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WHAT ARE THE MACROECONOMIC EFFECTS OF ASSET PURCHASES?†

Abstract

We examine the impact of large scale asset purchase announcements 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 2009M3 to 2014M5. We identify an asset purchase announcement shock with four different identification schemes, always leaving the reactions of real GDP and CPI unrestricted, to test whether these variables react to asset purchases. We then explore the transmission channels of this policy. The results suggest that an asset purchase announcement of 1% of GDP leads to a statistically significant rise of .58% (.25%) and .62% (.32%) rise in real GDP and CPI for the US (UK). In the US, this policy is transmitted through the portfolio balance channel and a reduction in household uncertainty. In the UK, the policy seems to be mainly transmitted through the impact on investors' risk appetite and household uncertainty.

JEL Classification: E50, E51 and E52
Keywords: Bayesian VAR and unconventional monetary policy

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† These Discussion Papers report on research carried out by, or under supervision of the External Members of the Monetary Policy Committee and their dedicated economic staff. Papers are made available as soon as practicable in order to share research and stimulate further discussion of key policy issues. However, the views expressed in this paper are those of the authors, and not necessarily those of the Bank of England or the Monetary Policy Committee. We gratefully acknowledge the comments of Ricardo Reis, Jonathan Wright and an anonymous referee. We are also grateful to Alina Barnett, Fabio Canova, Richard Portes, Jochen Schanz, Matthew Tong, Rohan Churm, Helene Rey, Tao Zha and participants at the European Central Bank conference on international spillovers and the National Bank of Poland workshop on identification in macroeconomics for useful suggestions on an early version of this paper.

1 Introduction

In response to the 2008-9 financial crisis, both the Bank of England and the Federal Reserve introduced large-scale asset purchases (LSAP) of government debt as an additional policy instrument. A number of academic studies have examined what the effects of this unconventional policy were. For example Chung et al. (2012) used the Federal Reserve Board's macroeconomic model to show that real GDP and inflation were respectively three and one percent higher as a result of US LSAPs. Kapetanios *et al.* (2012) used a range of BVAR methods to explore the effects of the Bank of England's purchases, finding that GDP and CPI were raised by 2.5% and 1.5% as a result of the first round of asset purchases in the UK.

In this paper we take previous work on asset purchases in four new directions. First, in contrast to most existing studies, we examine three proposed mechanisms through which asset purchases influence output and prices. Secondly, the passage of time, together with use of monthly data, allows us to estimate our model using only data since March 2009 when the policy was first introduced. This makes our results less susceptible to bias from the introduction of the new policy regime and hence the Lucas Critique than any other empirical study of this issue. We also explore whether our results are materially affected if the acute phase of the crisis, in 2009, is omitted from our data. Thirdly, effects found in VAR-based studies (e.g. Kapetanios *et al.* (2012)) were identified on the assumption that asset purchases led to a rise in real GDP and CPI only through their impact on the long-term interest rate. Instead, we use four different identification schemes to identify asset purchase shocks, leaving both the transmission mechanism and the responses of real GDP and CPI unrestricted. Thus we do not exclude *a priori* the possibility implied by Eggertson and Woodford (2003) that, except as a result of signaling the path of future short-term interest rate movements, asset purchases have no impact on GDP or CPI; we also estimate how much of the impact is transmitted through the bond and equity markets. Finally, all existing VAR studies rely on the imposition of either Litterman or time-varying parameter priors. Our analysis is carried out using a completely non-informative prior, avoiding possible bias from priors that are set too tight to let the data speak.

Theoretically, asset purchases might affect demand through three different mechanisms. The first is the so-called portfolio balance channel (Vayanos and Villa, 2009). This relies on the presence of investors with a preferred habitat for a given maturity in the government bond market. If this is the case, purchases of long-term government debt have the effect of reducing yields on debt of the maturities purchased, through their impact on term premia. An alternative mechanism, proposed by Bauer and Rudebusch (2014), is the signaling mechanism - that asset purchases provide a signal that the policy interest rate is going to remain at its effective lower bound for longer. This channel, working through the level of the average expected short-rate underlying a bond, also implies some decline in long rates. Thus, while both mechanisms result in lower government bond yields, they clearly work through different parts of yield curve. A third possible transmission mechanism is that asset purchases reduce uncertainty about financial markets and the economy more generally. All of these channels might lead to wealth effects from higher asset prices and induce households and firms to reduce precautionary savings, raise investment and also take more risk.

Our modeling framework allows us to explore which mechanisms may play a role by including relevant variables in the VAR one-by-one. If the portfolio balance (signaling) mechanism is behind the reduction in government bond yields, one should observe a relatively greater reaction of government debt yields (interest rate futures) at longer (shorter) maturities. We include both variables to establish whether either mechanism is relevant. We also include the VIX and a weighted average of implied interest rate futures' volatilities (MOVE) to examine the impact on uncertainty and risk-taking.

It is well known that estimates from small VARs may be subject omitted variable bias. Effects attributed to asset purchase announcements could therefore be merely a reflection of other coincident economic developments. We show that our reported estimates are robust to including range of variables as additional controls in our VARs and also that our conclusions about significance are robust to a range of different possible definitions of the asset purchase announcements.

Our results suggest that an asset purchase announcement shock worth 1% of nominal GDP, leads a rise of about .58% (.25%) of real GDP and .62% (.32%) in CPI in the US (UK). These findings are encouraging, because they suggest that asset purchases can be effective at stabilising output and prices.

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 on the CPI, but for the UK, our results suggest that the impact on the CPI is almost three times as large as the effect reported in these studies. The implied UK inflation-output trade-off is larger than in the US, meaning that the same change in GDP would have a greater impact on UK inflation. These estimates are, however, in line with studies of conventional monetary policy for the UK and US. Furthermore, from 2010 until late 2013, the UK's Monetary Policy Committee was repeatedly surprised by unexpectedly high inflation. Our results suggest that some of this may have been a consequence of previously under-estimating the effects of asset purchases on inflation. Interestingly, we also find that, if we estimate our model using data from before as well as after asset purchases started, the estimated effect on inflation is similar to that found in earlier studies.

We find evidence that US asset purchases influence yields on medium and long-term government debt, but not interest rate futures, which implies a role for the portfolio rebalancing, rather than the signaling, channel. In contrast, UK purchases do not have clear impacts on either interest rate futures or long rates. In both countries there is evidence that announcements have the effect of reducing financial market uncertainty.

This can reflect either real economic uncertainty (Bloom, 2009) or investors' risk appetite (Adrian and Shin (2010), Bruno and Shin (2012)). The effects on household survey measures of uncertainty and the BBB less AAA corporate bond spread suggest that the former is the relevant channel for the US, while the latter is more relevant for the UK. Thus it is clear that the transmission mechanism differs in different countries and policy-makers should not assume that the experience of one country will necessarily be replicated in another. Nevertheless, in a counter-factual exercise, we show that transmission through equity and government bond markets accounts for about a half of the overall impact of asset purchases on both GDP and CPI in both the UK and US.

The remainder of this paper proceeds as follows. Section two explains our model and discusses the details of our identification schemes. Section three presents the results and section four concludes.

2 Methodology and data

We use the following VAR model:

$$\boldsymbol{Y_t} = \boldsymbol{\alpha_c} + \sum_{k=1}^{L} \boldsymbol{A_k Y_{t-k}} + \boldsymbol{e_t} \qquad \boldsymbol{e_t} \sim \boldsymbol{N}(\boldsymbol{0}, \boldsymbol{\Sigma}) \qquad (1)$$

where $\boldsymbol{Y_t}$ is a vector of the following endogenous variables: the announcement of asset purchases divided by nominal GDP; the log of CPI; the log of real GDP; the yield on the 10-year government bond and the log of real equity prices at time $\boldsymbol{t}$. $\boldsymbol{A_k}$ is the array of coefficients associated with the corresponding lagged vector of variables for lag $\boldsymbol{k}$. $\boldsymbol{e_t}$ is a vector of residuals at time $\boldsymbol{t}$. This is assumed to be normally distributed with variance-covariance matrix $\boldsymbol{\Sigma}$. When the time-series dimension is small, estimates of $\boldsymbol{A_k}$ are likely to be imprecise. Previous work has addressed this problem by relying on Bayesian methods of inference and imposing a Litterman (1986), or time-varying parameter, prior. But there is always the risk that tight priors dominate information from the data. Our approach avoids this problem.[1] We use a completely non-informative Normal Inverse-Wishart prior, following the approach in Uhlig (2005).[2] We assume a lag length, $\mathbf{L}$, of two throughout.[3]

2.1 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 using a matrix $\boldsymbol{C_0}$, such that $\boldsymbol{C_0 e_{c,t}} = \boldsymbol{\varepsilon_{c,t}}$. We use four ways of inferring $\boldsymbol{C_0}$, zero restrictions, sign restrictions, a combination of zero and sign restrictions, and finally sign variance decomposition restrictions.

We first identify asset purchase shocks using 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 these are reasonable identification assumptions. 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, on the grounds that such restrictions do not naturally emerge from DSGE models. Canova and De Nicolo (2002), Faust and Rogers (2003) and Uhlig (2005) have therefore proposed identifying shocks by means of the

[1]Jarcocinski and Marcet (2013) propose imposing priors on the growth rates of variables, as oppose to priors on parameters, as the least controversial way to impose priors in small sample VARs. This approach is clearly very sensible in normal times, when variables such as real GDP and CPI inflation grow at a steady and predictable pace. During a period of economic turbulence however, the tightness on these priors may need to be loosened substantially to accommodate actual data outturns. It is also unclear how to set a credible growth rate prior for asset purchase announcements, the long rate and real equity prices during this time. To accommodate all of this uncertainty, it is necessary to set very loose priors, in which case the results are similar to those from the non-informative prior we adopt in this paper.

[2] Uhlig (2005) sets the hyper-parameters for the prior equal to zero to ensure that it is completely non-informative. This is identical to estimating the mean parameters via OLS and generating Bayesian credible sets through Monte Carlo simulations, which is the approach that we follow. See appendix D of his paper for more information.

[3] *Ex ante* lag length tests such as the Hannan-Quinn or BIC criterion suggest a lag length of 2. 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.

implied signs of the impulse responses that they produce.[4] Clearly, for identification restrictions of this type to be valid, they need to be strongly supported by economic theory. In standard DSGE models (e.g. Eggertson and Woodford, 2003), quantities of asset purchases do not affect either GDP or CPI. However, in the presence of financial frictions, such as 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 long-term bonds, by reducing term premia. But even in the absence of frictions, announcements of asset purchases can signal that the short-term interest rate is going to stay lower for longer (Bauer and Rudebusch, 2014). The associated reduction in the expected average short-term interest rate over the maturity of given government bond, will therefore also lead to a fall in the long-term interest rate. In other words, both the portfolio balance and the signaling mechanism lead to a decline in the long-term interest rate. This is the first identification assumption that we need to make. Secondly, lower yields on longer maturity bonds are likely to lead to some reallocation towards other assets, such as equities, leading to a rise in real equity prices. Thus our definition of an asset purchase shock is that it leads to lower yields on long-term rates 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 in expected profits and thus to a rise in real equity prices. The sign restrictions we use to identify an aggregate supply shock are identical, other than assuming that prices fall rather than rise. 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). Unless otherwise noted, we impose all sign restrictions upon impact and one month thereafter with the exception of asset purchase

[4] 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.

announcements, where we impose the sign restriction upon impact and for five months thereafter here and also in identification schemes III and IV.

Table 1 – Identification scheme II

	y Log real GDP	p Log CPI	AP Asset Purchases	i_t Long Interest Rate	sp_t Log Real Equity Price
Supply Shock	+	−		+	+
Demand Shock	+	+		+	+
Asset Purchase Shock	?	?	+	−	+

This table shows the sign restrictions imposed in identification scheme II

In identification scheme II, the assumption is that asset purchases affect the real economy via portfolio rebalancing from long-term government bonds into equities, to distinguish them from aggregate supply and aggregate demand shocks. But *a priori* it is not clear to what extent the mechanisms that are required for asset purchases to affect the yield on long-term government debt operate in reality. More importantly, to distinguish asset purchase from aggregate supply shocks, it was necessary to assume that long-term interest rates rise in response to an aggregate supply shock. Theoretically, a positive aggregate supply shock may lead to a rise in investment, competition for funds and higher bond yields, but also a decline in bonds yields as a result of the monetary policy reaction to lower consumer prices. Empirically, Dedola and Neri (2007) and Peersman and Straub (2009) examine the reaction of the short-term interest rate in response to technology shocks in SVARs for the US and Euro Area, respectively. Peersman and Straub (2009) show a positive medium-term reaction of the short rate to technology shocks, while Dedola and Neri (2005) find no significant effect. While the long-rate restrictions are therefore consistent with their results, we nevertheless drop them in identification scheme III below.

Table 2 – Identification scheme III

	y Log real GDP	p Log CPI	AP Asset Purchases	i_t Long Interest Rate	sp_t Log Real Equity Price
Supply Shock	+	−	0		
Demand Shock	+	+	0		
Asset Purchase Shock	?	?	+		+
Uncertainty shock			+		−

Table 2 shows the sign and zero restrictions imposed in identification scheme III.

This is possible, 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 equity prices is sufficient to distinguish these shocks from asset purchases. Given that monetary policy makers do not observe aggregate demand or supply shocks within a month, 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, perhaps as a result of a coincident financial crisis. Unlike demand and supply, these types of shocks can be observed in real time. This identification scheme is summarised in table 2 and referred to as identification scheme III throughout. We implement it with the procedure in Arias, Rubio-Ramirez and Waggoner (2014), who generalise the standard QR restrictions algorithm to include zero restrictions as well.

Identification schemes I – III rely on the idea that shocks can be distinguished based on restrictions on impulse responses. But it is also possible to use variance decomposition restrictions to separate different economic shocks (Faust and Rogers, 2003; Uhlig, 2005). The idea here is that a shock that is variable-specific should explain the largest fraction of the variance in that variable.[5] For example, short-rate shocks should explain the greatest fraction of the forecast error variance in the short-term interest rate. In identification scheme IV, we therefore require that asset purchase announcement shocks explain the largest fraction of variation in asset purchases upon impact and with a three period delay. This allows us to drop the zero restrictions and we do not have to rely on sign restrictions on the long rate to separate aggregate demand and supply shocks either. We implement this scheme in a similar fashion to identification scheme II, with the QR approach by Rubio-Ramirez, Waggoner and Zha (2010), but rather than keeping impulse responses which are consistent with a particular sign, we only keep those consistent with the variance decomposition restrictions in table 3.

Table 3 – Identification scheme IV

	y Log real GDP	p Log CPI	AP Asset Purchases	Variance Decomposition Restrictions
Supply Shock	+	−		$\frac{Var(Shock)}{Var(Asset\ Purchases)} < MAX(\frac{Var(Shock)}{Var(Asset\ Purchases)})$
Demand Shock	+	+		$\frac{Var(Shock)}{Var(Asset\ Purchases)} < MAX(\frac{Var(Shock)}{Var(Asset\ Purchases)})$
Asset Purchase Shock	?	?	+	$\frac{Var(Shock)}{Var(Asset\ Purchases)} = MAX(\frac{Var(Shock)}{Var(Asset\ Purchases)})$

Table 3 shows the sign and variance restrictions imposed in identification scheme IV.

