the associated FOMC meeting (usually 6 days, Romer and Romer, 2004). If the public information is not reflected in the capacity utilization index, then UM would contain an *anticipated* component of monetary policy. Again, the similarity of our estimated exchange rate responses for Germany and the UK to those of Faust et al. (2003) suggests that any distortion is small. Recall that the futures price changes are based on the information set only 1 business day prior to an FOMC meeting. Altogether, the evidence for the exchange rate (and other variables as we see later) tends to support the validity of the two different identification approaches.

The IRFs for the U.S.$ exchange rate versus other currencies largely bear out the results for the German mark and the British pound. The contemporaneous and maximum appreciations of the U.S.$ are slightly larger against the French franc (and the statistical significance of the results is strongest in this case), but slightly smaller against the Italian lira and the Japanese yen. In each of these cases, the IRF is relatively flat over the horizons of 6–24 months. The confidence intervals show that substantial delays in exchange rate overshooting cannot be ruled out, mirroring the results for the mark and pound. A different pattern of results is observed in the case of the US$/CN$ exchange rate. In this case, the maximum appreciation is less than 1% and decays within 6 months (a similar finding is reported in Kim, 2001).

One explanation for the limited adjustment of the US$/CN$ exchange rate is provided in Fig. 2, which plots IRFs for foreign 3 month t-bill rates. The degree of interest rate pass-through is relatively high in the Canadian case. This is not surprising, given the strong economic linkages between Canada and the U.S. For example, pass-through to Canadian interest rates exceeds unity for most of the first six months after a U.S. monetary policy shock, whereas pass-through to German and British interest rates does not exceed unity at any horizon. The powerful transmission of U.S. monetary shocks to Canadian interest rates limits the scope for profitable arbitrage in the foreign exchange market and therefore the extent of exchange rate adjustment. The endogeneity of Canadian monetary policy with respect to U.S. monetary policy is noted in Kim and Roubini (2000), who document maximum interest rate pass-through equal to roughly half the level that we report for Canada in Fig. 2.

The other foreign interest rate responses in Fig. 2 appear plausible. Our estimates of pass-through to German and British interest rates are generally similar to those of Faust et al. (2003). The IRFs that we report are close to the maximum of Faust et al. 's bounds in the German case and close to the midpoint of the bounds in the British case.

The deviation from uncovered interest parity (UIP) is a useful summary of the U.S. t-bill rate, foreign t-bill rate, and exchange rate estimated IRFs. It takes the form:

$$IRF(r,h) - IRF(r^*,h) - 4\cdot[IRF(s,h+3) - IRF(s,h)] \tag{3}$$

where the first argument of each IRF denotes the relevant endogenous variable and the second argument denotes the relevant IRF horizon.[22] These conditional (viz., post-monetary shock) UIP deviations for each country pair are plotted in Fig. 3, along with confidence intervals based on 68% coverage. The dominant feature of these plots is the large excess return on U.S. assets following an exogenous policy contraction, particularly in the models featuring Germany, the U.K., and France. At short horizons, the excess return exceeds 5 percentage points on an annual basis in the case of the U.S./U.K. model. A comparison of the foreign t-bill responses in Fig. 2 with the U.S. t-bill responses in Fig. 4 indicates that during the first few months the interest rate differential between the U.S. and the foreign country is positive in all cases (except Canada) – the persistent effect of an exogenous policy contraction contributes to UIP deviations. However, these interest rate differentials are small in comparison to the UIP deviation and are generally eliminated within 6 months as interest rate convergence occurs.

Instead, the main driver of excess returns appears to be the exchange rate, which typically appreciates for most of the first six months and therefore amplifies excess returns on U.S. assets. Such exchange rate behavior is an example of the well-known forward premium anomaly associated with the delayed adjustment of the exchange rate. The results also indicate some sluggishness in exchange rate adjustment at longer horizons. In particular, despite the relatively fast reversal of any interest rate differential in favor of U.S. assets, mean reversion of the exchange rate commences at horizons between

[22] Since the fixed income assets (t-bills) have three-month horizons, we use the three-month change in the log exchange rate. The log exchange rate change must then be annualized (times 4), to match the interest rate quotation.

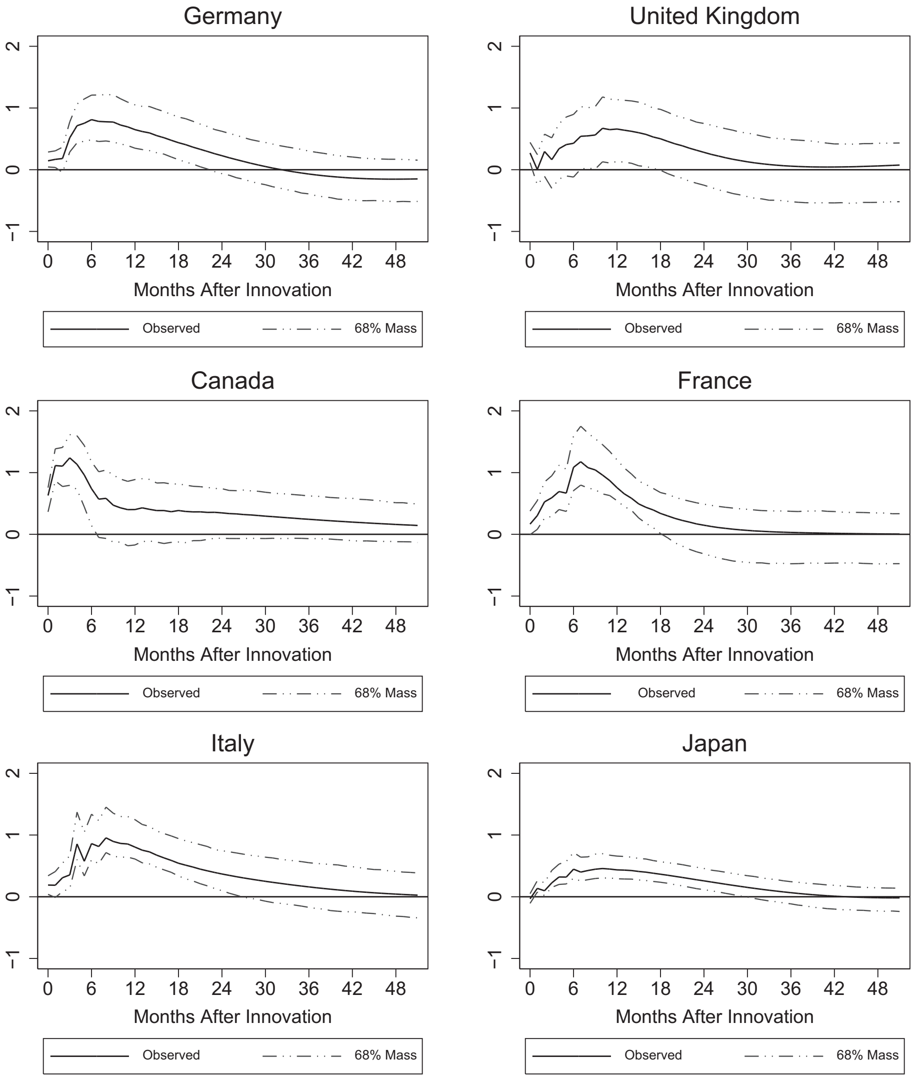

Fig. 2. Foreign 3 month t-bill/interbank rate impulse response. Note: Experiment is a 1 percentage point US interest rate innovation. All responses are in percentage points. US interest rate measure is the US 3 month t-bill rate and its innovation is derived from the monthly difference of the average, daily-cumulated UM. 1000 bootstrap replications.

