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Roland Meeks

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Capital regulation and the macroeconomy: Empirical evidence and macroprudential policy

Roland Meeks*

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Abstract

We present new evidence on the macroeconomic effects of changes in microprudential bank capital requirements, using confidential regulatory data from the Basel I and II regimes in the United Kingdom. Our central result is that an increase in capital requirements lowered lending to firms and households, reduced aggregate expenditure and raised credit spreads. A financial accelerator effect is found to have amplified the macroeconomic responses to shifts in bank credit supply. Results from a counterfactual experiment that links capital requirements to house prices and mortgage spreads indicate that tighter macroprudential policy would have had a moderating effect on house price and mortgage lending growth in the early 2000s, with easier monetary policy acting to offset its contractionary effects on output.

Keywords: bank lending and the macroeconomy; bank capital regulation; housing market; macroprudential policy; Basel III

JEL codes: E51, E58, G21, G38

*Bank of England, University of Essex, Centre for Applied Macroeconomic Analysis (CAMA) and Centre for Macroeconomics (CfM). Email: roland.meeks@bankofengland.co.uk. I am grateful to Aaron Clements-Partridge for his help getting this project started, and to two anonymous referees for their constructive comments and suggestions. Thanks also to Piergiorgio Alessandri, James Cloyne, Sebastian De-Ramon, Paul Fisher, Matteo Iacoviello, Benoit Mojon, Brian Quinn, Valery Ramey, Michael Straughan, and Karl Walentin, and to seminar participants at the Banca d'Italia, European Central Bank, University of Essex, University of Glasgow, 46th MMF Conference, CFCM Conference on Effective Macroprudential Instruments, 2015 EEA meetings, 2015 ASSA meetings, and in particular my discussant Michał Kowalik. The views in this paper are my own, and should not be attributed to the Bank of England, the Monetary Policy Committee, Financial Policy Committee, or the Prudential Regulation Authority Board.

1 Introduction

Equity capital has special importance for banks. Compared to non-financial firms, banks fund a relatively small proportion of their assets using it.[1] Prudential regulators have a long history of setting down minimum standards for it.[2] And during the financial turmoil in advanced economies that began in 2007, the UK government alone put £37 billion of it into the banking system (HM Treasury, 2009). In this paper we quantify the impact of regulation-induced changes in bank capital on the macroeconomy, study the interaction between regulatory and monetary policies, and assess post-crisis reforms to the Basel Accords that grant regulators macroprudential powers over minimum capital standards (Basel Committee on Banking Supervision, 2010a).

The central empirical questions we address—whether aggregate variables respond to changes in bank capital, and if so, whether active adjustments in capital requirements might be a useful policy tool—are far from settled.[3] The reason is that answers to these questions have not been straightforward to obtain. The first difficulty is that most variation in bank capital is likely to be the result of disturbances to macroeconomic variables, such as output or interest rates. These variables affect capital directly by causing variation in retained earnings and in the prices of assets held in bank trading books (the 'bank capital channel', Gambacorta and Mistrulli, 2004). The same disturbances also affect credit demand, creating an identification problem. While specific one-off events have provided some convincing evidence of a channel from changes in bank capital to pockets of economic activity, via lending, progress has otherwise been limited by a lack of suitable instruments.[4]

The second difficulty lies in isolating changes in bank capital caused by regulation. In

[1] In the UK, for example, quoted and unquoted equity together make up a little over half of the financial liabilities of non-financial firms (ONS Blue Book, various issues). Banking system equity makes up between 4-6% of their liabilities, as measured by their regulatory simple leverage ratio (Bank of England Financial Stability Report, various issues).

[2] Capital requirements date back to the mid-19th Century. Countries have historically set a wide variety of restrictions including fixed minimum levels of capital, minimums that depended on the population in a bank's operational locale, and from the early 20th Century minimum proportions of liabilities (Grossman, 2010, Ch. 6). Since the introduction of the Basel Accords in 1988, capital requirements on banks in jurisdictions that adopted the international rules have been formulated in terms of the ratio of capital to risk-weighted assets.

[3] Theoretical arguments rest on there being an economically large deviation from the Modigliani-Miller irrelevance proposition, leading higher capital requirements to raise bank funding costs (Miller, 1995). If such costs are passed through to borrowers, a reduction in credit, and by extension aggregate expenditure, may result. Comparative analysis of models incorporating financing frictions on banks does offer theoretical support to the proposition that changes bank capital can have significant macroeconomic effects (Guerrieri, Iacoviello, Covas, Driscoll, Kiley, Jahan-Pavar, Queralto Olive, and Sim, 2015).

[4] See for example Peek and Rosengren (2000) (commercial real estate construction activity), and Ashcraft (2005) (county-level real activity in Texas). These event-type studies provide a high level of econometric credibility, but by their nature have a scope that is limited in time and place. An influential earlier literature examined the introduction of leverage restrictions and risk-based capital requirements in the U.S. as part of the first Basel Accords; see Berger and Udell (1994), Hancock and Wilcox (1997, 1998).

most jurisdictions, such changes have been infrequent, leading researchers to rely instead on qualitative measures of regulatory stringency (Peek, Rosengren, and Tootell, 2003; Bassett, Lee, and Spiller, 2013). Where systematic reviews of individual banks' capital requirements did take place, the effects of regulation on bank-level loan supply can be estimated. But for a model to be useable in formulating stabilization policies, it must provide estimates of the 'total' effect of a shift in bank capital on loan supply, taking into account feedbacks between the banking system and the macroeconomy. This is not possible with a purely bank-level analysis of capital requirements and lending (Francis and Osborne, 2009a; Aiyar, Calomiris, and Wieladek, 2016).

In this paper we claim to go some way towards resolving these problems. Over the 1990-2008 period covered in this study, regulators required individual banks operating in the UK to hold capital in excess of the time-invariant minimum levels set down in the Basel Accords. Regulators operated a system in which there was variation in capital requirements both over time and across banks.[5] We combine this confidential regulatory data with aggregate bank lending and bank capital series, and with a set of macroeconomic variables, then estimate a standard macroeconomic vector autoregression (VAR) under a set of restrictions that identify a microprudential policy shock.

The restrictions we formulate to identify regulation-induced shocks to banking system capital exploit features of the institutional framework in which microprudential supervisors operated, as well as more-conventional assumptions on timing. First, for policy and operational reasons, supervisors did not respond to contemporaneous developments in the macroeconomy, and so we impose that condition on the model. We instead allow policy to respond to its second round influence on banking variables, which appear with a delay. Next, capital requirements were confidential to the supervisor-firm relationship, precluding a direct macroeconomic response. We impose the restriction that capital variables have no direct effect on macroeconomic variables, but rather influence aggregate outcomes via a bank lending channel. Last, we assume that there is a delay between the announcement of a regulatory change, and the resulting change in bank lending policy.