At present the transmission mechanisms of asset purchases are not sufficiently well understood to devise an identification scheme which would allow us to identify asset purchase announcement shocks perfectly. It is for this reason that we sequentially relax the strongest identification restrictions from the first scheme to the last one. Our first

[5] Our approach is similar in spirit, but not technique, to the penalty function approach first proposed in Uhlig (2005), which is designed to maximise the variance forecast error decomposition of a variable, by penalising (or ruling out) impulse responses which are deemed to be unreasonable. Arias, Rubio-Ramirez and Waggoner (2014) show that this approach tends to impose additional restrictions, even on variables that were left unrestricted and leads to artificially narrow impulse response bands. We implement their technique, the same as in Rubio-Ramirez, Waggoner and Zha (2010) and just select decompositions that are associated with the maximum variance forecast error decomposition of our main variable of interest, but we do not rule out any responses in the way that Uhlig (2005) does.

identification scheme relied on zero restrictions, which were then relaxed by adopting sign identification restrictions in identification scheme II. The possibly incredible restrictions on the long-term interest rate were then relaxed in identification scheme III, by using both zero and sign restrictions. Finally, we were able to drop the zero restrictions by relying on variance decomposition restrictions instead in identification scheme IV. Despite this pecking order, it is nevertheless not possible to claim that one scheme is necessarily better identified or preferable to another. As a result we study the effects of asset purchases in all four cases paying particular attention to results which are significant with at least three of the four schemes adopted in this paper.

2.2 Data

All of the VAR models in this paper are estimated on monthly data for the period when asset purchases were an active policy tool in both the UK and the US, from 2009m3 to 2014m5. Monthly real GDP data for the UK are provided by the National Institute of Economic and Social Research (Mitchell, Smith, Weale, Wright and Salazar, 2005) and the series used is fully consistent with the September 2014 vintage of official quarterly data[6], while monthly real GDP data for the US are taken from Macroeconomic Advisers.

The monthly indices of consumer prices are the official measures published in the two countries. UK Value Added Tax, an important fiscal contributor to CPI movements, was reduced from 17.5 per cent to 15 per cent in January 2009, raised to 17.5 per cent in January 2010 and raised further to 20 per cent in January 2011. In order to avoid the distortions introduced by these erratic price movements we work with the monthly CPIY index produced by the Office for National Statistics; this measures price movements excluding the immediate effects of changes to indirect taxes. For the United States we use the consumer price index published by the Bureau of Labour Statistics. Real equity prices are calculated by obtaining monthly averages of daily data for the FTSE100 and S&P500

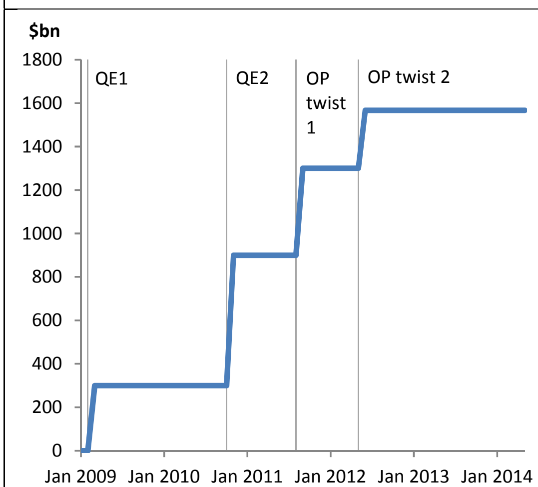
[6] This was the first vintage of data which included all of the changes in national accounts associated with the introduction of SNA 2008/ ESA 2010. This has changed the path of real GDP in the UK substantially, for example the 0% growth recorded in Q4 2011 was revised up to a growth rate of 0.5%.

obtained from Thomson DataStream and deflating by the CPI for the US and CPIY for the UK.

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 referred to as Operation Twist) as additional asset purchases, attaching the same weight to them as asset purchase announcements 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 section 3.4. The asset purchase series we use are shown in figure 1 below. Unlike the UK, the US also announced open-ended asset purchases. The effects of these are also explored further in section 3.4. The series showing the stock of actual assets purchased for the UK has been kindly provided by the Bank of England. For the US, these data are taken from the Federal Reserve Bank of St. Louis Data Archive (FRED).

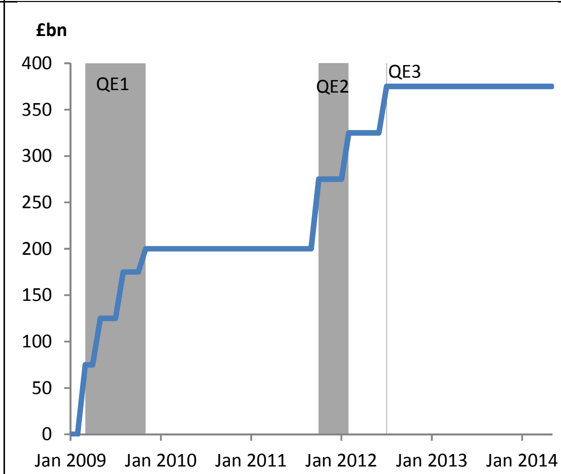
Figure 1: Asset Purchase Announcements in US and UK

US Asset Purchase Announcements



Source: FOMC minutes.

UK Asset Purchase Announcements



Source: MPC minutes.

This figure shows the timing of asset purchase announcements in US and UK, together with the total amount of assets purchased at book value. The US data do not include announcements about open-ended purchases. These are discussed in section 3.4.

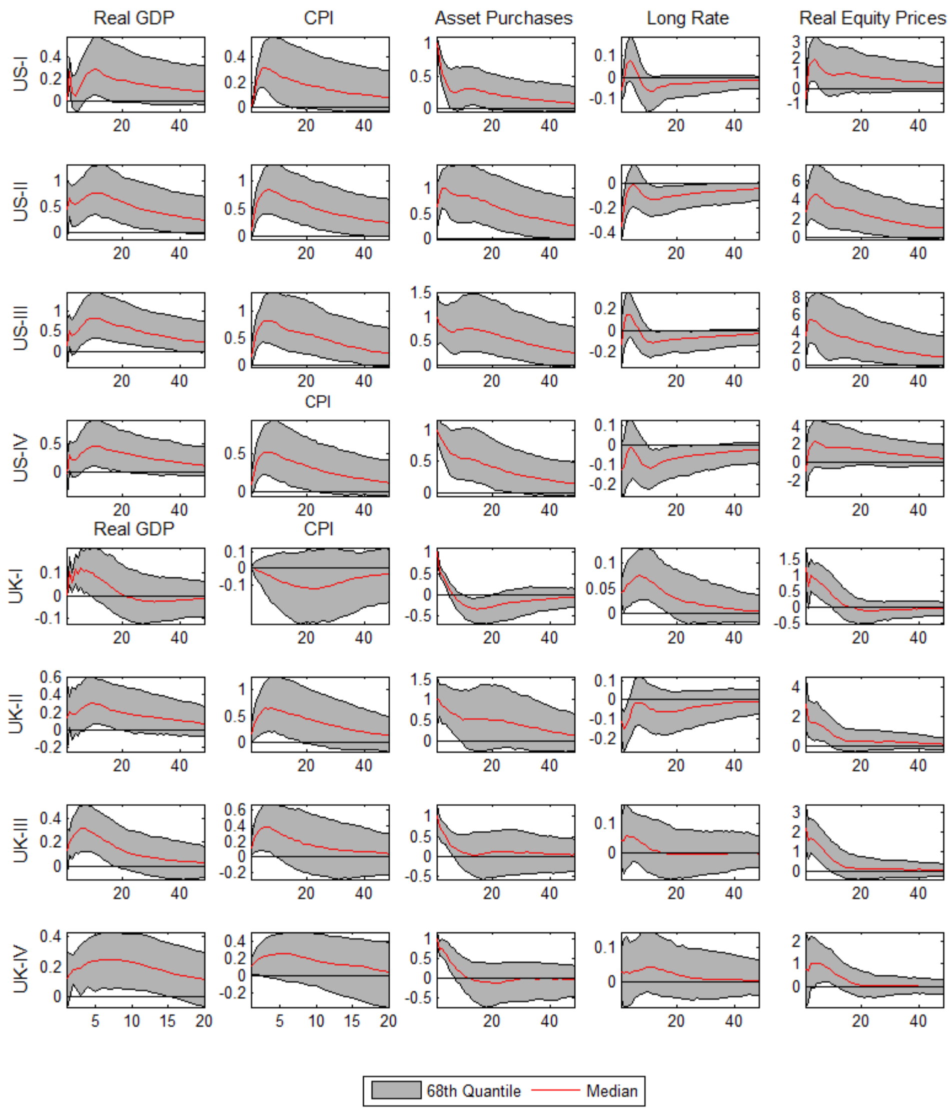
We also examine to what extent asset purchases had an impact on other variables in order to explore the three possible transmission mechanisms. As discussed by Christensen and Rudebusch (2012), the portfolio rebalancing channel implies a compression of term premia, while the signaling channel leads to a decline in the average expected level of short-term interest rates over the maturity of a bond. Either channel would result in a decline in the long-term government bond yield. But if the portfolio balance channel is the main transmission channel, one would expect a relatively larger impact on the yields of twenty and thirty-year government bonds. The signaling mechanism, on the other hand, implies a relatively stronger reaction of the Overnight Index Swap (OIS) futures of the three-month interest rate, six months, one year and two years ahead. We also examine the impact of our identified asset purchase shocks on two financial market indicators of uncertainty: the VIX (implied stock market volatility) and the MOVE (weighted average of implied interest rate volatilities at different horizons). To tease out whether movements in these variables reflect economic uncertainty or investors' risk appetite, we also look at a survey measure of household uncertainty and the BBB-AAA corporate bond spread. Details of the data are provided in appendix D.

3 Results

3.1 Main results

Figure 2 shows the results for both countries for each of our four identification schemes, with the row labels indicating the scheme in question. Table A1 in Appendix A shows the maximum impacts of the median and * next to the figure indicates that zero is outside the associated 68% Bayesian credible set.

Figure 2: Results for the standard specification



This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase announcement as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

An inspection of figure 2 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 identification scheme I for CPI in the UK. For both countries the maximum values for the impact on both GDP and CPI are higher with identification schemes II, III and IV than they are with scheme I. This probably reflects the role that economic theory plays in identifying the effects with these schemes. Averaging across all four schemes, however, we find that the maximum impact on GDP is 0.58 in the United States and 0.25 in the United Kingdom (Table A1). The figures for the CPI are 0.62 and 0.32, respectively.

To compare our multipliers to those presented in previous work, we compare the effects of US and UK QE1 implied by the impulse responses in those studies, to the ones implied by the impulse responses in this paper. 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 100 basis points in the spread between the long-term and short-term interest rate. It is then easy to see that the estimates in those papers imply a rise of 1.08 (2.5) percent and 0.9 (1.5) percent in GDP and CPI in the US (UK), respectively. During QE1, the Federal Reserve and the Bank of England engaged in government bond purchases worth two and fourteen per cent of annualised 2009Q1 GDP, respectively. Based on the estimates in this paper, this would lead to a rise of 1.12 (3.08) percent and 1.2 (4.2) percent in US (UK) real GDP and CPI, respectively. When the effect of MBS purchases is included, the estimates for US real GDP and CPI become 1.4 and 1.5. For the UK, the impact on real GDP is similar to previous work, but CPI response is almost three times as large. This difference is not statistically significant but produces estimates for the inflation-output trade-off which are more in line with previous studies of conventional monetary policy in VARs for the UK.[7]

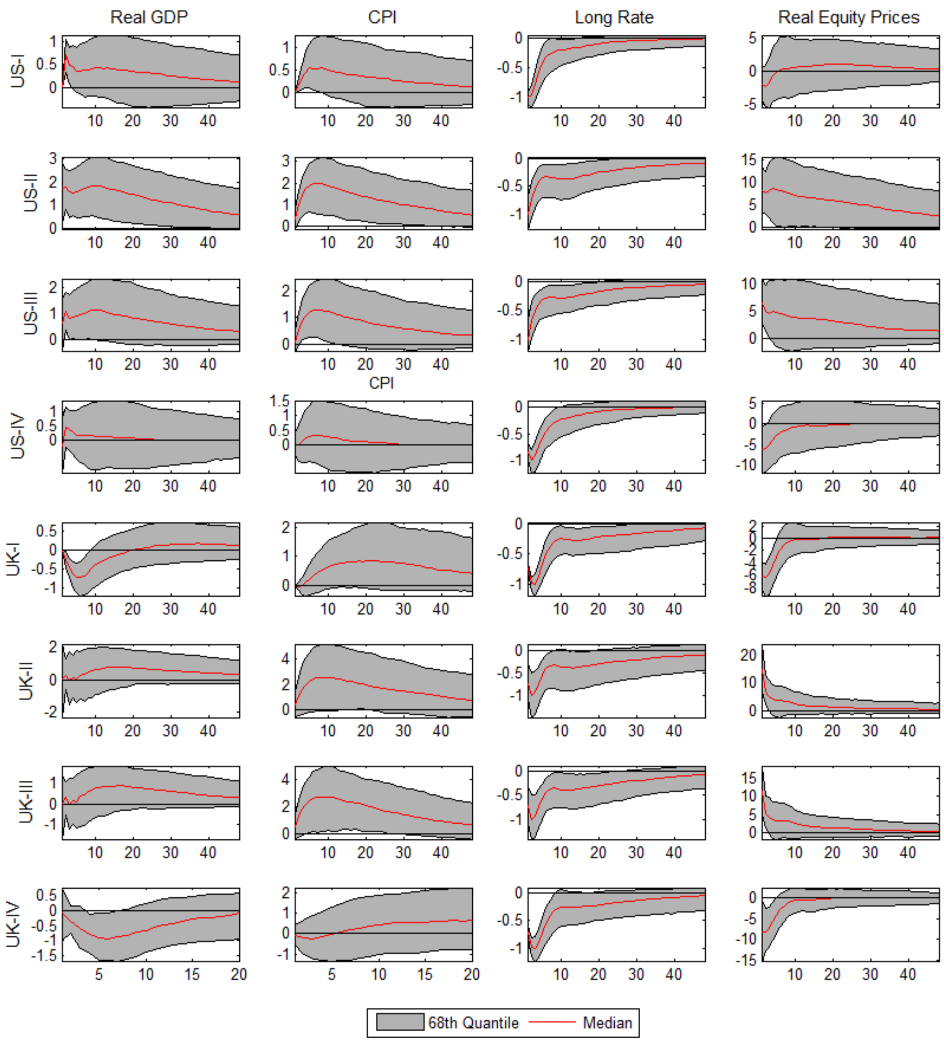
These quantitative differences may arise as a result of the identification scheme or the data on which the model was estimated. We explore both of them below. First, we modify our identification schemes to correspond as closely as possible to those adopted by previous work. Baumeister and Benati (2013) and Kapetanios et al (2012) assume that asset purchases lead to a decline in the long-short rate spread, a rise in real GDP and CPI and no reaction of the short rate to identify their spread shock. Since our models are

[7] These calculations and a description of the previous work can be found in appendix C.

estimated over a period when the short-term interest rate was constant in both countries, this corresponds to a shock to the long rate in our setup. We therefore replace asset purchase announcements in our VAR with the long rate and apply exactly the same identification schemes to the long rate.[8] Figure 3 shows the impulse responses from this exercise and table A2 of Appendix A their maximum impact. An unexpected shock leading to a 100 basis points decline in the long rate has the effect of raising both GDP and CPI by about 1.06 percent in the United States. These quantitative magnitudes are almost identical to the effect of QE1 reported by Baumeister and Benati (2013) and the QE1 impact implied by our asset purchase announcement shocks. But it is not clear that long rate shocks have an impact on real activity in the UK. Taken together with the results from our baseline specifications, this suggests that UK asset purchases most likely affect real GDP and CPI through channels other than the long-term interest rate. But these results, and quantitative estimates of QE1 which are very similar to previous work and the estimate of QE1 obtained with our approach, are consistent with the idea that US asset purchase announcements affect real activity through their impact on long-term government bond yields. In other words, the spread identification scheme used by previous work might be correct for the US, but is probably not right for the UK.