24 and 30 months in the cases of the German mark, the French franc, the Italian lira and the Japanese yen. Only the U.S.$ exchange rates versus the British pound and Canadian dollar exhibit mean reversion at shorter horizons. Overall, our results confirm the empirical robustness of delayed exchange rate adjustment, while at the same time providing evidence for a shorter time to maximum appreciation in response to a contractionary U.S. monetary policy shock.

3.2. Foreign output responses

In Fig. 5, we present the IRFs for foreign industrial production following a U.S. monetary contraction. For Germany and the U.K., the dominant feature of the response to an unanticipated and exogenous U.S.

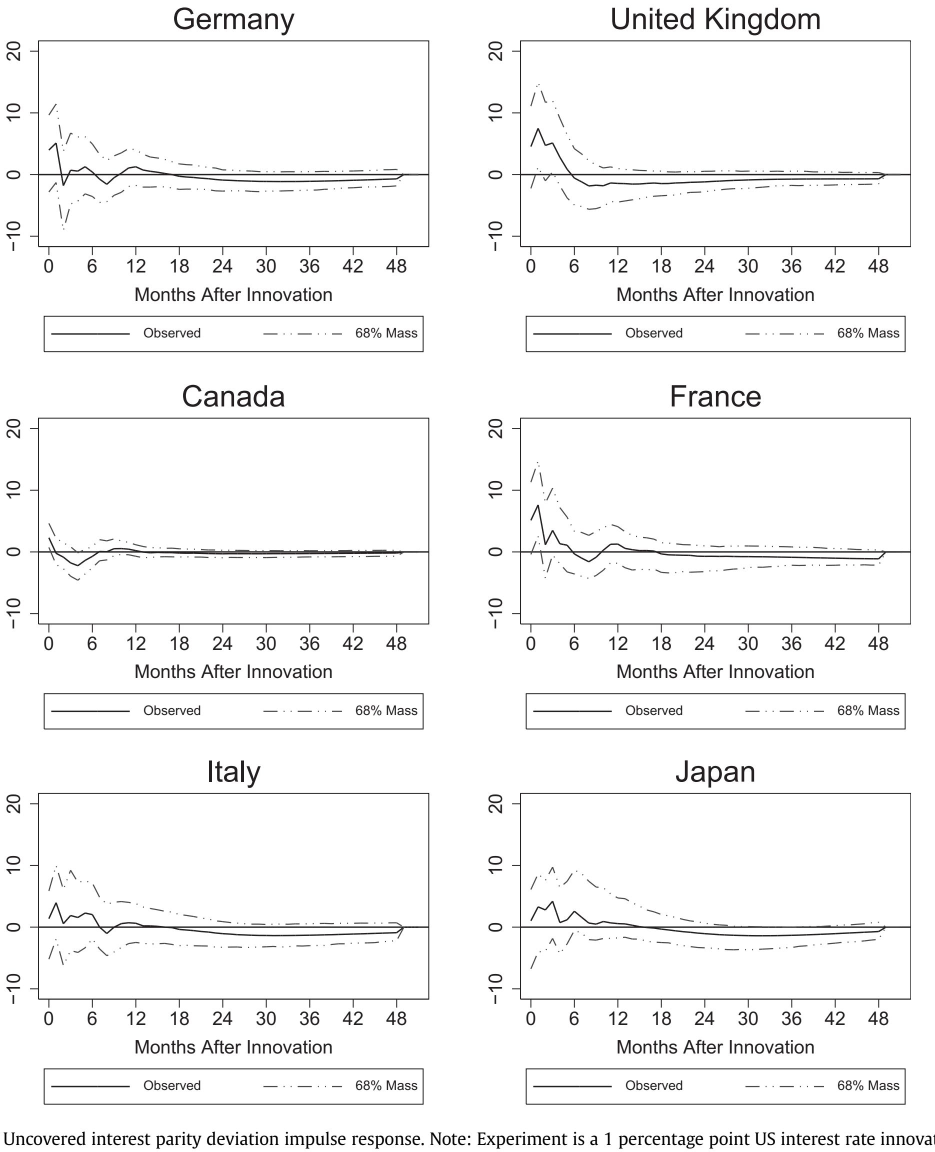

Fig. 3. Uncovered interest parity deviation impulse response. Note: Experiment is a 1 percentage point US interest rate innovation. All responses are in percentage points. US interest rate measure is the US 3 month t-bill rate and its innovation is derived from the monthly difference of the average, daily-cumulated UM. 1000 bootstrap replications.

monetary policy contraction are maximum declines in industrial production equal to one percentage point. The magnitudes of these output declines are both firmly within the bounds for the maximums reported by Faust et al., 2003). However, the timing for the U.K.'s maximum decline is approximately 20 months earlier than the timing for the maximum point of the bounds (13 as opposed to around 35 months). The timing of the German maximum responses is similar across the two identifications (around 35 months).

In our results, the main difference in the industrial production responses across Germany and the U.K. occurs during the first 12 months. German industrial production actually increases during this period, while U.K. industrial production is initially stable and then falls rapidly, reaching its trough after one year. One way to understand these differences is in terms of competing expenditure-switching effects (towards non-U.S. output) and expenditure-reducing effects (lower output and spending in the

