



BANK OF ENGLAND

External MPC Unit

Discussion Paper No. 42

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April 2014



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Abstract

We examine the impact of large-scale asset purchases of government bonds on real GDP and the CPI in the United Kingdom and the United States with a Bayesian VAR, estimated on monthly data from 2009 M3 to 2013 M5. We identify an asset purchase shock with sign and zero restrictions. In contrast to the impulse response analysis in previous work, the reactions of real GDP and CPI are left unrestricted, so as formally to test whether these variables are affected by asset purchases. We then explore the transmission channels to the domestic economy and emerging markets. Our results suggest that asset purchases have a statistically significant effect on real GDP with a purchase of 1% of GDP leading to a .36% (.18%) rise in real GDP and a .38% (.3%) rise in CPI for the United States (United Kingdom). In the United States, this policy lowers yields on long-term government bonds and the real exchange rate. In the United Kingdom, on other hand, interest rate futures and measures of financial market uncertainty are more affected. There is also some evidence that emerging market sovereign bond and corporate bond spreads decline, with industrial production rising in response a positive asset purchase shock in either country.

Key words: Unconventional monetary policy, Bayesian VAR, Hierarchical prior, Litterman prior.

JEL classification: E50, E51, E52.

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Information on the External MPC Unit Discussion Papers can be found at www.bankofengland.co.uk/research/Pages/externalmpcpapers/default.aspx

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I. Introduction

After policy rates fell close to zero in response to the global financial crisis of 2008-2009, the scope for further conventional monetary policy easing was exhausted. As a result both the Bank of England and the Federal Reserve embarked on large scale asset purchases (LSAPs) of government and financial securities. This paper explores the impact of asset purchases on the economies of the United Kingdom and the United States. In contrast to earlier work, it uses only data series since the programmes of asset purchases began in early 2009, and it adopts identification schemes which make no prior assumptions about the effect of the policy on output and inflation.

Studies that examine the impact of unconventional monetary policy on the wider macro-economy typically adopt Bayesian VAR methods or use structural macroeconomic models. An example of the latter is Chung et al. (2012), which uses the Federal Reserve Board's US macroeconomic model to examine the possible impact of US LSAPs and finds that real GDP and inflation were respectively three and one percent higher as a result the Federal Reserve's asset purchase policy.

Our paper is more closely associated with the Bayesian VAR literature. The first study that examines the macroeconomic impact of LSAPs in the UK and the US with this approach is Baumeister and Benati (2013). Based on the argument that LSAPs in these two countries were designed to lower long rates once short rates could not be reduced further, they identify a 'pure' spread shock, which compresses the spread between the long and the short rate, while leaving the short rate unchanged. The identification assumptions they impose in their impulse response analysis are that the spread does not increase, output and prices do not fall and that the central bank's policy rate does not react upon impact. This last identification assumption is justified by the fact that, in the relevant circumstances, policy rates cannot be reduced further and are, in effect, constant. They find that the spread shock had a significant impact on output growth and inflation in the US and the UK. With a focus on the UK, Kapetanios, Mumtaz, Stevens and Theodoris (2012) adopt the same identification scheme and use three different models to examine the impact of QE: a large Bayesian VAR with a rolling window, a change-point (Markov-Switching) parameter VAR and a Bayesian time-varying parameter VAR. All of their models suggest that the first UK LSAP (also known as QE1), via their effect on the spread, had a significant effect on output and inflation.

Both of these two papers are subject to three possible criticisms. First, they both identify a spread shock, but the extent to which this type of shock actually reflects asset purchases is not clear. Secondly, as part of their identification scheme, they assume that output and prices react to their unconventional policy shock, rather than testing whether these variables actually react to asset purchases. Finally, they rely on time-varying parameter models with slowly evolving coefficients, meaning that the results could be partially affected by pre-crisis data, since it is unclear to what extent such models will pick up either the structural change likely to have taken place during the recessions and associated with the onset of the 'Great Recession' or the change in the conduct of monetary policy. But it is important to point out that these choices were made mainly out of necessity. When these studies were undertaken, time series on unconventional monetary policy, real activity and prices were still short.

In contrast, we estimate our model on monthly data from 2009m3, when asset purchases started, to 2013m5. Our results are therefore based entirely on the period in which asset purchases took place, and, importantly, after the various government interventions in the banking system in both countries. There is, of course, a good reason for the lack of previous work in this area and that is the short time series associated with the chosen sample period. We address this problem by adopting Bayesian methods of inference and exploit two different priors, which have been specifically developed for this case: the Litterman prior (1986) assumes that persistent variables follow a random walk, while the hierarchical panel VAR prior, proposed in Jarocinski (2010), assumes that both countries have the same mean in their autoregressive parameters. A second advantage of this framework is that we can examine larger VAR models, which means that we are also able to study the impact of asset purchase shocks on other domestic and international variables as well. Thus, we identify an asset purchase shock, in an attempt to ensure that the shock indeed reflects asset purchases. The credible identification of such shocks is not an easy task and we therefore use zero restrictions, sign restrictions and a combination of the two for this purpose. In the first identification scheme, based on zero restrictions only, the main identification assumptions are that prices and output react with a lag and that asset purchases react to output and prices, but to no other shock contemporaneously. In the second and third identification schemes, we assume the presence of either signalling or portfolio rebalancing effects, and use them to identify asset purchase shocks.

Regardless of identification scheme, we leave the response of output and prices always unrestricted, to test whether these variables react to asset purchase shocks.

Economic theory suggests that asset purchase policy can affect the macroeconomy through various channels. Bauer and Rudebusch (2011) argue that announcements of asset purchases signal that short-term interest rates will stay lower for longer and interest rates at short maturity will be affected most, since monetary authorities cannot credibly commit to keep interest rates low too far into the future (Krishnamurthy and Vissing-Jorgensen, 2010). On the other hand, the portfolio rebalancing channel, based on the presence of preferred habitat investors, suggests that either yields close to those maturities that are actually purchased or those with the highest interest rate risk should be affected most through the impact on scarcity and duration, respectively. We explore the empirical relevance of these channels, by including futures of the three month interest rate one year, two years and three years ahead, as an additional variable in the VAR, one variable at a time. Similarly, we examine the impact on the 5-year, 20-year and 30-year yields on government bonds. To the extent that portfolio rebalancing may lead to an effect on other asset classes, we also study the impact on corporate bond yields, the real exchange rate and real house prices. A final transmission channel of unconventional monetary policy is that it may reduce uncertainty about the future path of interest rates (Key, Weale and Wieladek, 2014) and the economy in general, which is why we also examine the impact on the VIX and a weighted average of implied interest rate futures' volatilities. Finally, we also study the impact on corporate and sovereign bond spreads, real share prices, capital flows and industrial production in emerging market economies, to examine to which extent these countries have been affected by asset purchase policy in the UK and the US.

The work closest to ours is Gambacorta, Hoffman and Peersman (2013), who examine the macroeconomic impact of central bank balance sheet expansion in 8 countries for the period 2008m1 to 2011M6. Their paper is focused on the impact of all of the types of central bank policies, which led to central bank balance sheet expansion. They pool their data and assess how these policies affected output and prices. To avoid dynamic heterogeneity bias, they use the mean group estimator to estimate their panel VAR. Our paper differs from theirs in several important dimensions. First, we focus solely on asset purchases after the global financial crisis, as different policies may have different macroeconomic effects and it is unclear whether pooling across countries is appropriate.

This criticism could, of course, also be applied to asset purchases in the UK and the US. It is for this reason that we focus on purchases of government bonds¹, though both of our proposed estimators allow for country-specific heterogeneity in the transmission mechanism. Furthermore, Rebucci (2003) demonstrates that when applied to panel VAR estimation, the mean group estimator is biased when the time-series dimension as large as fifty, as in their paper. Our Bayesian hierarchical estimator is unbiased even when the time-series dimension is short (Hsiao, Pesaran and Tahmiscioglu, 1999). Finally, when they identify their ‘unconventional’ monetary policy shock, they also restrict output and prices to be zero upon impact. In contrast, given the inherent uncertainty about the impact of asset purchase shocks on these variables, we choose to leave them unrestricted.

It is well known that small VARs are subject to omitted variable bias problems. Identified asset purchase could therefore be merely a reflection of other coincident economic developments, such as domestic fiscal policy, the Euro Area crisis, real oil prices or the ECB’s monetary policy actions. We show that our reported estimates are robust to including each of these variables as additional controls in our VARs. Furthermore, unlike interest rate policy, for which the announcement coincides with implementation, asset purchases were announced before they were made. We therefore show that our base-line results are robust to using the actual amounts purchased rather than announcements. The nature of asset purchases in the UK makes it easy to generate a time series of announcements. But this is more complicated for the US, where the Federal Reserve purchased a variety of assets, including long-term government bonds and mortgage-backed securities (MBS), extended the maturity² of its balance sheet maturity and announced open-ended purchases. In our basic specification, we treat maturity extension (‘Operation Twist’) in the same way as asset purchase announcements. Our results are robust to putting a smaller weight on ‘Operation Twist’, and adding the present value of open-ended government bond and MBS purchase announcements to the asset purchase series.

The main question that we want to answer in this paper is whether asset purchases have an impact on real GDP and prices. Our results suggest that, at the median, an asset purchase shock that results in an announcement worth 1% of nominal GDP, leads a rise

¹ Our results are robust to including purchases of mortgage backed securities as well.

² The Federal Reserve sold treasury securities at the short-end, to treasury securities at the long-end, of the yield curve, while keeping the size of its balance sheet constant. We refer to this as an extension in the maturity of the Federal Reserve’s balance sheet.

of about .36% (.18%) of real GDP and .38% (.3%) in CPI in the US (UK). These findings are encouraging, because they suggest that asset purchases can be effective in stabilising output and prices. The implied UK Phillips curve is steeper than in the US, meaning that the same change in output would have a relatively greater impact on UK inflation. For real GDP, our calculations suggest that these figures are similar to what Baumeister and Benati (2013) and Kapetanios et al (2012) report for their UK and US real GDP responses to spread shocks. For the US, we also find a similar effect the on CPI, but for the UK, our results suggest that the impact on the CPI is more than twice as large as the effect reported in Baumeister and Benati (2013) and Kapetanios et al (2012). For the UK, asset purchase announcements have an impact on interest rate futures in the UK and measures of financial market uncertainty, suggesting that ‘signalling’ is an important transmission channel. For the US, only long-term yields and the real exchange rate react to asset purchase shocks, which implies a relatively greater role for the portfolio rebalancing channel. We do not find an impact on capital flows to emerging markets, but there is an effect on sovereign and corporate spreads, as well as industrial production in those countries. One potential explanation for this pattern is that UK and US asset purchase policy stabilised economic conditions in the target export markets of these countries. If this is the correct explanation, then one would not necessarily expect a negative spillover effect on emerging market economies from UK and US asset sales, so long these are accompanied by economic growth in these countries.

The remainder of this paper proceeds as follows. Section two discusses the methodology behind Bayesian VAR models and the details of our identification schemes. Section three presents the results and section four concludes.

2. Methodology and data

The VAR³ model we propose is the following:

$$Y_{c,t} = \alpha_c + \sum_{k=1}^L A_{c,k} Y_{c,t-k} + e_{c,t} \quad e_{c,t} \sim N(\mathbf{0}, \Sigma_c) \quad (1)$$

where $Y_{c,t}$ is a vector of the following endogenous variables: the announcement of asset purchases scaled by nominal GDP; the log of CPI; the log of real GDP; the yield on the

³ The description of most the components of our proposed model closely follows the presentation of Jarocinski (2010). See his work for more details on the remaining parts of the model.

10-year government bond and the log of real equity prices for country c , with the total number of countries C , at time t . $A_{c,k}$ is the array of coefficients associated with the corresponding lagged vector of variables for lag k and country c . $e_{c,t}$ is a vector of residuals for country c at time t . This is assumed to be normally distributed with variance-covariance matrix Σ_c . When the time-series dimension is small, estimates of $A_{c,k}$ are likely to be imprecise. While one way of addressing this problem is to include pre 2009m3 data, that would carry the risks that our estimates could be biased by coincidence of the various government interventions in the banking system in response to the global financial crisis. Instead, we follow previous work and rely on Bayesian methods of inference to address the sample size issue with two alternative prior assumptions: The Litterman (1986) prior⁴ and the panel VAR hierarchical prior. The former imposes the prior assumption that non-stationary variables follow a random walk, while the latter imposes the prior of a common mean across countries. To ensure robustness across prior assumptions, we estimate our model subject to each prior separately. We assume a lag length, L , of two throughout.⁵

2.1 The Litterman and panel VAR prior

In general, prior beliefs on VAR coefficients can be expressed as

$$E[(A_{ij,c,k})] = \delta_{i,j,c,k} \quad V[(A_{ij,c,k})] = \left\{ v \frac{\lambda^2 \sigma_{i,c}^2}{k^2 \sigma_j^2} \right\} \quad (2)$$

$\delta_{i,j}$ is the prior mean for the VAR coefficient in row i , column j in country c at lag length k . $v \frac{\lambda^2 \sigma_{i,c}^2}{k^2 \sigma_j^2}$ is the corresponding prior variance. A smaller prior variance means that larger weight is put on the prior relative to the data. The values of this variance, v , σ_i^2 , λ , σ_j^2 and k are typically calibrated. Following the approach set out in Kadiyala and Karlsson (1997), v is typically set to unity, as this allows researchers to relax the assumption of the

⁴ Indeed, the observation to parameter ratio for each VAR equation in our application of the Litterman prior (1986) is 49/11, while Leeper, Sims and Zha (1996) and Banbura, Giannone and Reichlin (2010) use ratios of 423/ 260 and 1703/492, respectively, which is considerably smaller than in our application.

⁵ Ex-ante lag length tests such as the Hanan-Quin or BIC criterion suggest a lag length of 2. Similarly, if the VAR is estimated with the correct lag length, the residuals should follow a white-noise process and autocorrelation tests on the residuals of each equation of the VAR suggests that this is the case.

diagonal variance-covariance that is typically embedded in the standard Litterman prior (1986). λ is the key parameter determining the tightness of the prior. If $\lambda = 0$, then the posterior coefficient estimate of $A_{ij,c,k}$ from this model will coincide with the prior, $\delta_{i,j}$. On the other hand, if $\lambda \rightarrow \infty$, the prior structure is not binding and the posterior estimate will coincide with the OLS estimate. The parameterisation of $V[(A_{ij,c,k})]$ has the convenient property that the degree of tightness can be summarised in one parameter, λ . But this comes with one drawback: the coefficients in $A_{c,k}$ may have different magnitudes. In specifying a single parameter that determines the degree of tightness, there is therefore the risk that some coefficients are allowed to differ from the prior by a small fraction of their own size, while others can differ by orders of magnitude. Following Jarocinski (2010) and an analogous procedure for the Litterman (1986) prior, we use the ratio $\frac{\sigma_{i,c}^2}{\sigma_{j,c}^2}$, as a scaling factor for each coefficient, where c is the country, i the equation and j the number of the variable regardless of lag. $\sigma_{i,c}^2$ is the estimated variance of the residuals of an auto-regression for the endogenous variable in equation i , of the same order as the VAR, and is obtained pre-estimation. $\sigma_{j,c}^2$ is the corresponding variance for variable j and obtained in an identical manner. To the extent that unexpected movements in variables will reflect the difference in the size of VAR coefficients, scaling by this ratio of variances allows us to address this issue.

The first prior that we explore within this framework is, the by now standard, Litterman (1986) prior. In his original paper, Litterman (1986) sets $\delta_{i,j,c,k} = 1$ if $k = 1$ and $j = i$ for all of the variables, assuming that they all behave like a random walk. But Banbura, Gianonne and Reichlin (2010) argue that this is not appropriate for stationary variables, for which they suggest setting $\delta_{i,j,c,k} = 0$, which is the prescription that we follow here. Typically, the value of λ is set to a small number, reflecting the researchers' belief that the prior reflects the properties of the data. The results may depend on the value of λ and it is uncertain what the right value of this parameter for a given VAR model is. Previous work has suggested two different ways to estimate λ . Banbura, Giannone and Reichlin (2010) solve numerically for the value of λ that provides the smallest root mean square

forecast errors. Most recently, Primiceri, Giannone and Lenza (2013) propose treating this parameter as a hierarchical parameter within the VAR model and show how to estimate it by maximising the likelihood function of this model. This is indeed the approach that we follow here. We first use their approach to estimate the λ associated with the highest likelihood and then in a second step use the dummy variable approach presented in Banbura, Giannone and Reichlin (2010) to implement this model.

When faced with short time series, the strategy typically adopted by researchers is to pool data across countries to improve statistical inference. However, in the presence of country fixed effects, estimates of lagged dependent variable models can be biased, either due to the small dimension of the time series (Nickell, 1981) or the presence of different dynamics in different countries (Pesaran and Smith, 1995). Pesaran and Smith (1995) propose the mean group estimator for this purpose, which relies on estimating the model in question country by country and subsequently averaging across countries to rid the pooled estimate of the bias. Indeed, this is the approach followed by Hoffman, Peersman and Gambacorta (2013) in their examination of unconventional monetary policy. However, Rebucci (2003) shows that VARs estimated in this manner display substantial bias in the dynamics, even if the time-series dimension is fifty. Hsiao, Pesaran and Tahmiscioglu (1999) show, for the case of an auto-regressive process, that the Bayesian hierarchical panel estimator is unbiased even when the time-series dimension is short. In recent work, Jarocinski (2010) extended this estimator to a panel VAR setup. The prior in (2), is then set such that $\delta_{i,j,c,k} = \bar{A}_{ij,k}$, reflecting the assumption that the coefficients are centred around a common mean. In this case, λ reflects the degree of dynamic heterogeneity, with a high value of λ resulting in country-by-country estimation, while a value of zero will result in pooling. We follow the approach presented in Jarocinski (2010) to the letter and treat λ as a hyperparameter, which is estimated from the data. We implement this model with his Gibbs sampling approach by taking 1,100,000 draws from the posterior, discarding the first 100,000 as burn-in, and retaining every 1000th draw for inference.

2.2 Identification

The challenge for structural VAR models is to disentangle orthogonal, structural economic shocks, $\boldsymbol{\varepsilon}_{c,t}$, from the correlated reduced form shocks $\boldsymbol{e}_{c,t}$. This is typically achieved with the help of a matrix \boldsymbol{C}_0 , such that $\boldsymbol{C}_0\boldsymbol{e}_{c,t} = \boldsymbol{\varepsilon}_{c,t}$. As discussed below, we recover \boldsymbol{C}_0 either with short-run zero restrictions, short-run sign restrictions or a combination of the two.

In our first identification scheme, we identify asset purchase shocks with a lower-triangular scheme, with asset purchases ordered after real GDP and prices, but before all of the other variables. The identifying assumptions are therefore that output and prices react with a lag and that aside from responding to these two, asset purchases do not react to any other variable upon impact. Given that our data are monthly, we argue that this is not an unreasonable identification assumption. If the identification assumptions are correct, then this approach will be useful, not only for testing for the statistical significance of output and prices, but also to examine the reaction of long rates and real stock prices. This identification scheme is referred to as ‘Identification Scheme – I’ for the rest of the paper.

On the other hand, VAR identification schemes that employ timing exclusion restrictions have been criticised in recent years, based on the grounds that such restrictions do not naturally emerge from DSGE models. Canova and De Nicrolo (2002), Faust and Rogers (2003) and Uhlig (2005) have therefore proposed identifying shocks based on the implied signs of the impulse responses that they produce. For example, researchers that use this approach typically identify an expansionary monetary policy shock by assuming that it leads to an expansion of output, a rise in the price level and a fall in the short rate. Clearly, for identification restrictions of this type to be valid, they need to be strongly supported by economic theory. In standard DSGE models, quantities of asset purchases do not matter. However, in the presence of financial frictions, such as the imperfect substitutability between long and short bonds (Harrison, 2012) or preferred habitat investors (Vayanos and Villa, 2009), economic theory does suggest that a rise in asset purchases will lead to a fall in the interest rate on longer maturity bonds. This is the first identification assumption that we need to make. Secondly, lower yields on longer maturity bonds are likely to lead to some portfolio balancing towards other assets, such as

equities, leading to a rise in real equity prices. In other words, our definition of an asset purchase shock is that it leads to lower yields on long-term government bonds and a rise in equity prices. The other shocks that we identify are an aggregate demand shock, which would typically lead to a rise in prices and output. The rise in prices, together with the fact that firms may require greater finance for production, is likely to lead to a non-negative response of the long interest rate. The rise in demand would also lead to a rise in real share prices. The sign restrictions we use to identify an aggregate supply shock are identical, other than assuming that prices fall instead. This identification scheme, which we refer to as scheme II throughout the paper, is summarised in Table 1 and implemented with the QR approach presented in Rubio-Ramirez, Waggoner and Zha (2010).