[8] For identification scheme I, we order the long rate after output and prices, but before real equity prices. For identification scheme II, we assume that the long rate falls and real equity prices rise in response to unconventional monetary policy. This allows us to distinguish this shock from aggregate demand and supply shocks, for which the identification is unchanged. For the identification scheme III, we assume that aggregate supply and demand shocks cannot affect the long rate contemporaneously. The risk/uncertainty shock is then identified as a shock that leads to a decline in real equity prices and the long rate, while the unconventional monetary policy shock is identified as a shock that leads to a decline in the long rate and rise in real equity prices. Finally, in identification scheme IV, we assume that a long rate shock should explain the greatest fraction in the long rate forecast error variance decomposition. All other restrictions remain the same.

Figure 3: Results for a shock to the long rate



This figure shows, for each of the variables list above, the median impulse responses in response to a one percent decline in the long rate, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

In Appendix B we show that these results do not depend on the inclusion of the first round of asset purchases in the data set. Figure B1 shows results estimated over the period 2010m3-2014-m5. This omits the period when the financial crisis was at its most extreme. Our results are not greatly affected, suggesting that the impact of the second and third rounds of purchases in the UK and US was not very different from the impact of the first round. In other words, our evidence suggests that asset purchases did not become less effective over time. Figure B2 looks at results estimated from 2007m1-2014m5. We now find that the effects on GDP are larger than in figure 1. There is no significant effect on CPI in the UK with any of the identification schemes although the median impulse remains positive in all four cases. The inclusion of UK data before asset purchases were introduced might therefore explain why previous work found a smaller effect on CPI inflation; indeed our estimates for this period imply an inflation/output trade-off of 0.37 which is closer to the value of 0.6 found by Kapetanios *et al.* (2012). This confirms our view that analysis over this extended period may be subject to the Lucas critique: in this case it seems to bias the UK inflation response to unconventional monetary policy to be substantially lower than it is actually is. For the US, Tables B1 and B2 show that the quantitative magnitudes are larger, but the relative impact on output and inflation remains the same as in the base line case.

In summary, this suggests that the observed differences to previous work seem to arise from both the inclusion of pre-asset purchase data and the difference in identification schemes, but these issues create substantially larger biases for the UK than the US.

3.2 What is the evidence on the mechanisms by which asset purchases affect real activity?

Economic theory suggests that asset purchase policy can affect interest rates through three different channels:

1) Portfolio rebalancing. If investors have preferred habitats, then 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. This should be reflected in a reduction of the term premium rather than a reduction in expected future spot rates. An alternative friction which leads to similar effects comes from the presence of transaction costs leading to imperfect substitutability in the government bond market (Harrison, 2012).

2) Signaling future interest rates. Bauer and Rudebusch (2014) argue that any announcement of unconventional policy means that interest rates will be kept at the zero lower bound for longer.[9] In other words, the expected average short-term interest rate will decline as a result of the announcement. A reduction in either component, the term premium (portfolio rebalancing) or the average expected short-term interest rate (signaling) will lead to a decline in the long-term interest rate. But portfolio rebalancing, through the impact on term premia, is likely to have a relatively larger impact on twenty and thirty-year maturity government bond yields, while the signaling channel should be reflected in movements of short-term interest rate futures. We include all of these variables into our model to asses which effect is stronger.

3) A reduction in uncertainty. If asset purchases make people more confident that the monetary authorities have a means of supporting the economy despite the fact that short-term interest rates are at the zero lower bound, then the perceived variance of future output will decline. This is likely to be reflected as a decline in measures of financial market uncertainty. This can be interpreted as either a reduction in household uncertainty about durable consumption or greater risk-taking by investors, as they search for yield. Both of these channels can support demand by raising consumption and reducing financing costs for riskier firms in the real economy.

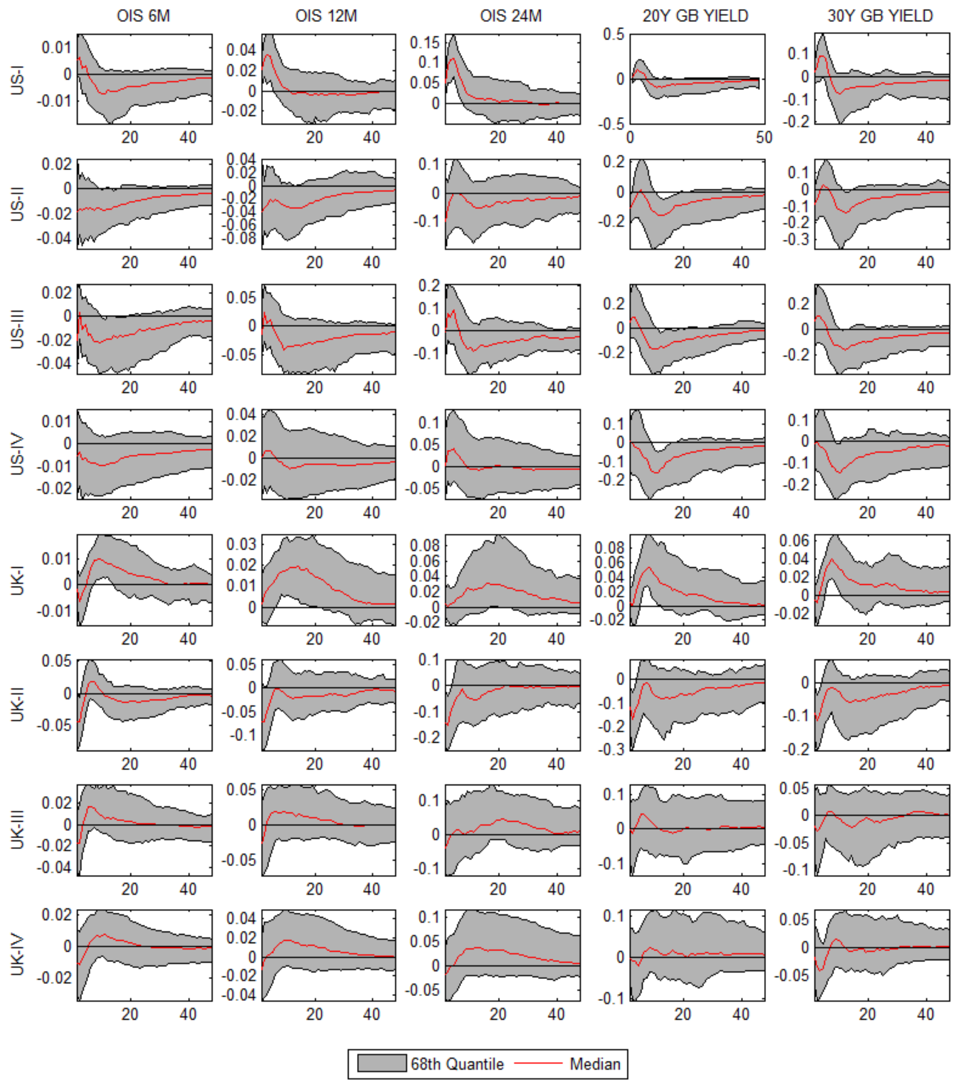
The reduced-form nature of structural VARs does not allow us to decompose the estimated impacts into contributions from these different transmission channels directly. We can, however, identify variables which we would expect to be significantly affected

[9] 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.

by asset purchase shocks if each of these mechanisms plays a role in the transmission of this policy.

The extent to which each transmission mechanism should operate in each country clearly depends on the presence of financial frictions in the government bond market. The average maturity of government bonds was 4.2 and 14 years in the US and UK government bond markets at the end of 2007, which implies greater liquidity premia (transaction costs) in the US government bond market. Similarly, Asian central banks are natural preferred habitat investors in the US government bond market (Turner, 2012). This descriptive evidence would suggest a greater *ex ante* role for the portfolio balance channel in the US, rather than the UK. To examine if this is the case, we include the yields on government bonds of twenty and thirty years maturity, as well as the three-month rate, six, twelve and twenty-four months ahead, as the 6th variable in the VAR. The results are shown in Figure 4 and the maximum impacts in table A3 of Appendix A.

Figure 4: Results for Portfolio Balance & Signaling Channels



This figure shows, for each of the variables list above, the median impulse responses in response to an unexpected one percent asset purchase announcement as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses. OIS 6M, OIS 12M and OIS 24M are the 3-month rate, 6 months, 12 months and 24 months ahead. 20Y and 30Y GB YIELD are the yields on 20 and 30 year government debt.

We can see in table A3 that, for US, two sets of results shows significant effects with at least three of the identification schemes. Twenty-year bond rates in the United States are influenced by asset purchase shocks with all four schemes, while thirty year bond yields are significant in three out of four schemes. While negative effects are found more generally, these cannot be regarded as significant. Table A3 also illustrates the problems associated with relying on only one identification scheme. In particular, identification scheme two suggests that most of the included variables are significant, particularly for the UK.

Looking first at the US, figure 2 shows that the impact on twenty-year debt yields is also present with ten-year debt, although of course for scheme II that was an identifying assumption. Interest rate futures tend to move in the right direction, but are not statistically significant. A reasonable conclusion is that these results provide evidence for the portfolio balance channel operating in the US, or at least that asset purchases affected security yields on long-term fixed-interest debt. In the UK there is greater sensitivity of the results to the identification scheme. Both the long-term government bond yield and interest rate futures react in a statistically significant manner only with scheme II. Thus the responses of OIS and long-term rates do not, overall, provide good evidence that either portfolio balance or signaling play significant roles in the UK.

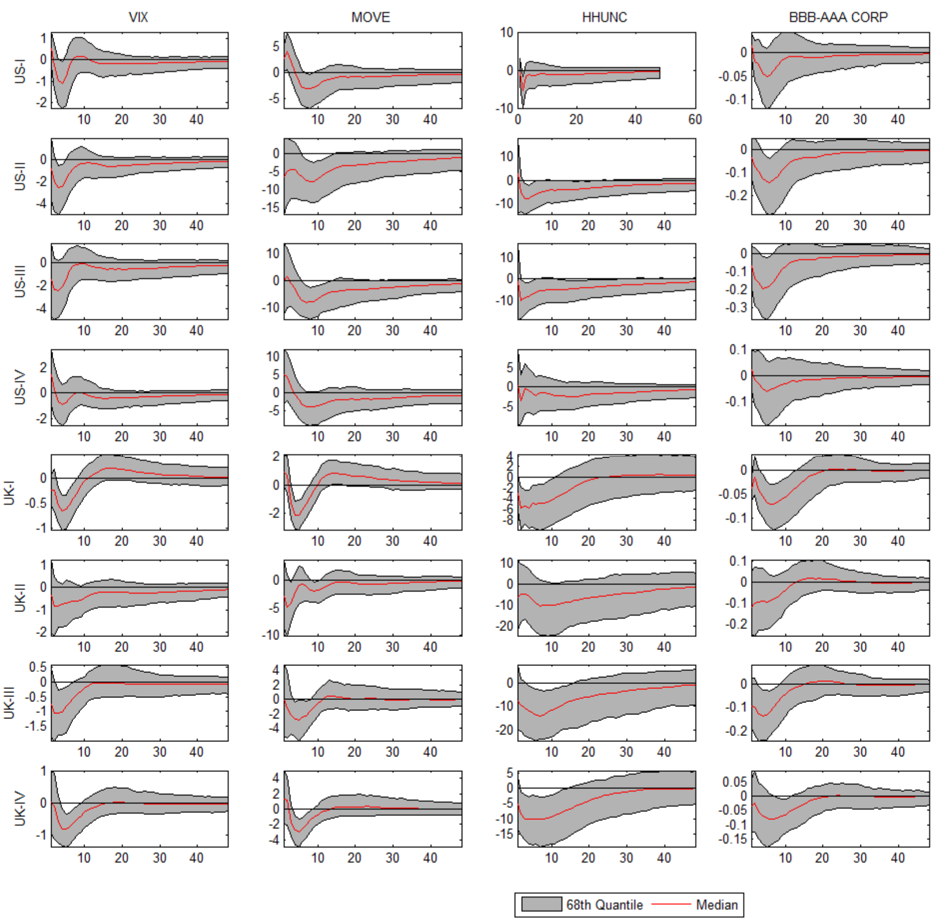
Christensen and Rudebusch (2012) use several dynamic term structure models to decompose the movements in long rates which they associate with asset purchases. They find that, in the US the movements were largely the result of expected future short rates (*signaling*) while in the UK falls in term premia dominated (*portfolio balance channel*). There are several reasons why our results may differ from those of Christensen and Rudebusch (2012). First of all, they estimate their models using daily data stretching from the late 1980s until the end of 2010, since a long sample is needed to mitigate biases in the estimation of their model. As a result they need to make the assumption that asset purchase announcements are just normal shocks to the Treasury bond market. However, if this assumption is violated, their results may be susceptible to the Lucas Critique. Secondly and perhaps more pertinently, they look at the immediate impacts of announcements of asset purchases at daily frequency, while we are interested in movements at lower frequencies. Thirdly, their model assumes that the path of interest rates can be represented by a Brownian motion. This assumption may be invalid for the shorter end of the yield curve whose path might be explicitly constrained by the zero lower bound.

Asset purchases can, of course, also have a direct impact on the real economy by reducing uncertainty about the future interest rate path and the macroeconomy in general. Deaton (1992) shows how uncertainty depresses the current level of consumption and Dixit and Pindyck (1994) how it reduces investment. This suggests that uncertainty, like signaling, has effects on demand which do not need to be transmitted through financial markets, although a part of their transmission is likely to be through their impact on financial markets.