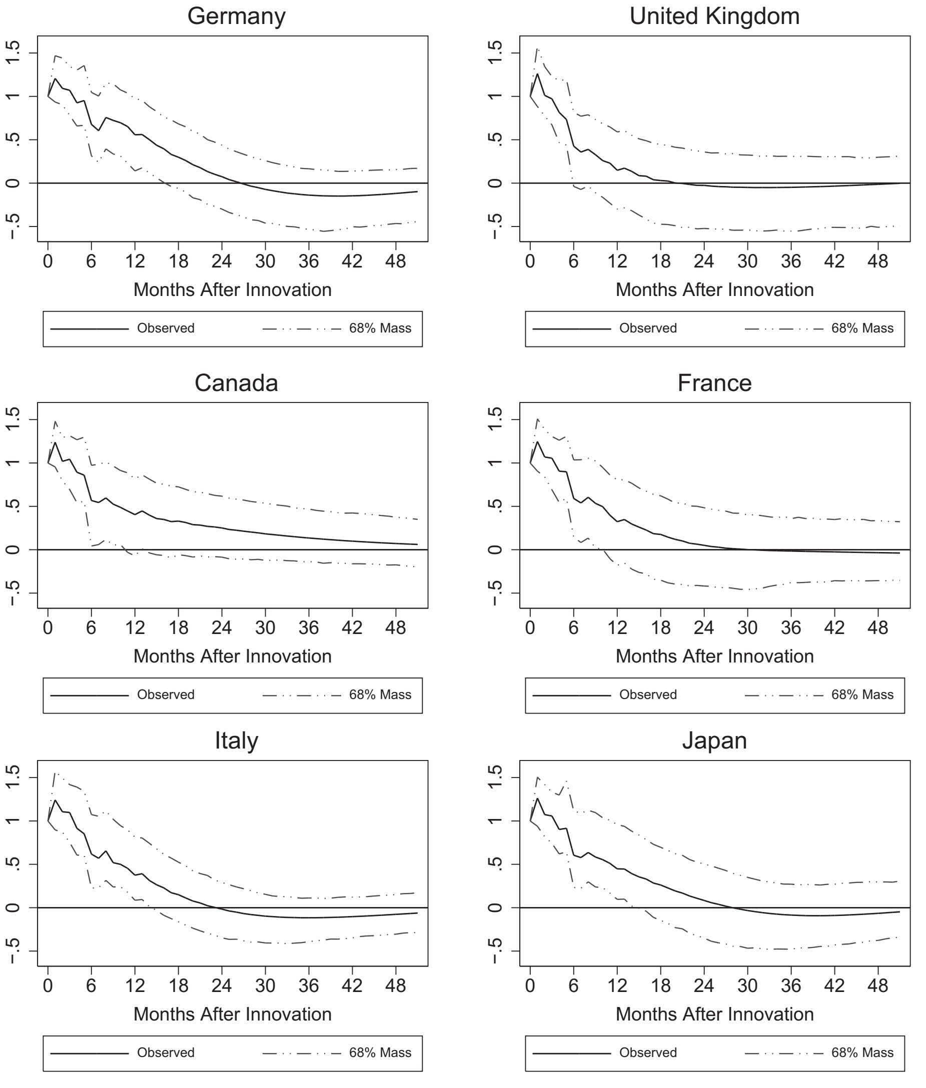

Fig. 4. US 3 month t-bill rate impulse response. Note: Experiment is a 1 percentage point US interest rate innovation. All responses are in percentage points. US interest rate measure is the US 3 month t-bill rate and its innovation is derived from the monthly difference of the average, daily-cumulated UM. 1000 bootstrap replications.

U.S. reduces demand for non-U.S. output). The U.K. may be subject to a more powerful direct negative income effect after a policy induced slowdown in the U.S. because the U.S. accounts for a larger fraction of U.K. exports than German exports. This argument requires that expenditure-reducing effects dominate expenditure-switching effects, because differences in trade shares would also suggest a more powerful expenditure-switching effect in favor of U.K. goods. Of course, other explanations are also possible. For example, although the size of exchange rate and foreign interest rates responses were comparable across Germany and the U.K., suggesting that they do not account for different output effects, cross-country differences in the elasticity of output with respect to interest rate and exchange rate changes could account for the results.

Although they all indicate declines in output, the evidence from the other countries is not decisive in discriminating between alternative explanations. The IRFs indicate a high probability of recession in

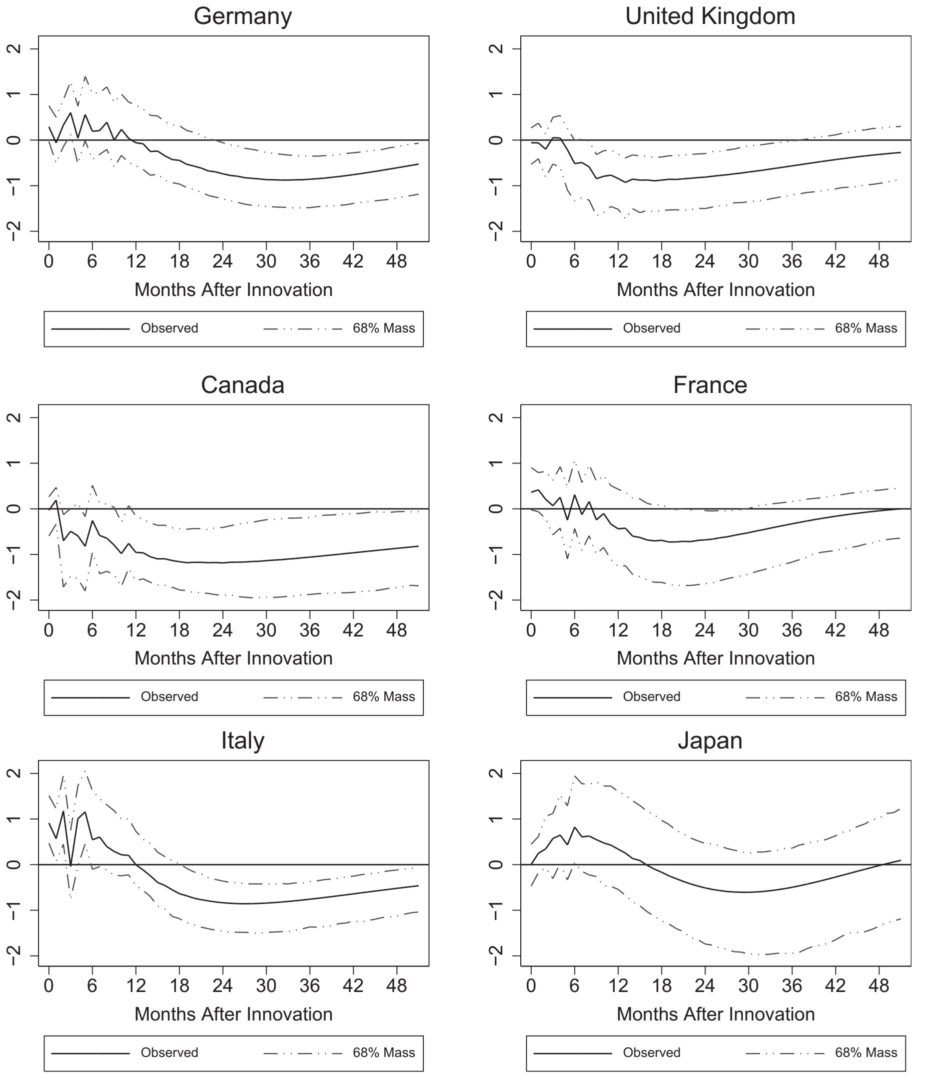

Fig. 5. Foreign industrial production index impulse response. Note: Experiment is a 1 percentage point US interest rate innovation. All responses are in percentage points. US interest rate measure is the US 3 month t-bill rate and its innovation is derived from the monthly difference of the average, daily-cumulated UM. 1000 bootstrap replications.

Canada during the first 6–12 months, while the opposite appears true for France and Italy. Such differences are consistent with stronger direct trade links between the U.S. and Canada than between the U.S. and continental European countries. However, they may also be due to the high level of interest rate pass-through to Canada and the muted response of the Canadian dollar. Furthermore, the responses in Fig. 5 suggest output increases in Japan at first, even though the U.S. is a major trading partner for Japan and is therefore likely to exert a strong expenditure-reducing effect.

3.3. U.S. output, price, and reserve responses

We now turn to the responses of U.S. domestic macroeconomic variables to a U.S. monetary policy contraction. In Fig. 6, we report the IRFs for U.S. industrial production for each model. The estimated