Table 1 – Identification scheme II

	y	p	AP	i_t	sp_t
	Log real GDP	Log CPI	Asset Purchases	Long interest rate	Log real Share Price
Supply Shock	≥ 0	$0 \leq$		≥ 0	≥ 0
Demand Shock	≥ 0	≥ 0		≥ 0	≥ 0
Asset Purchase Shock	?	?	≥ 0	$0 \leq$	≥ 0

In identification scheme II, the assumption is that asset purchases affect the real economy via portfolio rebalancing from long-term government bonds into shares to distinguish them from aggregate supply and aggregate demand shocks. But, it is not clear to what extent the financial frictions that are required for asset purchases to affect the yield on long-term government debt hold in reality.

Table 2 – Identification scheme III

	y	p	AP	i_t	sp_t
	Log real GDP	Log CPI	Asset Purchases	Long interest rate	Log Real Share Price
Supply Shock	≥ 0	$0 \leq$	0		
Demand Shock	≥ 0	≥ 0	0		
Asset Purchase Shock	?	?	≥ 0		≥ 0
Uncertainty shock			≥ 0		$0 \leq$

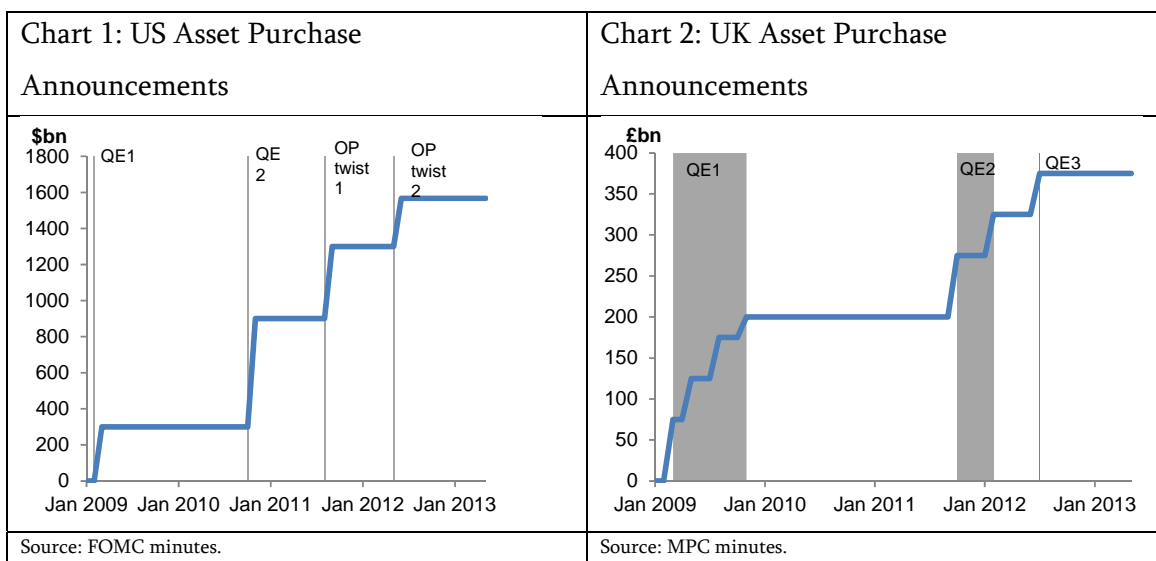
An alternative school of thought suggests that the main effect of asset purchases comes from the signal they send about short-term yields, namely that they are not about to rise any time soon (Bauer and Rudebusch, 2011). In that case real share prices would still react, but it is not clear whether long-term interest rates would. Indeed, it is possible to drop the restriction on long-term interest rates, as long as one is willing to make the assumption that asset purchases do not react contemporaneously to aggregate demand and aggregate supply shocks. In that case, the restriction on real share prices is sufficient to distinguish these shocks from asset purchases. Given the high frequency of our data, the assumption of a zero contemporaneous reaction of asset purchases to aggregate demand and supply shocks is not unrealistic. An additional advantage is that this allows us to identify a fourth shock, namely a rise in uncertainty/risk premia. This shock is identified as a decline in real equity prices, to which the monetary policy authority reacts with a rise in asset purchases. This identification scheme is summarised in table 2 and referred to as Identification scheme III throughout. This last identification scheme is implemented with the procedure proposed in Arias, Rubio-Ramirez and Waggoner (2013), who generalise the standard QR restrictions algorithm to include zero restrictions as well.

2.4 Data

All of the VAR models in this paper are estimated on monthly data for the period when asset purchases were an active policy of both the UK and the US, from 2009m3 to 2013m5. Monthly real GDP for the UK are provided by the National Institute of Economic and Social Research (Mitchell, Smith, Weale, Wright and Salazar, 2005), while monthly real GDP data for the US are taken from Macroeconomic Advisers. Monthly CPI data are obtained from the ONS and the BEA, for the UK and US respectively. Real equity prices are calculated by obtaining monthly averages of daily data for the FTSE100 and S&P500 obtained from Thomson DataStream and deflating by the CPI.

The asset purchase announcement series are constructed in the following manner: For the UK, we just cumulate the announcement of asset purchases over time. For the US, we treat asset purchases associated with the maturity extension programme (also known as Operation Twist) as additional asset purchases, attaching the same weight to them as asset purchases of government bonds that were financed with the issue of central bank reserves. It is of course possible that the weight attached to them should be smaller, and

the effect of that is explored in the robustness section. Both of the asset purchase series we use are shown in charts 1 and 2 below. Unlike the UK, the US also announced open-ended asset purchases. The effects of these are explored further in the robustness section. The stock of actual assets purchased for the UK has been kindly provided by the Bank of England for the purposes of this investigation. For the US, these data are taken from the Federal Reserve Bank of St. Louis Data Archive (FRED).



We also examine to which extent asset purchases had an impact on other macroeconomic variables, namely for yields on 5, 20 and 30 year government bonds, as well as Overnight Index Swap (OIS) futures of the 3 month interest rate, one year, two years and three years ahead for both the UK and the US. The other macroeconomic variables on which we examine the impact of our identified asset purchase shocks are: the VIX (implied stock market volatility), the MOVE (weighted average of implied interest rate volatilities at different horizons), corporate bond yields, the real exchange rate and real house prices.

Finally, we investigate whether emerging market economy variables react to asset purchases in the US and the UK. For this exercise, we study the impact on the monthly average of sovereign (EMBIG) and corporate (CEMBIG) spreads, taken from JP Morgan, and the monthly average of MSCI Emerging market share prices, expressed in either US dollars or Pound Sterling, and deflated by the US and UK CPI, respectively. To assess the impact on capital flows, we use either flows into emerging market economy mutual funds

provided by EPFR in Dollars or the sum of monthly total flows, constructed as the sum of the trade balance and the change in international reserves for the largest emerging market economies: Brazil, China, India, Indonesia, Mexico, South Africa and Turkey. Both of these variables are expressed as ratios of nominal GDP in 2007 for these countries. The industrial production variable for emerging markets is a GDP-weighted average for these countries. More details can be found in appendix A.

3. Results

3.1 Main results

Figure 1 shows the results for the United States. Each row shows impulse responses based on a different VAR specification. The specifications differ in identification scheme and type of prior used in the estimation. For example, 'Litterman – I', refers to a specification where the model was estimated for the United States only with the Litterman prior and identification scheme I imposed. Similarly, 'Panel-III' refers to a specification with the panel VAR prior and identification scheme III imposed. An inspection of figure 1 clearly suggests that regardless of identification scheme, real GDP and the CPI always rise in response to an asset purchase shock. This effect is statistically significant throughout, except for specification 'Panel – I' for the CPI. The maximum effects on real GDP and CPI, as well as the extent to which that is statistically significant, are reported in Table 3. An average of them suggests that quantitatively, real GDP and CPI rise by about .36% and .38% following a 1% rise in the ratio of assets purchased to GDP.

Table 3 – Maximum effect on Real GDP and CPI

Model/ Variable	Litterman I	Litterman II	Litterman III	Panel I	Panel II	Panel III	Average across models
GDP (US)	0.23**	0.56**	0.47**	0.10*	0.49**	0.28*	.36
GDP (UK)	0.06*	0.26*	0.14*	0.08*	0.34**	0.21**	.18
CPI (US)	0.25**	0.67**	0.57**	0.02	0.45*	0.31*	.38
CPI (UK)	0.01	0.61*	0.31*	0.06*	0.45*	0.39**	.30
CPIexVAT (UK)	0.02	0.67**	0.41**	0.09*	0.43**	0.41**	0.34

Note: Table shows maximum median effect on real GDP and CPI for the US/UK. Each column shows results from a different estimator and identification scheme. For example, 'Litterman I' means that the model was estimated subject to the Litterman prior and the asset purchase shock identified with a Choleski decomposition scheme. Each column shows the effect on a different economic time series, either real GDP or CPI for the US and the UK. The final column shows the impact on the UK CPI with the impact of VAT excluded from the time series. **/* signifies statistical significance at 90 / 68 quantile bands.

Figure 2 repeats the same exercise for the UK. As before, regardless of estimator or identification scheme, real GDP and the CPI show statistically significant and positive responses to asset purchase shocks, with the exception of specification 'Panel – I' for the CPI. Table 3 shows that, a 1% rise in the ratio of assets purchased to GDP raises the level of real GDP by about .18% and the price level by about .3%. In the UK, the government first lowered and then subsequently raised VAT during this time period. To ensure that our CPI estimates are not contaminated by the mechanical impact of this fiscal policy, we repeated our analysis with a UK CPI series that excludes the impact of VAT. The final row of table 3 shows that the average CPI impact is even higher in this case.

Figure 1: Results for USA

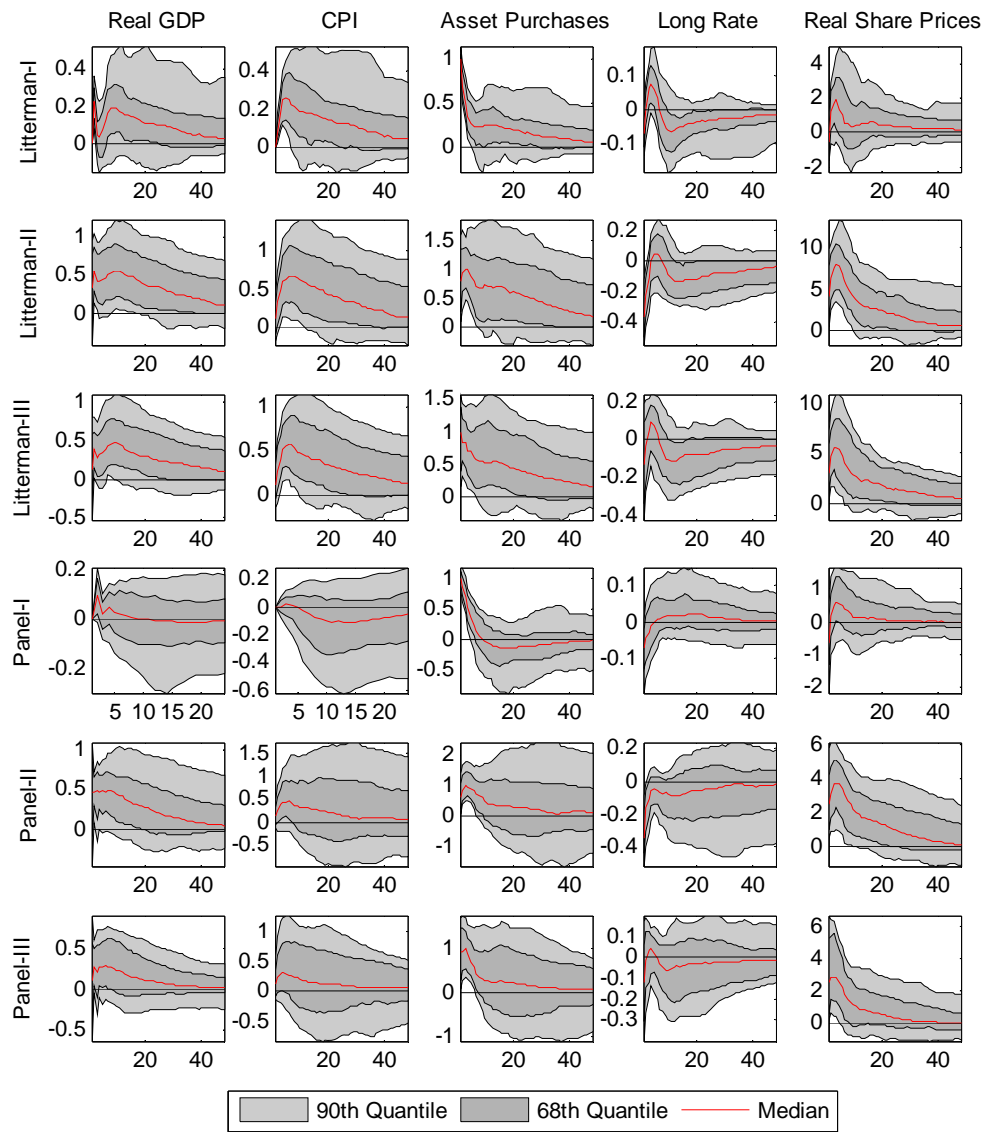
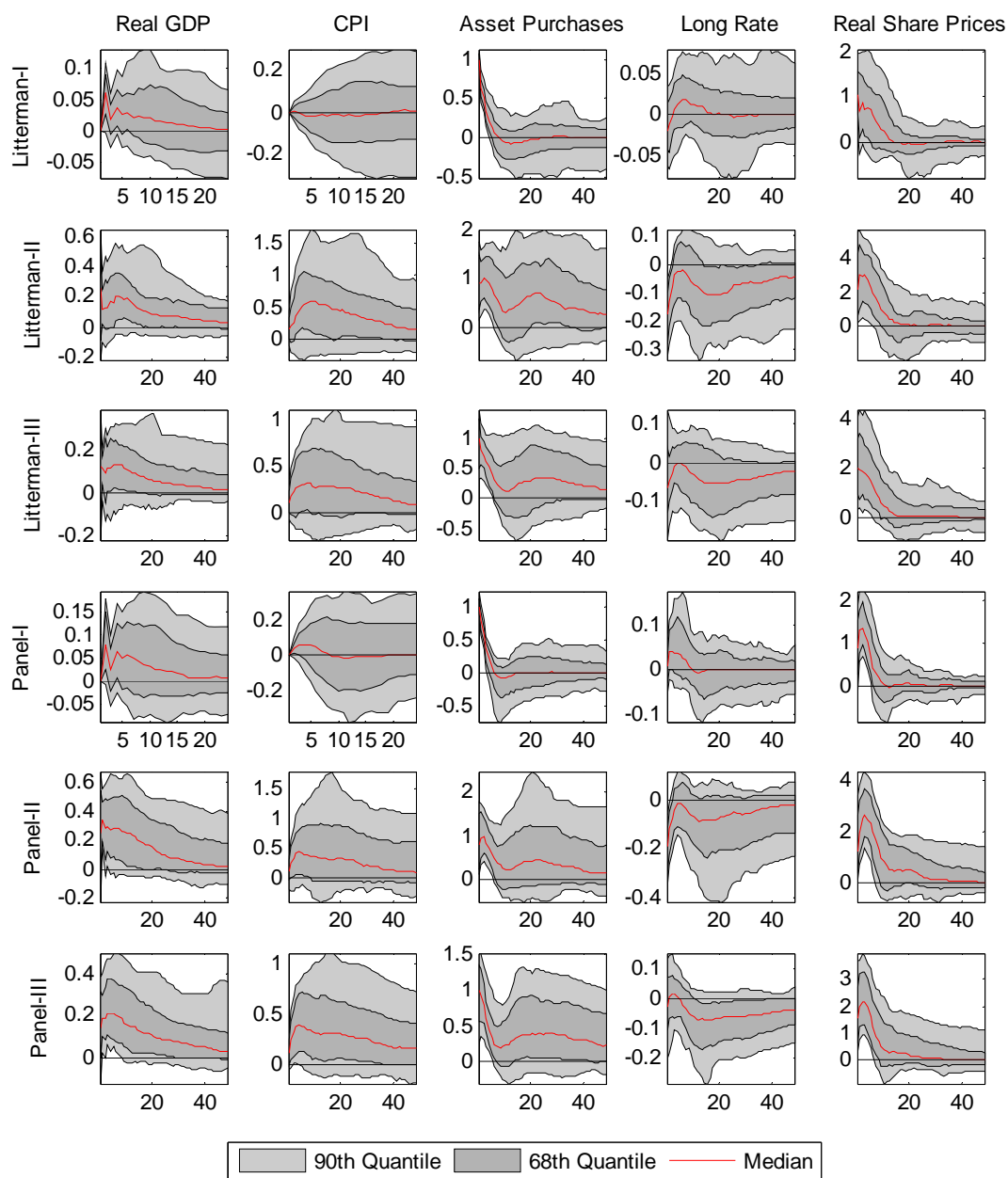


Figure 2: Results for UK



Previous studies that use BVAR methods to examine the impact of unconventional monetary policy on real GDP and CPI are Baumeister and Benati (2013) and Mumtaz, Kapetanios, Stevens and Theodoris (2012). In their impulse response analysis, both of these studies identify a ‘spread shock’ which leads to a decline in the long-term (10-year) yield on government bonds, does not affect the short-term interest rate, and leads to a rise

in output and inflation and use a time-varying parameter and a Markov-switching VAR, respectively, for that purpose. Baumeister and Benati (2013) find that a 100 basis points decline in the spread shock leads to rise of about 1.8 (1.8) percent and 1.5 (1.4) percent in UK (US) output and inflation. The impulse responses for the ultimate MS-VAR regime, which most closely corresponds to our VAR sample period, presented in Kapetanios et al (2012) imply that the same size spread shock would lead to a rise of 2.5% in real GDP and 1.5% in CPI inflation.

In contrast to these papers, we choose to leave the reaction of output and CPI unrestricted in each identification scheme for one simple reason: Unlike the case of conventional monetary policy, it is still uncertain, both empirically and theoretically, whether real GDP and the CPI should react to asset purchase policy or not. The fact that asset purchase policy has a stastically significant impact on real GDP and, most of the time, on the CPI regardless of identification scheme, despite identification restrictions weaker than in previous work, suggests that asset purchase policy can indeed be an effective monetary policy instrument.

Table 4 – Comparison of estimated impact of QE1 across various studies

Study/ Variable	Baumeister and Benati (2013)	Kapetanios, Mumtaz, Stevens and Theodoris (2012)	Weale and Wieladek
Real GDP (US)	1.08		.72 (1.61**)
Real GDP (UK)	1.8	2.5	2.52
CPI (US)	.84		.76 (1.12**)
CPI (UK)	1.5	1.5	4.2

Note: The estimated impact figures have been calculated in the following way. Baumeister and Benati (2013) suggest that the first round of asset purchases in the US, QE1, led to a decline in the spread, defined as the difference between the short-term (3-month) and long-term (10-year) interest rate, of about 60 bps. Their impulse response for the US (figure 6 in their paper) suggest that following a 100 basis points fall in the spread, output rises by 1.8% and CPI inflation by 1.4%. Multiplying these figures by .6 (their proposed impact of US QE1) yields the corresponding numbers in the table above. Kapetanios et al (2012) cite evidence suggesting that UK QE1 led to a compression in the spread of about 100 basis points. The impulse responses in their paper (figure 4) suggest this size spread compression would lead to rise in output of 2.5% and rise in inflation of 1.5%. In our study, we express asset purchases as a fraction of 2009Q1 annualised GDP, meaning that QE1 resulted in purchases of about 14% and 2% for the UK and US, respectively. The figures in the ‘Weale and Wieladek’ column can therefore be obtained by multiplying 14% and 2% by the average effect (column 8) reported in table 3. Figures followed by ** include the impact of mortgage backed securities for the US. In that case the ratio of securities purchased (both Treasury bond and MBS) is 7% and should be multiplied by the average of the effects for real GDP (.23) and CPI (.16) reported in row 7 (Treasury + MBS AP – 18 months) of tables 7 and 8, to obtain the figures in the table above.