We explore the impact on two measures of financial market uncertainty for that purpose: the implied volatility of the share price index (VIX) and interest rate futures (swaptions) in each country (MOVE). Two of the interpretations taken by previous work are that these measures reflect real economic uncertainty (Bloom, 2009) or investor's risk appetite (Bruno and Shin, 2012). To disentangle these two different interpretations, we also include household survey measures of uncertainty and the BBB-AAA corporate bond spread into our model. We include these variables, again one at a time, as a sixth variable in our VAR model to establish whether there is a significant response to asset purchase shocks. We would expect this if either of these channels plays a role. The results for are shown in figure 5 and the maximum values are found in table A4 of Appendix A.

Figure 5 and table A4 demonstrate that both the VIX and MOVE show significant movements in the UK, while only MOVE does so in the US. Bloom (2009) argues that the VIX is a reflection of uncertainty. On the other hand, Adrian and Shin (2010), Bruno and Shin (2013) and Miranda-Aggripino and Rey(2013) argue that the VIX is a reflection of investor's risk appetite. Interestingly, the reaction of household uncertainty over durable purchases suggests that the first interpretation is relevant for both countries. Similarly, the fact that corporate bond spreads react significantly in three of the four identification schemes for the UK only is stronger evidence that the risk-taking channel plays a role in the UK.

Figure 5 – Results for the Uncertainty Channel



This figure shows the response functions of the VIX, the MOVE, a measure of household uncertainty (HHUNC) and the spread between BBB and AAA corporate bonds to an asset purchase shock. Results are shown for both US and UK for each of our four identification schemes. Five hundred simulations were used to generate the responses.

3.3 How important is transmission through bond and equity markets?

Ideally, our framework would also allow us to examine the quantitative significance of each transmission channel. This might be possible if individual variables in the VAR were to capture a transmission channel, in both an economic and statistical sense, entirely. In that case, one way to quantify the effect of that transmission channel would be to set the feedback coefficients from that variable to output and prices to zero and repeat the impulse response analysis. The difference between the counterfactual and original impulse response would indicate the quantitative contribution of a given transmission channel. Ludvigson, Steindel and Lettau (2002) propose this methodology to quantify the impact of wealth effects, due to monetary policy, on consumption. We adopt their approach, setting the feedback coefficients of the long rate and real equity prices on real GDP and prices to zero, so as to examine how far the effects of the policy are transmitted through these markets. In principle, these variables could reflect all three transmission channels.

Using Ludvigson, Steindel and Lettau's (2002) approach we are able to set an upper limit to the role played by portfolio balance effects as reflected in bond yields and real equity prices. We keep all of the draws of the VAR coefficients retained by identification schemes I-IV, used to generate figure 2, and set the auto-regressive coefficients on the ten year rate and on real equity prices to zero in the equations for GDP and CPI. While this allows us to shut off the feedback from financial markets to real activity, it is also a counterfactual exercise, since agents would have reacted differently in the absence of the financial market feedback. Doing this we find the maximum response for GDP falls from 0.58 to 0.31 in the US and 0.25 to 0.14 in the UK. For CPI the corresponding falls are from 0.62 to 0.31 in the US and 0.32 to 0.10 in the UK. Thus in broad terms we can say that bond and equity channels account for about half of the impact of asset purchases on output and inflation in both countries. This sets an upper limit for the role of that component of portfolio balance channel which operates through bond and equity market movements.

3.4 Robustness

3.4.1 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 reflect 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, the ratio of the ECB's total assets on its balance sheet to Euro Area GDP, the trade balance to GDP ratio and the real exchange rate one by one in our VAR. The impulse response charts are shown in the Appendix B, figures B3 to B9 while tables B3 to B9 make clear the maximum effects and statistical significance.

A comparison of the base results for the mean maximum effects in table A1 with those in tables B3 to B9 shows that these results are robust to the inclusion of additional variables. For the United States all four identification schemes continue to show a significant impact on CPI; for the United Kingdom scheme I does not show a significant effect, just as it did not in our basic model. However, in the majority of cases our main effects of interest are statistically different from zero and they are quantitatively very similar to the estimated effects from the model presented in figure 2.

3.4.2 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 whether 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 open-ended 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.

We considered six possible alternatives for the United States; only the first of these is also relevant to the United Kingdom. Looking at the amount of assets purchased rather than the announcements we showed (figure B10) that the impulse response effects remained significantly above zero in all four cases.

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 through the issue of central bank reserves. While it is clearly difficult to pinpoint the right weight for Operation Twist announcements, we also explore results with a weight of one half, probably a reasonable lower bound, in figure B9. These results are similar to our baseline results, but not significant for identification scheme IV for the real GDP response. If the portfolio channel is an important part of the transmission mechanism in the United States, as the earlier results suggest, then perhaps it is not surprising that significance is reduced when Operation Twist is down-rated.

The Federal Reserve Board 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 announcements. 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 asset purchase announcement is therefore to calculate the present value[10] of an asset that pays $US 45 bn each month, for thirty-six months. This suggests that the economic impact of the open-ended asset purchase announcement was about $US 1217bn. Financial markets may of course take a

[10] We used the yield on the 10-year government bond in the month prior to the announcement as the discount rate for our calculation.

different view and an examination of OIS rate futures data suggest that they expected a rise in the three 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 $US 702bn, which is similar to the impact of the second asset purchase announcement ($US 600bn). In December 2013, the FOMC announced that the rate at which assets were purchased would slow. We treat this announcement as an unwinding of the open-ended purchases, meaning that we subtract $US 1217bn and $US 702bn from the total asset purchase announcement series for the eighteen and thirty-six months cases, respectively. This is shown in figures B12 and B13. The GDP response is significant with all four schemes while the CPI response is significant with the first three, regardless of how long we assume open-ended purchases to last.

In addition to government bonds, the Federal Reserve also purchased large quantities of mortgage-backed securities. Most of these purchases were made before March 2009, when government bond purchases began, and from September 2012, when open-ended purchases of mortgage backed securities a rate of US$ 40bn USD per month were announced. We follow the same approach as for government bonds and add and then subtract the present discounted value of mortgage-backed securities for eighteen and thirty-six months respectively. We also add the MBS purchases before 2009 m3 to our series. The impact of asset purchases (figures B14 and B15) on output in particular is still statistically significant in at least three out of four identification schemes. While the evidence for CPI inflation is weaker, the largest mass of the 68% Bayesian credible sets is above zero.

A comparison of figures B12-B15 with figure 1 and tables B12 to B15 with table A1 suggests that the coefficients in these four variants are generally lower. This is to be expected since the total sum of purchases is larger, and we should not look for robustness in terms of the coefficients. Our findings about statistical significance are, however, robust to the definition of asset purchases used.

4 Conclusion

In response to the 'Great Recession', central banks deployed a range of novel monetary policy tools, but their impact on the economy is still not well understood. In this paper we study purchases of government bonds by the Bank of England and the Federal Reserve. We find significant effects on GDP and CPI from asset purchases without making the identifying assumption that the effects are positive and with models estimated only on the period in which asset purchases were carried out; models estimated over a longer period may be more susceptible to the Lucas critique. Unlike earlier work, our estimates do not rest on prior assumptions about the nature of the coefficients in the VAR; they are entirely driven by the data.

Our results suggest that, at the median, an asset purchase shock that results in the central bank purchasing government bonds worth 1% of nominal GDP, leads a rise of about .62% (.25%) of real GDP and .58% (.32%) in CPI in the US (UK). These results are robust to including a number of different additional variables in the VAR. Similarly, using the actual amount of assets purchased as the main variable of interest and, for the US, various perturbations to the definition of asset purchases makes little difference to our findings. Our estimates of the impact of asset purchases on real GDP and CPI are 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 notable exception: For the UK, we find that the CPI response is almost three times larger than documented by previous work; the implied inflation-output trade-off is, however, consistent with studies of the conventional monetary policy transmission mechanism and helps explain why UK inflation was higher than expected after asset purchases began.

For the United States we find that long-term bond yields respond to asset purchases while short-term swap rates do not. This suggests that the portfolio balance may play a role while it is unlikely that signaling is important. Asset purchases reduce measures of financial market uncertainty, such as the MOVE or the VIX, in both countries. We find this to be a reflection of a reduction in household uncertainty about durable good purchases in both countries. In addition, in the UK there is a rise in the appetite for risk. This suggests that the uncertainty channel (Bloom, 2009) may be relevant for both countries, and the risk-taking channel (Bruno and Shin, 2013) for the transmission mechanism in the UK. In both countries we find that about a half of the effect of the policy is transmitted through bond and equity markets.

Our results have important implications for both research and policy. They suggest that identification schemes appropriate for one country may not be suitable for another. Similarly, we show that there is substantial heterogeneity in transmission mechanisms across countries. One should not expect asset purchases to have the same effect everywhere. Finally our results indicate that results based on time-series data from before the start of asset purchases may be affected by the Lucas Critique.

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Appendix A – Tables showing Maximum Impact of Impulse Response Functions and their Significance

Table A1: Maximum Impact of Asset Purchase Shocks (percentage points)

	I	II	III	IV	Mean
US GDP	0.28*	0.77*	0.82*	0.46*	0.58
US CPI	0.32*	0.84*	0.82*	0.51*	0.62
UK GDP	0.12*	0.30*	0.32*	0.25*	0.25
UK CPI	0.00	0.65*	0.38*	0.26*	0.32

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, when the estimation periods is 2010m3 – 2014m5. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets

Table A2: Maximum Impact of Long Rate Shocks (percentage points)

	I	II	III	IV	Mean
US GDP	0.70*	1.82*	1.14*	0.56	1.06
US CPI	0.55*	1.98*	1.29*	0.40	1.06
UK GDP	-0.23*	1.04	1.24	-0.11	0.66
UK CPI	1.20*	3.47*	3.82*	0.95	2.36

This table shows the maximum effects on real GDP and CPI of negative long rate shocks. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table A3: Maximum Impact of Asset Purchase Shocks on OIS and Long-term Government Bond Rates (percentage points)

	I	II	III	IV	Mean
US OIS 6M	-0.01	-0.02	-0.02	-0.01	-0.01
US OIS 12M	0.00	-0.04	-0.04	-0.01	-0.02
US OIS 24M	0.00	-0.10	-0.09	-0.01	-0.05
US 20Y GB Yield	-0.10*	-0.16*	-0.17*	-0.16*	-0.15*
US 30Y GB Yield	-0.07	-0.14*	-0.16*	-0.14*	-0.13*
UK OIS 6M	-0.01	-0.05*	-0.02	-0.01	-0.02
UK OIS 12M	0.00	-0.07	-0.02	-0.01	-0.03
UK OIS 24M	0.00	-0.15*	-0.04	-0.02	-0.05
UK 20Y GB Yield	0.00	-0.17*	-0.01	-0.02	-0.05
UK 30Y GB Yield	-0.01	-0.11*	-0.03	-0.04	-0.05

This table shows the peak effects of the median impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, for all of the variables listed in the table. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets. OIS 6M, OIS 12M and OIS 24M are the 3-month rate, 6 months, 12 months and 24 months ahead. 20Y and 30Y GB YIELD are the yields on 20 and 30 year government debt.

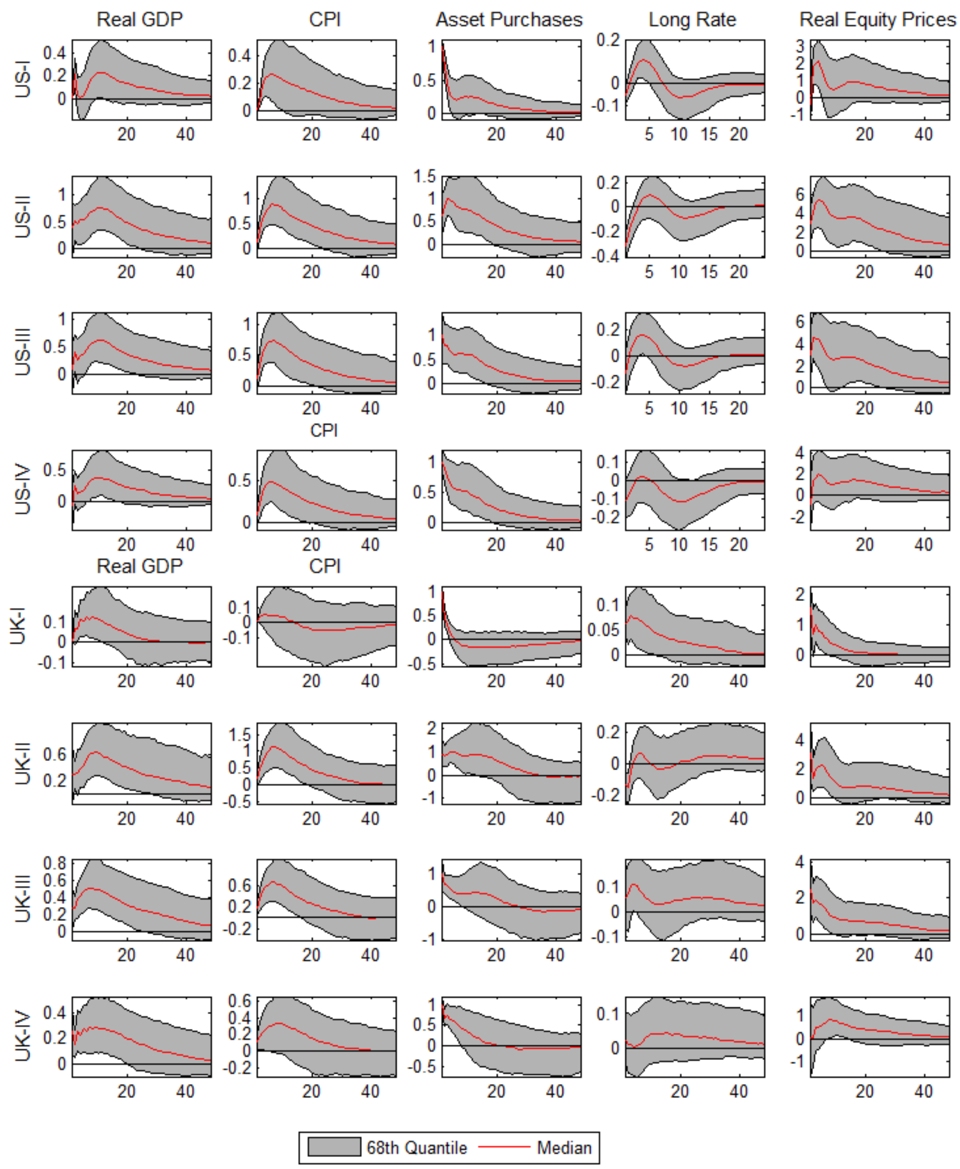
Table A4: Maximum Impact of Asset Purchases on Indicators of Uncertainty (percentage points)