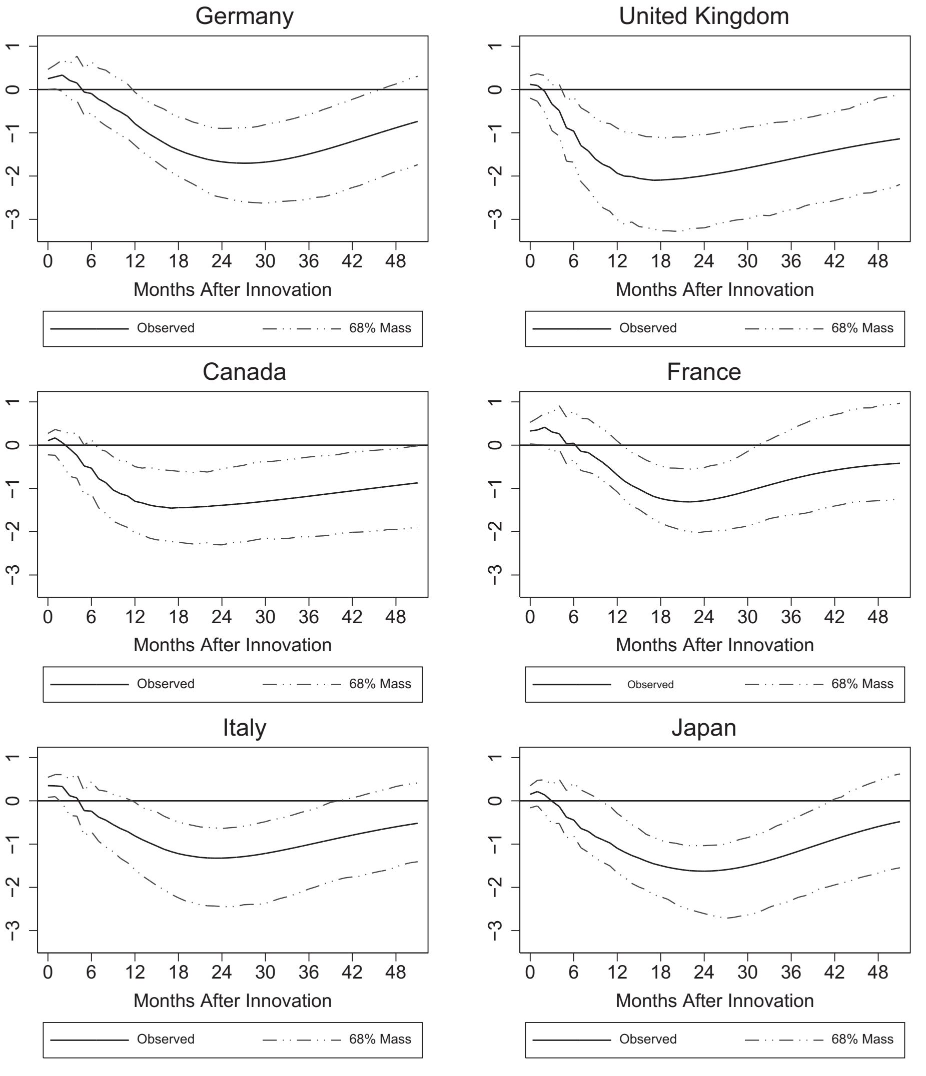

Fig. 6. US industrial production index impulse response. Note: Experiment is a 1 percentage point US interest rate innovation. All responses are in percentage points. US interest rate measure is the US 3 month t-bill rate and its innovation is derived from the monthly difference of the average, daily-cumulated UM. 1000 bootstrap replications.

maximum reduction in output is approximately two percentage points when the U.K. is the foreign country and slightly less than two percentage points when Germany is the foreign country. These estimates are close to the top end of the range of Faust et al. 's partially identified responses. The ranges of estimates for the time of the maximum domestic output response are comparable to those implied by the partial identification scheme. The other four bilateral models indicate slightly smaller output reductions, equal to about 1.5 percentage points.

A comparison of these output effects with those reported by Romer and Romer (2004) is informative. First, as in the results presented by the Romers, there is initially a short-lived increase in output following a monetary policy contraction. One explanation is that the identification does not remove all endogenous policy changes, even after extending the orthogonalization to include additional unemployment forecasts and the capacity utilization terms.[23] Second, domestic output's trajectory after a monetary contraction follows a U-shape, as in Romer and Romer (2004). The turning point of the U-shape occurs either at the same time or slightly earlier than in the Romers' results. However, the magnitude of the output reduction at the turning point is only half that estimated by Romer and Romer (2004).

There are many possible explanations for the differences in the magnitudes of the output responses between Fig. 6 and those reported by Romer and Romer (2004). In addition to a different (but related) shock series, the models that we estimate for industrial production use different sample periods, different lag orders, and a broader range of controls. To make progress towards a specific explanation, we estimated two alternative sets of results, each based on a slightly different method for deriving the shocks but otherwise identical to our baseline implementation. The first variant uses the orthogonalization described in Section 2.1 to generate shocks at the meeting frequency, but converts those shocks to a monthly frequency through cumulating within months (the method used by Romer and Romer). The second variant uses the aggregation method described in Section 2.1, but generates the underlying shocks using the orthogonalization specification employed by Romer and Romer – the additional unemployment forecasts and the capacity utilization terms used in Section 2.1 are omitted. The results from this exercise (not reported) indicate that the aggregation method is the main source of differences in the results. In particular, aggregating shocks via Romer and Romer (2004)'s method leads to U.S. output decreases up to twice as large as those in Fig. 6, which is comparable to those reported by Romer and Romer (2004). We also found that the output declines were slightly more delayed than in the baseline case. In contrast, we found that varying the set of controls used in the orthogonalization but preserving our baseline aggregation method lead to output declines of a similar order of magnitude to those in Fig. 6. However, we did find that this method lead to initial output puzzles that were somewhat more persistent than in the baseline case, taking 5–10 months to dissipate. This suggests that the capacity utilization index emphasized by Giordani (2004) plays some role in identifying the effects of monetary policy shocks in our baseline case, even after controlling for the Greenbook forecasts.

In Fig. 7, we report the IRFs for the U.S. Consumer Price Index (CPI). When the U.K. is the foreign country, the estimated price response is within the bounds reported by Faust et al. (2003). When Germany is the foreign country, there is more evidence of a price puzzle (a price increase in response to a monetary contraction) than in Faust et al. (2003)'s results. In the case of price responses from models featuring foreign countries other than Germany and the U.K., there is evidence for a price puzzle, lasting for up to 24 months.

Such results stand in stark contrast to those presented by Romer and Romer (2004), who find no change in the price level for 18–24 months, followed by statistically significant price declines of 5–6 percentage points. In this case, the main reason for the difference between the results that we present and those presented by Romer and Romer is not the orthogonalization underpinning the monetary policy shock identification or the method used to aggregate from meetings to months. In fact, versions of the results that use either the orthogonalization or the aggregation method employed in Romer and Romer (2004) generate even more pronounced price puzzles than those reported in Fig. 7. Moreover, the eventual price decreases are smaller than those displayed in Fig. 7. Instead, a crucial factor in determining the price responses is the number of lags of the exogenous shock measure included in the price equation. If the maximum order of the lag operator $B(L)$ in Eq. (2) is increased from 6 to 48 (matching the maximum lag length used in the Romers' price regressions), we obtain the set of IRFs for prices presented in Fig. 8. Excepting the case in which Italy is the Foreign country, price puzzles are absent and key features of the Romer and Romer results can be seen – namely, there is a flat price trajectory for 18–24 months, which is then followed by a price decline. The magnitude of the deflations at the 48 month horizon remain smaller than in the Romers' results, likely reflecting the broader range of controls included in the VAR price equation and also the different sample period (1974–2001 as opposed to 1969–1996).

Overall, the extensions of the Romer and Romer (2004) identification developed in this paper provide interesting insights concerning the effects of monetary policy on U.S. domestic variables

[23] Romer and Romer (2004) note that the output increase is mitigated (but not eliminated) after adding a dummy variable for April 1980 to the industrial production regression. This is also true of our results.