To compare our multipliers to those presented in previous work, one needs to translate the impact of asset purchases to the spread considered in previous studies. For that purpose, we compare the effects implied by UK and US QE1 implied by the impulse responses in previous studies, to the ones implied by the impulse responses in this paper. In particular, Baumeister and Benati (2013) and Kapetanios et al (2012) argue that the first round of asset purchases in the US and the UK, led to fall of about 60 and 100 basis points in the spread between the long-term and short-term interest rate. From table 3, it is then easy to see that the estimates in those papers imply a rise of 2.5 (1.08) percent and 1.5 (.9) percent in output and inflation in the UK (US), respectively. During QE1, the Bank of England and the Federal Reserve engaged in government bond purchases worth of 14 and 2 per cent of annualised 2009Q1 GDP, respectively. Table 3 suggests that, based on the estimates in this paper, this would lead to a rise of 2.52 (.72) percent and 4.2 (.76) percent in UK (US) real GDP and CPI, respectively. For the US, these estimates are smaller than reported by previous work, though they exclude the possible impact of mortgage securities. Once that effect is included (see table 3), the estimates for US real GDP and CPI rise to 1.61 and 1.12, respectively. For the UK, interestingly, the impact on real GDP is almost identical to previous work; the implied CPI response is more than twice as large, but this difference is not statistically significant. This finding is robust to excluding VAT from the CPI series.

3.2. How do other variables react to asset purchase shocks?

Economic theory suggests that asset purchase policy can affect interest rates through three different channels: 1) Bauer and Rudebusch (2011) argue that any announcement of unconventional policy means that interest rates will be kept at the zero lower bound for longer.⁶ 2) According to the portfolio rebalancing channel, in the presence of asset market frictions, asset purchases will either affect yields with the highest interest rate risk or yields at the maturity purchased through the impact on duration and scarcity, respectively.

Clearly, given the lack of public information on the exact composition of assets bought during each announcement, it is not really possible to assess the strength of the scarcity channel. But by examining the impact on interest rate futures and long-term

⁶ This channel also suggests that since central banks cannot credibly commit to a policy stance too far into the future (Krishnamurthy and Vissing-Jorgensen, 2010), asset purchase policy should have a relatively greater impact on the short, relative to the long, end of the yield curve.

government bond yields, we may be able to provide support for either the signalling or the portfolio rebalancing channel. For this purpose, we examine the impact on OIS interest rate futures of the 3 month USD or GBP rate 12 month, 24 month and 36 months ahead, as well as the yields on government bonds of 5/20/30 year maturity, by adding them one-by-one to our VAR model. Figure 3 shows the results for the US. The response of 3 month OIS rate 12 months, 24 months and 36 month ahead is not statistically significant with any specification and sometimes reacts with the wrong sign. But due to the introduction of forward guidance in the US during our sample period, which is likely to affect future rates at the horizons we are interested in, it is not possible from these results to rule out any role for the signalling channel as an important transmission mechanism of asset purchases. A formal distinction between asset purchase and forward guidance policy is beyond the scope of this paper and left for future research. On the other hand, yields on 20 and 30 year Treasuries show a statistically significant and negative response in most specifications. This suggests that in the US, the portfolio rebalancing channel seems to be an important transmission mechanism of unconventional monetary policy. Figure 4 repeats this exercise for the UK, where the response of yields on long-dated gilts is not robust across all of the specifications. On the other hand, the response of the 3 month OIS rate 12 and 24 months ahead is statistically significant, negative and robust across all of the specifications. This suggests that in the UK, the signalling channel is relatively more important.

Figure 3: Results for USA

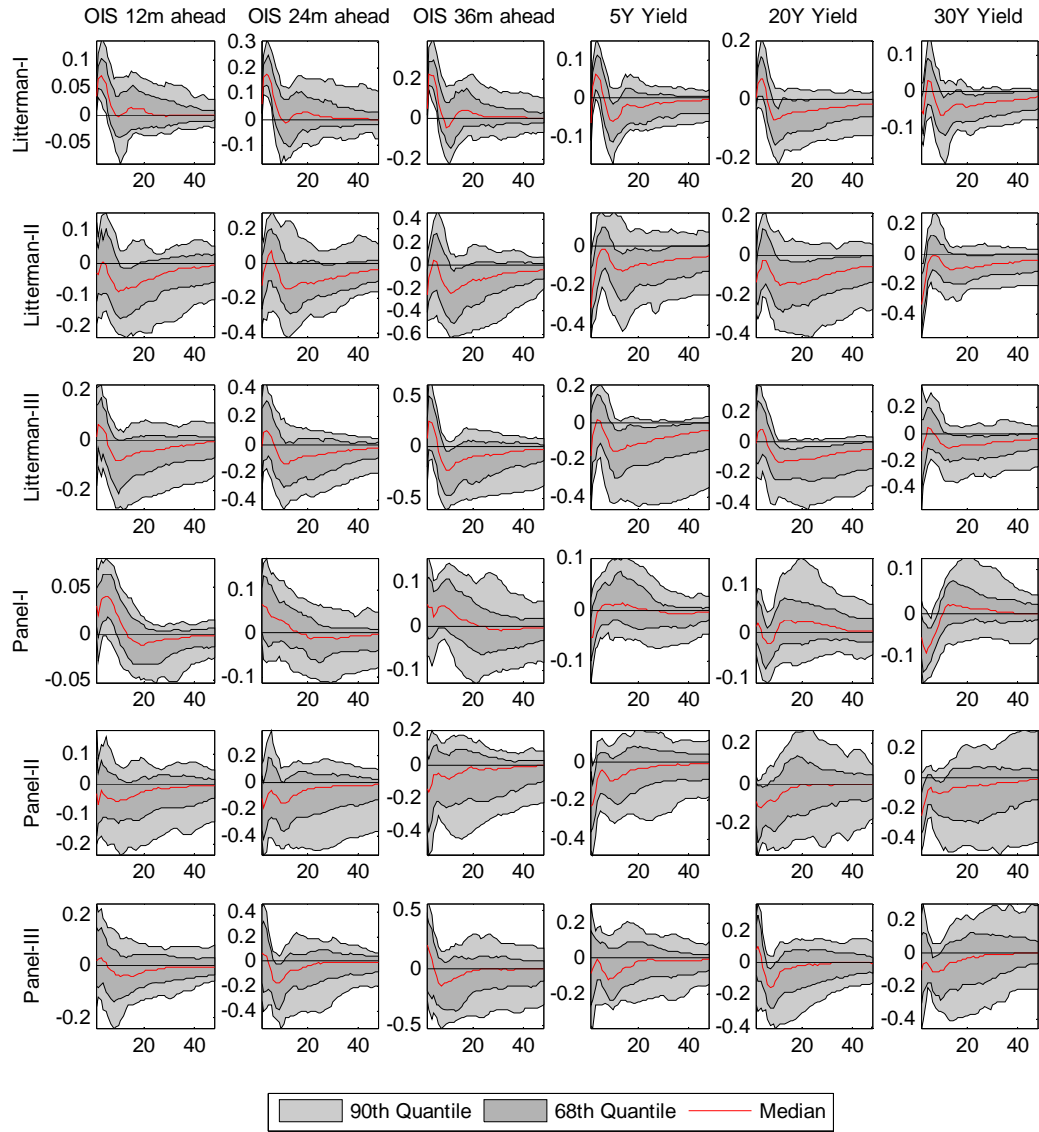
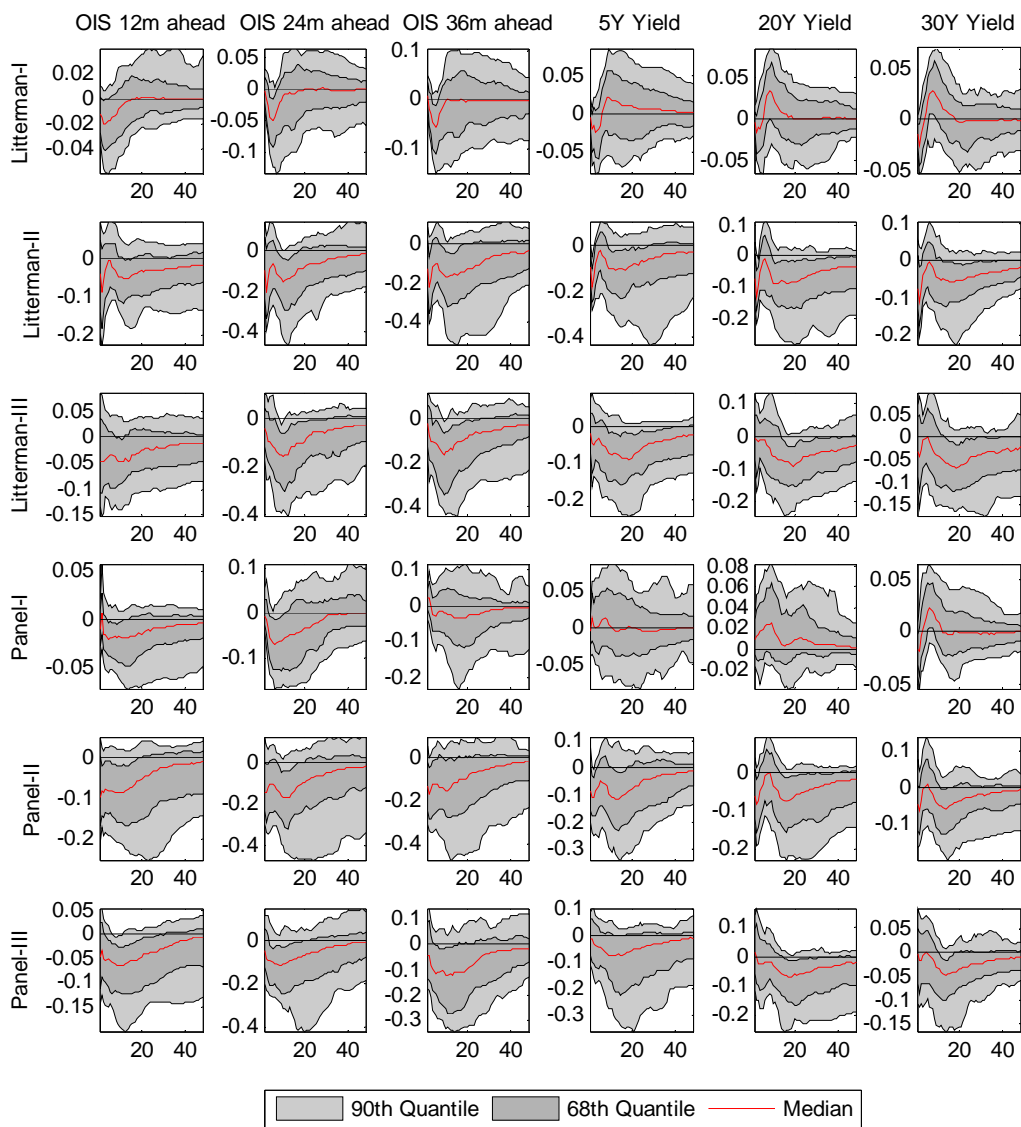


Figure 4 – Results for UK

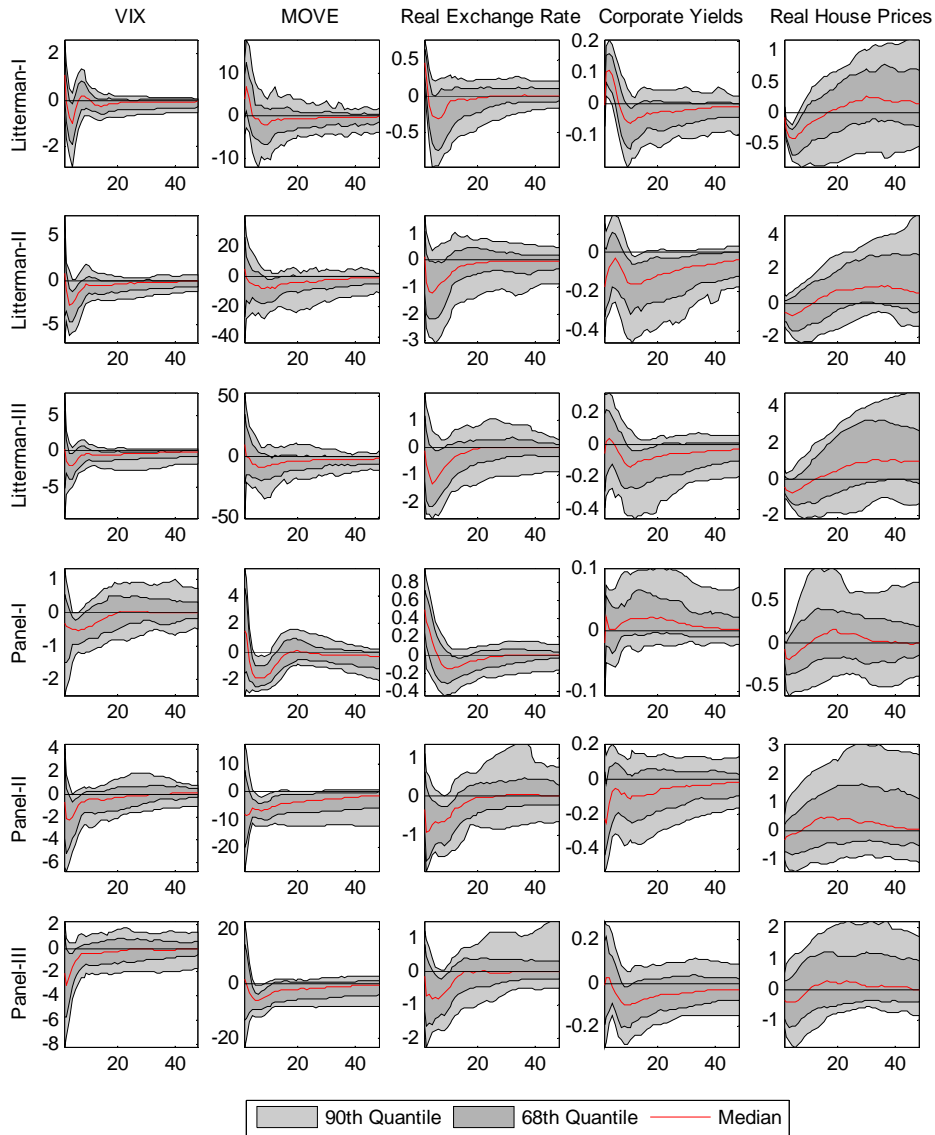


Asset purchases can, of course, also have an impact on the real economy by reducing uncertainty about the future interest rate path and the macroeconomy in general. Additionally, they may also reduce yields on corporate bonds and the real exchange rate, through the portfolio rebalancing channel at home and abroad. Finally, to the extent that effective long-term (short-term) interest rates on mortgage rates are affected, real house prices in the US (UK) may rise in response to them.⁷ To test for any of these transmission

⁷ Given that the vast majority of mortgages in the US are at 30-year maturity, while those in the UK track short-term rates fairly closely, one would expect US (UK) real house prices to react relatively more to changes in long-term (short-term) mortgage rates.

channels, we include the following variables, again one at a time, as a sixth variable in our VAR model: the implied volatility of the share price index (VIX) and interest rate futures (swaptions⁸) in each country (MOVE), corporate bond yields, the real exchange rate and real house prices. The results for the US are shown in figure 5. The only two variables that react with the expected sign in most specifications are the VIX and the real exchange rate.

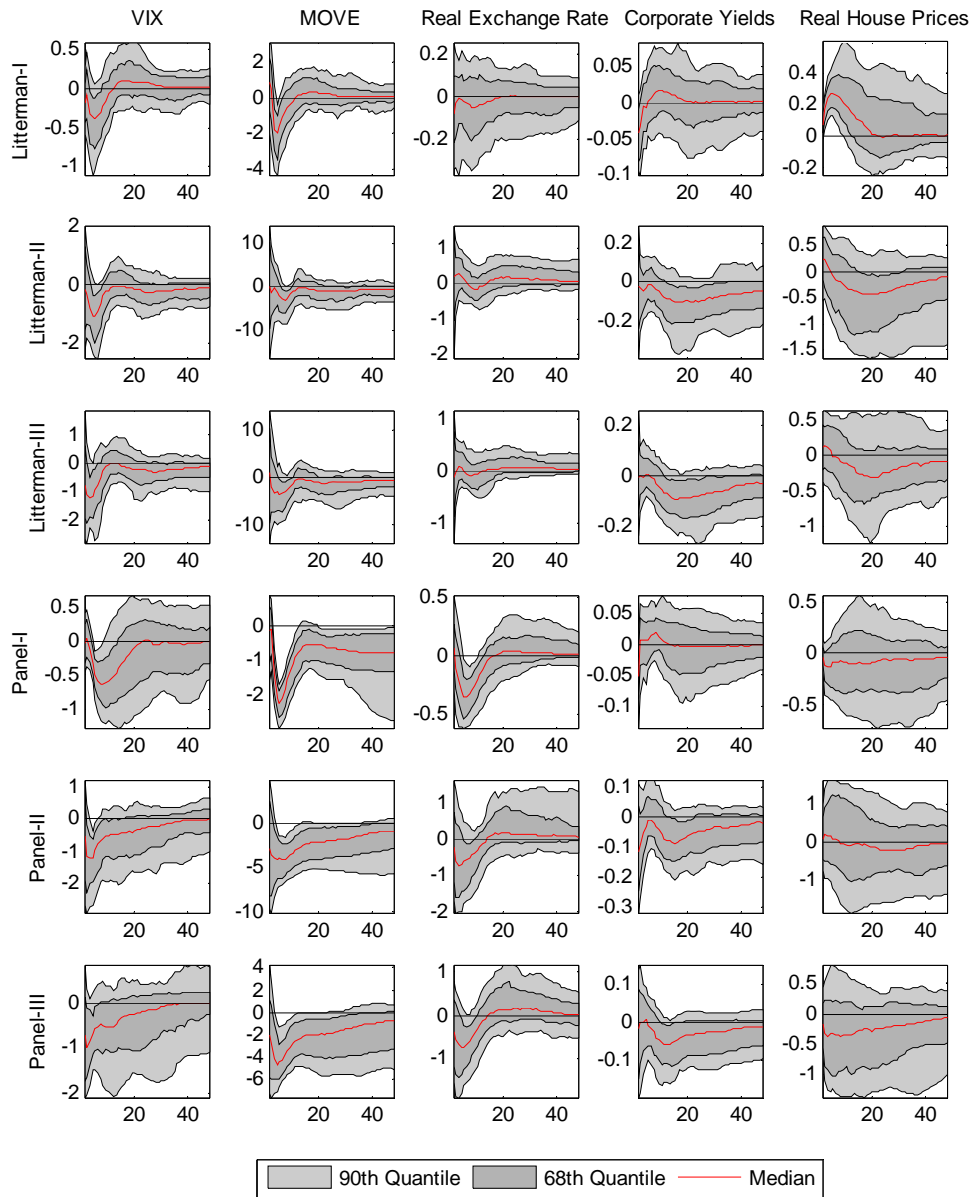
Figure 5 – Results for the US



⁸ A swaption is an option granting its owner the right to enter into an underlying interest rate swap. For horizons of greater than 12 months only swaptions are available, as opposed to options, which is why they are used in the calculations of the MOVE index.

The results for the UK are shown in figure 6. Clearly, only the VIX and MOVE react to asset purchases in the UK. This is in line with the importance of the signalling channel in the UK documented previously: A reduction in the implied volatility of interest rate swaptions will typically raise certainty that interest rates will remain at their low levels going forward.