Our central finding is that changes in microprudential capital requirements on banks have statistically and economically important spill-overs to the macroeconomy. A tightening of capital requirements reduces credit growth to households and non-financial firms, and raises spreads on home mortgages and on corporate bonds. Housing market activity is damped down by the regulatory action, which results in both lower average house prices and a higher

[5] Francis and Osborne (2009b) provide a description of the institutional environment, and summarise trends in UK banking capitalisation. The Bank of England was responsible for banking regulation prior to 1997, with the Financial Services Authority (FSA) in charge thereafter. The Prudential Regulatory Authority, a subsidiary of the Bank, took over from the FSA in April, 2013. However, the earlier date of December 2008 marks a distinct change in FSA policy to an 'Enhanced Prudential Regime', and so we end our analysis in 2008:Q3 (see Bailey, 2012).

proportion of mortgages in arrears. We also report important interactions between prudential and monetary policies. Systematic monetary policy easing acts to cushion the effect of changes in prudential policy on output, which for a 50 basis point increase in the average required capital ratio is a little over 0.2% lower than trend, two-to-three years after the shock. In the absence of a monetary policy response, peak output declines are larger, at roughly 0.3%. These findings indicate that the microeconomic frictions that lead bank equity finance to be costly are of macroeconomic relevance. And they complement a growing literature that identifies credit markets as a source of aggregate fluctuations, as in Gilchrist and Zakrajšek (2012), Meeks (2012), and Walentin (2014).

To help inform the conduct of policy with time-varying capital requirements as a macroprudential tool (the so-called counter cyclical buffer found in Basel III), we go on to report the results of a counterfactual simulation exercise. The exercise is motivated by the shortage of direct experience with the tool, and complements the literature that uses DSGE or macroeconometric models to analyse macroprudential policy (Angelini, Neri, and Panetta, 2014; Akram, 2014). We find that a macroprudential rule linking capital requirements to house prices and mortgage spreads would have led to a substantially higher aggregate capital ratio, and would have had a modest moderating influence on credit growth and house prices, prior to the financial crisis of 2008.

The VAR model that we specify resembles those adopted by Berrospide and Edge (2010), Iacoviello and Minetti (2008), and Walentin (2014) in that macroeconomic and banking factors both appear, but it includes a somewhat richer set of variables to account simultaneously for bank balance sheet dynamics, and credit, housing market, and other macroeconomic conditions. We share with Bassett, Chosak, Driscoll, and Zakrajšek (2014) and Mésonnier and Stevanovic (2015) the goal of isolating the effect of shifts in the supply of bank lending on the economy at large. But the present paper is unique in isolating the effect of microprudential regulatory action on aggregate conditions.

The econometric estimates we present exploit bank-level variation in required capital ratios to sharpen our estimates of the relationship between changes in regulation and changes in bank lending. Formally, estimates from bank-level panel data inform the prior parameter distribution of a standard Bayesian VAR for macroeconomic aggregates. The idea of combining micro and macro information via a Bayesian prior was previously employed in the context of a DSGE model by Chang, Gomes, and Schorfheide (2002). Our combined micro-macro estimation approach includes as a special case the 'plug in' method adopted by De Graeve, Kick, and Koetter (2008), but rather than treating micro estimates as fixed parameters, allows the additional information present in aggregate data to affect aggregate dynamics, and an appropriate assessment of parameter uncertainty. The part of our analysis that deals with bank-level data is similar to that in Francis and Osborne (2009a), Labonne and Lamé (2014), and Aiyar, Calomiris, and Wieladek (2014, 2016). But whereas those studies confined their interest to an analysis of the bank-level effect of regulatory action, our study instead uses them as a springboard for an analysis of the broader macroeconomic effect of regulation.

An alternative approach to incorporating micro information into the estimation of an aggregate VAR is to augment the model with statistical factors, extracted from institution-level balance sheet data. In such a factor-augmented VAR (FAVAR), the dynamic properties of the common components of important banking variables are modeled alongside an array of macroeconomic data, see Jimborean and Mésonnier (2010) and Buch, Eickmeier, and Prieto (2010). Something of a drawback of the FAVAR approach is that first stage extraction of principal components does not deal well with the type of rotating panel data typically encountered in practice.[6]

Our approach to identification is standard in the VAR literature, and rests on institutional facts particular to the UK regulatory environment. An alternative idea is to identify shocks at the micro level, and then to aggregate them in order to assess their macroeconomic effects. Examples may be found in Amiti and Weinstein (2013), who use matched bank-firm loan data, and Bassett, Chosak, Driscoll, and Zakrajšek (2014), who use bank-level survey responses on loan demand. However, the micro-identification approach requires adequate controls for bank- and firm-level credit demand to be found, and these are lacking in the UK case.

The rest of this paper is organized as follows. Section 2 gives details of the data that is used in the empirical work. Section 3 discusses the institutional arrangements under which microprudential policy was set. Section 4 summarises bank-level evidence on the relationship between capital regulation and lending and sets out the macroeconomic model used in the main analysis. The principal results on the macroeconomic impact of capital regulation can be found in section 5, while section 6 reports on the results of a counterfactual experiment in which capital requirements are set according to a macroprudential rule. Section 7 presents robustness checks on the estimation method and identification, and section 8 concludes.

2 Data

Two categories of information are used in the analysis: aggregate macroeconomic data, and micro banking data.[7] The banking data is a group-consolidated level panel covering some 19 years. The panel is unbalanced and rotating, principally due to multiple merger and takeover

[6]This limitation has lead Jimborean and Mésonnier and others to filter out banks which enter or exit over their sample period, including due to mergers, which raises concerns of sample selection bias.

[7]Details of the data and its sources can be found in Appendix A.

events.[8] After filtering to remove banks who advanced little or no direct loans to firms or households, the dataset contains 644 observations on 21 UK banking groups, treating pre- and post-merger banks as separate entities (see Table A.2). The sample is dominated by larger banks, whose influence over aggregate lending is correspondingly of greatest importance.