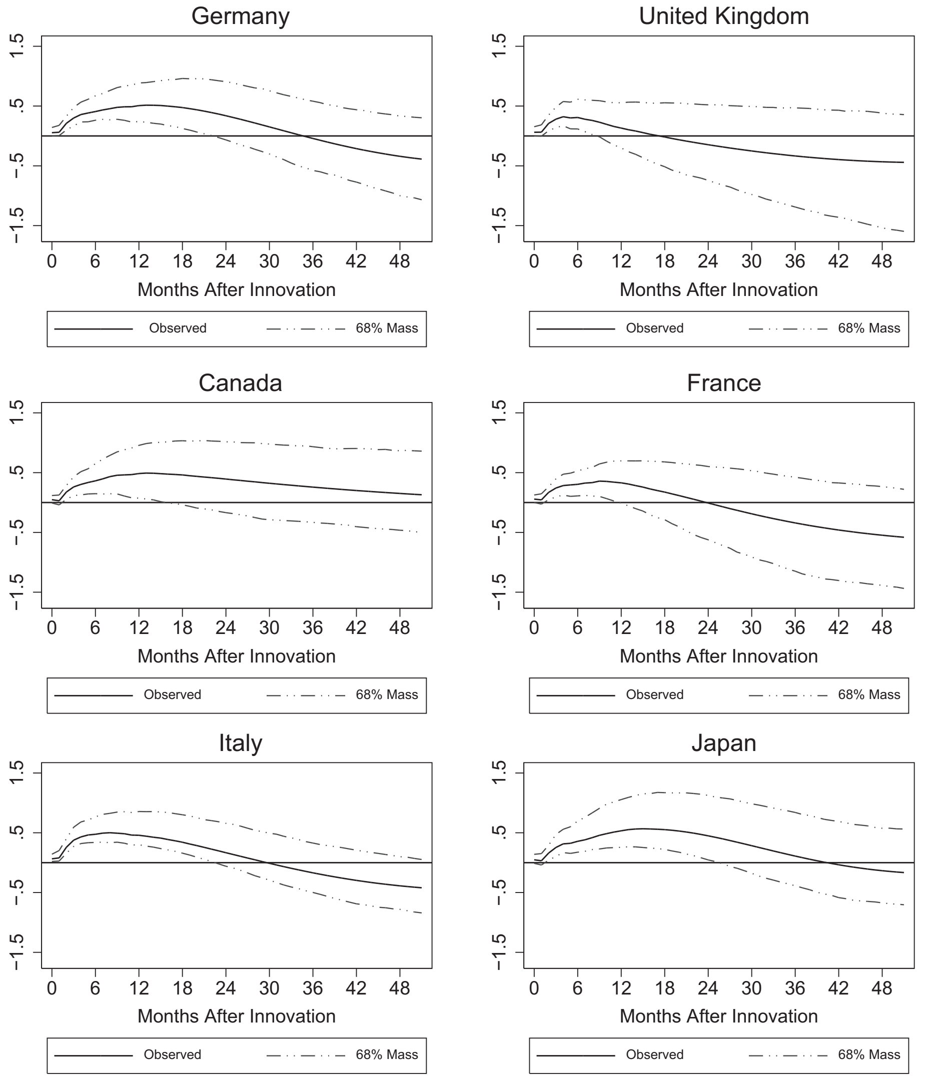

Fig. 7. US consumer price index impulse response. Note: Experiment is a 1 percentage point US interest rate innovation. All responses are in percentage points. US interest rate measure is the US 3 month t-bill rate and its innovation is derived from the monthly difference of the average, daily-cumulated UM. 1000 bootstrap replications.

(independent of the open economy context). The qualitative features of the results reported by Romer and Romer (2004) are robust to including the additional unemployment forecasts and the capacity utilization index in the orthogonalization, and to updating the sample. However, the magnitude of the output response appears to depend on the method of aggregation from FOMC meetings to months, and the avoidance of a price puzzle and evidence for eventual deflation depends on including a sufficient number of lags of the policy measure in the model (direct effects).

Finally, in Fig. 9, we present IRFs for the ratio of non-borrowed reserves to total reserves following an unanticipated and exogenous U.S. monetary policy contraction. For each bilateral VAR, there is a statistically significant two percentage point reduction in the ratio, which dissipates within 12–18 months. This is consistent with changes in the FOMC target being implemented via open market

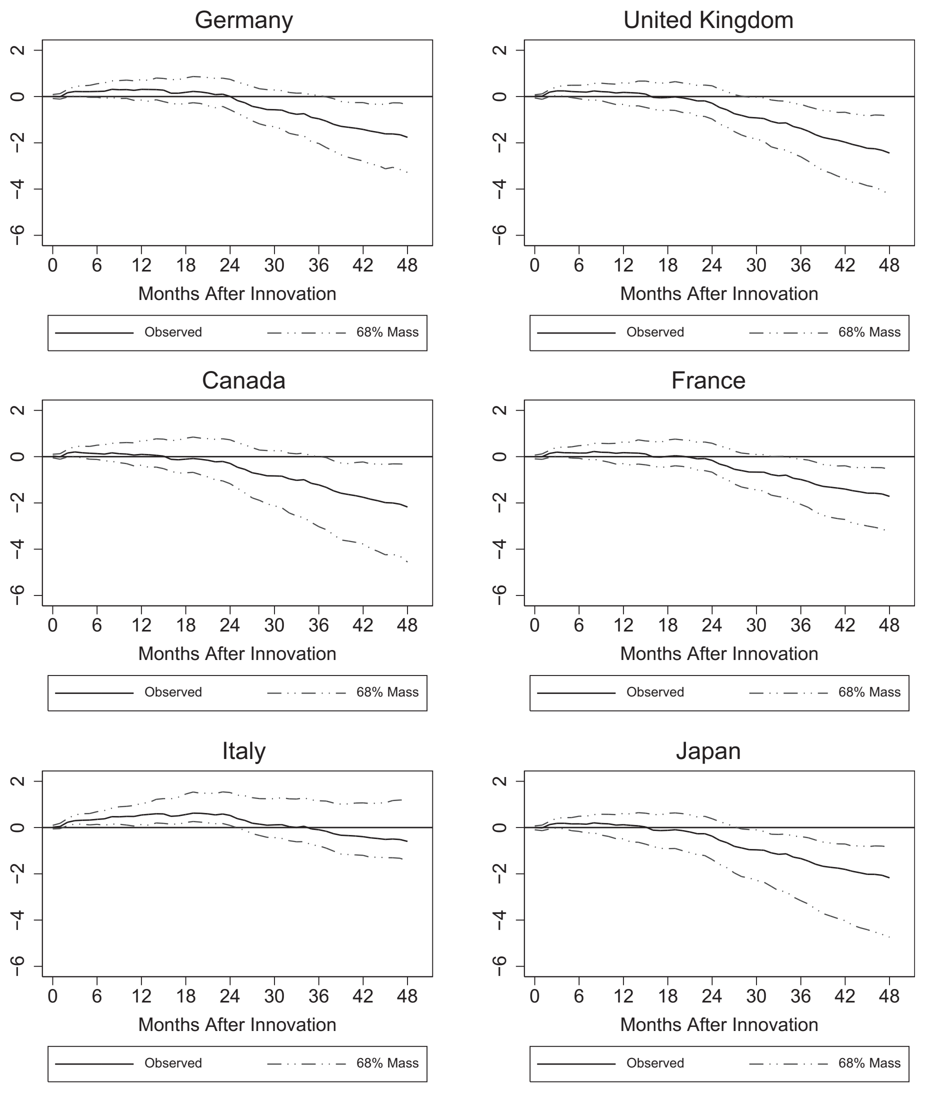

Fig. 8. US consumer price index impulse response, from baseline VAR with 48 lags of the exogenous policy measure (UM). Note: Experiment is a 1 percentage point US interest rate innovation. All responses are in percentage points. US interest rate measure is the US 3 month t-bill rate and its innovation is derived from the monthly difference of the average, daily-cumulated UM. 1000 bootstrap replications.

operations that reduce the stock of non-borrowed reserves in the banking system. In line with the mean reversion of the US interest rate responses in Fig. 4, this policy change is reversed within 18 months. Faust et al. (2003) report a similar correlation between interest rates and the fraction of non-borrowed reserves in total reserves, but their evidence for a large and statistically significant relationship is less strong than in the results that we present in Fig. 9.