Figure 6 – Results for the UK



3.3 Are shocks transmitted across borders?

A question of recent policy interest is to which extent unconventional monetary policy spills over into emerging economies. For example, Fratzscher, Doluca and Straub (2012) argue that US quantitative easing has increased global liquidity and led to capital flows to emerging market economies. In this section, we examine the transmission of asset purchase shocks in each country on several variables in emerging market economies, namely dollar-denominated sovereign (EMBIG) and corporate (CEMBIG) bond spreads to US treasury bonds of the same maturity, the natural logarithm of real share prices, equity and bond flows into emerging market mutual funds, a measure of total capital flows to, as well as the natural logarithm of industrial production⁹ in, those countries. These results are shown in figures 5 and 6. Both UK and US asset purchase policy have a statistically significant and negative effect on corporate and sovereign yield spreads, as well as a positive and statistically significant effect on real share prices, in most specifications. Furthermore there is a statistically significant and positive effect on industrial production in almost all specifications. Typically it is argued that these asset price and real economy reactions are the result of a search for yield from advanced to emerging economies and are transmitted through capital flows. Interestingly, in nearly all of the specifications, neither the EPFR nor the total capital flows measure shows a statistically significant and positive reaction. This suggests that while there may have been some spillover to emerging markets, it does not necessarily seem to happen via greater capital inflows. Indeed, it is equally likely that a reduction in uncertainty about demand in advanced economies as a result of asset purchases could be responsible for the pattern observed in the impulse responses in figures 5 and 6, but a distinction between the various channels to emerging market economies is beyond the scope of this work.

⁹ Industrial production is a GDP weighted average of the industrial production indices of the largest emerging markets: Russia, Brazil, China, India, Indonesia, Mexico and Turkey.

Figure 7: The effects of US shocks on EME variables

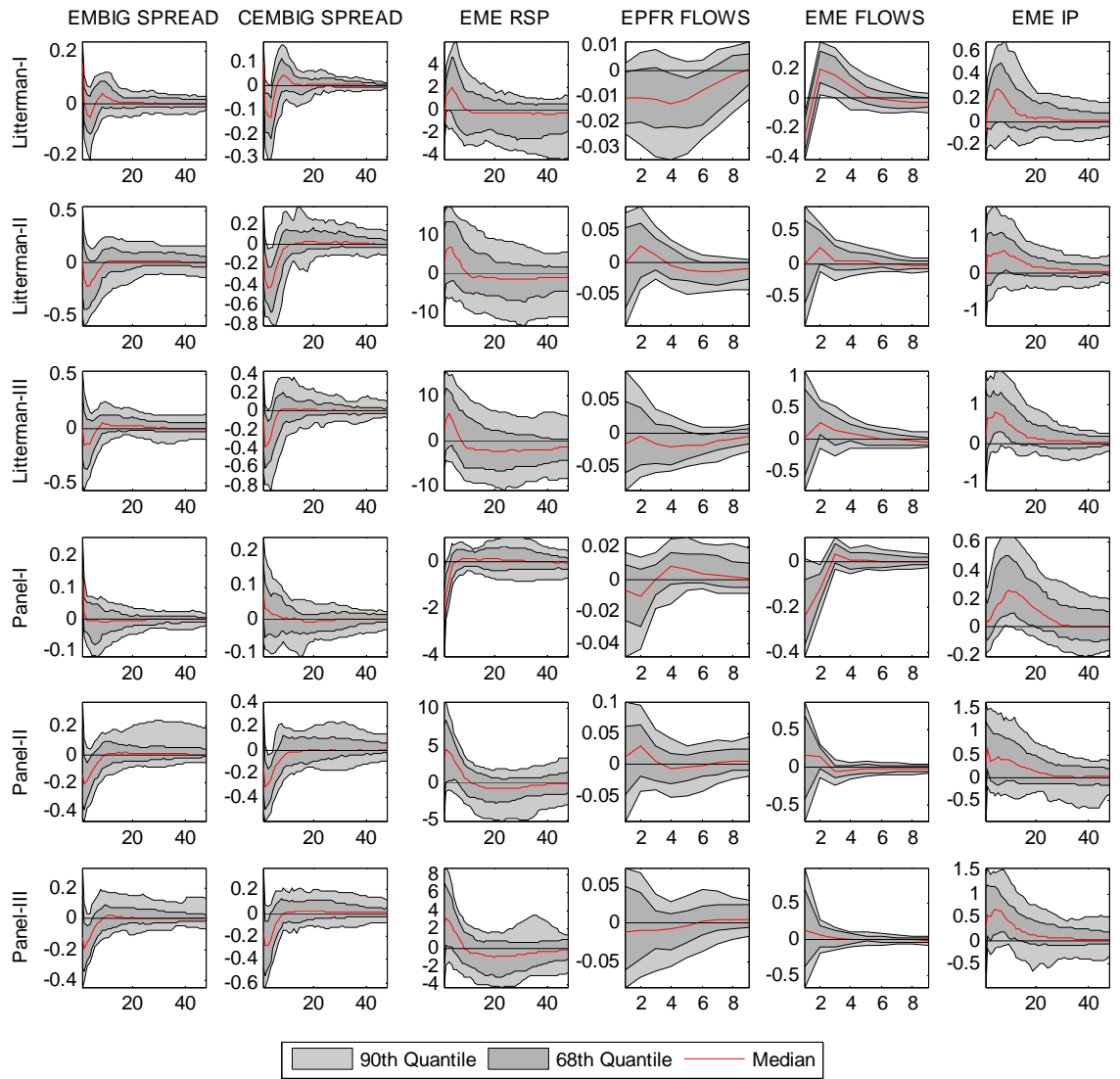
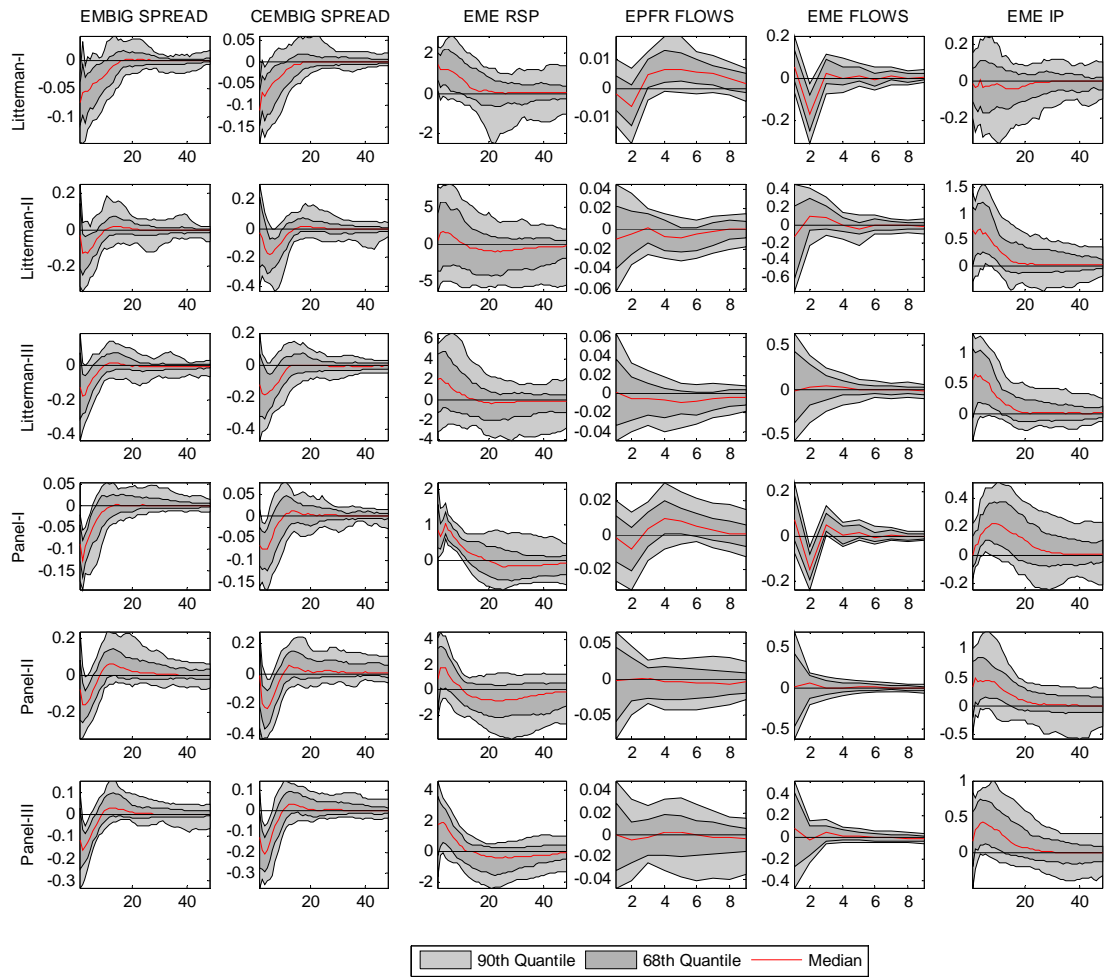


Figure 8: The effects of UK shocks on EME variables



3.3 Robustness

3.3.1 Omitted variable bias

Due to the short sample size, our baseline model consists of five variables. But it is well known that small VARs may suffer from omitted variable bias. In particular, the asset purchase shock may be reflecting the reaction of the monetary authority to other coincident economic developments, such as domestic fiscal policy, the Euro Area crisis, real oil prices and monetary expansion by the European Central Bank. To examine if this is the case, we therefore include the domestic government budget balance to GDP ratio, the public debt to GDP ratio, the spread between Italian and German 10-year government bond yields, the natural logarithm of real oil price in US dollars/UK sterling and the ratio of the ECB's total assets on its balance sheet to Euro Area GDP one by one in our VAR. For brevity, we only show the maximum effect on real GDP and the CPI, together with an indication of statistical significance, in tables 5 and 6. All of the corresponding figures can be found in appendix B. Rows one to five in table 5 show the impact on US real GDP and statistical significance, when the government budget balance to GDP ratio, the public debt to GDP ratio, the Euro Area spread, the real oil price and the ratio of the ECB's total assets to Euro Area GDP are included as additional variables, respectively. Rows six to ten show the corresponding impact on UK real GDP. The average across all of the specifications in each row is similar to, and most of the time actually greater than, the averages of .36 (US) and .18 (UK) obtained with the base line model in table three. Table six repeats this exercise for the impact on the CPI. As with real GDP, the results are similar to the average effects reported in table three. Overall, this suggests that omitted variable bias does not seem to be a problem in our model.

Table 5 – Maximum impact and statistical significance on real GDP

Model/ Additional variable included	Litterman I	Litterman II	Litterman III	Panel I	Panel II	Panel III	Average
Gov. budget balance (US)	0.31**	0.63**	0.60**	0.12**	0.57**	0.43*	0.44
Public debt (US)	0.15**	0.50**	0.44*	0.10*	0.50**	0.21	0.32
Euro Area spread (US)	0.21**	0.50**	0.52*	0.15**	0.57**	0.34*	0.38
Real oil price (US)	0.26**	0.72**	0.61**	0.17**	0.57**	0.41**	0.46
ECB balance sheet (US)	0.20**	0.52**	0.48**	0.10**	0.61**	0.43*	0.39
Gov. budget balance (UK)	0.06*	0.28*	0.20**	0.08**	0.37**	0.25*	0.21
Public debt (UK)	0.06**	0.18*	0.16*	0.10*	0.42**	0.27*	0.20
Euro Area spread (UK)	0.07*	0.26*	0.18*	0.13**	0.37**	0.23**	0.21
Real oil price (UK)	0.07*	0.23**	0.19**	0.15**	0.35**	0.24**	0.20
ECB balance sheet (UK)	0.05*	0.19*	0.14*	0.11**	0.32**	0.22**	0.17

Note: Table shows maximum median effect on real GDP for the US/UK. Each column shows results from a different estimator and identification scheme. For example, 'Litterman I' means that the model was estimated subject to the Litterman prior and the asset purchase shock identified with a choleski decomposition scheme. Each row shows the effect of including a different control variable as the 6th variable in the VAR. In particular, 'Gov. budget balance' means that all models in that row have been estimated including the government budget balance to GDP ratio as an additional control variable. 'Public debt' means that all models in that row have been estimated including the public debt to GDP ratio as an additional control variable. 'Euro Area spread' means that all models in that row have been estimated including the Italian to German government bond spread on 10-year government bond yields as an additional control variable. 'Real oil price' means that all models in that row have been estimated including the natural logarithm of the real oil price, expressed in domestic currency (USD and GBP respectively) and deflated by the CPI, as an additional control variable. 'ECB Balance sheet' means that all models in that row have been estimated including the ECB's Total assets to Euro Area GDP ratio as an additional control variable. (US)/(UK) means that results for the US/UK are presented. **/* signifies statistical significance at 90 / 68 quantile bands.

Table 6 – Maximum impact and statistical significance on CPI

Model/ Additional variable included	Litterman I	Litterman II	Litterman III	Panel I	Panel II	Panel III	Average
Gov. budget balance (US)	0.34**	0.71**	0.68**	0.03	0.53*	0.50*	0.46
Public debt (US)	0.18**	0.58**	0.64**	0.02	0.52*	0.43*	0.39
Euro Area spread (US)	0.27**	0.64**	0.67**	0.05*	0.47*	0.58*	0.45
Real oil price (US)	0.29**	0.72**	0.74**	0.09*	0.44*	0.55**	0.47
ECB balance sheet (US)	0.26**	0.71**	0.65**	0.01	0.67*	0.72*	0.50
Gov. budget balance (UK)	0.01	0.75**	0.47*	0.05	0.74**	0.43*	0.41
Public debt (UK)	0.07	0.46*	0.32*	0.04*	0.61*	0.47**	0.33
Euro Area spread (UK)	0.01	0.66*	0.41*	0.05**	0.30	0.21	0.27
Real oil price (UK)	0.01	0.63*	0.39*	0.07**	0.45*	0.30*	0.31
ECB balance sheet (UK)	0.02	0.55	0.40*	0.05	0.54*	0.36*	0.32

Note: Table shows maximum median effect on the CPI for the US/UK. Each column shows results from a different estimator and identification scheme. For example, 'Litterman I' means that the model was estimated subject to the Litterman prior and the asset purchase shock identified with a choleski decomposition scheme. Each row shows the effect of including a different control variable as the 6th variable in the VAR. In particular, 'Gov. budget balance' means that all models in that row have been estimated including the government budget balance to GDP ratio as an additional control variable. 'Public debt' means that all models in that row have been estimated including the public debt to GDP ratio as an additional control variable. 'Euro Area spread' means that all models in that row have been estimated including the Italian to German government bond spread on 10-year government bond yields as an additional control variable. 'Real oil price' means that all models in that row have been estimated including the natural logarithm of the real oil price, expressed in domestic currency (USD and GBP respectively) and deflated by the CPI, as an additional control variable. 'ECB Balance sheet' means that all models in that row have been estimated including the ECB's Total assets to Euro Area GDP ratio as an additional control variable. (US)/(UK) means that results for the US/UK are presented. **/* signifies statistical significance at 90 / 68 quantile bands.

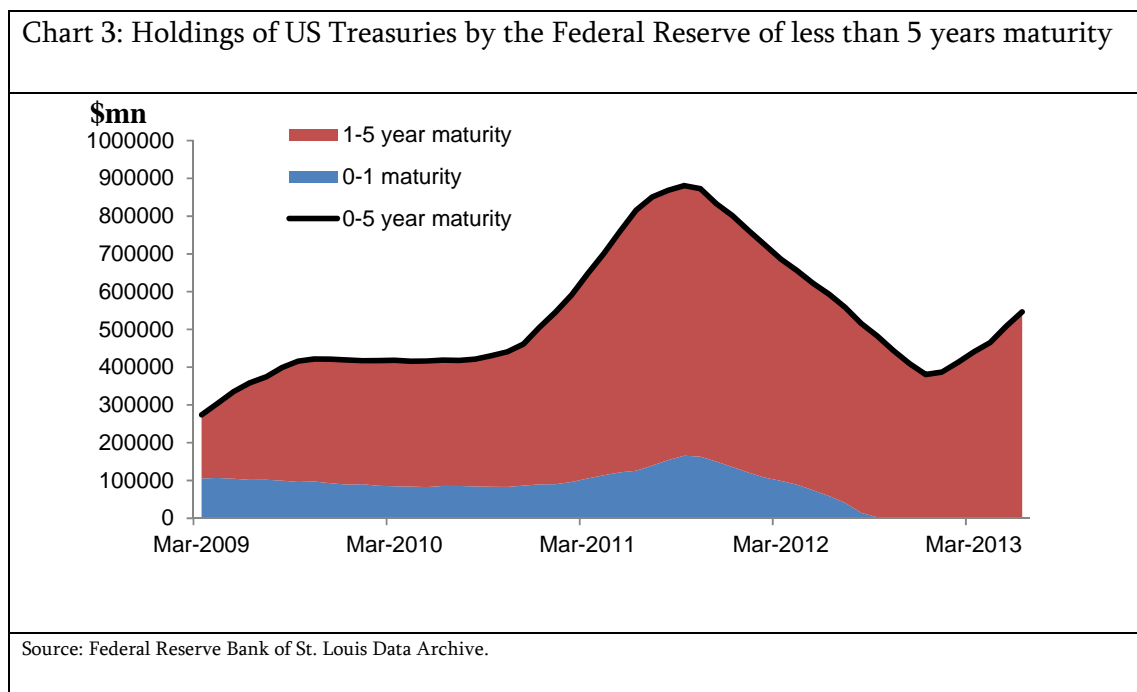
3.3.2 – Definition of the announcement series

Our empirical analysis assumes that macroeconomic variables tend to respond to announcements, rather than, actual asset purchases. But it is worth verifying if our results are robust to using the actual amount of assets purchased instead. Similarly, in contrast to the UK, the nature of asset purchases in the US has changed over time, with the Federal Reserve engaging in Operation Twist and opened purchases, as well as purchases of mortgage-backed securities. This means that we had to make a number of assumptions to create the asset purchase announcement series for the US and we show that our results are robust to all of them in this section. For brevity, we only show the maximum effect on real GDP and CPI, together with an indication of statistical significance, in tables 7 and 8. All of the corresponding figures can be found in appendix B.

The scarcity channel of asset purchase policy suggests that only the yields of assets that are actually bought, or expected to be bought in the future, should react to the policy. If this is correct, then actual assets purchased, rather than announced, should be used in the VARs estimated in this paper. The results from this exercise for real GDP and the CPI for the UK and the US are presented in the first two rows of tables 7 and 8. As before, asset purchase policy in both countries has a statistically significant and positive impact on real GDP and the CPI, though quantitatively, the effects seems larger.

In the construction of our asset purchase announcement series, we made the assumption that announcements associated with the Federal Reserve's maturity extension programme (also known as Operation Twist) receive the same weight as asset purchases of government bonds that were financed with the issuance of central bank reserves. This does, of course, not have to be the right procedure, particularly since Treasury bonds purchased as part of US QE1 were sold off as a result. Chart 3 illustrates that the Federal Reserve's holdings of short-term treasury debt rose to around \$US 880 bn and then fell to \$US 380 bn. Taken at face value, this suggests that the announcement of \$US 667 bn was associated with a contemporaneous unwinding of shorter-term assets purchased during US QE1 of about \$US 500 bn. This calculation suggests that a more reasonable weight on Operation Twist announcements is a quarter, as oppose to one. While it is clearly difficult to pinpoint the right weight for Operation Twist announcements precisely, a quarter is probably not an unreasonable lower bound. The third row in tables 7 and 8 reports results from VAR estimates with an announcement series that attaches a weight of a quarter on

the Operation Twist announcements. Asset purchase policy now has a statistically significant effect on real GDP in all but one specification, but with a larger impact than before. The effect of CPI is only statistically significant in four out of the six possible specifications.



The Federal Reserve also announced open-ended purchases of government bonds at a rate of \$US 45 bn per month in 2012. It is unclear how to translate the magnitude of this announcement to one that is comparable to other US asset purchase announcement. At the time of the announcement, guidance was also provided that the federal funds rate would stay low until unemployment had reached the 6.5% threshold. FOMC minutes that accompanied the announcement suggested that this would be met in 2015, implying that purchases would continue for at least three years. One way of calculating the economic impact of the open-ended QE announcement is therefore to calculate the present value of an asset that pays \$US 45 bn each month, for thirty-six months. This suggests that the economic impact of the open-ended QE announcement was about 1217bn USD. Financial markets may of course take a different view and an examination of OIS rate futures data suggest that they expected a rise in the 3 month OIS rate twenty-four, but not twelve months ahead. Assuming that open-ended QE will expire after eighteen months yields an economic impact of about 702bn USD, which is similar to the

impact of QE2 (600bn USD). Rows four and five in tables 7 and 8 show estimates for VAR models, where the present value of openended asset purchases lasting either eighteen or thirty-six months has been added to the announcement series. For real GDP, there is a positive and statistically significant effect of asset purchase policy in all specifications, with quantitatively smaller impacts. For the CPI response, the effects are not statistically significant anymore, in particular when asset purchases are assumed to last thirty-six months.