Of central interest in this study is the confidential information on how much capital supervisors required banks to fund themselves with, over and above the Basel minimum. Breaches of this additional requirement, referred to as 'individual capital guidance' (ICG), would trigger regulatory action, so the ratio of regulatory capital (including the ICG) to risk weighted assets is referred to as the 'trigger ratio'.[9] In addition to the required capital ratio, we have information on banks' published capital ratio, constructed as the ratio of tier 1 or 'core' capital to risk-weighted assets. We also make use of two measures of bank lending: to private non-financial corporates (PNFCs), and secured mortgage lending to households. Both credit variables are measured in terms of the flow of new lending in the current quarter (which differs from the change in the stock of lending due to write-offs and other items) scaled by the stock of loans outstanding in the previous quarter.[10]

The macroeconomic data includes a set of standard core variables (in levels): log real gross domestic product, the log consumer price index and the Bank of England base rate. In common with Walentin (2014) and Iacoviello and Minetti (2008), who also build models on UK data, we include average house prices and mortgage spreads. In addition, we include the proportion of households 6 months or more in arrears.[11] As a rough proxy for the marginal cost of external finance for corporations, we use the spread between average investment grade corporate bond yields and 10 year gilts. We construct aggregate counterparts of the bank-level capital and trigger ratios by taking the weighted average across banks each period; with weights determined by banks' lending share.[12] The aggregated data, plotted in Figure 1,

[8]Davies, Richardson, Katinaite, and Manning (2010) detail some history of UK banking sector consolidation. Most consolidation involving smaller firms occurred in the early 1990s. For example, Midland Bank was purchased by HSBC in 1992; Lloyds absorbed the TSB and Cheltenham & Gloucester Building Society in 1995. The Lerner index for the UK banking industry rose from a little above 0.1 in the early-1990s to 0.25 in the mid-1990s. It was only a little higher than that at the end of our sample in 2008.

[9]Throughout, the Basel minimum requirement was a risk-asset ratio of 8%, of which at least 4% had to be tier 1 capital. The ICG framework was initially implemented under the Basel I regime, but was extended under Pillar 2 of Basel II (introduced in 2004).

[10]Aggregate counterparts of the lending series are obtained from Bankstats, and are based on a moderately larger sample of lenders than those in the micro data. From the late 1990s onwards, the Bank of England has collected securitization adjusted data on lending stocks, and we use these throughout. Securitization made a negligible contribution to UK lending prior to that time. We refer the reader to Bridges, Gregory, Nielsen, Pezzini, Radia, and Spaltro (2014) for additional description of the micro dataset, and details of its underlying sources.

[11]The principal reason for including arrears is to explain the data in the early 1990s, when a significant housing bust and high interest rates led a large number of UK households to fall behind on repayments, and to nearly 350,000 homes being repossessed. These factors continued to depress mortgage lending long after a general economic recovery was underway.

[12]The results below are near identical when using the unweighted series, so we report only on the weighted series.

naturally inherit the relatively short history of the underlying micro data. The central point of note is that micro variation in trigger ratios is not averaged away by aggregation, as Aiyar, Calomiris, and Wieladek (2014) also remark. Moves in the trigger ratio are relatively infrequent over the first part of the sample, but tend to be large; ignoring zero and very small changes, the average quarterly change in the system-wide capital requirement was 15 basis points over the full sample.

3 Institutional background and identification

Under the UK microprudential regime in force over the roughly two decades following the introduction of the first Basel Accords, bank supervisors did not have the authority to change minimum capital standards across the board. Instead, changes to the level of system-wide capital requirements were a by-product of changes to the requirements placed on individual banking groups. The institutional practices in place in the UK indicate that such changes were not made in response to current macroeconomic news. Under Basel rules, the business cycle was just one amongst a panoply of risks not captured by minimum (Pillar 1) standards for supervisors to consider. Little concrete guidance was given on where and how to account for it.[13] But of greater significance was the manner in which supervision was actually practiced. Supervisors aimed to avoid abrupt changes in regulation, except in extreme circumstances, through frequent contact with regulated banks.[14] Macroeconomic conditions played into supervisors' thinking about bank health, but macroeconomic news was not in itself a reason for immediate regulatory action. Indeed, the micro data reveals only a handful of quarters where all the changes in requirements—which affected roughly one in six banks each quarter—went in the same direction, as might be expected if supervisors did respond to a common macroeconomic factor.[15]

[13]The risks to be covered by supervisory review under Basel II Pillar 2 included: concentrations of credit risk; interest rate risk in the banking book; and operational, reputational and strategic risk. The Basel documents speak of being 'mindful' of the state of the business cycle, but also that Pillar 1 requirements already account for 'uncertainties ... that affect the banking population as a whole' (Basel Committee on Banking Supervision, 2006, paras. 726 and 757). See also the discussion in Aiyar, Calomiris, and Wieladek (2016).

[14]Under the pre-1997 Bank of England regime, this fact is evident from the infrequent adjustment of capital requirements observed in Figure 2. If, as seems possible, banks were able to anticipate regulatory action, as a result of their ongoing dialogue with bank supervisors, the estimated effects of actual changes in capital requirements would naturally be attenuated. On the other hand, none of the bank-level variables included in the regressions summarized in Table 1, in particular lending growth, were significant predictors for the trigger ratio. Under the post-1997 FSA regime, formal supervisory reviews were conducted at set two-year intervals, all but ruling out direct reactions to macroeconomic news. I am grateful to Brian Quinn and Michael Straughan for their guidance on operational practices.

[15]Nevertheless, Aiyar, Calomiris, and Wieladek (2014) have argued that the result, if not the intention, of supervisory actions was to produce counter-cyclical movements in aggregate capital requirements. They contend that regulators operated a *de facto* macro-prudential regime (between 1998 and 2007), pointing to evidence that 'average capital requirements across the banking system were ... strikingly counter-cyclical' (p. 10). They report a correlation