	I	II	III	IV	Mean
US VIX	-1.11*	-2.57*	-2.42	-0.93	-1.76
US MOVE	-3.06*	-7.90*	-9.40	-3.97	-6.08
US HHUNC	-5.44*	-7.87*	-10.06*	-3.48	-6.71
US BAA-AAA Spread	-0.05	-0.14*	-0.19*	-0.06	-0.11
UK VIX	-0.67*	-0.84	-1.06*	-0.81*	-0.85
UK MOVE	-2.10*	-4.79*	-2.83*	-3.01*	-3.18
UK HHUNC	-5.82*	-10.30	-14.02*	-10.30*	-10.11
UK BAA-AAA Spread	-0.07*	-0.12	-0.14*	-0.08*	-0.10

This table shows the maximum effect of asset purchase shocks on the percentile responses of the MOVE, the VIX, the household measure of uncertainty (HHUNC) and the BBB-AAA corporate bond spread. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

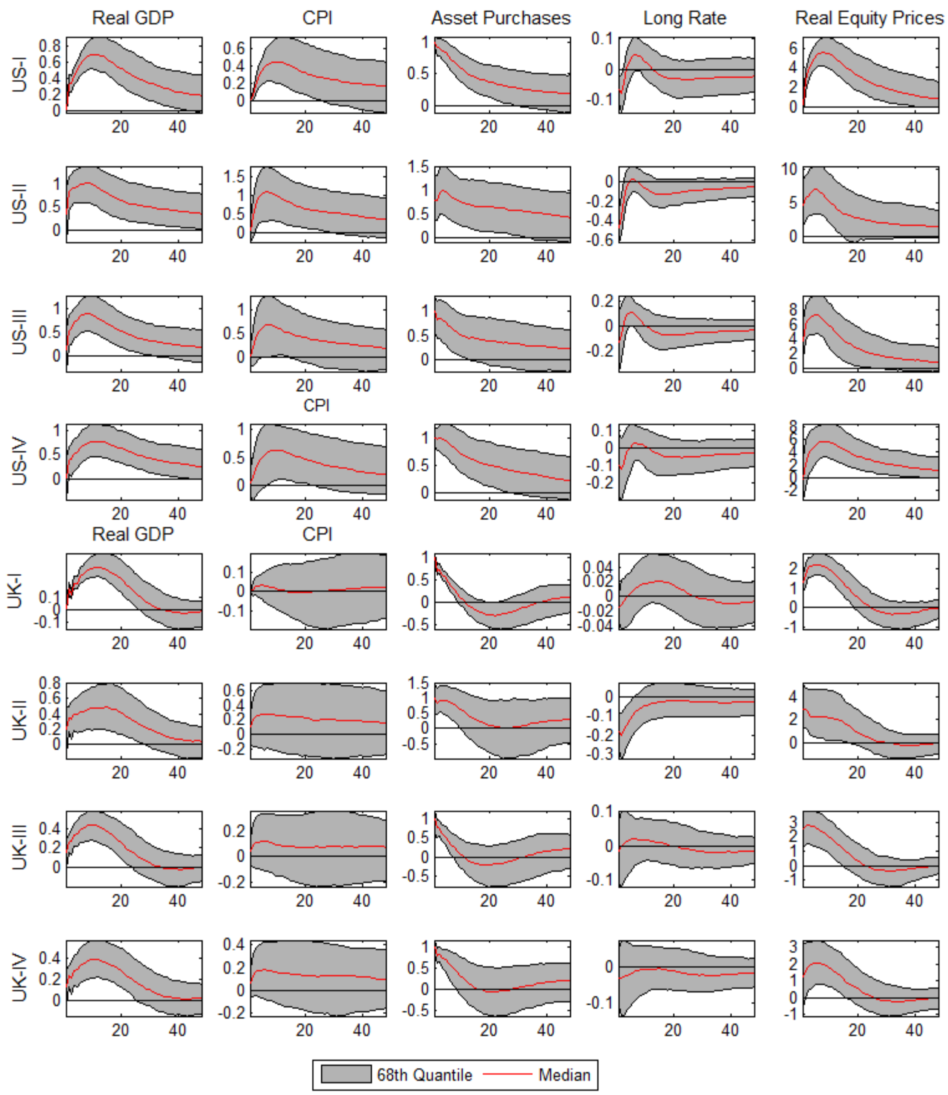
Appendix B – Additional Tables and Figures

Figure B1: Results for Short Sample (2010m3-2014m5)



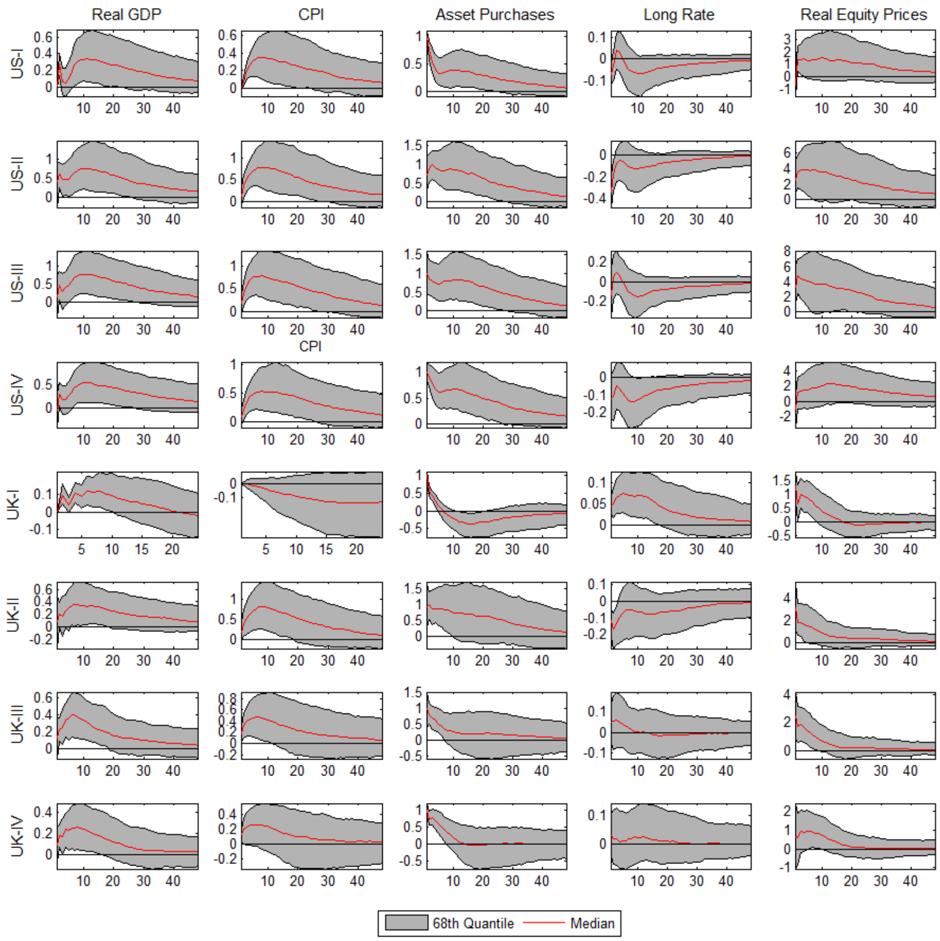
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase announcement as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. The estimation period is 2010m3 – 2014m5. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B2: Results for Long Sample (2007m1-2014m5)



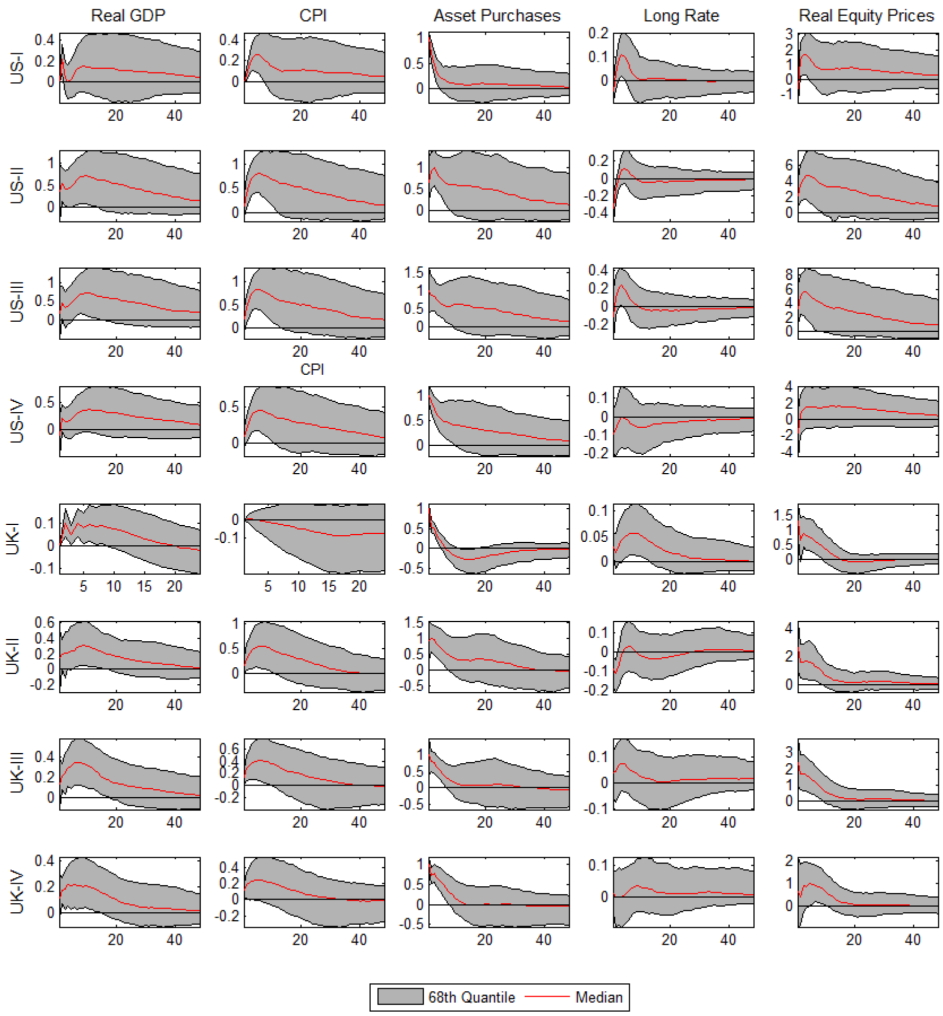
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase announcement as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. The estimation period is 2007m1 – 2014m5. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B3: Results with Gov. Budget Balance as Control Variable



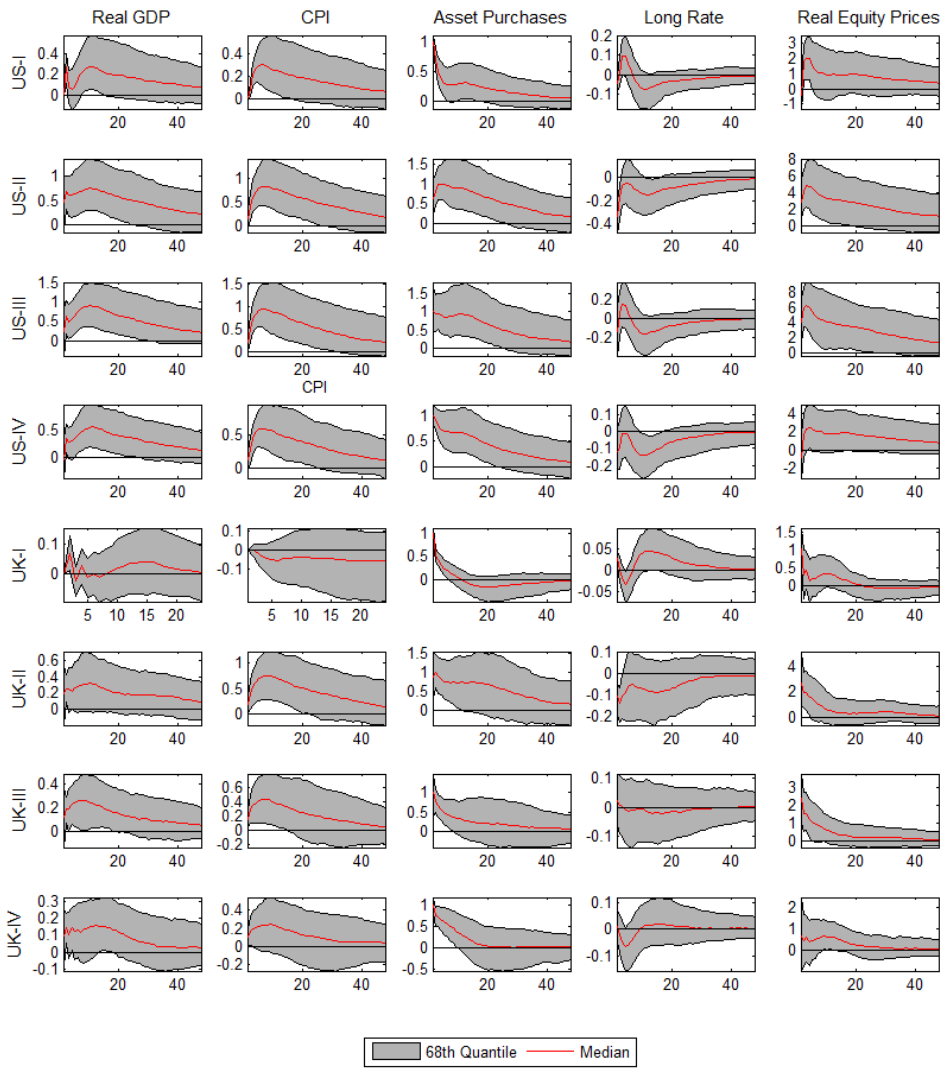
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase announcement as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. The ratio of the budget balance to GDP is included as a control variable, but not shown. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B4: Results with Public Debt to GDP ratio as Control Variable Maximum Impact (percentage points)