Table 7 – Maximum impact and statistical significance on real GDP

Model/ Change to Asset purchase announcement series	Litterman I	Litterman II	Litterman III	Panel I	Panel II	Panel III
Assets Purchased (UK)	0.10*	0.35*	0.26**	0.14**	0.38**	0.24**
Assets Purchased (US)	0.37*	0.66*	0.51*	0.25**	0.79**	0.43*
Smaller weight on Op. Twist Announcement	0.24**	0.59**	0.47*	0.04	0.74**	0.34*
Openended AP – 18 months	0.12**	0.45**	0.27*	0.09**	0.46**	0.28*
Openended AP – 36 months	0.05*	0.36*	0.15*	0.09*	0.33**	0.19**
Treasury + MBS AP – 18 months	0.08*	0.31*	0.23*	0.09**	0.44**	0.25**
Treasury + MBS AP – 36 months	0.03	0.21**	0.12*	0.05*	0.30**	0.18**

Note: Table shows maximum median effect on real GDP for the US/UK. Each column shows results from a different estimator and identification scheme. For example, ‘Litterman I’ means that the model was estimated subject to the Litterman prior and the asset purchase shock identified with a Choleski decomposition scheme. Each row shows a different modification to the asset purchase series in the VAR. In particular, ‘Assets Purchased’ means that all models in that row have been estimated with the actual amount of assets purchased, as oppose to announced, to GDP ratio as the dependent variable. ‘Smaller weight on Op. Twist Announcement’ means that we put a weight of .25, as oppose to 1, on announcements associated with Operation Twist in constructing the asset purchase announcement series. ‘Openended AP – 18 months/36 months’ means that the baseline asset purchase series for the US has been augmented with the present-value of open-ended purchases, assuming that agents believe that they will last 18/36 months upon announcement. ‘Treasury + MBS AP – 18 months/ 36 months’ repeats the previous exercise but adding purchases of mortgage-backed securities as well. **/* signifies statistical significance at 90 / 68 quantile bands.

Table 8 – Maximum impact and statistical significance on the CPI

Model/ Change to Asset purchase announcement series	Litterman I	Litterman II	Litterman III	Panel I	Panel II	Panel III
Assets Purchased (UK)	0.24**	0.58*	0.78**	0.43**	0.89*	0.76**
Assets Purchased (US)	0.59**	0.69**	0.68**	0.31**	0.53*	0.42*
Smaller weight on Op. Twist Announcement	0.33**	0.78**	0.60*	0.03	0.62	0.50**
Openended AP – 18 months	0.08*	0.45*	0.31*	0.00	0.35	0.18
Openended AP – 36 months	0.02	0.29*	0.13	0.00	0.18	0.15
Treasury + MBS AP – 18 months	0.02	0.29*	0.20	0.00	0.22	0.25*
Treasury + MBS AP – 36 months	0.00	0.17*	0.10	0.00	0.16	0.12

Note: Table shows maximum median effect on CPI for the US/UK. Each column shows results from a different estimator and identification scheme. For example, 'Litterman I' means that the model was estimated subject to the Litterman prior and the asset purchase shock identified with a Choleski decomposition scheme. Each row shows a different modification to the asset purchase series in the VAR. In particular, 'Assets Purchased' means that all models in that row have been estimated with the actual amount of assets purchased, as oppose to announced, to GDP ratio as the dependent variable. 'Smaller weight on Op. Twist Announcement' means that we put a weight of .25, as oppose to 1, on announcements associated with Operation Twist in constructing the asset purchase announcement series. 'Openended AP – 18 months/36 months' means that the baseline asset purchase series for the US has been augmented with the present-value of open-ended purchases, assuming that agents believe that they will last 18/36 months upon announcement. 'Treasury + MBS AP – 18 months/ 36 months' repeats the previous exercise but adding purchases of mortgage-backed securities as well. **/* signifies statistical significance at 90 / 68 quantile bands.

In addition to government bonds, the other type of asset that the Federal Reserve purchased in large quantities were mortgage-backed securities. Most of these purchases were made before March 2009, the beginning of government bond purchases, and in September 2012, open-ended purchases of mortgage backed securities were announced at a rate of 40bn USD per month. Below we explore to which extent the inclusion of announcements of mortgage-backed security purchases into the US asset purchase series described in the paragraph above makes a difference to our results. As before, the open-ended nature of these purchases requires us to make assumptions about the perceived length of purchases to calculate their economic impact upon announcement. The results are shown in last two rows of tables 7 and 8. Clearly, the effect on real GDP is still statistically significant and positive, while the effect on CPI seems less well determined.

4. Conclusion

In response to the ‘Great Recession’ and the fact that policy rates had been reduced to the lowest practical level for both the UK and the US, central banks deployed a range of novel monetary policy tools. The impact of these new, frequently referred to as ‘unconventional’, monetary policies on the economy is still little understood. In this paper we focus on purchases of government bonds by the both the Bank of England and the Federal Reserve and propose three different schemes to identify asset purchase shocks and understand their impact on the macroeconomy with two different Bayesian VAR models. Importantly, unlike in the impulse response analysis of all previous work in this area, we do not restrict the reaction of either the CPI or real GDP in any of our identification schemes. This allows us to test formally whether output and the price level react to asset purchases. To examine the relative importance of the signalling, versus the portfolio rebalancing, channel of unconventional monetary policy, we also examine the impact on OIS rate futures and twenty/thirty year yields on government debt, by including each of them as a additional variables, one at a time into our model. In a similar spirit, we also include measures of financial market uncertainty, the real exchange rate, corporate bond yields and real house prices. Finally, we use our framework to examine the impact on corporate and sovereign bond spreads, real share prices, industrial production in and capital flows to emerging market economies.

Our results suggest that, at the median, an asset purchase shock that results in the central bank purchasing government bond worth 1% of nominal GDP, leads a rise of about .18% (.36%) of real GDP and .3% (.38%) in CPI in the UK (US). These results are robust to including domestic fiscal policy, the spread on 10-year Italian to German government bonds, real oil prices and the size of the ECB’s balance sheet as additional variables in the VAR. Similarly using the actual amount of assets purchased as the main variable of interest and, for the US, different assumptions regarding the treatment of the Federal Reserve’s maturity extension programme (also known as Operation Twist), open-ended purchases of government bonds or MBS purchases make little different to our findings. Interestingly, a back of the envelope calculation suggests that the quantitative size of the impact on real GDP and CPI is similar to studies that identify unconventional monetary policy as a compression in the spread between the long and the short rate (Baumeister et al, 2013; Kapetanios et al, 2012), with one exception: For the UK, we find that the CPI response is more than twice as large as documented by previous work,

though this finding is, of course, subject to considerable uncertainty and the difference is probably not statistically significant.

We also find that, for the UK, asset purchases have a statistically significant impact on OIS rate futures, suggesting that the signalling channel is important. In contrast, for the US we only find a reaction of government yields of 20 and 30 year maturity, suggesting that the portfolio rebalancing channel is important. But due to the possible impact of US forward guidance on OIS rate futures, lack of statistically significant responses does not allow us to conclude that the signalling channel is not present in the US. Furthermore, our findings show that unconventional monetary policy can lead to a reduction in uncertainty, measured as the implied volatility of the stock market, in both countries and even uncertainty about the path of future interest rates in the UK. For the US, there is also some evidence that asset purchases lead to a real exchange rate depreciation. Finally, we document that there is a robust, statistically significant and negative effect of UK and US asset purchases on sovereign and corporate bond spreads in emerging market economies, and a positive effect on industrial production in those countries. Interestingly, there is no evidence of a capital flows reacting, regardless of whether measured as flows into emerging market mutual debt and equity funds or as an approximate measure of total capital flows, defined as the difference between the trade balance and changes in international reserves.

Overall, these findings are encouraging, because they suggest that unconventional monetary policy in form of asset purchases, can be effective in stabilising output and prices. In the UK an important transmission channel seems to be through signalling that interest rates will stay lower for longer and a reduction in financial market uncertainty. In the US, the impact on government bond yields of long maturity, and hence the portfolio rebalancing channel, and the real exchange rate are important transmission channels. We do not find robust evidence for a reaction of capital flows to emerging market economies, but dollar denominated sovereign and corporate bond spreads and industrial production in these economies do seem to react to asset purchase policy in the UK and the US. One explanation for this pattern is a reduction in uncertainty about demand in advanced economies, and hence the target export markets of emerging market economies, as a result of asset purchase policy. If this is true, then one would not necessarily expect a negative spillover effect on emerging market economies from UK and US asset sales, so long these are the result of robust economic growth in these countries.

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Appendix A – Data

Table A1 – Data

Variable	Source and transformation for the US	Source and transformation for the UK
Real GDP	Monthly GDP from Macroeconomic Advisers; Expressed in natural logarithm	Monthly GDP from Mitchell et al (2001); Expressed in natural logarithm
CPI	Monthly seasonally adjusted Consumer Price Index for all items from FRED (CPIAUCSL); Expressed in natural logarithm	Monthly Seasonally adjusted CPI from the Bank of England database; Expressed in natural logarithm
Asset purchase announcements	Minutes of the Federal Open Market Committee (FOMC); Scaled by annualised 2009Q1 GDP	Minutes of the Monetary Policy Committee (MPC); Scaled by annualised 2009Q1 GDP
5-year/10-year/20-year/30-year yield on government bonds	Monthly average of the 5/10/20/30 - year Yield on US Treasury Bonds taken from DataStream (USBD5/10/20/30Y)	Monthly average of the 5/10/20/30 -year Yield on UK Gilts taken from the Bank of England website
Real share prices	Monthly average of S&P500 index from DataStream (S&PCOMP), divided by CPI and expressed in natural logarithms	Monthly average of FTSE100 index from DataStream (FTSE100), divided by CPI and expressed in natural logarithms
12m/24m/36m OIS rate	Monthly average of option (swaption) value for the 3-month US Dollar/ UK Pound OIS (Over night index Swap) rate 12 (24 and 36) months ahead from Bloomberg	
VIX	Monthly average of the CBOE Volatility Index taken from FRED	Monthly average of the implied volatility of the FTSE 100 taken from the Bank of England database
MOVE	Monthly average of the implied volatility index for interest rate swaptions. Constructed by assigning a weight of .2/.2/.4/.2 to the implied volatilities of the one month USD/GBP LIBOR rate 2 years/ 5 years/ 10 years and 30 years ahead, taken from Bloomberg.	
Corporate bond yields	Monthly average of the 10-year yield on AAA rated US corporate bond yields taken from DataStream (TRUCCY)	Monthly average of the 10-year yield on AAA rated UK corporate bond yields taken from DataStream (TRBCCY)
Real Exchange Rate	Monthly average of the US/UK real effective exchange taken from the Bank of International Settlements Effective FX rate database	
Real house prices	Monthly OFHEO house price index, deflated by CPI. Expressed in natural logarithms.	Monthly Land Registry house price index, deflated by CPI. Expressed in natural logarithms.
EMBIG Spread	This is a weighted index of sovereign dollar denominated bonds in Emerging Market economies, expressed as a spread to US treasury bonds at the same maturity. Taken from Morgan Markets.	
CEMBIG Spread	The description is identical to the EMBIG spread, except that this refers to <i>corporate</i>	

	bonds. Taken from Morgan Markets.	
Real EME share prices	FTSE provides a Value-weighted index of Emerging market economy share prices in USD/GBP. This is taken from DataStream (AWALEG\$/£), deflated by the US/UK CPI and expressed in natural logarithms.	
EPFR Capital flows	EPFR provides flows to emerging market economy debt and equity mutual funds in USD at weekly, which we average to monthly, frequency. We then scale this by the sum of USD GDP of Brazil, China, India, Indonesia, Mexico and South Africa in 2007.	
Total Capital flows	The IMF IFS provides the dollar value of import, exports and international reserves at monthly frequency. Under the assumption that in the short-run the trade balance tracks the current account, one can back out an approximate monthly measure of capital flows by subtracting the change in international reserves from the trade balance. We sum this 'capital flow variable' for Brazil, China, India, Indonesia, Mexico and South Africa, and then divide it by sum of 2007 USD GDP for these countries to obtain our measure of total capital flows.	
Emerging market industrial production	The IMF IFS provides indices of industrial production at monthly frequency for Brazil, China, India, Indonesia, Mexico and South Africa. We construct an emerging market economy GDP-weighted average for these countries, based on their relative size in 2007.	
Government budget balance to GDP Ratio	US/UK GOVERNMENT PRIMARY BALANCE AS % OF GDP (AR) SADI is taken from the OECD Economic Outlook database at quarterly frequency and then linearly interpolated to monthly frequency.	
Public debt to GDP Ratio	Total Public Debt as Percent of Gross Domestic Product from FRED (GFDEGDQ188S) obtained at quarterly frequency, then linearly interpolated to monthly frequency.	General government consolidated gross debt had been taken from the UK Office of National Statistics (BKPX) at quarterly frequency. The series is then seasonally adjusted via X12. This is then divided by annualised UK nominal GDP at quarterly frequency. The resulting ratio is linearly interpolated to monthly frequency.
Euro Area Spread	Defined as the difference in yields on 10-year government debt between Italy and Germany. Monthly averages of daily yields have been obtained from DataStream (ITBRYLD/GBBD10Y)	
Real Oil Prices	Crude Oil Prices: West Texas Intermediate (WTI) from FRED (MCOILWTICO); Deflated by CPI and expressed in natural logarithms.	Crude Oil Prices: Brent Europe from FRED (MCOILBRETEU); Deflated by CPI and expressed in natural logarithms.
ECB Balance Sheet	Monthly average of Total Assets of the ECB, taken from the ECB Statistical Warehouse. Then expressed as a ratio to 2009Q1 Euro Area GDP.	