3.4. Robustness

We now investigate the robustness of our results along four dimensions: (1) controlling for an episode in which U.S. monetary policy may have been directly endogenous to the exchange rate and

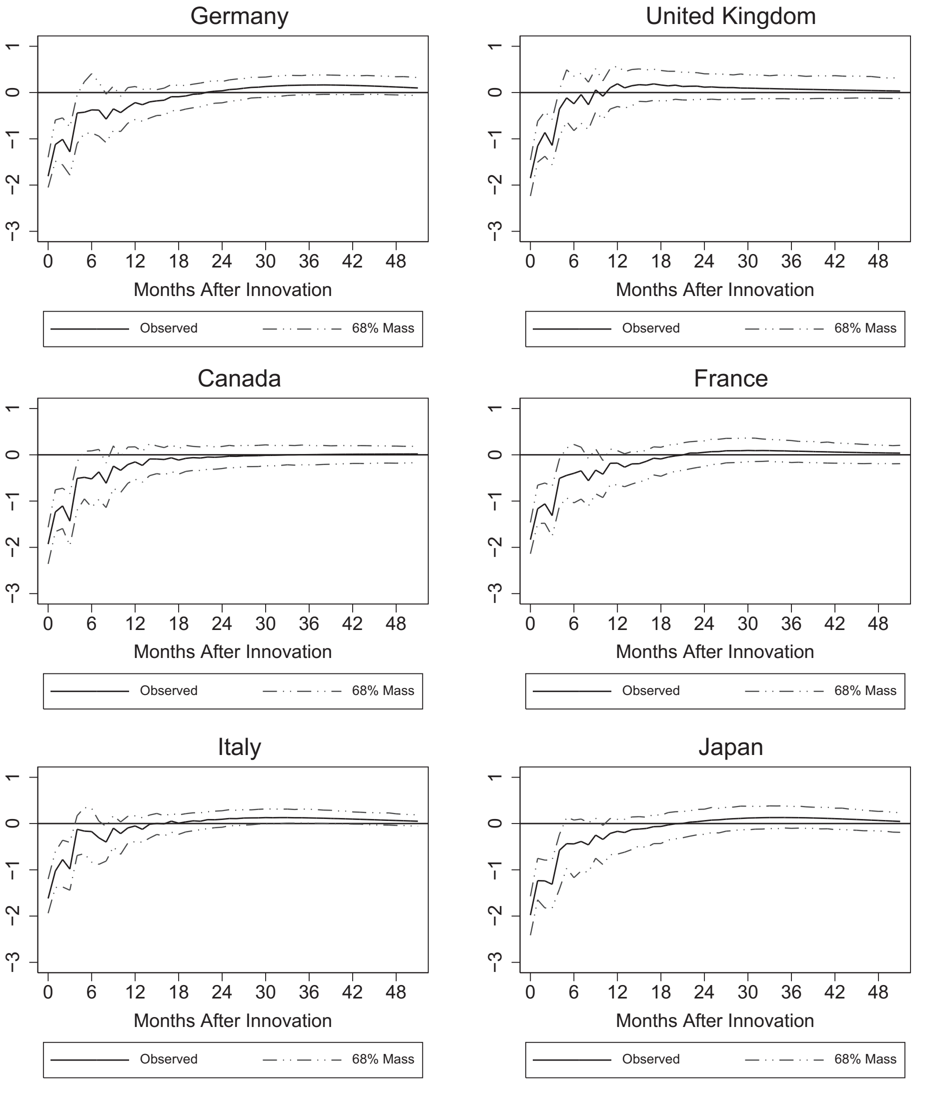

Fig. 9. US non-borrowed reserves to total reserves ratio impulse response. Note: Experiment is a 1 percentage point US interest rate innovation. All responses are in percentage points. US interest rate measure is the US 3 month t-bill rate and its innovation is derived from the monthly difference of the average, daily-cumulated UM. 1000 bootstrap replications.

other open economy variables; (2) using a sub-sample that excludes post-1992 data; (3) estimating the model in I(0) space through differencing each of the nonstationary variables; (4) generating the IRFs using the method of local projections (Jorda, 2005) rather than powering up the estimated VAR coefficients.

First, recall that a maintained assumption in our identification is that the target federal funds rate responds to the exchange rate and other open economy variables only to the extent that those variables influence the Greenbook forecasts for growth, inflation and unemployment and/or the capacity utilization index. If, for example, movements in the value of the dollar induce changes in the target federal funds rate without or before affecting the Greenbook forecasts, the UM shocks will be contaminated by endogenous policy interventions. One episode in which this may have been the case is the months spanning the end of 1984 and the start of 1985, when the FOMC repeatedly cited the strength of the dollar as one reason for easing policy (see Romer and Romer, 2004 and the sources referenced in their discussion). In order to address this issue, we added six impulse dummies to our baseline model in equation (2), one for each of the months covered by the final quarter of 1984 and the first quarter of 1985. This step reduces the impact of these observations on the estimated IRFs.[24] Our results were generally robust to this extension of the model. The maximum appreciation of the U.S.$ versus the British pound and the Canadian dollar increased somewhat relative to the baseline case, consistent with some attenuation of the baseline responses when monetary policy is used to stabilize currency fluctuations. However, in other cases, exchange rate responses were largely unchanged, as were the responses of foreign interest rates and other open economy variables. In general, it appears that estimates of the effects of monetary policy are not distorted by episodes in which the exchange rate may have been a target of policy.

The second robustness test that we implemented entailed estimating the IRFs for the sub-sample 1974m1–1992m8. This sub-sample excludes the post-1996 period in which we pooled announced changes in the target federal funds rate with the Romers' narratively identified changes in the target. It also excludes the exchange rate crises that saw the British pound and Italian lira exit the European Exchange Rate Mechanism (ERM) and the bands for the French franc widened, and the creation of the eurozone, which includes Germany, France and Italy, in 1999. Our core results were generally robust in this case. Interestingly, U.S.$ appreciations versus the currencies of Britain, Italy, and France (the three countries affected by some turbulence in foreign exchange markets in 1992) were larger but more delayed than in the baseline case. We also found that the evidence for a U.S. price puzzle was much weaker in the shorter sample.

The next robustness test involved replacing each element of the Z matrix with its first difference. Such a transformation maps the data to I(0) space, and thereby rules out spurious correlation amongst I(1) variables as a driver of our results (see the discussion in Section 2.2). We then simulated IRFs for the variables in first-differences, and cumulated the results to provide level responses comparable to those in our baseline results. Our main findings were:

1. The impact appreciations of the U.S.$ following a monetary tightening were similar to our baseline VARs, except in the case of the Japanese yen, against which the impact U.S.$ appreciation was about one quarter that observed in our main results.
2. The peak appreciations were generally larger (exceptions occur for the US$ exchange rate against the Canadian and Japan currencies) and occur 3–4 months later than in the baseline results (an exception occurs for the US$ exchange rate versus the French franc, for which the maximum appreciation occurs after 8 months, compared to 18 months in the baseline results).
3. The depreciation of the US$ following an initial appreciation occurs more gradually and is often incomplete at the 48 month horizon. Following the initial exchange rate appreciation, the IRFs from the first-differenced VARs tend to resemble those for the US$ and French franc exchange rate in the baseline results.
4. Foreign interest rate responses are very similar to the corresponding results from the baseline VARs during the first 12 months. At longer horizons, the responses decay, but not to the same extent as in the baseline results.
5. Foreign output responses exhibit the biggest changes relative to the baseline results. The initial rises in foreign output that occur for Italy and Japan in the baseline results are smaller, but more persistent (even at 48 months, output is slightly positive in these two cases). Results for other countries are qualitatively similar to those in the baseline cases, although the maximum output declines for Canada and France are noticeably smaller.