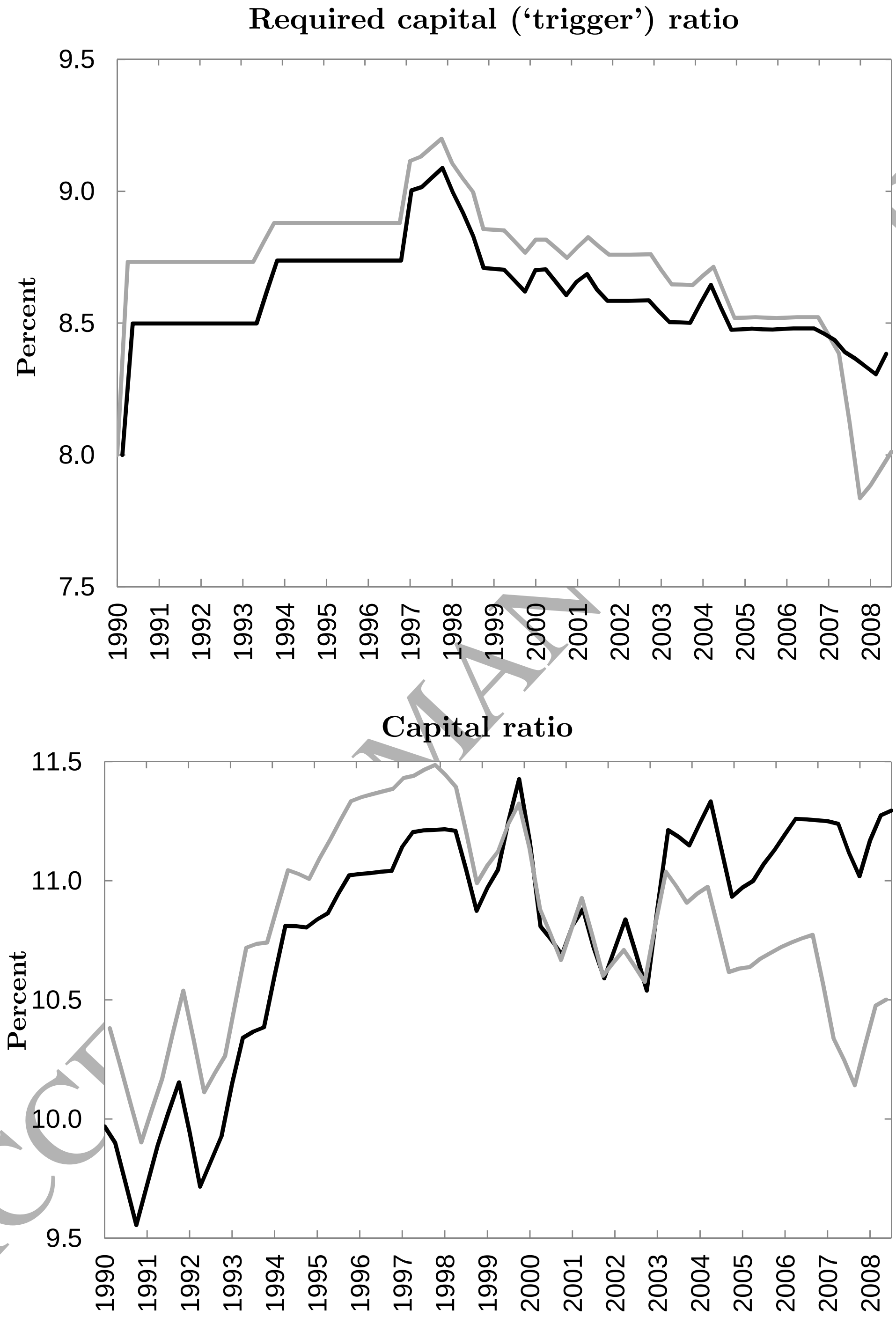


Figure 1. Required and actual banking system capital ratios. *Note:* black line — weighted by share of lending; gray line – – unweighted (simple average). See Appendix A for additional detail.

A second important institutional consideration is that bank trigger ratios were not public information, but were rather communicated privately between the supervisor and the individual regulated institution. Changes in the trigger ratio were therefore not directly observed by the public, although an individual bank's response to such a change naturally could be. In our empirical work we therefore exclude direct channels from changes in capital requirements to the macroeconomy, while allowing indirect channels through bank lending to operate, with a one quarter lag. Perhaps the most substantive aspect of this exclusion restriction is the assumption that monetary policy did not respond directly to changes in microprudential regulation, particularly before 1997, when the Bank of England had responsibilities for both monetary and microprudential policies.[16] Although a strict separation existed between these functions, effectively preventing the routine flow of regulatory information to other areas of the Bank, we nevertheless examined the official record of Monetary Policy Committee meetings as a check. There is no mention of capital requirements or of banking system capital until September 2007; references remain infrequent thereafter, and do not appear to have had a direct bearing on the monetary policy decision.[17] This is understandable given that the only instance of modest banking instability that the UK experienced in this period was amongst small- and medium-size banks during 1991-1994 (see Logan, 2001), and given the removal of direct supervisory powers from the central bank after independence in 1997. Although not conclusive, the official record does not provide evidence that contradicts our assumption.

4 The empirical model

The tool we adopt to investigate the macroeconomic impact of prudential policy is a structural VAR. The advantage of the VAR approach is that it captures complex dynamic interactions between banking and macro variables, while imposing few restrictions. The mid-size VAR that we work with, containing 11 variables, two lags and an intercept requires us to estimate

between the average trigger ratio and annual GDP growth of between 0.44 and 0.64, depending on the weighting scheme used in aggregation. On our 1989-2008 sample and weighting trigger ratios by UK lending share, the correlation is 0.40. Of course, unconditional correlations do not say anything about marginal effects after controlling for bank-specific factors; but in the panel regression (C.1) for the trigger ratio, current GDP growth is insignificant, with a t-ratio of 0.13.

[16]The power to set monetary policy rested with the Chancellor of the Exchequer until the Bank was granted operational independence in 1997.

[17]Official minutes of the Monetary Policy Committee meetings are available from June 1997; prior to that, minutes of the monthly meetings between the Chancellor of the Exchequer and the Governor of the Bank of England are available from April 1994. The sole mention of prudential regulation during the sample period we consider is contained in the minute of the January, 2008 meeting (para. 4): '[B]anks were becoming more cautious about expanding their balance sheets ... [and] the introduction of the new Basel II regulatory regime for all banks at the beginning of 2008 ... might have a knock-on effect on their willingness to lend'.

a large number of parameters.[18] Dense parameterization can in practice lead inference to be unstable. Under the Bayesian approach to estimation, a prior distribution for the parameters that contains substantive information along some, but not necessarily all, dimensions is used to help overcome this difficulty.

4.1 Estimation and inference

Letting $\mathbf{y}_t$ be a vector containing the $m = 11$ aggregate variables listed in section 2 and summarized in Table A.1, and $\mathbf{x}_t = (\mathbf{y}_{t-1}^\intercal, \ldots, \mathbf{y}_{t-p}^\intercal, 1)^\intercal$ be a vector of lag terms, the structural VAR(p) is given by:

$$\mathbf{y}_t^\intercal \mathbf{A} = \mathbf{x}_t^\intercal \mathbf{F} + \boldsymbol{\nu}_t^\intercal, \quad \boldsymbol{\nu}_t \sim N(0, \mathbf{I}) \tag{1}$$

where $\mathbf{A}$ summarises the contemporaneous relationships between the elements of $\mathbf{y}_t$, $\boldsymbol{\nu}_t$ is a vector of independent stochastic disturbances, and $\mathbf{F} = (\mathbf{F}_1^\intercal, \ldots \mathbf{F}_p^\intercal, \mathbf{c})^\intercal$ collects together both the intercept vector $\mathbf{c}$ and the lagged autoregressive matrices. Individual structural equations are read down columns of $[\mathbf{A}^\intercal; \mathbf{F}^\intercal]^\intercal$, with variables in rows.