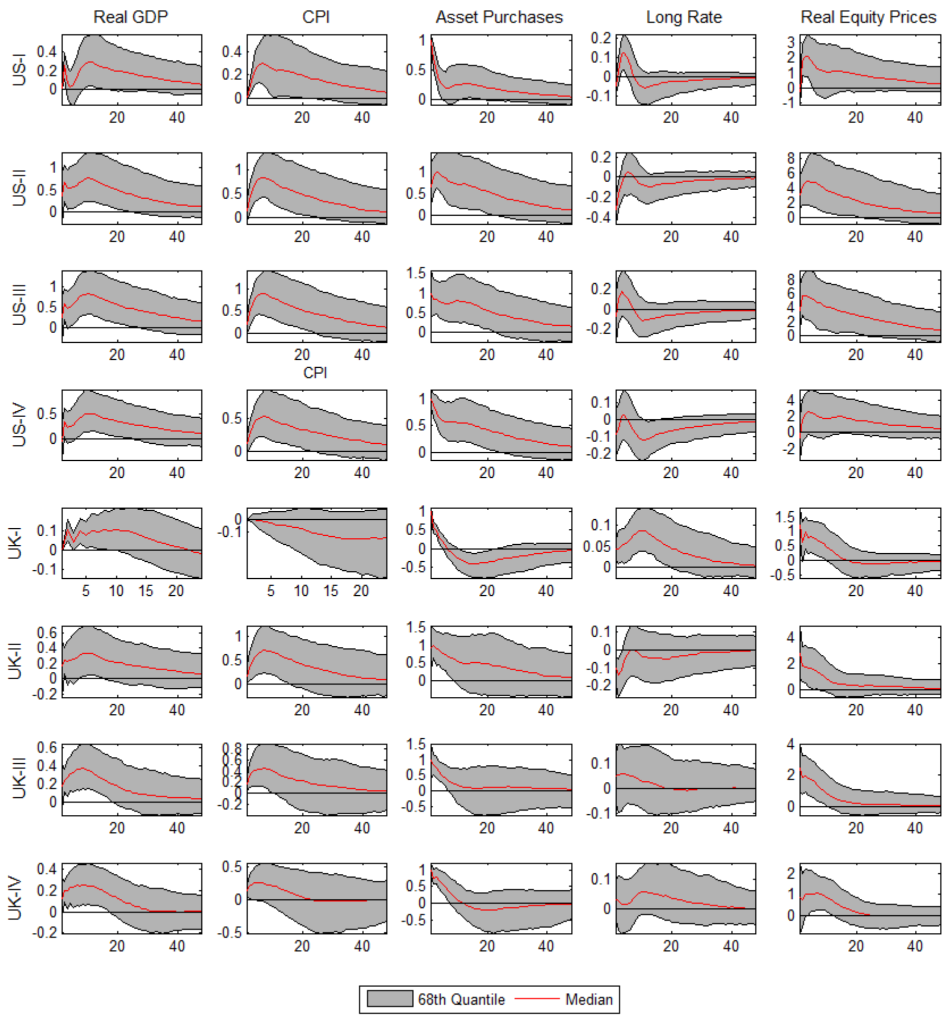
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase announcement as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. The ratio of public debt to nominal GDP is included as a control variable, but not shown. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B5: Results with the Real Oil Price as Control Variable



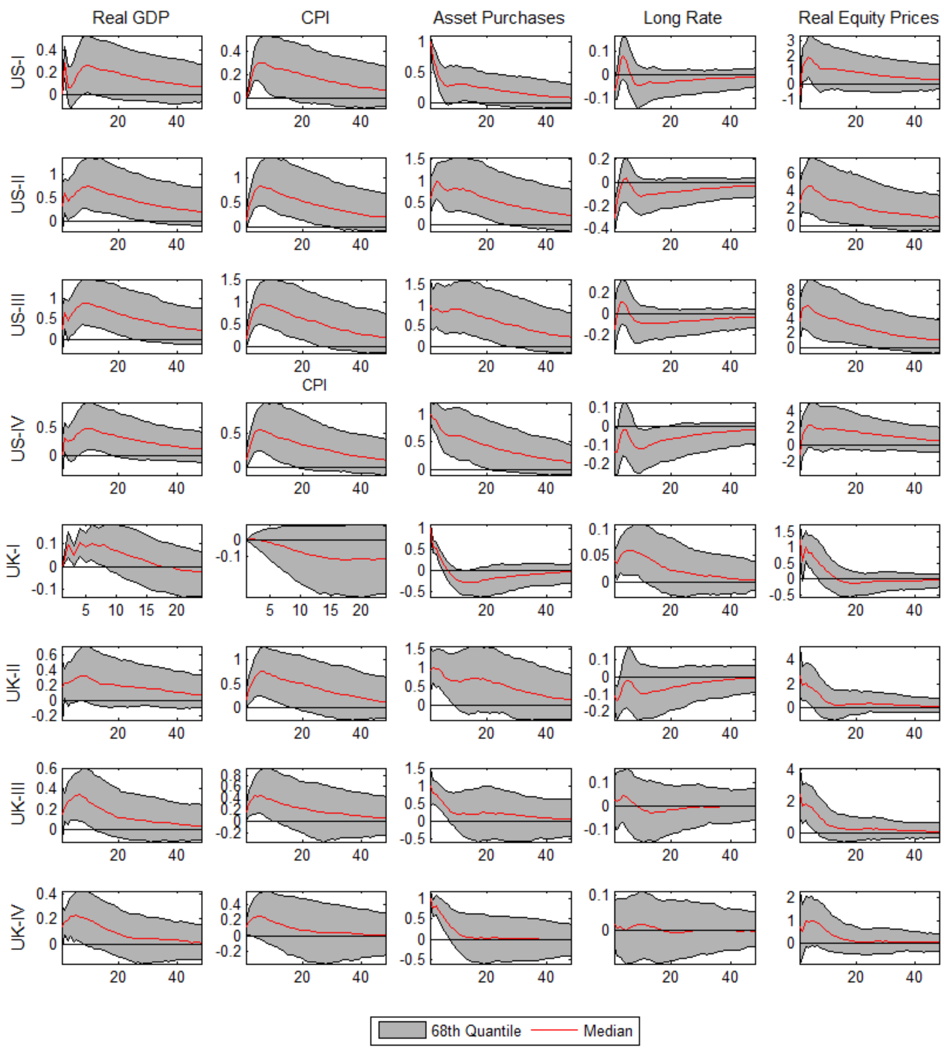
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase announcement as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. The real oil price is included as a control variable, but not shown. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B6: Results with the Italian to German 10-year Government Bond Yield Spread as Control Variable



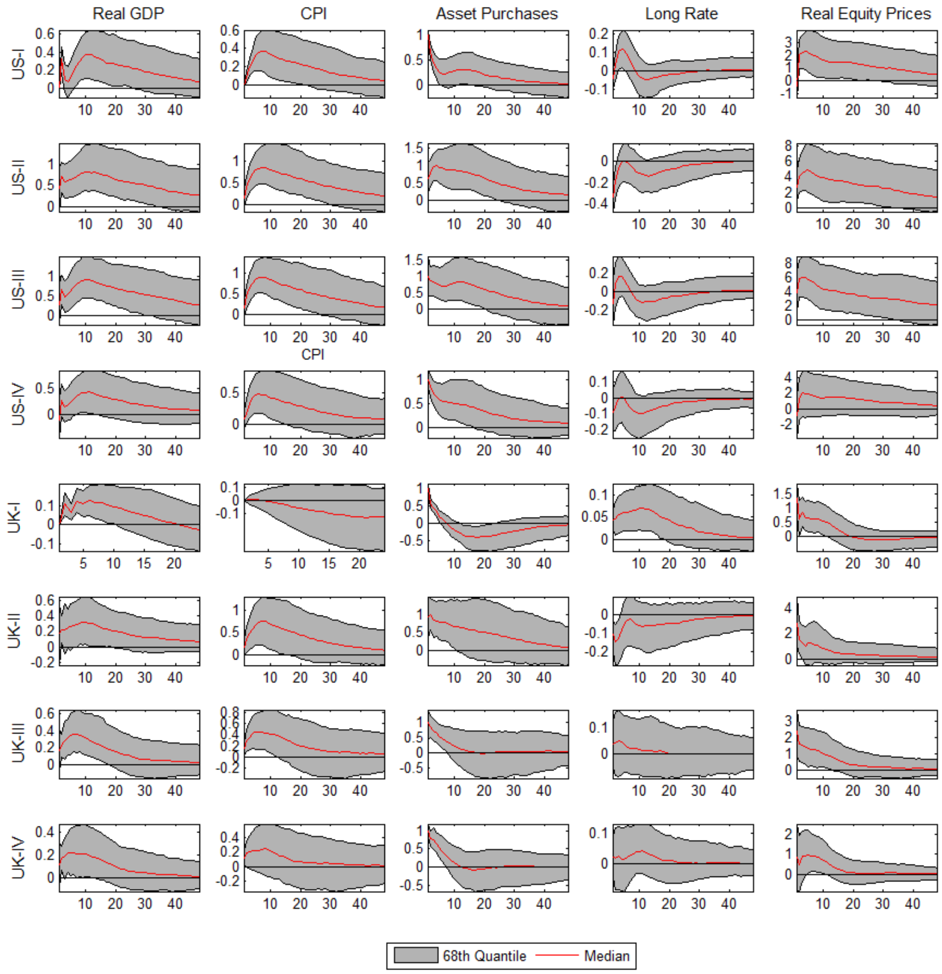
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase announcement as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. The Italian to German 10-year government bond yield spread is included as a control variable, but not shown. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B7: Results with ECB Total Assets to Euro Area GDP Ratio as Control Variable



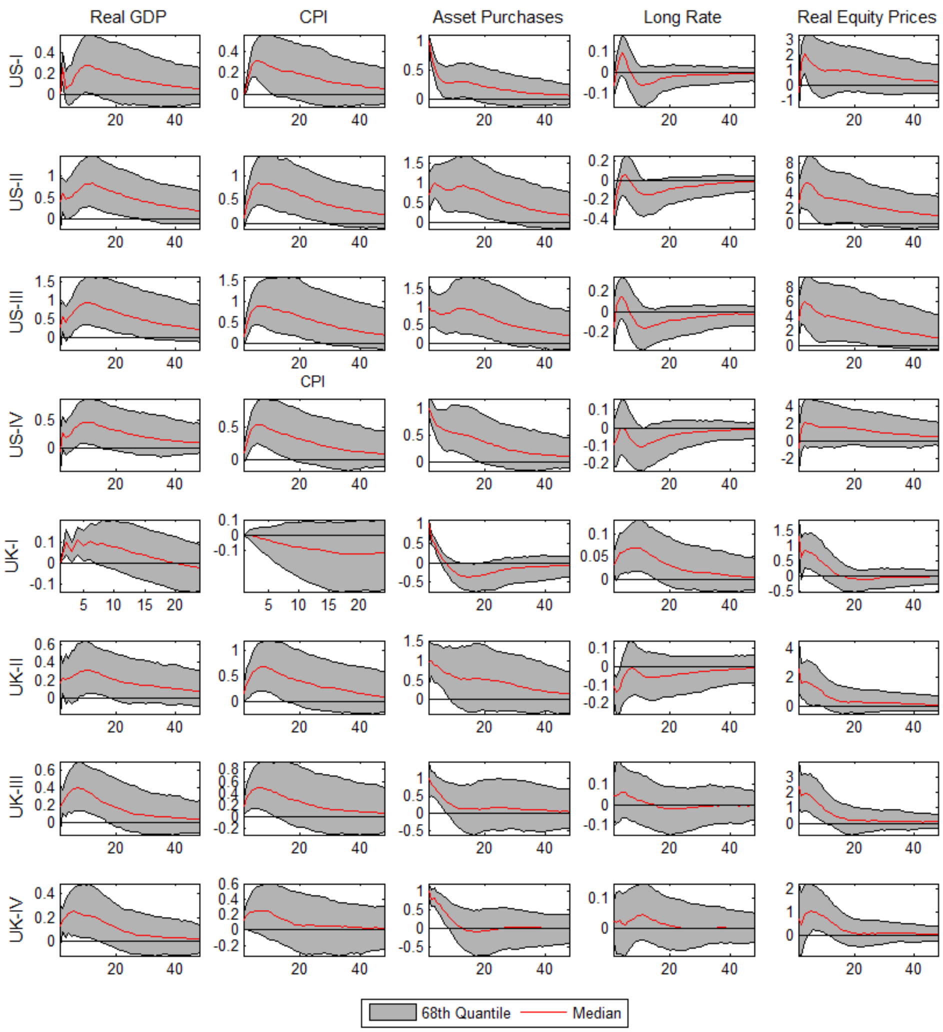
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase announcement as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK. The ECB's total assets to nominal Euro-Area GDP is included as a control variable, but not shown. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B8: Results with Real Exchange Rate as Control Variable



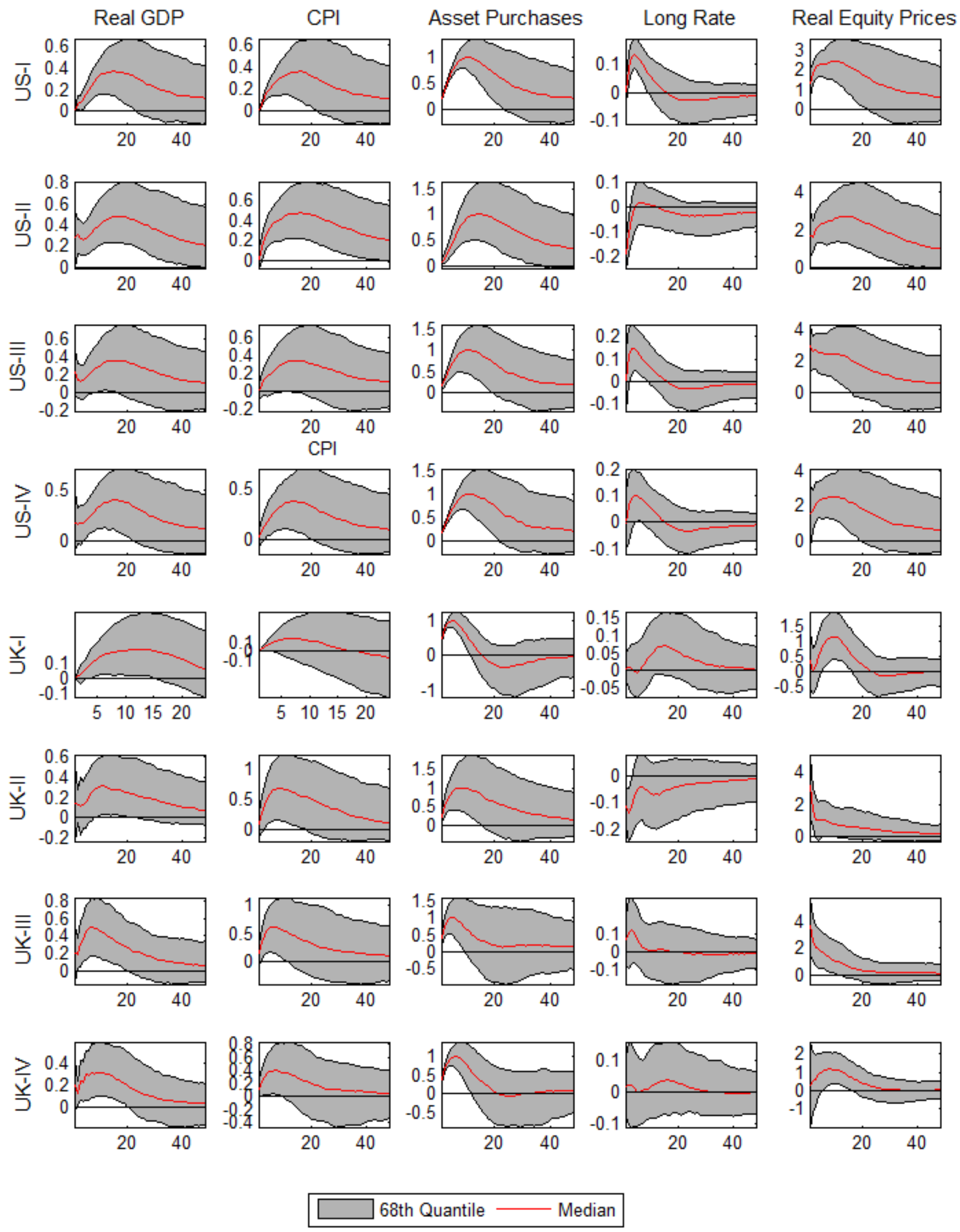
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK, with the real exchange rate as a control variable. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B9: Results with the Trade Balance to GDP ratio as Control Variable