Appendix B – Additional figures

Figure B1: Results for USA with Gov. Budget Balance as control variable

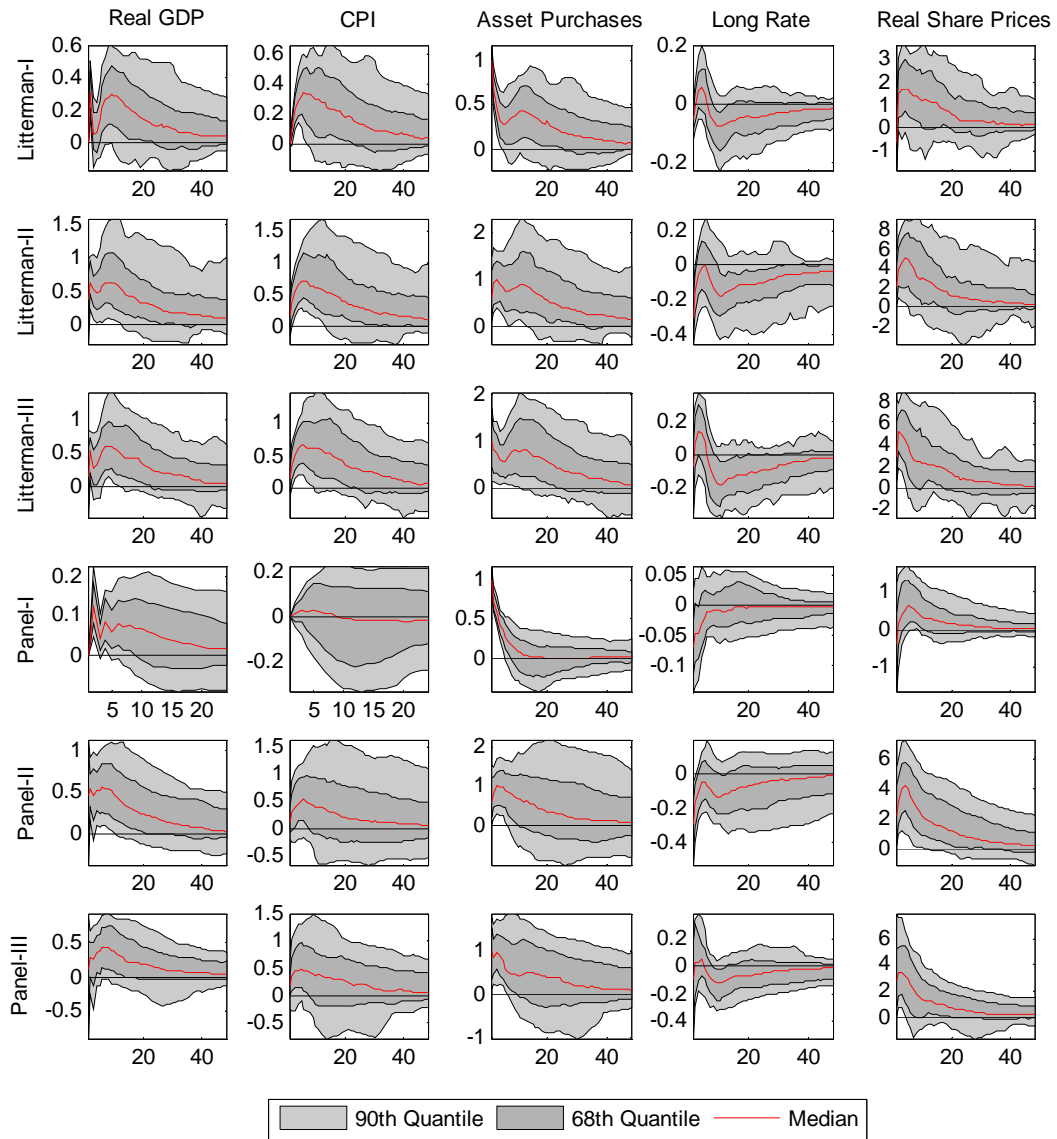


Figure B2: Results for UK with Gov. Budget Balance as control variable

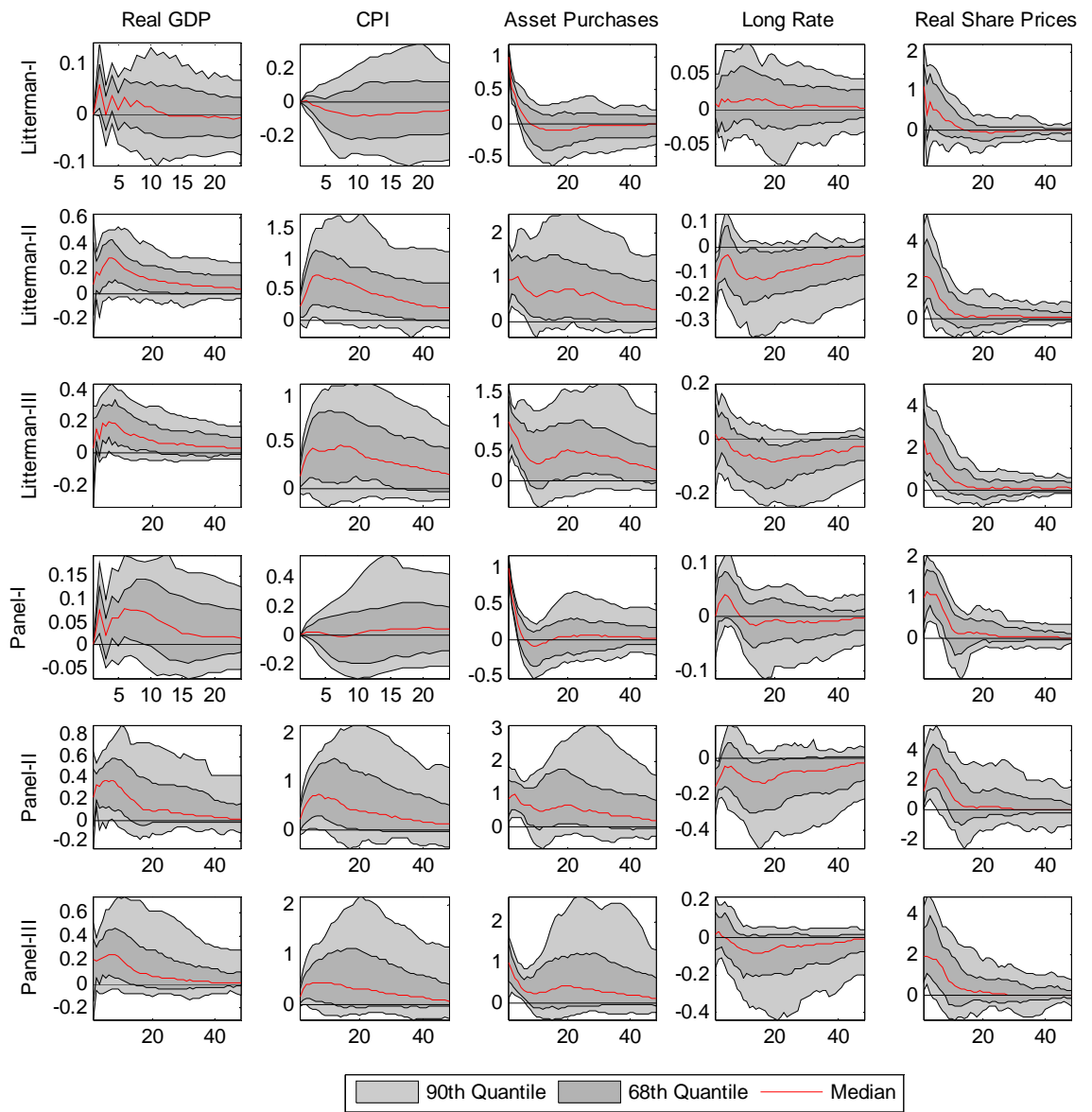


Figure B3: Results for USA with Public Debt to GDP ratio as control variable

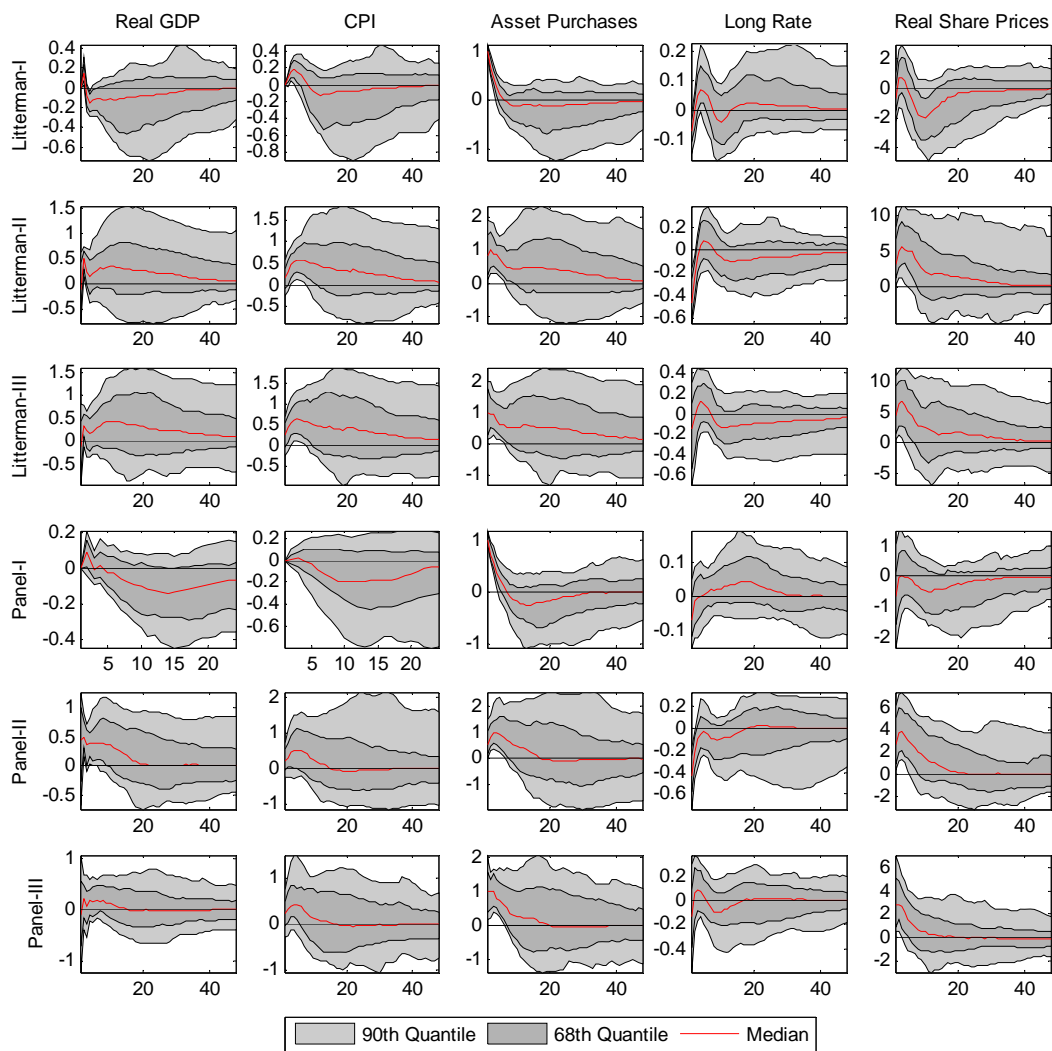


Figure B4: Results for the UK with Public Debt to GDP ratio as control variable

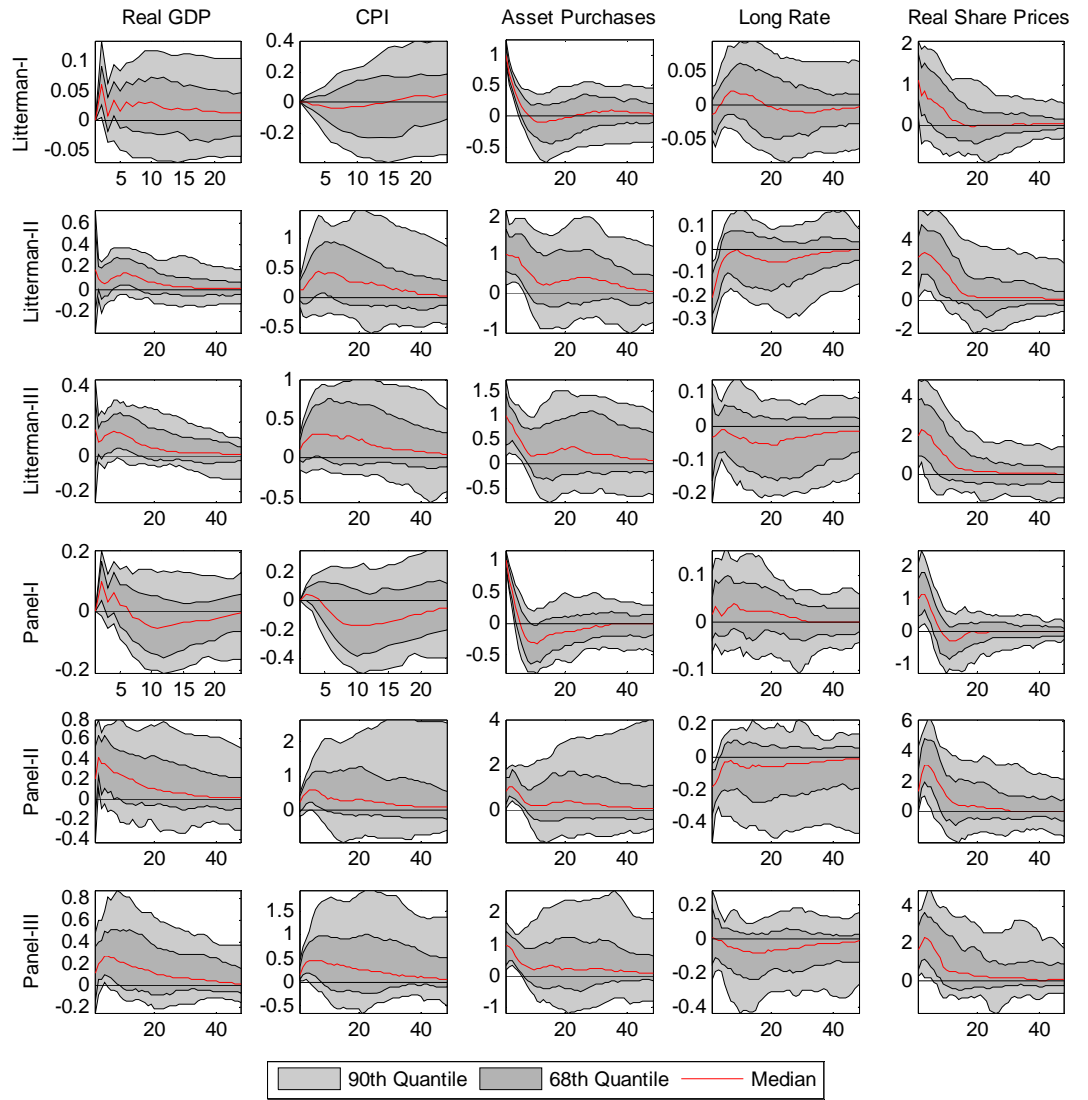


Figure B5: Results for USA with the Italian to German 10-year government bond yield spread as control variable

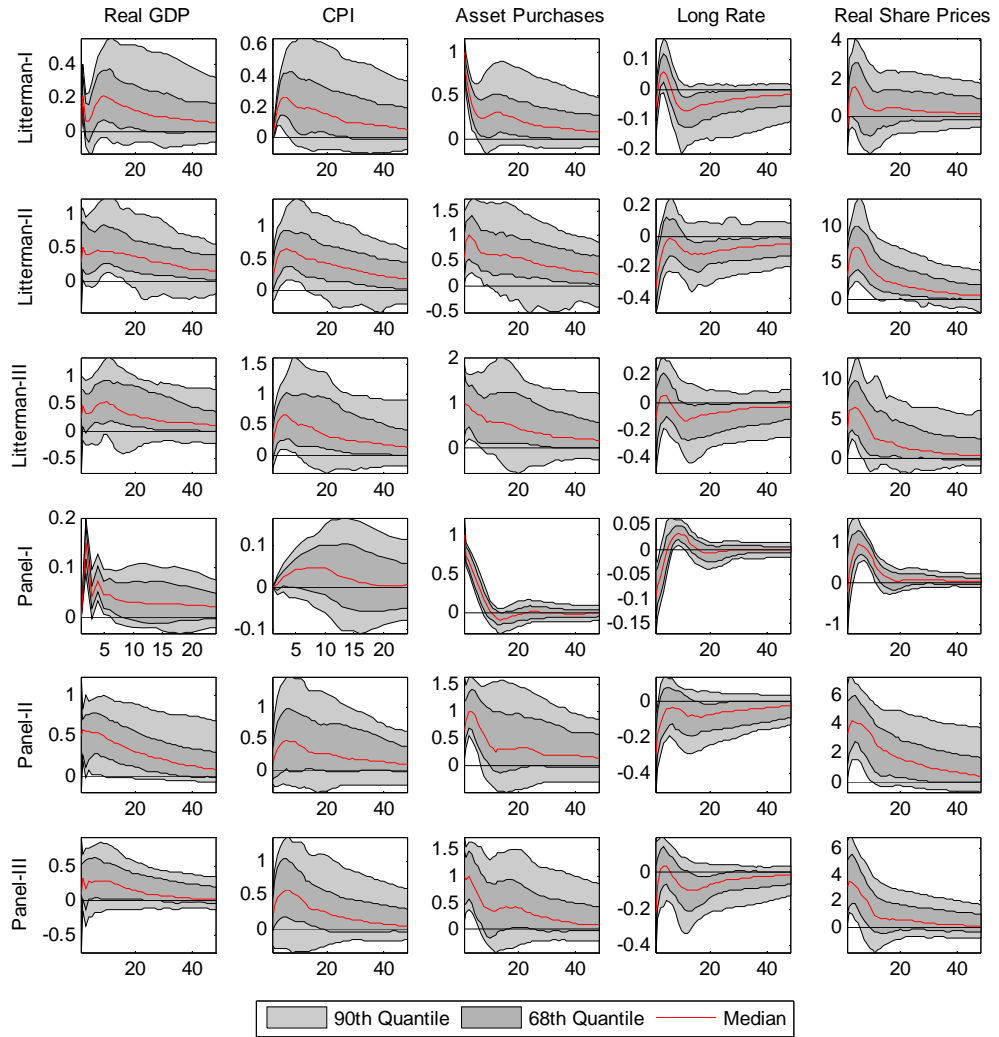


Figure B6: Results for UK with the Italian to German 10-year government bond yield spread as control variable