Following Sims and Zha (1998), we take structural equations to be *a priori* independent. Then denoting columns of the $\mathbf{A}$ and $\mathbf{F}$ matrices by lower case letters, for each equation i the prior parameter distributions can be written:

$$\mathbf{a}_i \sim \mathsf{N}(\mathbf{0}, \underline{\mathbf{S}}_i) \quad \text{and} \quad \mathbf{f}_i | \mathbf{a}_i \sim \mathsf{N}(\underline{\mathbf{B}}\mathbf{a}_i, \underline{\mathbf{H}}_i), \quad i = 1, \ldots, m \tag{2}$$

where $\underline{\mathbf{S}}_i$ and $\underline{\mathbf{H}}_i$ are prior covariance matrices, and $\underline{\mathbf{B}}$ summarizes beliefs about reduced-form dependencies between variables (see Appendix B). To set $\underline{\mathbf{B}}$ in the prior conditional distribution in (2) we make selective use of information from a subset of the variables in the sample data $\mathbf{Y} = [\mathbf{y}_1, \ldots, \mathbf{y}_T]^\intercal$ observed before 1990, and from the micro data described in section 2, which we collect into $\mathcal{X}_0$. We maintain the assumption that $\mathbf{Y}$ and $\mathcal{X}_0$ are independent, conditional on the parameters $\boldsymbol{\theta} \coloneqq (\mathbf{a}_i, \mathbf{f}_i)_{i=1}^m$.[19] The posterior density function is then given by $p(\boldsymbol{\theta}|\mathbf{Y}) \propto p(\mathbf{Y}|\boldsymbol{\theta})p(\boldsymbol{\theta}|\mathcal{X}_0)$, with posterior inference based on the output of the Waggoner and Zha (2003) Gibbs sampler. An appealing aspect of the posterior parameter estimates is that they are a function of both micro and macro information. The conditional independence assumption considerably simplifies the analysis, at the cost of making what may be a somewhat crude approximation. The robustness checks reported in section 7 therefore report on how varying the weight placed on prior information affects the main results.

[18] More or fewer lags were not strongly favoured by the model's marginal likelihood, but in practice longer lags caused the bank-level estimates to become unreliable.

[19] Applied Bayesian analyses frequently draw on non-sample data to formulate priors, making the same implicit conditional independence assumption. Our application follows the same rationale as that of Chang, Gomes, and Schorfheide (2002, p. 1502), wherein (B.1) and (C.1) are the equivalents to their micro and macro models.

4.2 Capital requirements and lending at the bank level

The relationship between minimum bank capital requirements, bank capital, and lending to households and firms underpins the effect of prudential policy on aggregate activity. At the individual bank level, several hundred changes to trigger ratios are recorded in our sample (see Bridges, Gregory, Nielsen, Pezzini, Radia, and Spaltro, 2014, Table B). These provide ample variation to estimate the relationships between capital and lending, which we use to set elements of $\underline{\mathbf{B}}$. Moreover, with the individual data we are able to control for both observed and unobserved heterogeneity, the absence of which might otherwise cause omitted variable bias. As our sample is focused on larger banks, we do not allow for size-related dynamic heterogeneity.

Table 1 presents reduced-form fixed effects regression results for each bank-level variable.[20] The first two columns report on bank lending equations. Mortgage lending growth is moderately persistent, likely due to banks' reluctance to make sudden changes in consumer lending policy, whereas corporate lending growth shows lower persistence. The signs on capital variables in the lending equations are as expected, with a higher trigger ratio acting to slow growth both in secured and corporate credit, and a higher capital ratio acting to increase them. The trigger ratio is statistically significant in the corporate lending equation, but not in the secured lending equation. However, the results indicate that there are indirect channels linking the capital requirements to mortgage lending through interactions between components of banks' loan portfolios: in particular, when a bank makes a higher volume of corporate loans, there is a statistically significant reduction in mortgage lending.

The second two columns report on bank capital equations. Both actual and required capital ratios are estimated to be highly persistent, consistent with infrequent adjustment of the latter. The estimates show that a higher trigger ratio tends to substantially raise banks' capital ratios, consistent with banks acting to restore the buffer of capital held above the regulatory minimum (the long-run multiplier is statistically indistinguishable from unity, indicating one-for-one pass through from requirements to actual capital ratios; see also Francis and Osborne, 2009a). None of the observable controls appear to explain variation in the trigger ratio itself; the exception is lags of the actual capital ratio, which enter with a small long-run multiplier.

4.3 Other prior information

The other information we use to set the prior is intended to counteract potential bias arising from the short history at our disposal. The 18 year period covered by our bank-level data

[20]The results presented here are closely related to those reported in Francis and Osborne (2009a), Aiyar, Calomiris, and Wieladek (2014) and Bridges, Gregory, Nielsen, Pezzini, Radia, and Spaltro (2014). However these papers and ours contain differences in sample period, coverage, and/or the treatment of mergers. Therefore for completeness we present our own results here.

Table 1. Bank-level estimates of the relationships between minimum capital requirements, lending and capital.

	Dependent variable							
Regressor	Secured lending		PNFC lending		Capital ratio		Trigger ratio	
Secured lending	**0.534**	(8.51)	– 0.160	(1.17)	**0.025**	(2.43)	0.000	(0.15)
PNFC lending	**– 0.040**	(2.81)	**0.218**	(2.59)	– 0.002	(1.41)	0.000	(0.18)
Capital ratio	**0.120**	(2.74)	0.300	(0.74)	**0.794**	(17.65)	**0.026**	(2.99)
Trigger ratio	– 0.037	(0.20)	**– 2.18**	(2.23)	**0.234**	(2.56)	**0.897**	(34.88)
Bank-level controls	yes		yes		yes		yes	
Time fixed effects	yes		yes		yes		yes	
R^2 (within)	0.484		0.282		0.904		0.920	
$N \times T$	644		644		644		644	

Note: Table shows within-group estimates for regressions of each of the dependent variables given in the column headings on two lags of each of the regressors given in rows (equation (C.1)). Sums of coefficients on lags shown. Absolute value of robust t–statistic in parentheses. Bank-level controls: Ratio of risk-weighted to total assets; ratio of tier 1 to total (tier 1 plus tier 2) capital; provision ratio; loan:deposit ratio; size (total assets). Sample: N = 21 (see Table A.2), 1989:4–2008:3. Further details may be found in Appendix C.