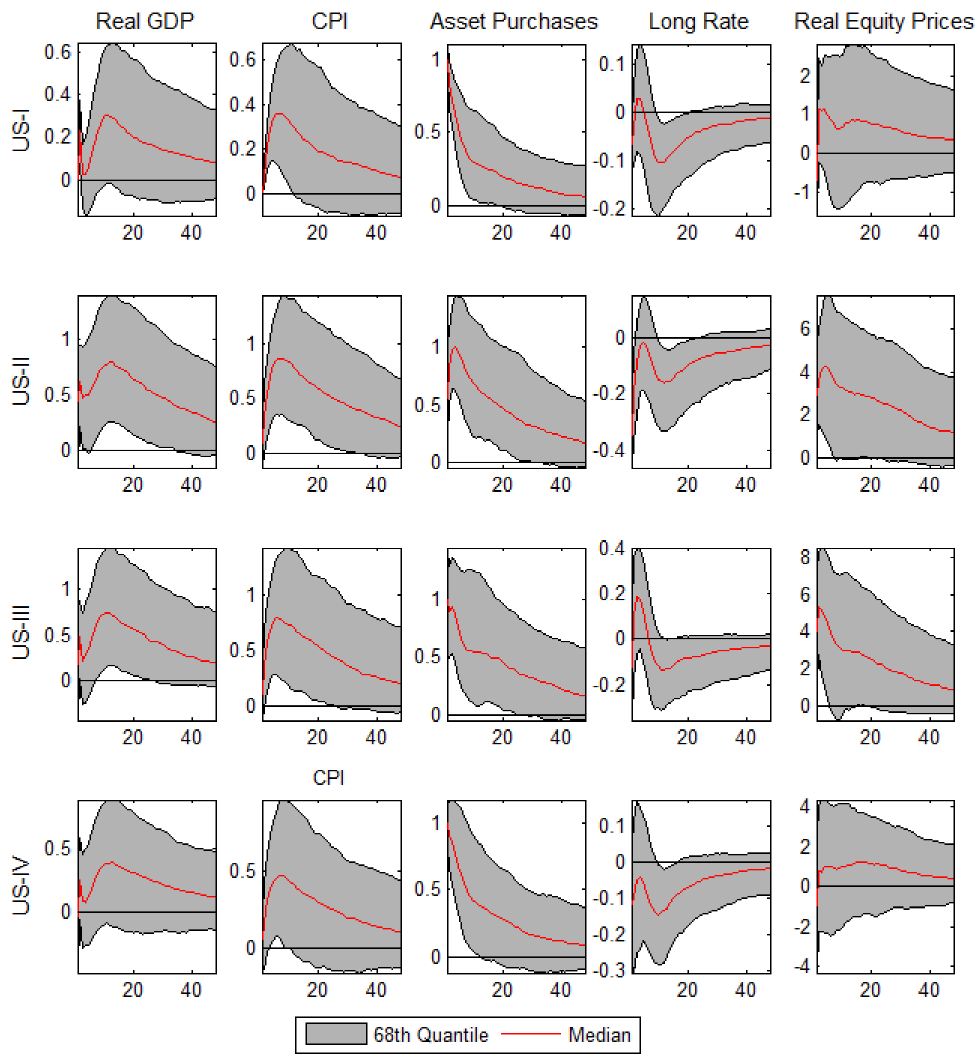
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK, with the real exchange rate as a control variable. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B10: Results with amount of assets purchased



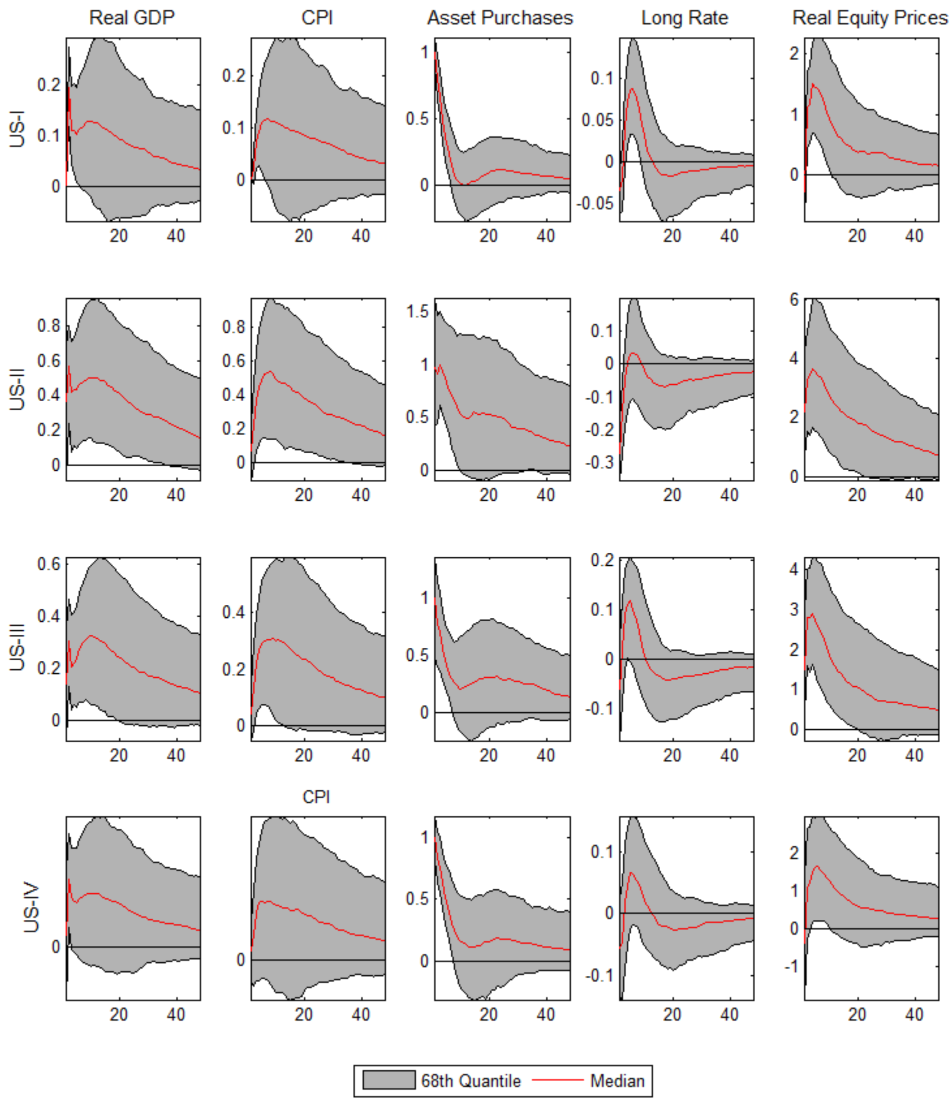
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK, with the baseline asset purchase announcement series replaced by actual amounts of asset purchased. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B11: Results for US with half weight on Operation Twist Announcements



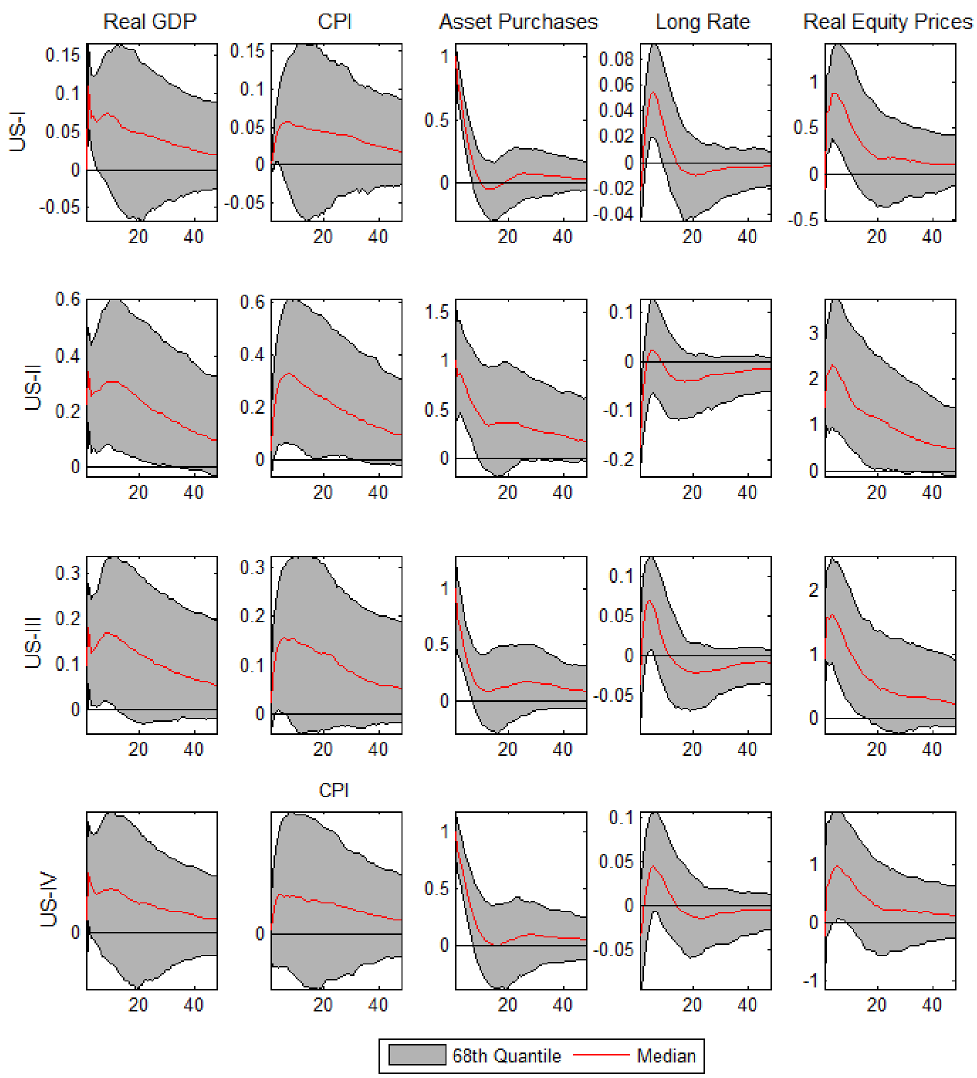
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK, with the baseline asset purchase announcement series replaced by a series where we put a weight of half on the Federal Reserve's Operation Twist. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B12: Impact of US Open-ended Asset Purchases assumed to last 18 months



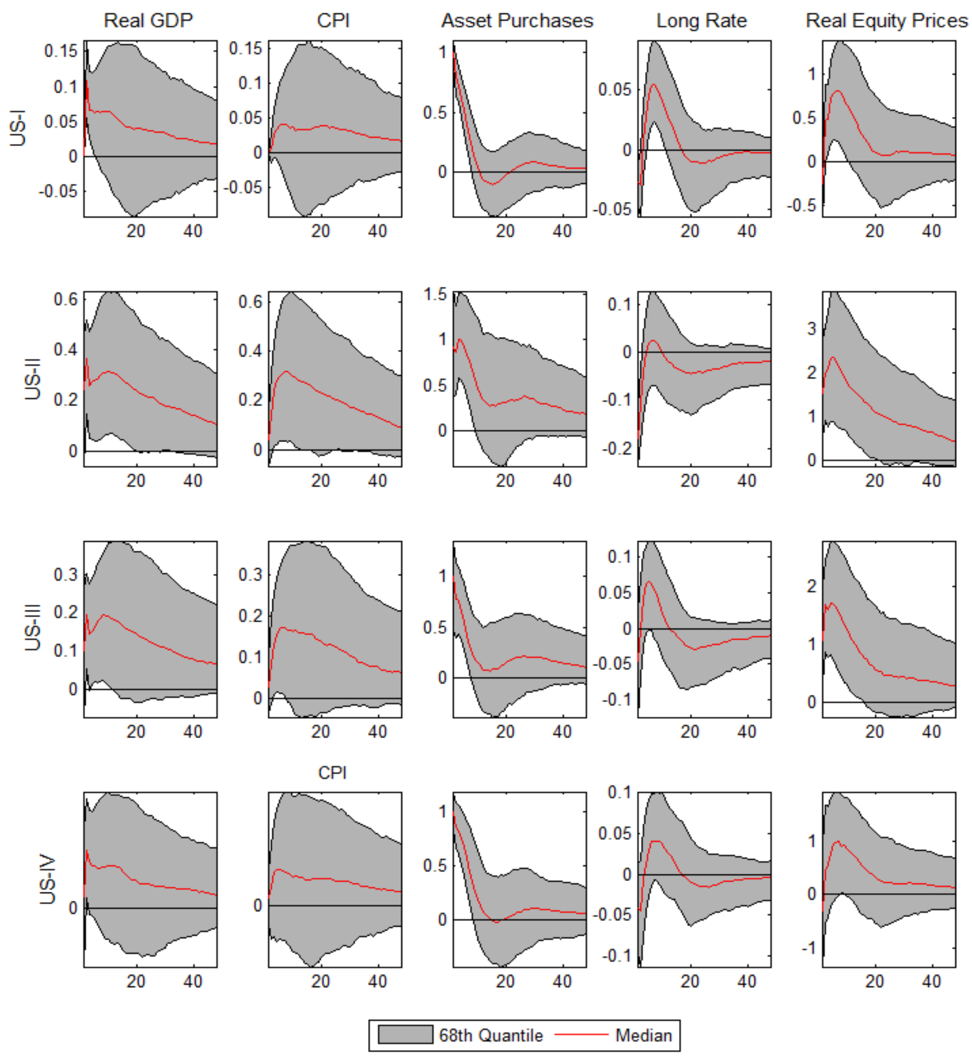
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK, with the baseline asset purchase announcement series replaced by a series where we add the impact of present discounted value of open-ended treasury purchases assuming that they last 18 months. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B13: Impact of US Open-ended Asset Purchases assumed to last 36 months