[24] Strictly speaking, given that $B(L)$ is of order 6, the model should include 6 lags of each impulse dummy in order to completely remove the six observations in question from the likelihood function. As this would consume a substantial number of degrees of freedom, we opted not to include the full set of impulse dummy lags.

Overall, the evidence from models in first-differences indicates that our baseline results for exchange rates and foreign interest rates are generally robust to spurious correlation concerns. The results for foreign output exhibit greater sensitivity, indicating that some caution regarding the baseline estimated responses for this variable may be merited.

The final robustness exercise that we performed entailed generating the IRFs via the method of local projections, as discussed by Jorda (2005). This involves evaluating the h-period response of a variable to a monetary policy shock by means of a direct h-step forecasting regression in which the controls are the elements of the VAR information set, including the exogenous policy shock. Jorda (2005) shows that this method can yield more accurate estimates of the IRFs than the method of powering up the VAR coefficients, particularly at longer horizons and in instances in which the VAR is misspecified (incorrect number of lags). The IRFs constructed via local projections generally confirmed our baseline results, although two points of difference should be noted. First, the maximum appreciation of the U.S.$ against the German mark was three time larger than in Fig. 1, but occurred much later (after 24 months). Second, the price reductions observed in the U.S. four years after a monetary contraction were typically two to three times larger than in the baseline case. Recall that we attributed the lack of strong evidence for price declines in our baseline results to the use of 6 exogenous shock lags in the empirical model. After increasing the number of UM lags to 48, price puzzles were generally absent and significant price reductions occurred after 4 years, matching the results of Romer and Romer (2004). The fact that IRFs derived via local projections yield stronger evidence of price declines even when using 6 lags of the policy measure is consistent with Jorda (2005)'s claim that the local projection method for generating impulse responses is more robust to lag order specification than the usual method of powering up estimated VAR coefficients.

4. Commonalities across two U.S. monetary policy identification schemes

One of the main results established in the previous section was that the open economy consequences of U.S. monetary policy shocks identified by the procedure discussed in Section 2.1 are similar to the results of the partial identification developed by Faust et al. (2003), which leverages restrictions from high frequency regressions and *a priori* bounds on IRFs. In particular, the contemporaneous and maximum U.S.$ appreciations versus the German mark and British pound were within the confidence intervals established by the partial identification, as were the estimated timings of the maximum effects. As noted in Section 1, the high frequency data restrictions employed in the partial identification take the form of spot exchange rate and spot and forward interest rate responses to monetary policy shocks measured from changes in the price of the current-month federal funds futures contract around FOMC meetings.[25] Given that the estimated effects are inputs to the partial identification, one explanation for the similarities between our results and those documented by Faust et al. (2003) is that the federal funds futures-identified shock series and our identified shock series share common features. In order to explore such an idea in greater detail, we present a simple comparison of the two series at the FOMC meetings frequency.

Faust et al. (2003) calculate policy surprises from futures price data for a sample of 61 FOMC meetings that span the period March 1994–October 2001.[26] Using data purchased from the Chicago Board of Trade, we calculated the change in the closing current-month contract's price from the day prior to an FOMC meeting to the day of the meeting (trading closes at 4 p.m. Eastern Standard Time and FOMC announcements occur at 2:15 p.m. Eastern Standard Time). This series was then divided by the

[25] The current-month futures contract settles based on the average realized federal funds rate for the month. On any particular day in the month, the contract's price reflects the average federal funds rate during days of the month that have already passed and the expected rate for days remaining in the month. As such, anticipated monetary policy moves should not affect the contract price at the time of implementation. Then, price changes that do occur on FOMC days must be linked to unanticipated policy changes (see Kuttner, 2001).

[26] The sample start date is determined by the FOMC's February 1994 decision to publicly announce a target federal funds rate immediately after each meeting. Prior to this date, it is less clear exactly when news of policy intentions reached the market, and hence which movements in futures prices should be used to define policy shocks.

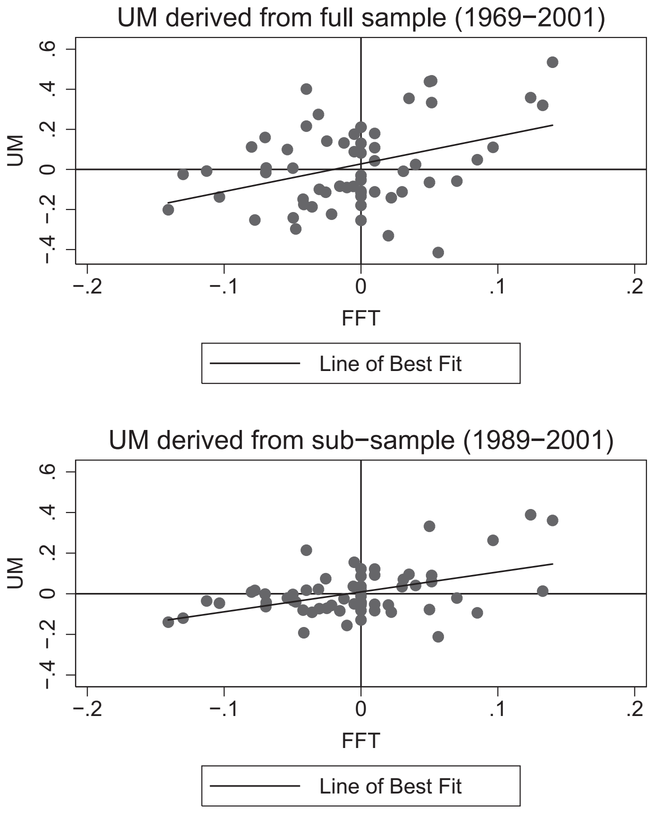

Fig. 10. Comparison of futures' market identified and UM identified US MP shocks. Note: The scatterplots use data over the period 1994m03–2001m10. Data are in percentage points. FFT denotes the federal funds futures' price identified shock and UM represents the shock derived from the narrative evidence, Greenbook forecasts, and capacity utilization measure. Details are described in the main text.

fraction of days in the month remaining, to obtain a measure of monetary shocks at the meetings frequency. The scaling controls for the fact that contracts settle according to the average federal funds rate for the month, such that a monetary shock occurring part way through the month does not exert a one-for-one effect on the contract price.[27]

In the upper panel in Fig. 10, we plot the UM shock at each meeting against the corresponding futures-identified shocks (denoted FFT). In the lower panel, we plot the same relationship except that UM is obtained through estimating Eq. (1) for the period 1989–2001 (the sub-sample for which the federal funds futures market has been in existence). The lines of best fit indicate a positive association between the two series. This is confirmed by the simple correlation statistics, which are 0.38 based on the full sample UM and 0.47 based on the sub-sample UM. According to either the UM shocks or the futures-identified shocks, the largest policy contraction during the 1994–2001 period occurred after

[27] The scaling takes on a large value towards the end of a month, magnifying any measurement errors in the contract prices (measured to the nearest basis point). We followed Faust et al. (2003) in measuring the shock from the change in the price of the next month's futures contract rather than the current-month contract whenever a meeting occurred after the 22nd of the month.

the meeting on the 15th November 1994.[28] However, there is disagreement concerning the magnitude of the shocks. The full sample UM shock is more than three times the size of the futures-identified shock. More generally, the UM shocks are of larger absolute size than the futures-identified shocks. For example, the standard deviation of UM is 21 b.p., while that of the futures-identified shocks is 6 b.p. (the standard deviation of the 1989–2001 sub-sample UM shocks is 12 b.p.). Clearly, a measure of monetary shocks derived through orthogonalizing target federal funds rate changes with respect to the Greenbook information and the capacity utilization terms retains elements of target rate changes that are excluded from a measure of shocks derived from market expectations that are implicit in futures prices. Interestingly, despite the greater variation in the UM series, there is no evidence of greater real-time predictability of the UM shocks as compared to the futures-identified shocks. In fact, a regression of either FFT or UM on their own lags for the last 12 meetings over the period 1994–2001 does not yield coefficients that are significant, either individually or in combination.