encompasses a single business cycle recovery, and a single downturn, which makes statistical detection of a 'medium-term' financial cycle, lasting on average around 16 years problematic (Drehmann, Borio, and Tsatsaronis, 2012). Failing to capture medium-term relationships between output and credit over-weights an unusual period in the early 1990s that combined a strong economic recovery with weak bank mortgage lending associated with a major housing bust (on which, see Muellbauer and Murphy, 1997). We therefore set elements of $\underline{\mathbf{B}}$ corresponding to interactions between macroeconomic and bank lending variables to point estimates from an auxiliary reduced-form VAR run on 1975:Q1-1989:Q4 data.[21]

4.4 Identifying restrictions

As it stands, the model in (1) embodies no restrictions, aside from the requirement that the $\mathbf{A}$ matrix be of full rank, and so is not identified. The schematic in Table 2 details how the identifying restrictions discussed in section 3 map into the VAR, partitioning $\mathbf{y}_t$ into four distinct blocks of variables: macroeconomic ('M'); bank lending ('B'); the aggregate bank capital ratio ('K'); and the trigger ratio (policy variable, 'P'). The restriction that macroeconomic variables

[21]The main impediment to extending the historical data before 1990 is the measurement of regulatory capital itself: regulatory treatment of capital, and reported capital ratios, were not on the same basis as afterward. The Basel Committee's framework for capital measurement, which cemented the role of risk weighting assets in capital adequacy assessments, was developed in the mid-1980s, and a bilateral US-UK capital adequacy agreement was concluded in 1987 (see Tarullo, 2008). The Bank of England detailed its proposed rules for implementation of Basel I in October, 1988. The Accord was fully introduced to UK law in 1990.

Table 2. Unrestricted and restricted VAR coefficients

Impact matrix **A**

Variables	M	B	P	K
M	×	×		×
B		×		×
P			×	×
K				×

Lag matrix $\mathbf{F}_\ell$

Variables	M	B	P	K
M	×	×		×
B	×	×	×	×
P		×	×	×
K		×	×	×

Note: Equations in columns, variables in rows. An × indicates position of non-zero coefficient blocks in the **A** and $\mathbf{F}_\ell, \ell = 1, \ldots, p$ matrices in equation (1). A blank entry indicates a zero restriction. M – macroeconomic variables; B – bank lending variables; K – system-wide capital ratio; P – trigger ratio (prudential policy variable).

affect capital regulation via their impact on lending and capital alone, and with a lag consistent with reporting delays on banking variables, is captured by the exclusions on **A** and $\mathbf{F}_\ell$ in the microprudential policy equation (the column headed 'P'). Second, the restriction that no macroeconomic variable is able to respond directly to the policy variable, either within the quarter or with a delay, means the trigger ratio is excluded from the macroeconomic block (the column headed 'M') of the VAR (see also Peek, Rosengren, and Tootell, 2003). By contrast, capital ratios respond immediately to changes in the trigger ratio, and loan quantities and proxies for the cost of credit respond with a lag, consistent with there being some delays in arranging new loans, and some stickiness in loan prices, as with the balance sheet dynamics reported for U.S. banks by Hancock, Laing, and Wilcox (1995).[22]

5 The macroeconomic impact of microprudential regulation

5.1 Dynamics following a regulatory shock

The principal findings of this paper relate to the macroeconomic effects of changes in microprudential capital requirements. The main experiment that we consider is an unanticipated increase in the trigger ratio, the minimum capital to risk-weighted asset ratio required by bank regulators. The shock is normalized to 50 basis points, somewhat larger than the average change to requirements in the data, but a plausible benchmark for the size of change that could be contemplated in future (see section 6). Figure 2 shows the responses of banking system variables, along with pointwise 68% error bands. Their responses under the prior are indicated by '+' symbols. The initial impact of the shock falls on the aggregate capital ratio. There is an immediate increase in this ratio of around 10 basis points, and it then continues to increase over a period of approximately 18 months. Surplus capital, having initially fallen, is therefore

[22] Based on the estimated Bayes factor for the restricted and unrestricted models the data overwhelmingly support, by a factor of 150, the over-identifying restrictions imposed on the VAR.

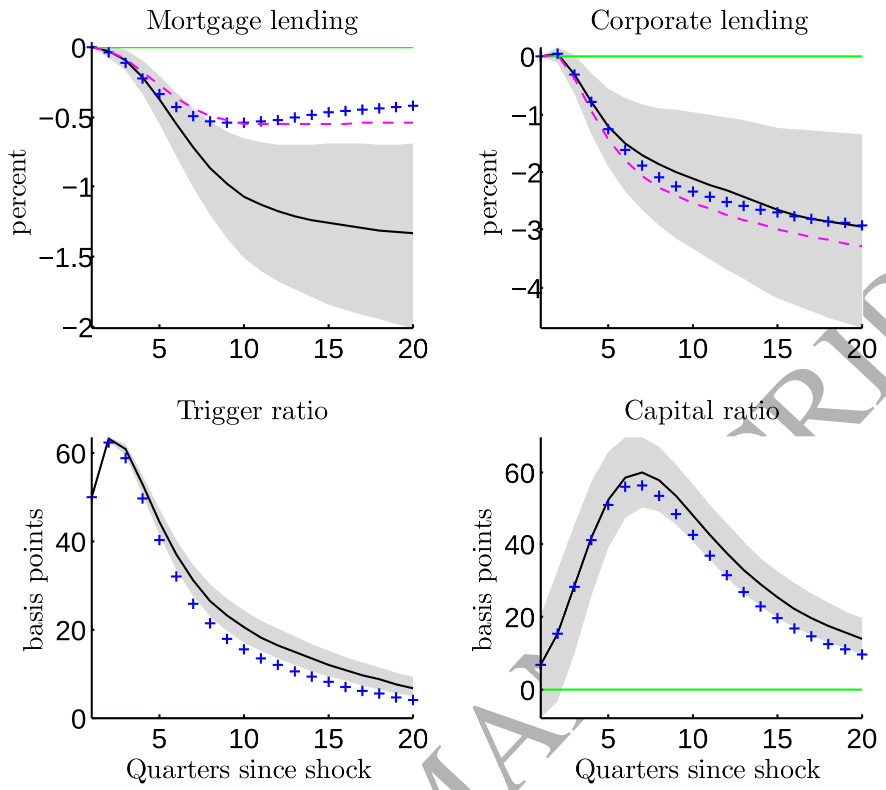


Figure 2. Banking system response to an unanticipated increase in aggregate capital requirements. *Note*: The panels depict the impulse response functions of aggregate lending and capital variables to an orthogonalized shock to the trigger ratio of 50 basis points. – Median response. The shaded area represents pointwise 16 to 84 percentile error bands. + Bank-level prior responses. −− VAR responses, with feedbacks closed down. The responses of household and corporate lending are cumulated growth rates.

rebuilt fairly rapidly, and has returned to its baseline value within two years.