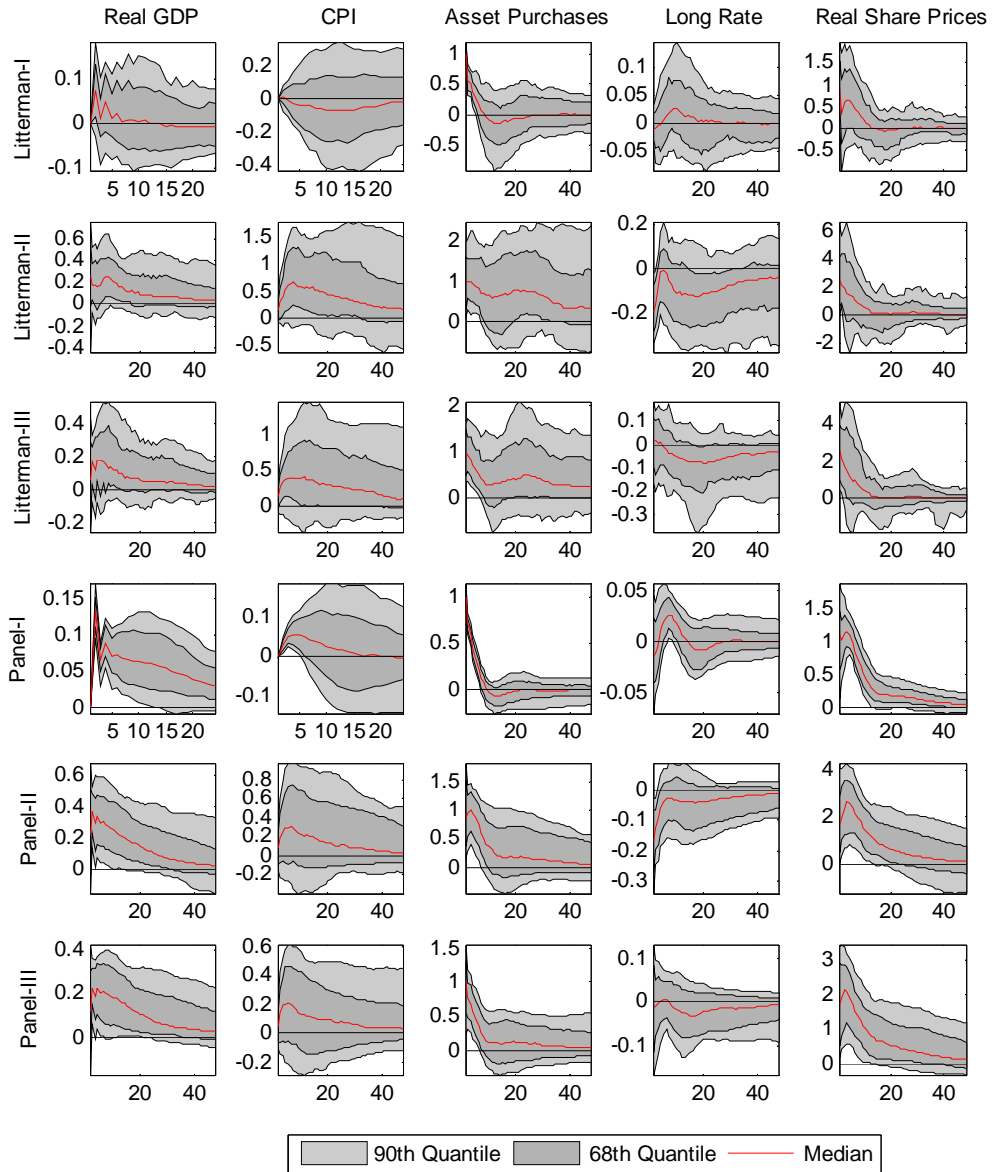


Figure B7: Results for USA with the real oil price as control variable

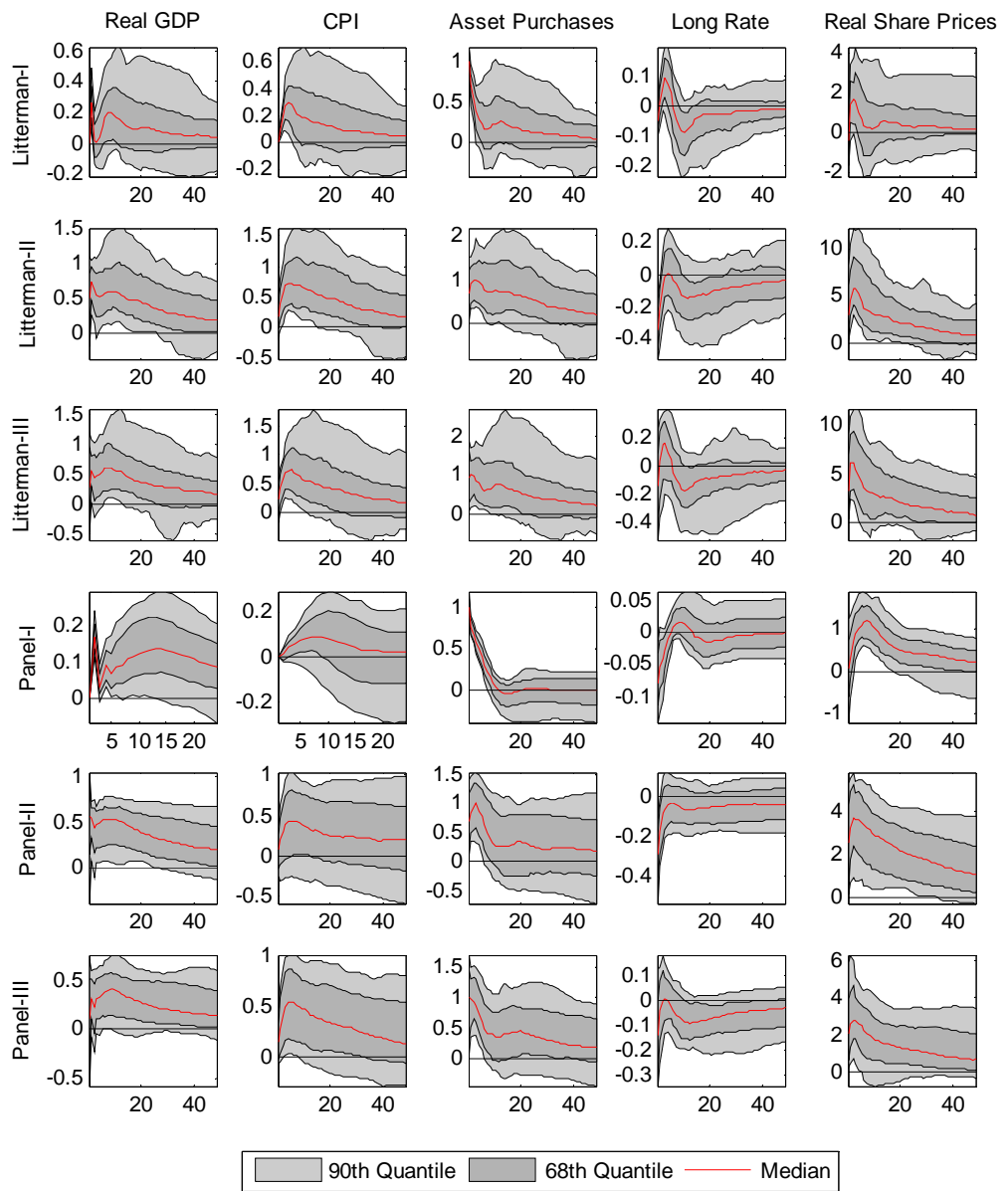


Figure B8: Results for UK with the real oil price as control variable

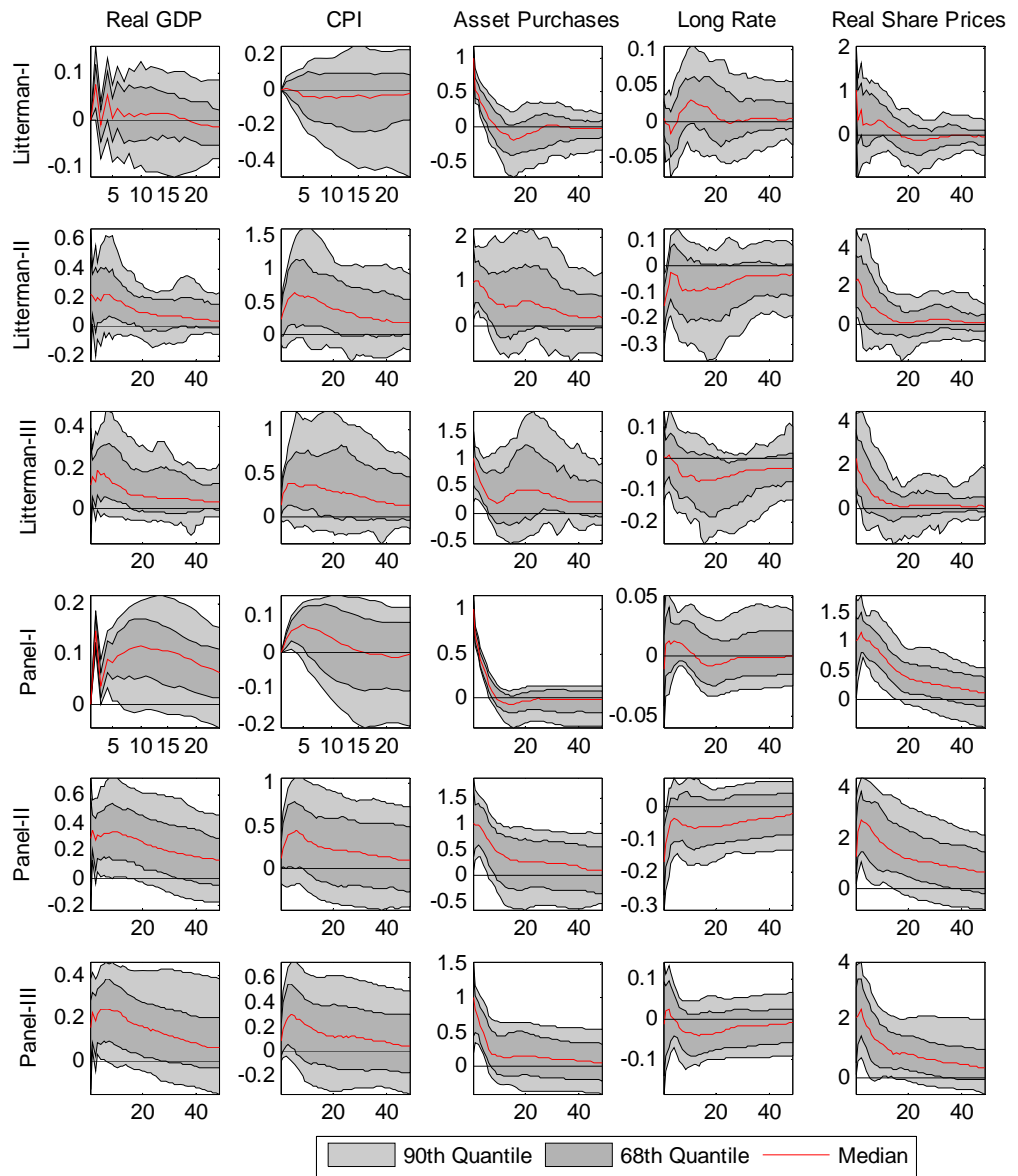


Figure B9: Results for USA with ECB total assets to Euro Area GDP ratio as control variable

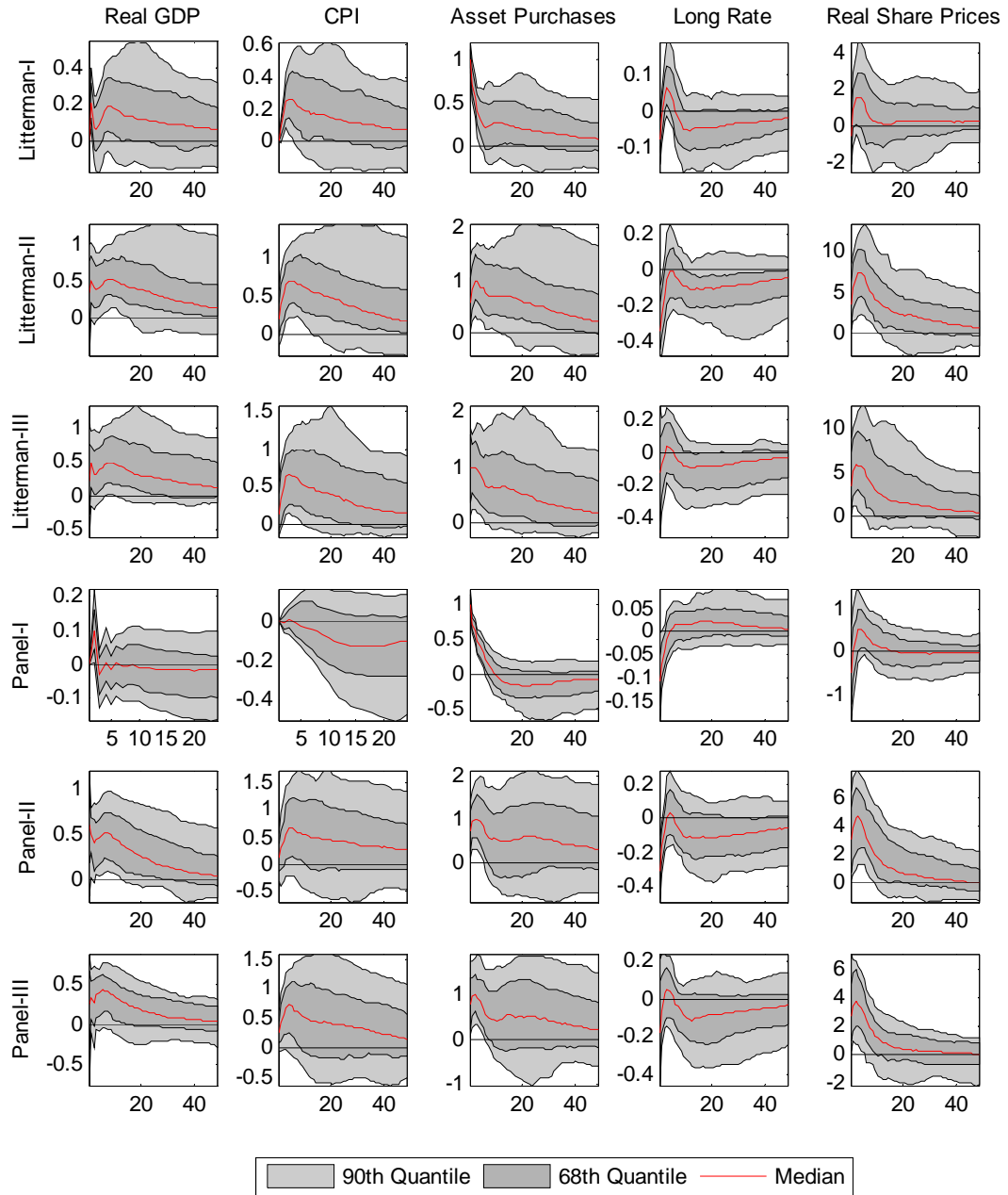


Figure B10: Results for UK with ECB total assets to Euro Area GDP ratio as control variable