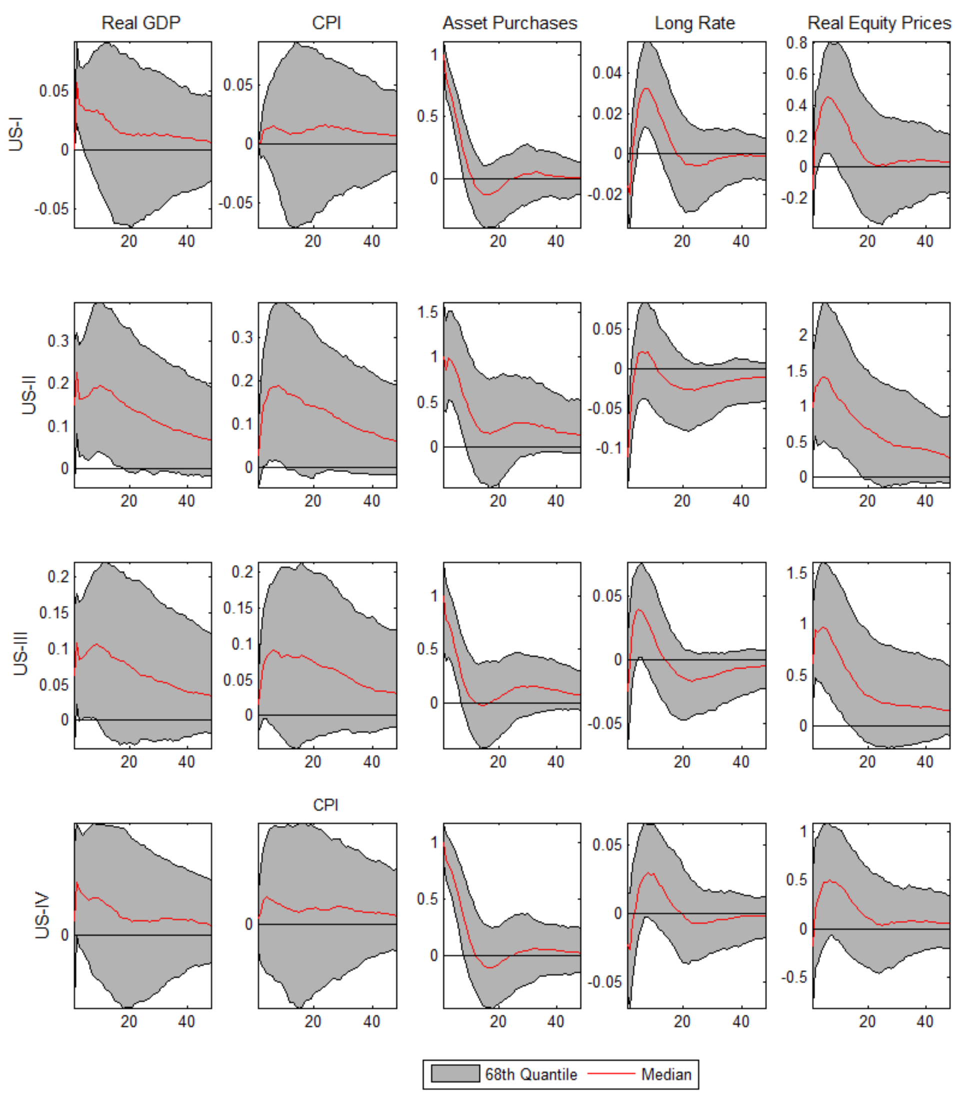
This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK, with the baseline asset purchase announcement series replaced by a series where we add the impact of present discounted value of open-ended treasury purchases assuming that they last 36 months. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B14: Impact of including Mortgage-backed Securities and all Open-ended Asset Purchases are assumed to last 18 months



This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK, with the baseline asset purchase announcement series replaced by a series where we add the impact of mortgage-backed securities (mbs) and the present discounted value of open-ended treasury and mbs purchases assuming that they last 18 months. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Figure B15: Impact of including Mortgage-backed Securities and all Open-ended Asset Purchases are assumed to last 36 months



This figure shows, for each of the variables in our model, the median impulse responses in response to an unexpected one percent asset purchase as a fraction of 2009Q1 GDP, together with 68% Bayesian credible sets. We show results for all four identification schemes for the US and the UK, with the baseline asset purchase announcement series replaced by a series where we add the impact of mortgage-backed securities (mbs) and the present discounted value of open-ended treasury and mbs purchases assuming that they last 36 months. 10,500 simulations, with the first 10,000 as burn-in, were used to generate the responses.

Table B1 Maximum Impact: Estimation Period 2010m3-2014m5 (percentage points)

	I	II	III	IV	Mean
US GDP	0.23*	0.77*	0.63*	0.38*	0.50
US CPI	0.27*	0.88*	0.73*	0.50*	0.59
UK GDP	0.12*	0.65*	0.51*	0.29*	0.39
UK CPI	0.05	1.13*	0.68*	0.33*	0.55

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, when the estimation periods is 2010m3 – 2014m5. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B2 Maximum Impact: Estimation Period 2007m1-2014m5 (percentage points)

	I	II	III	IV	Mean
US GDP	0.70*	1.02*	0.89*	0.78*	0.85
US CPI	0.44*	1.09*	0.69*	0.63*	0.71
UK GDP	0.35*	0.48*	0.43*	0.38*	0.41
UK CPI	0.03	0.28	0.12	0.18	0.15

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, when the estimation periods is 2007m1 – 2014m5. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B3: Maximum Impact with Government Budget Balance as a Control Variable (percentage points)

	I	II	III	IV	Mean
US GDP	0.33*	0.77*	0.78*	0.53*	0.60
US CPI	0.35*	0.80*	0.79*	0.53*	0.62
UK GDP	0.12*	0.36*	0.39*	0.26*	0.28
UK CPI	0.00	0.82	0.47*	0.26*	0.39

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, when the ratio of the government budget balance to nominal GDP is included as a control variable. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B4: Maximum Impact with the Ratio of Public Debt to GDP as a Control Variable (percentage points)

	I	II	III	IV	Mean
US GDP	0.22*	0.72*	0.71*	0.36*	0.50
US CPI	0.26*	0.80*	0.84*	0.46*	0.59
UK GDP	0.10*	0.31*	0.35*	0.22*	0.24
UK CPI	0.01	0.56*	0.41*	0.25*	0.31

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, when the ratio of public debt to nominal GDP is included as a control variable. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B5: Maximum Impact with the Real Price of Oil as a Control Variable (percentage points)

	I	II	III	IV	Mean
US GDP	0.27*	0.76*	0.92*	0.55*	0.63*
US CPI	0.30*	0.82*	0.94*	0.58*	0.66*
UK GDP	0.07*	0.32*	0.26*	0.16*	0.20*
UK CPI	0.00	0.75*	0.43*	0.24*	0.35*

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, when the real oil price is included as a control variable. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

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Table B6: Maximum Impact with the Italian to German 10-year Government Bond Yield Spread as a Control Variable (percentage points)

	I	II	III	IV	Mean
US GDP	0.29*	0.77*	0.85*	0.50*	0.60
US CPI	0.30*	0.83*	0.90*	0.54*	0.64
UK GDP	0.11*	0.34*	0.37*	0.25*	0.27
UK CPI	0.00	0.71*	0.46*	0.25*	0.36

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, when the Italian to German 10-year government bond yield spread is included as a control variable. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B7: Maximum Impact with the Ratio of ECB Total Assets to Euro Area GDP as a Control Variable (percentage points)

	I	II	III	IV	Mean
US GDP	0.29*	0.74*	0.89*	0.50*	0.60
US CPI	0.30*	0.82*	0.96*	0.57*	0.66
UK GDP	0.11*	0.33*	0.34*	0.23*	0.25
UK CPI	0.00	0.75*	0.44*	0.25*	0.36

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, when the ratio of ECB Total Assets to Euro Area GDP is included as a control variable. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B8: Maximum Impact with Real Exchange Rate as control variable (percentage points)

	I	II	III	IV	Mean
US GDP	0.27*	0.83*	0.93*	0.47*	0.63
US CPI	0.31*	0.84*	0.90*	0.53*	0.64
UK GDP	0.11*	0.31*	0.40*	0.25*	0.27
UK CPI	0.00	0.69*	0.50*	0.25*	0.36

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, with the real exchange rate as a control variable. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B9: Maximum Impact with Trade Balance to GDP ratio as control variable (percentage points)

	I	II	III	IV	Mean
US GDP	0.37*	0.81*	0.91*	0.42*	0.63
US CPI	0.37*	0.85*	0.91*	0.48*	0.65
UK GDP	0.12*	0.32*	0.35*	0.22*	0.26
UK CPI	0.01	0.75*	0.45*	0.24*	0.36

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, with the trade balance to GDP ratio as a control variable. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B10: Maximum Impact with Asset Purchases rather than Purchase Announcements Modeled (percentage points)

	I	II	III	IV	Mean
US GDP	0.37*	0.48*	0.36*	0.40*	0.40
US CPI	0.36*	0.47*	0.34*	0.38*	0.39
UK GDP	0.19*	0.32*	0.51*	0.32*	0.33
UK CPI	0.15*	0.69*	0.62*	0.40*	0.47

This table shows the peak effects of the median real GDP and CPI impulse response in response to a one percent rise in asset purchase announcement as a share of 2009Q1 GDP, with the baseline asset purchase announcement series replaced by actual amounts of asset purchased. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B11: Maximum Impact when Operation Twist Announcements are given a Half Weight (percentage points)

	I	II	III	IV	Mean
US GDP	0.30*	0.79*	0.74*	0.39	0.56
US CPI	0.36*	0.86*	0.79*	0.47*	0.62

This table shows the maximum effects on real GDP and CPI when the baseline asset purchase announcement series replaced by a series where we put a weight of half on the Federal Reserve's Operation Twist. Median peak effects are shown for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B12: Maximum Impact when US Open-ended Purchases are assumed to last 18 Months (percentage points)

Maximum Impact (percentage points)

	I	II	III	IV	Mean
US GDP	0.19*	0.57*	0.33*	0.23*	0.33
US CPI	0.12*	0.53*	0.31*	0.17	0.28

This table shows the maximum effects on real GDP and CPI when the baseline asset purchase announcement series is replaced by a series where we add the impact of present discounted value of open-ended treasury purchases assuming that they last 18 months. Median peak effects are shown for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B13: Maximum Impact when US Open-ended Purchases are assumed to last 36 Months (percentage points)

Maximum Impact (percentage points)

	I	II	III	IV	Mean
US GDP	0.11*	0.34*	0.18*	0.14*	0.19
US CPI	0.06*	0.33*	0.16*	0.08	0.15

This table shows the maximum effects on real GDP and CPI of negative long rate shocks when the baseline asset purchase announcement series is replaced by a series where we add the impact of present discounted value of open-ended treasury purchases assuming that they last 36 months. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B14: Maximum Impact when US Purchases of mortgage-backed securities are included and all Open-ended Purchases are assumed to last 18 Months (percentage points)

Maximum Impact (percentage points)

	I	II	III	IV	Mean
US GDP	0.11*	0.34*	0.18*	0.14*	0.19
US CPI	0.06*	0.33*	0.16*	0.08	0.15

This table shows the maximum effects on real GDP and CPI when the baseline asset purchase announcement series is replaced by a series where we add the impact of mortgage-backed securities (mbs) and the present discounted value of open-ended treasury and mbs purchases assuming that they last 18 months. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Table B15: Maximum Impact when US Purchases of mortgage-backed securities are included and all Open-ended Purchases are assumed to last 36 Months (percentage points)

Maximum Impact (percentage points)

	I	II	III	IV	Mean
US GDP	0.06*	0.22*	0.11*	0.07*	0.11
US CPI	0.02*	0.19*	0.09*	0.04	0.08

This table shows the maximum effects on real GDP and CPI when the baseline asset purchase announcement series is replaced by a series where we add the impact of mortgage-backed securities (mbs) and the present discounted value of open-ended treasury and mbs purchases assuming that they last 36 months. Median peak effects are shown for the US and the UK for each identification scheme, together with the mean across identification schemes. * indicates that at the time of the peak response the value of zero lies outside the 68% Bayesian credible sets.

Appendix C: The Relationship between GDP and CPI Effects

One way of comparing the plausibility of our results in this paper is to compare the implied ratio of the maximum effect on inflation to GDP with that found in other VAR studies of monetary policy. This exercise is presented in Table C1 for studies of asset purchase and conventional monetary policy. The results for the latter are taken from Cloyne and Huertgen (2014).

Table C1: Output and Inflation Effects of Monetary Stimulus

Country	Study	CPI Impact	GDP Impact	Ratio
United Kingdom				
Interest Rate*	Mumtaz et al (2011)	-1.15	-0.5	2.3
	Cloyne et al (2014)	-1	-0.6	1.7
Asset Purchases**	Weale and Wieladek (2015)	0.32	0.25	1.3
	Kapetanios et at (2012)	1.5	2.5	0.6
	Baumeister and Benati (2013)	1.5	1.8	0.8
United States				
Interest Rate*	Romer and Romer (2004)	-4.75	-3.1	1.5
	Bernanke and Mihov (1998)	-1.15	-0.8	1.4
	Christiano et al (1999)	-0.6	-0.7	0.9
	Bernanke et al (2005)	-0.7	-0.6	1.2
	Coibion (2012)	-3	-2.95	1.0
Asset Purchases**	Weale and Wieladek (2015)	0.62	0.58	1.1
	Baumeister and Benati (2013)	0.84	1.08	0.8

* An interest rate increase of one percentage point

** Asset purchases of one per cent of GDP for Weale and Wieladek. For Baumeister and Benati (2013)/Kapetanios et al (2012) we show the peak response to a one percent decline in the long-term to short-term rate spread.

A notable feature of the other studies of asset purchases is that they show a smaller inflation to GDP ratio than is typically found in studies of conventional monetary policy. Qualitatively, this is also the case for the corresponding ratio implied by our results, but the quantitative figures are much closer to those for conventional monetary policy in both countries.

It is, of course, possible that the inflation-output trade-off has changed since asset purchases were introduced. However, since Baumeister and Benati (2013) and Kapetanios et al (2012) were estimated using predominantly pre-crisis data, this is unlikely to account for these differences. This means that an explanation would have to be structured around the differences in the strength of the underlying transmission mechanisms between conventional and unconventional monetary policy. It is reassuring that our results have much less need of any such explanation because they are closer to the estimates of the relative responses to interest rate changes.

Additional References

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Appendix D – Data

Table D1 – 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
6m/12m/24m OIS rate	Monthly average of option (swaption) value for the 3-month US Dollar/ UK Pound OIS (Overnight index Swap) rate 6 and 12 (24) 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.	
Household Uncertainty	Monthly fraction of households of households citing future uncertainty as a reason for why today is a bad time to buy large durables, taken from the University of Michigan Survey of Consumers.	Fraction of GFK survey respondents indicating that uncertainty about the future affects consumer purchases.
Government budget balance to GDP Ratio	US/UK GOVERNMENT PRIMARY BALANCE AS % OF GDP (AR) SADJ 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 (MCOILBRENTEU); 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.	