The effect of capital movements on the cumulated stock of bank lending is rapid and significant. Secured household lending is close to 0.5% lower, relative to trend, at around 18 months, and non-financial corporate lending is around 1.5% lower. The estimated responses we observe are consistent with equity capital being costly for banks to raise, leading regulatory capital requirements to be a binding constraint on aggregate bank lending.[23] The rate of decline in loan growth levels off roughly coincident with the return of the aggregate buffer to its pre-shock level, consistent with the findings in Mésonnier and Stevanovic (2015). Because household secured and corporate categories attract high risk weights—50% and 100% respectively under

[23]Binding in the sense of influencing banks' lending behaviour. As noted above, banks maintain a buffer of capital above regulatory minima to avoid accidental breaches of 'hard floor' requirements. The existence of the buffer does not imply regulatory constraint is 'slack'.

Basel I—lower loan volumes entail a higher risk-based capital ratio, other things equal.[24]

Figure 2 further allows us to examine the responses from our VAR alongside those under the prior estimated from bank-level data. The comparison shows that the decline in mortgage lending is almost a full percentage point greater in the VAR case, while that for corporate lending is slightly smaller. The source of these differences can be understood by closing down the feedbacks that are present in the VAR, to make it behave like a bank-level model (dash lines): in this case the two sets of responses are closely aligned.[25] The macroeconomic response to the regulation-induced decline in lending plays back onto banking system itself, amplifying and propagating the initial impulse. As we will go on to demonstrate, it is this two-way interaction between the banking block and the macroeconomy that is key to understanding the effects of prudential policy on the economy at large.

Figure 3 summarises the responses of variables in the macroeconomic block.[26] Aggregate real expenditure declines in response to tighter bank credit conditions, consistent with the existence of credit constrained and bank-dependent agents—a fundamental tenet of financial accelerator theories (see Bernanke, Gertler, and Gilchrist, 1999). Changes in bank lending and in real expenditure propagate to broader financial conditions. Corporate spreads widen as bank credit supply contracts, which is consistent both with banks choosing to reduce high risk-weight assets by selling off corporate bonds, and with substitution by marginal bank borrowers into capital market funding. Consistent with a strong credit supply effect on the housing market, house prices decline by 1% relative to baseline, and arrears increase by approximately 0.05 percentage points (not shown). Mortgage spreads are initially flat, but after four quarters stay persistently above their pre-shock values. Consumer prices are marginally higher over the first two years, consistent with a cost channel operating through loan spreads, whereafter they undergo a noticeable decline, bringing about a systematic easing in monetary policy.

These patterns are in line with the responses of the U.S. economy to a bank credit supply shock reported in Bassett, Chosak, Driscoll, and Zakrajšek (2014). There, a shock that produces a 4% decline in lending capacity (loans outstanding and unused commitments) raises corporate bond spreads by 40 basis points, and causes a fall of up to 0.7% in real GDP, with offsetting movements in monetary policy. Qualitatively, these movements closely resemble the regulation-induced supply shift we identify (although we lack data on loan commitments). On a long

[24] A drawback of the linear VAR approach is that it cannot accommodate the possibility that the cost of raising equity may with the state of the business cycle, implying that the effects of trigger shocks are state-dependent. Rather, it delivers an average response to such shocks. For an investigation into the dependence of bank-level responses upon the level of aggregate credit growth, see Bahaj, Bridges, Malherbe, and O'Neill (2016).

[25] The prior and posterior distributions of the parameters in the bank lending block of the VAR are displayed in Figures E.1–E.4 of Appendix E.

[26] Appendix D displays estimated impulse response functions when the policy and capital variables enter the lending equations in first differences, rather than in levels.

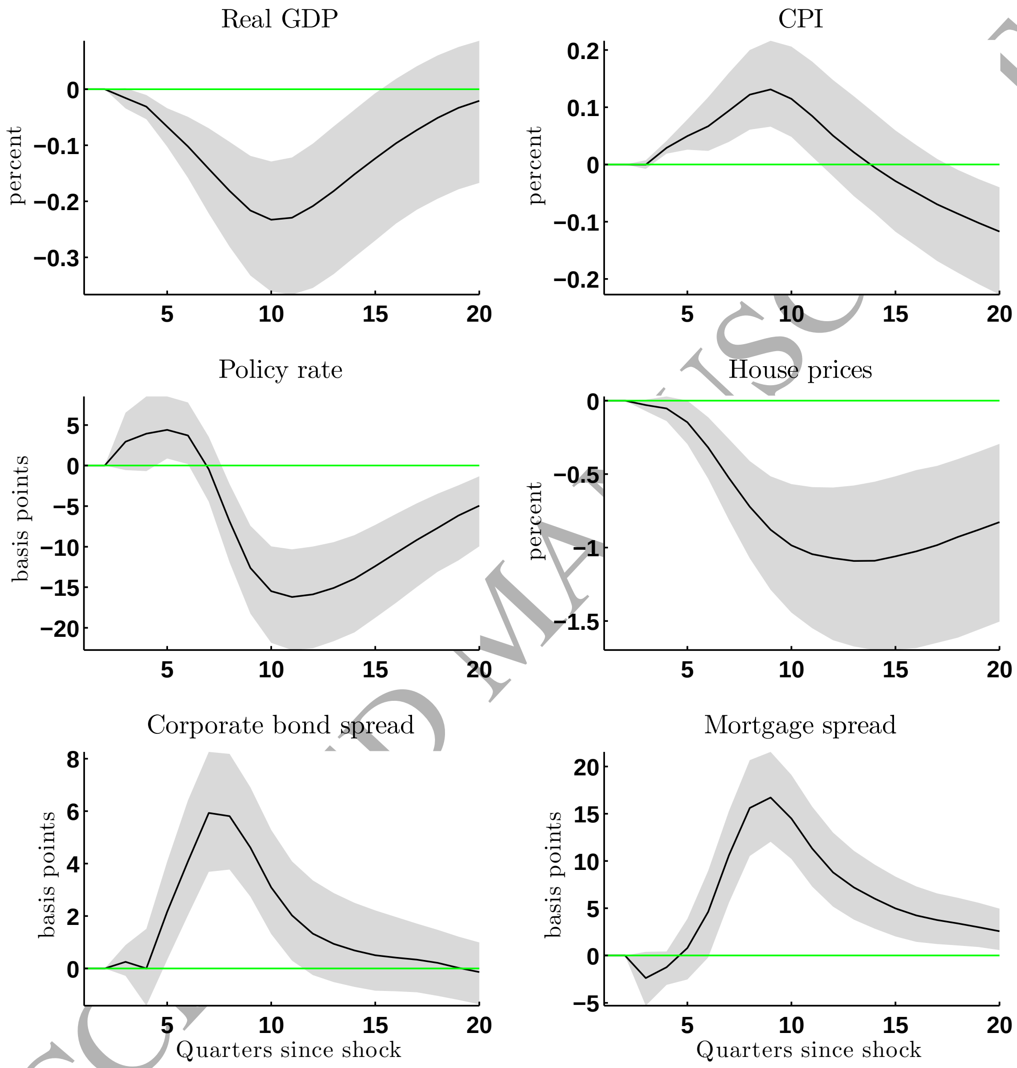


Figure 3. Macroeconomic response to an unanticipated increase in aggregate capital requirements. *Note*: The panels depict the impulse response functions of selected variables to an orthogonalized shock to the trigger ratio of 50 basis points. – Median response. The shaded area represents pointwise 16 to 84 percentile error bands.