Overall, while the results from the two identifications differ concerning the magnitude of U.S. monetary policy shocks in some instances, there is general agreement concerning the direction in which exogenous policy shocks operate at different points in time. As such, it appears that market expectations embodied in the prices of futures contracts and the information contained in the Greenbook forecasts and the capacity utilization index are comparable instruments for the purpose of eliminating anticipated and endogenous changes in U.S. monetary policy.[29] When high frequency futures market data cannot be used to identify U.S. monetary policy (e.g., prior to the creation of the market for futures contracts in 1988m10), the Greenbook forecasts and the capacity utilization index can be used as alternative levers for identification.

5. Conclusion

Monetary policy's open economy consequences are difficult to estimate due to the simultaneity inherent in the relationships between asset prices, such as interest rates and exchange rates. Moreover, the influence of relevant omitted or unobserved variables (such as inflation expectations) may complicate the interpretation of any estimated results. In this paper, we have extended the U.S. monetary policy identification strategy pioneered by Romer and Romer (2004) to overcome such obstacles to the estimation of the open economy consequences of U.S. monetary policy. The approach leverages narrative evidence, the Federal Reserve's real-time information (as captured by the Greenbook forecasts), and a proxy for latent policymaker perceptions of the economy's cyclical position (the capacity utilization index), to achieve identification. If these variables are predetermined, and adequately summarize the Federal Reserve's information set regarding its objectives and expectations, then the approach is able to recover the unanticipated and exogenous component of U.S. monetary policy. In particular, we require that open economy variables influence the Federal Reserve's monetary policy decisions only through their effects on expectations of the Federal Reserve's objectives (such as output growth, inflation, unemployment, and the output gap).

We included the identified U.S. monetary policy shock as an exogenous variable in a set of 6 bilateral monthly VARs, where the foreign country is one of the non-U.S. G7 members. After a contractionary U.S. monetary policy shock, we found that the U.S.$ exchange rate always appreciates, achieving a maximum appreciation within 1–20 months. Specifically, the results indicated that a one percentage point rise in the U.S. t-bill rate (from the identified shock) induces a contemporaneous U.S.$ appreciation which exceeds one percentage point. The U.S.$ appreciation continues for some months, creating large UIP deviations over a 6 month horizon. There is strong, positive interest rate pass-through from the U.S. to the foreign countries, with the maximum lying between 0.46 and 1.24 and occurring within 3–10 months. Foreign output responses are uniformly negative at horizons of 16

[28] A reading of the post-meeting statement by FOMC member Donald Kohn indicates that at the time the committee regarded a 50 b.p. increase as warranted in light of economic conditions, but that they implemented a 75 b.p. hike to preempt any need for further tightening at future meetings. There was a concern that such future tightening might have led to criticism that the FOMC had not been forceful enough in making decisions at previous meetings.

[29] As discussed earlier, for the post-1994 period, narrative evidence plays a tiny role in the identification since federal funds target changes are announced.

months, possibly indicating that the expenditure-reducing effects of the U.S. policy contraction dominate any expenditure-switching effects.

We also considered the robustness of Romer and Romer (2004)'s estimated effects of monetary policy on U.S. domestic variables. The response shapes were generally similar. However, the estimated response magnitudes were smaller, likely reflecting the greater number of conditioning variables in our estimated models. We also found that the U.S. price response was sensitive to the number of exogenous policy lags included in the model.

Our results are consistent with those reported in recent work by Faust et al. (2003) for the responses of Germany and the U.K. They partially identify U.S. monetary policy's effects via a combination of restrictions derived from high frequency regressions and *a priori* impulse response bounds. We compared our identified shocks with U.S. monetary policy shocks derived from the federal funds futures market, which is an important component of Faust et al.'s partial identification approach. There appears to be considerable overlap between the identifying information leveraged across the two identifications schemes, leading to the similarity of results. However, as evidenced by the larger set of countries we consider, our approach is more readily implementable. Moreover, it does not require that *a priori* bounds on the impulse responses be established, allowing the estimated responses to be unrestricted. Accordingly, applications of our identification approach should be relatively straightforward, enabling the open economy consequences of U.S. monetary policy to be explored for a wide variety of countries.

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Appendix. Supplementary table

Table A.1
Data sources.

Variable	Source
Bilateral dollar exchange rates	Board of Governors of the Federal Reserve System, http://federalreserve.gov/releases/h10/Hist/
Treasury-bill rates	International Financial Statistics, line 60C … ZF and line 60CS … ZF (UK only)
Consumer prices	International Financial Statistics, line 64 … ZF
Industrial production	International Financial Statistics, line 66 … CZF
Non-borrowed bank reserves plus extended credit	Board of Governors of the Federal Reserve System, http://federalreserve.gov/releases/h3/hist/h3hist1.htm
Total bank reserves	Board of Governors of the Federal Reserve System, http://federalreserve.gov/releases/h3/hist/h3hist1.htm
Federal funds rate target changes identified from narrative evidence	AER data archive, http://www.e-aer.org/data/sept04_data_romer.zip
Announced federal funds rate target changes (post-1996)	Board of Governors of the Federal Reserve System, http://federalreserve.gov/fomc/fundsrate.htm
Greenbook forecasts	The Federal Reserve Bank of Philadelphia, http://philadelphiafed.org/econ/forecast/
Capacity utilization index	The Federal Reserve Bank of St. Louis, http://research.stlouisfed.org/fred2/series/TCU/

Table A.2
Residual autocorrelation tests.

Germany		*United Kingdom*	
Equation	AR (1–8)	Equation	AR (1–8)
s	0.297	s	0.714
i	0.000	i	0.000
i^*	0.159	i^*	0.128
y^*	0.281	y^*	0.042
NBRX	0.000	*NBRX*	0.000
y	0.297	y	0.583
p	0.281	p	0.016
Canada		*France*	
Equation	AR (1–8)	Equation	AR (1–8)
s	0.637	s	0.201
i	0.000	i	0.000
i^*	0.001	i^*	0.052
y^*	0.111	y^*	0.943
NBRX	0.000	*NBRX*	0.000
y	0.254	y	0.083
p	0.006	p	0.099
Italy		*Japan*	
Equation	AR (1–8)	Equation	AR (1–8)
s	0.160	s	0.689
i	0.000	i	0.000
i^*	0.272	i^*	0.639
y^*	0.021	y^*	0.087
NBRX	0.000	*NBRX*	0.000
y	0.044	y	0.721
p	0.244	p	0.041

Notes: The figures reported are p-values from an F-test of the joint hypothesis that the errors are serially uncorrelated at lags 1–8.

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