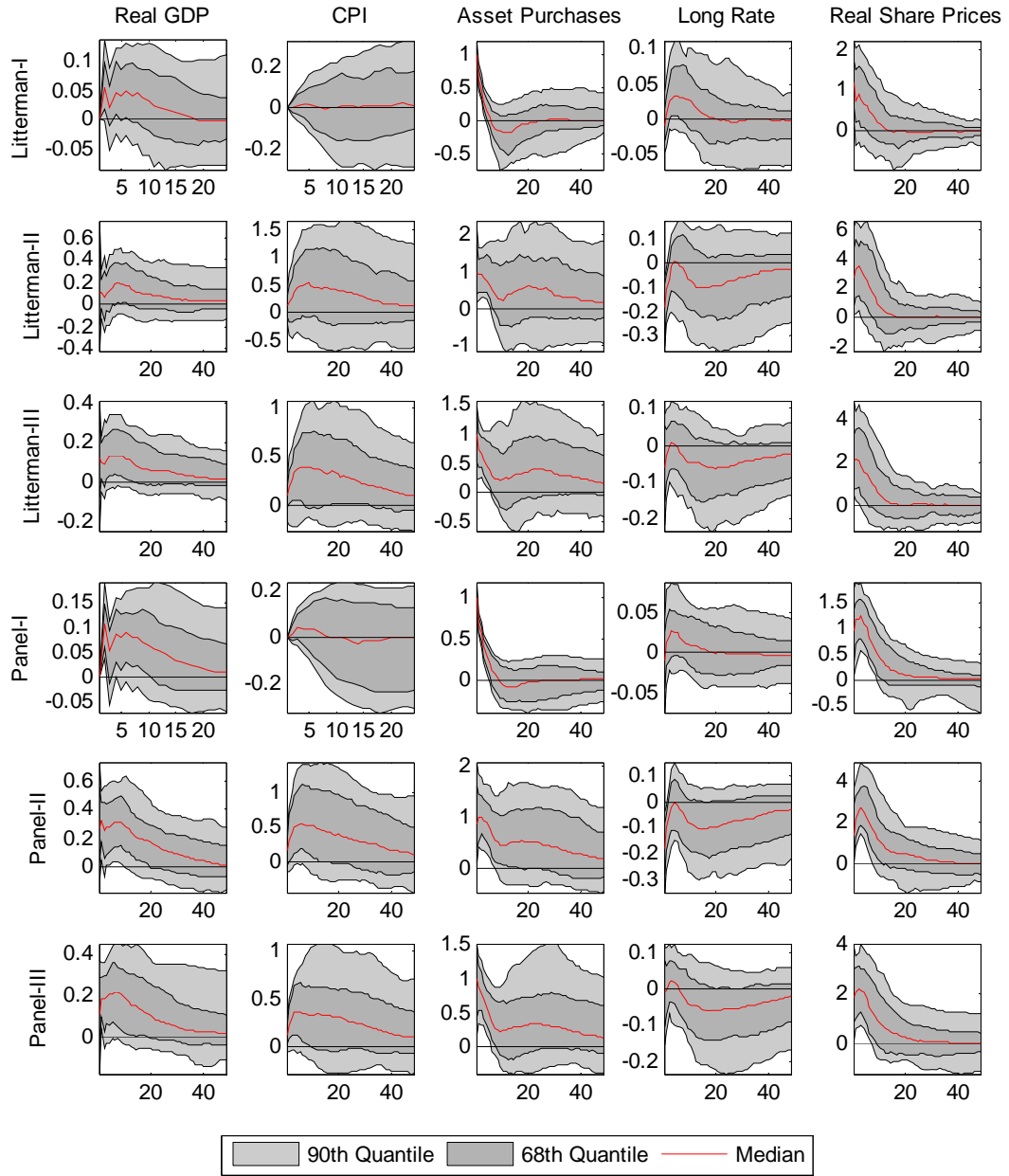


Figure B11: Results for USA with amount of assets purchased

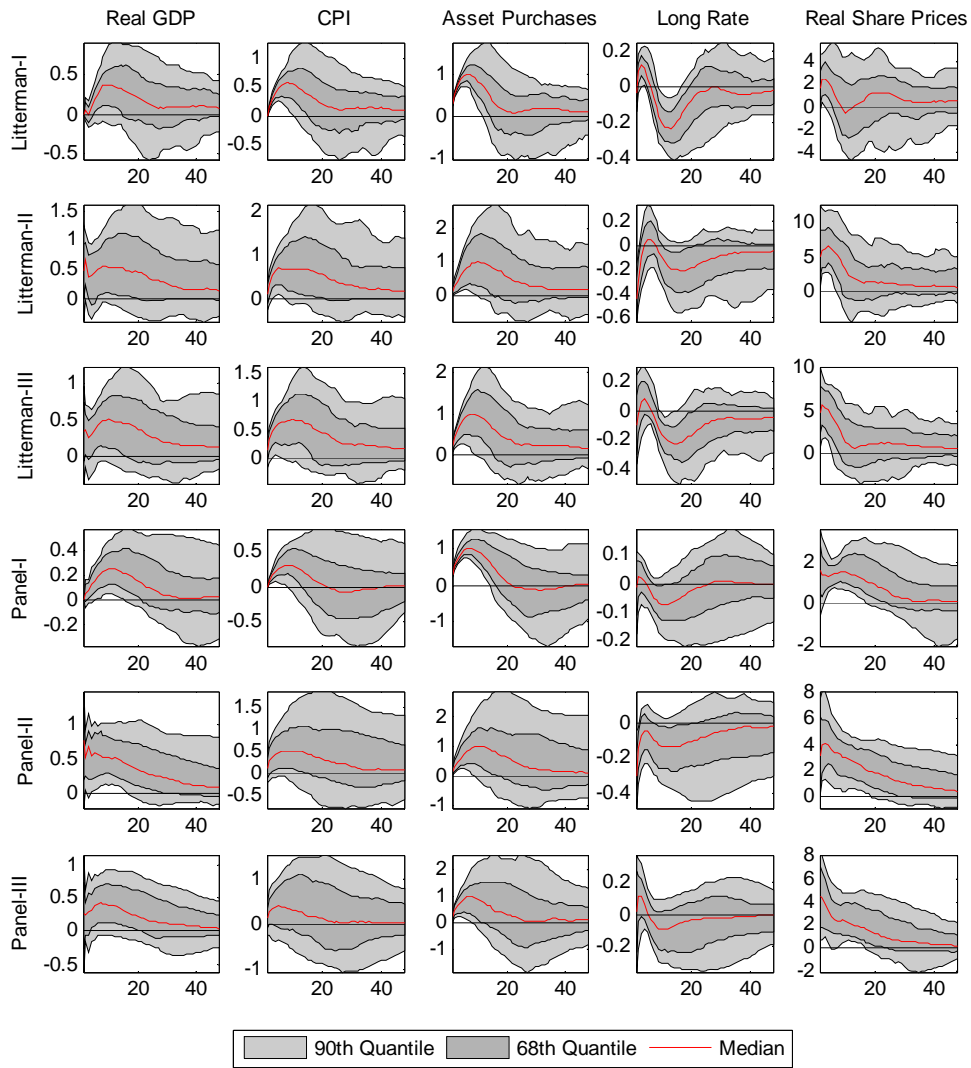


Figure B12: Results for UK with amount of assets purchased

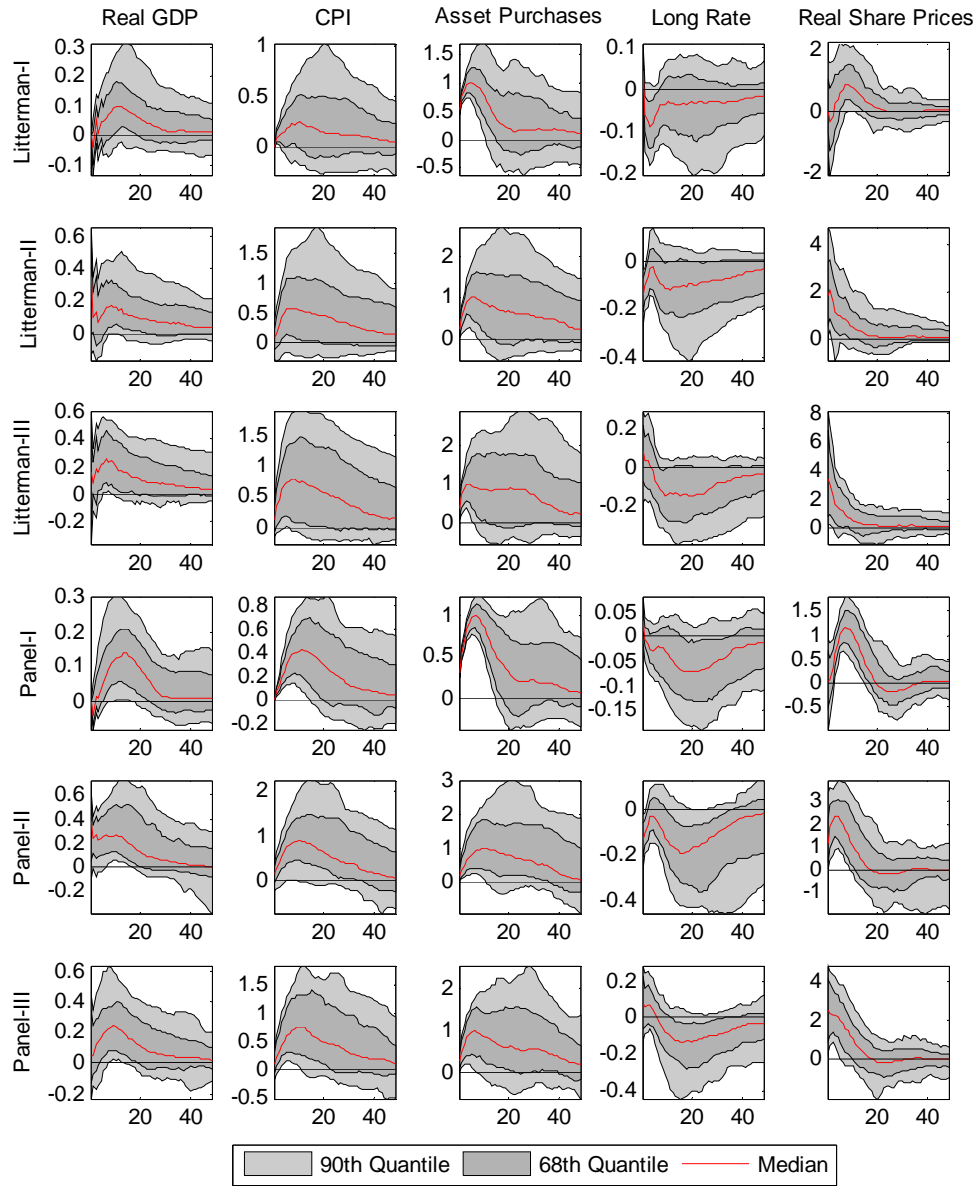


Figure B13: Results for US with smaller weight on Operation Twist Announcements

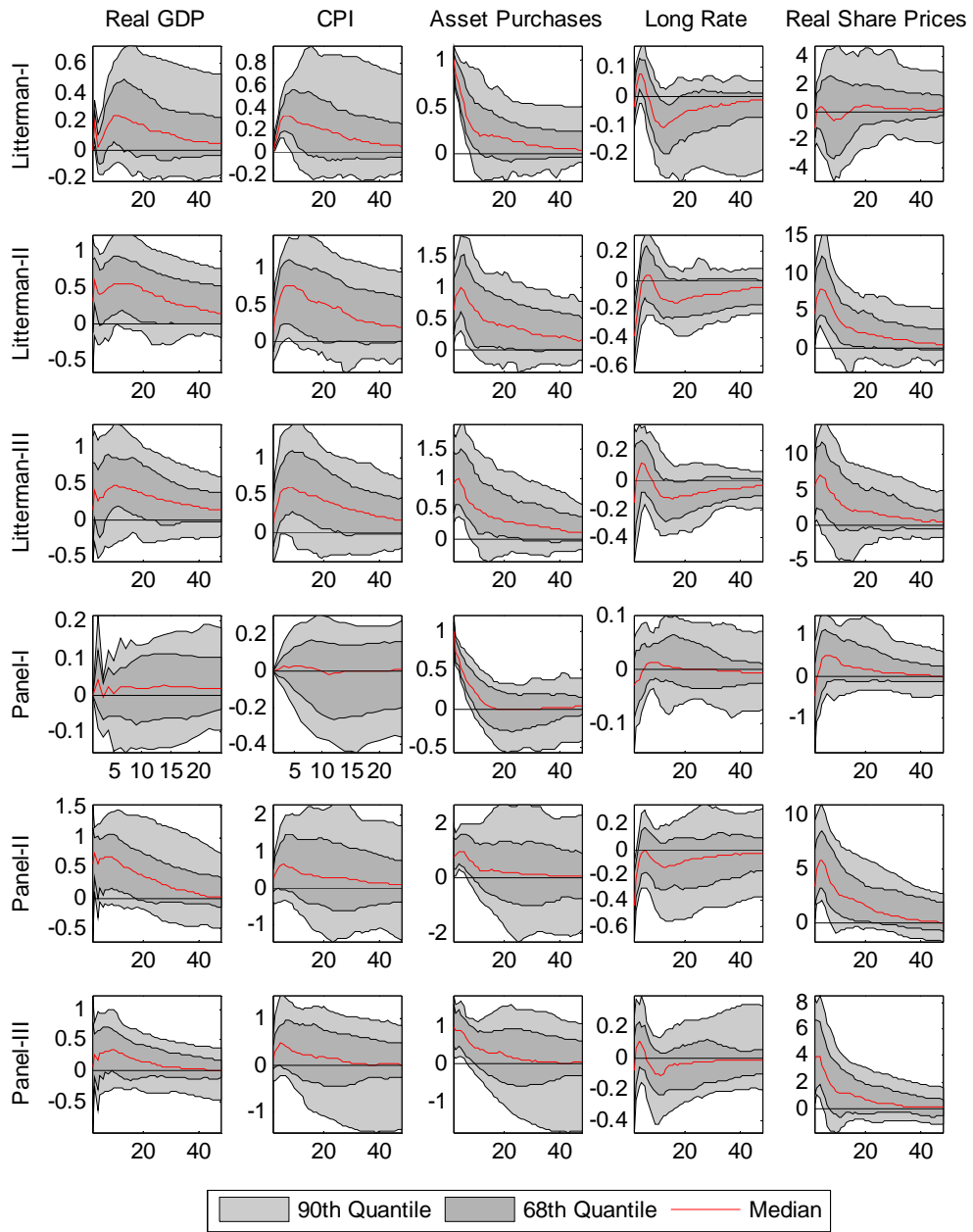


Figure B14 – Impact of US Openended Asset Purchases assumed to last 18 months

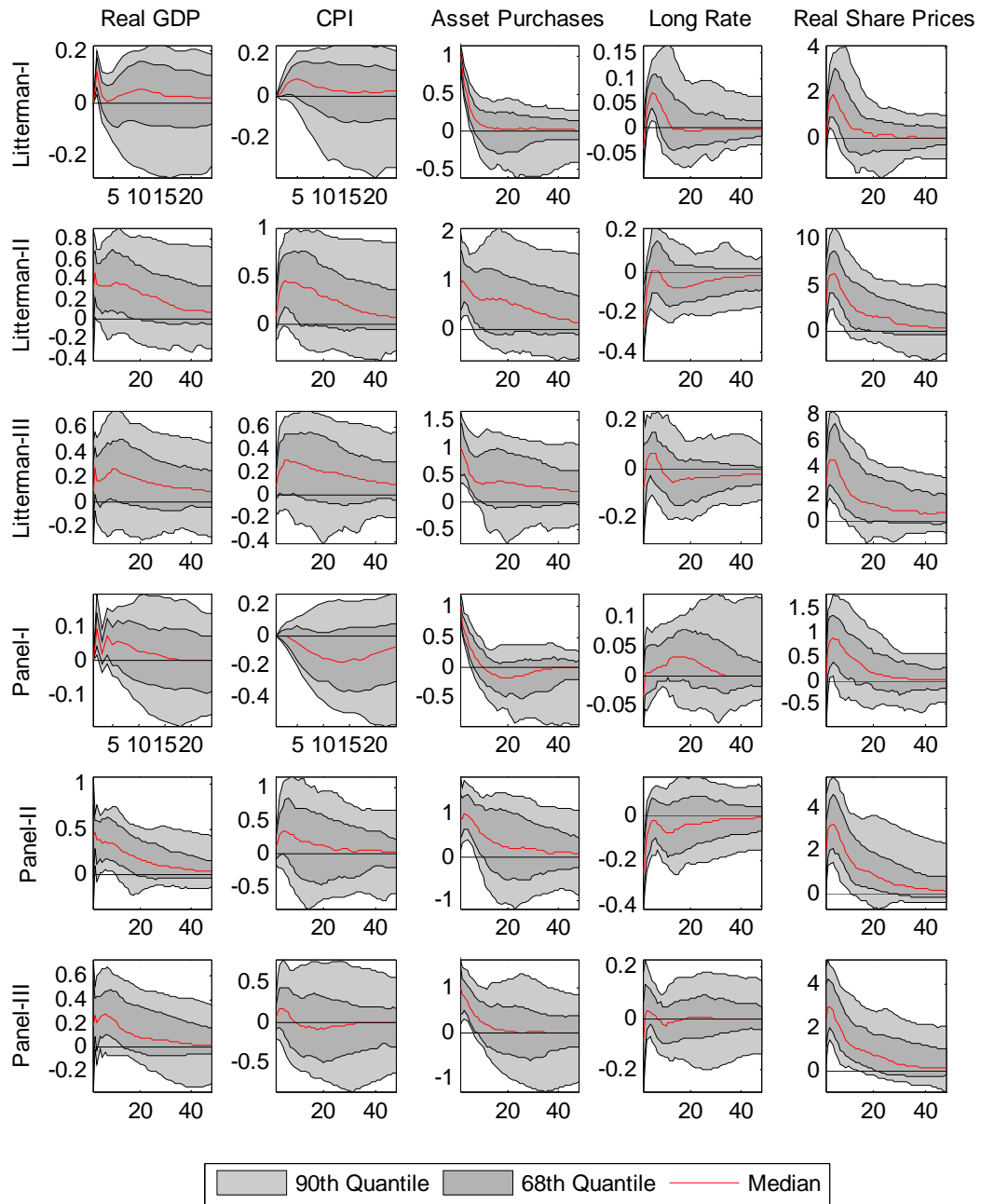


Figure B15 – Impact of US Openended Asset Purchases assumed to last 36 months

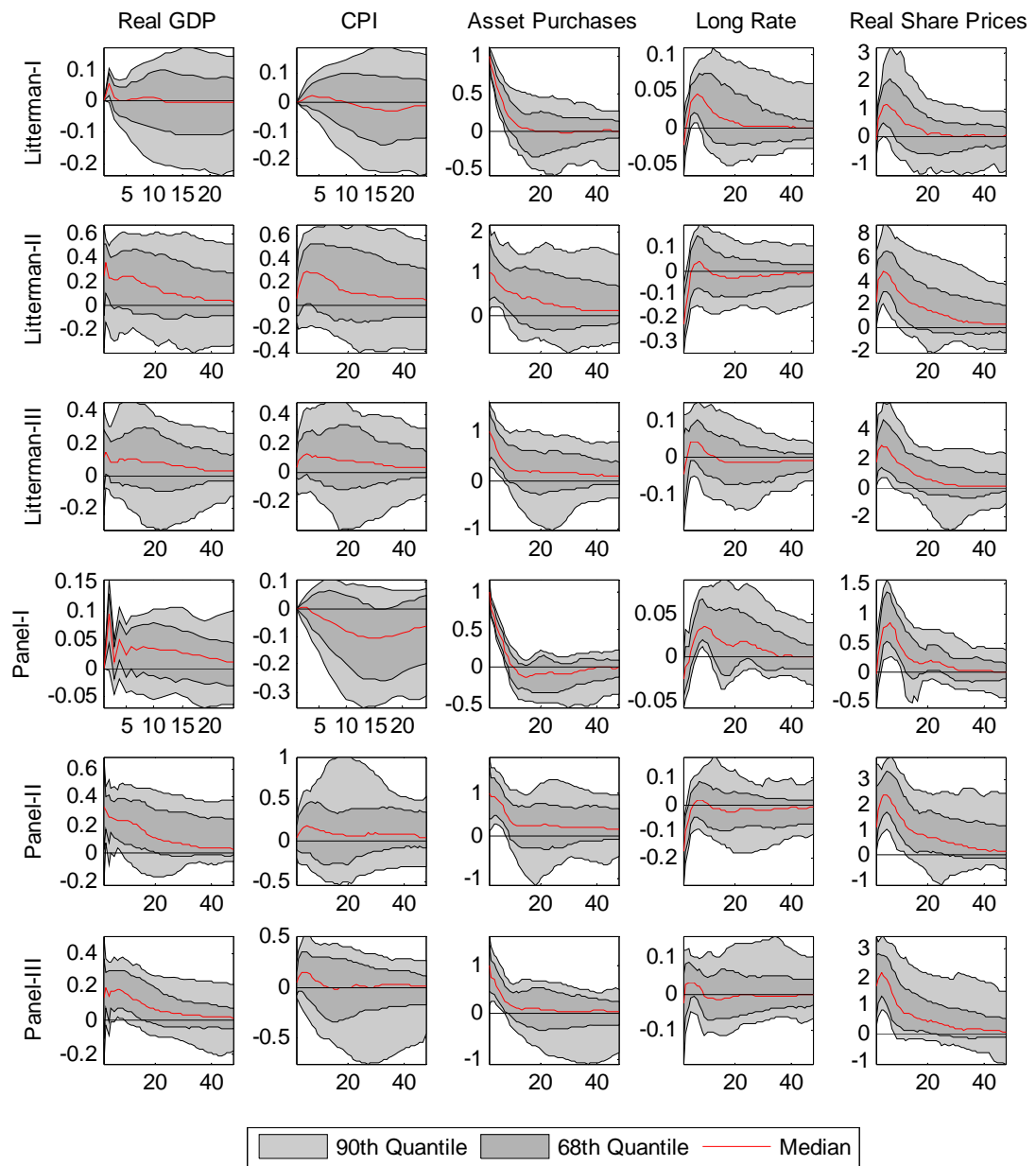


Figure B16 – Impact of Including MBS Asset Purchases assumed to last 18 months

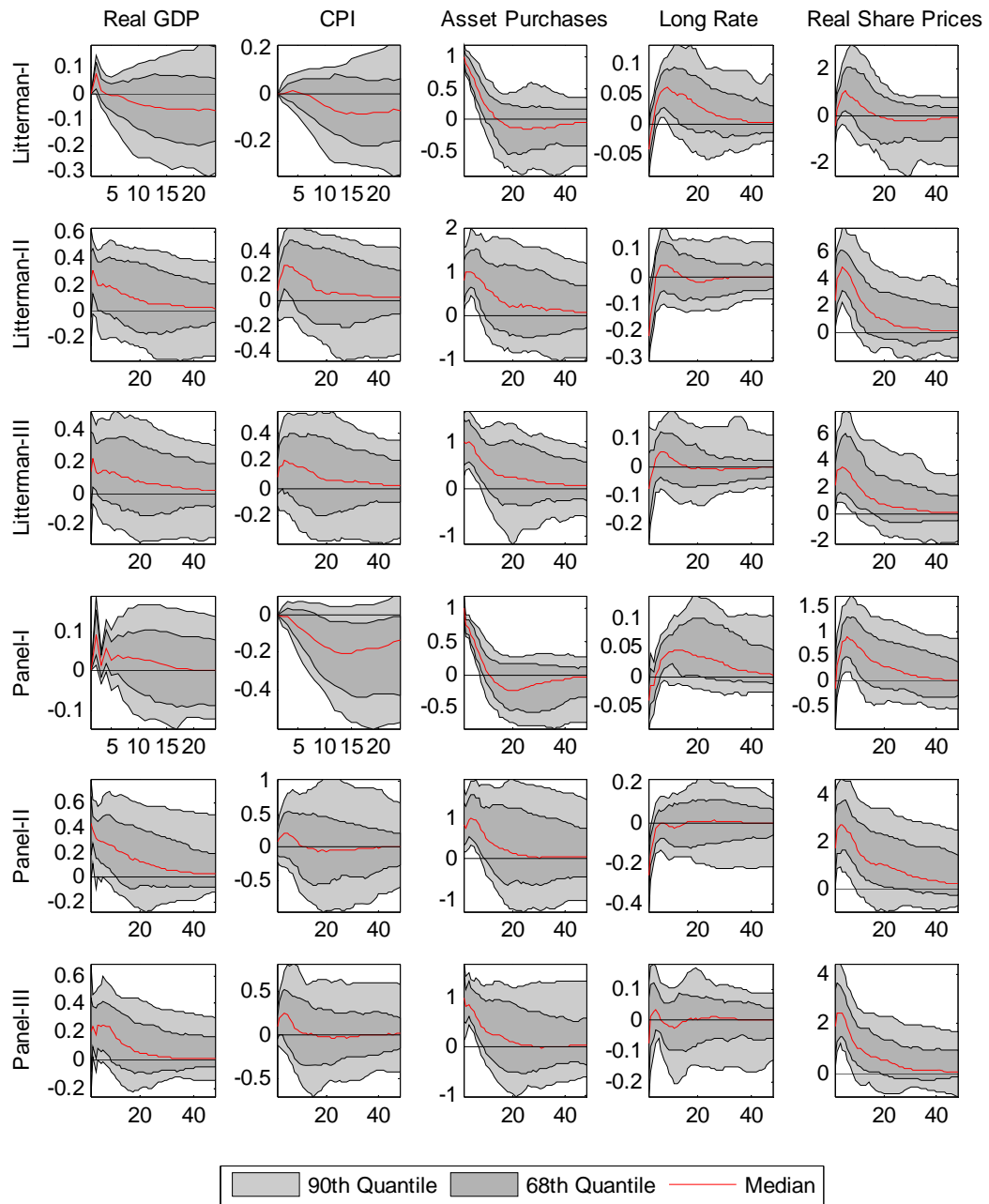
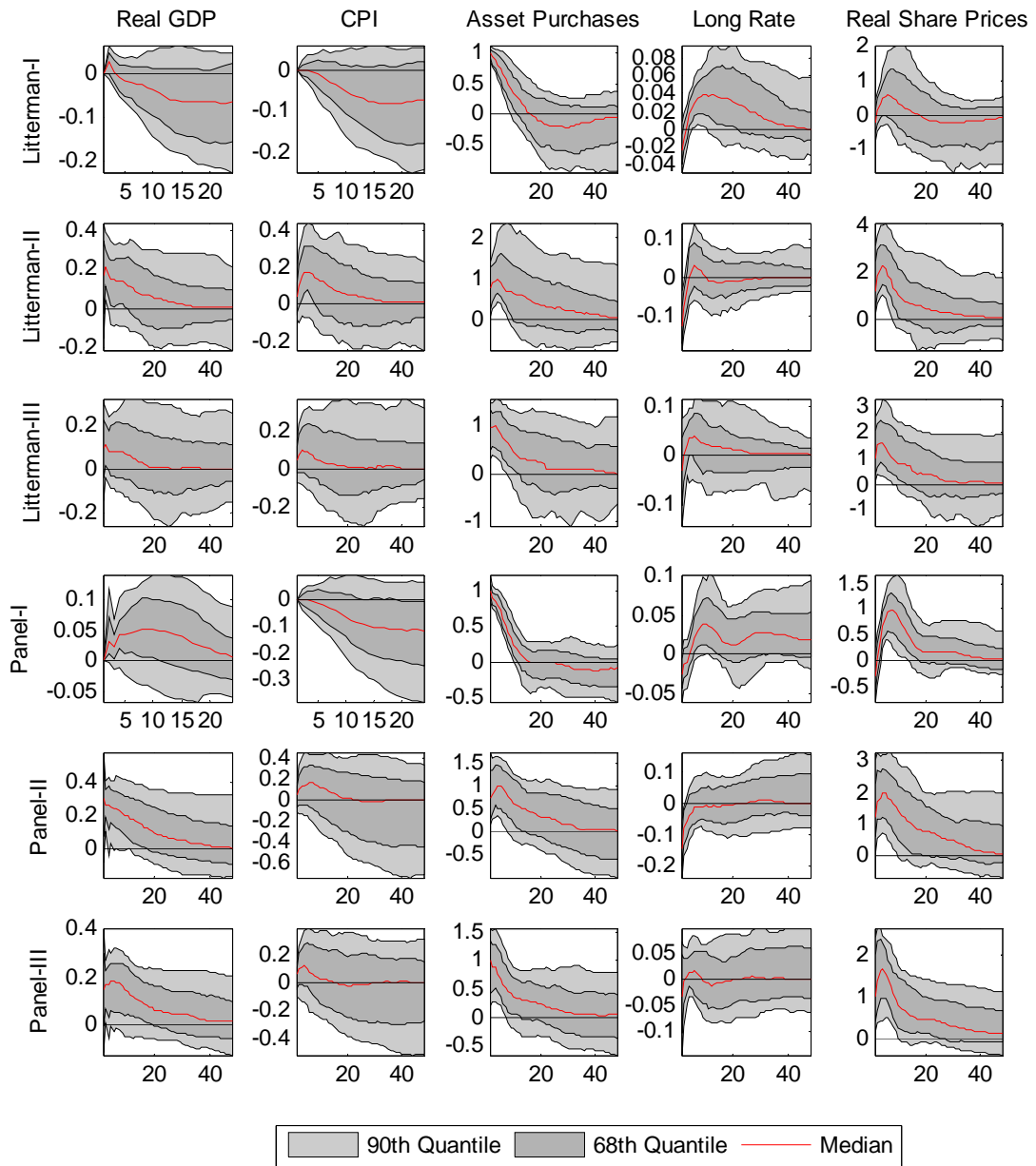


Figure B17 – Impact of Including MBS Asset Purchases assumed to last 36 months



Appendix C – The Gibbs sampler for the panel VAR model

Model (1) can be written as:

$$Y_c = X_c B_c + e_c$$

Jarocinski (2010) shows that based on the prior of a common mean, \bar{B} , the joint posterior of the model can be written as:

$$\prod_c |\Sigma_c|^{-\frac{T_c}{2}} \exp\left(-\frac{1}{2} \sum_c (y_c - \tilde{X}_c \beta_c)' (\Sigma_c^{-1} \otimes I_{T_c}) (y_c - \tilde{X}_c \beta_c)\right)$$

$$\lambda^{-\frac{CNK}{2}} \exp\left(-\frac{1}{2} \sum_c (\beta_c - \bar{\beta})' L_c^{-1} \lambda^{-1} (\beta_c - \bar{\beta})\right) \prod_c |\Sigma_c|^{-\frac{N+1}{2}} \lambda^{-\frac{v+2}{2}} \exp\left(-\frac{1}{2} \frac{s}{\lambda}\right)$$

where $\tilde{X}_c \equiv I_N \otimes X_c$, $y_c \equiv \text{vec}(Y_c)$, $\beta_c \equiv \text{vec}(B_c)$ and $\bar{\beta} \equiv \text{vec}(\bar{B})$. Based on this posterior, it is easy to derive the conditional distribution for the Gibbs sampler and estimation of the model. In particular, the model can be estimated by Gibbs sampling through iteratively drawing from the following distributions. The country-specific VAR coefficients β_c are drawn from:

$$p(\beta_c | \bar{\beta}, Y_c, \Lambda_c) = N((G_c)^{-1} (\Sigma_c^{-1} \otimes X_c') \text{vec}(Y_c) + \lambda^{-1} L_c^{-1} \bar{\beta}, (G_c^{-1})) \quad (C1)$$

where $G_c = \Sigma_c^{-1} \otimes X_c' X_c + \lambda^{-1} L_c^{-1}$. $\bar{\beta}$ is drawn from:

$$p(\bar{\beta} | \beta_c, \Lambda_c) = N((\lambda^{-1} \sum_c L_c^{-1})^{-1} \lambda^{-1} \sum_c L_c^{-1} \beta_c, (\lambda^{-1} \sum_c L_c^{-1})^{-1}) \quad (C2)$$

λ is treated as a hyper parameter and drawn from the following inverse gamma 2 distribution:

$$p(\lambda | \bar{\beta}, \beta_c, L_c^{-1}) = IG_2(s + \sum_c (\beta_c - \bar{\beta})' L_c^{-1} (\beta_c - \bar{\beta}), CNK + v) \quad (C3)$$

A completely non-informative prior with s and ν set to 0 results in an improper posterior in this case. We therefore set both of the quantities to very small positive numbers, which is equivalent to assuming a weakly informative prior. But it is important to point out that λ is estimated from the total number of country-specific coefficients that this prior is applied, namely the product of country (\mathbf{C}), equations (\mathbf{N}) and coefficients in each equation (\mathbf{K}). Given this large number of effective units, any weakly informative prior will be dominated by the data. Finally, the country-specific variance matrix of the residuals, Σ_c , is drawn from an inverse-Wishart distribution:

$$p(\Sigma_c | \beta_c) = IW(U_c' U_c, T_c) \tag{C4}$$

where $U_c = y_c - \tilde{X}_c \beta_c$ and T_c is the number of observations for each country.