sample of UK data, Barnett and Thomas (2014) likewise estimate that a credit supply shock that reduces lending growth by 1% raises corporate bond spreads by a similar amount, and lowers GDP growth by up to 0.1%. Their findings indicate a slightly weaker pass-through from bank credit to aggregate expenditure than estimated here (but they report larger effects on a post-1992 sub-sample).

Variance decompositions show that the majority of the variation in the trigger ratio at horizons up to a year is the result of regulatory shocks. At the two-year horizon, they account for about 16% of the variation in the capital ratio, and 2% of the variation in mortgage lending growth. But as large regulatory shocks were observed only infrequently, on average their contribution to fluctuations in the macroeconomy was—reassuringly—very small. Historical decompositions, which trace the cumulative impact of structural shocks at each date, indicate that regulatory shocks made modest contributions to movements in aggregate variables, particularly in the mid-1990s. Figure 4 shows that in the absence of changes in capital requirements, mortgage spreads would have been some 15 basis points lower and corporate bond spreads around 5 basis points lower than was the case. Mortgage lending growth was reduced by 0.1 annual percentage points, and corporate lending growth by some 0.3 percentage points. These effects fed through to house prices, which were lower by up to 1% as a result (not shown). The largest impact fell on the banking system capital ratio: it was 80 basis points higher in 1998 than in the absence of shocks, and 40 basis points lower in 2008.

In summary, we find that changes in regulatory capital requirements have real effects, consistent with the developing literature on the macroeconomic impact of financial shocks. Regulation was not, on average, an important source of aggregate fluctuations, but large regulatory shocks caused movements in mortgage and corporate bond spreads, house prices, and in particular the banking system capital ratio.

5.2 Feedbacks and financial accelerator effects

To better understand the transmission channels at play, in this section we unpick the full system responses described above using posterior simulations in which various endogenous variables are held constant at their baseline values by selectively setting coefficients to zero, as in Sims and Zha (1996).[27] Figure 5 indicates how the system responds in the absence of the financial accelerator mechanism, that is, holding mortgage and corporate bond spreads constant. For comparison, the baseline responses from Figures 2 and 3 are shown as dash lines. In this case we see that the decline in aggregate expenditure is about half as large as in the baseline case where spreads rise: Higher credit spreads act to amplify the regulatory

[27]These experiments are not intended to assess the plausibility of the implied restrictions, or to pose a counterfactual change in the structure of the economy (for which, see section 6). Rather, they are intended to highlight the role played by the dynamic responses of particular variables.

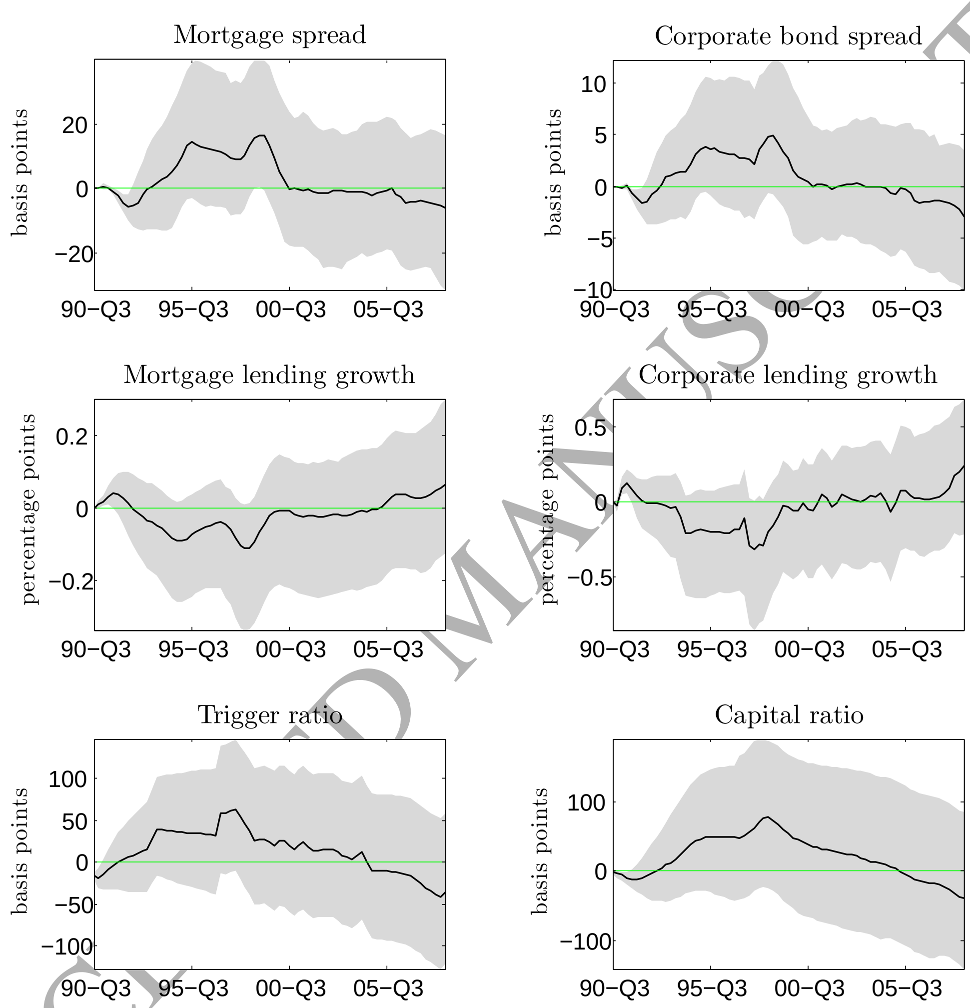


Figure 4. Historical contribution of regulatory shocks to path of selected variables. *Note*: The panels depict the difference between the actual path of each variable, and the path that would have been followed if regulatory shocks had be zero. – Median path. The shaded area represents pointwise 16 to 84 percentile error bands.

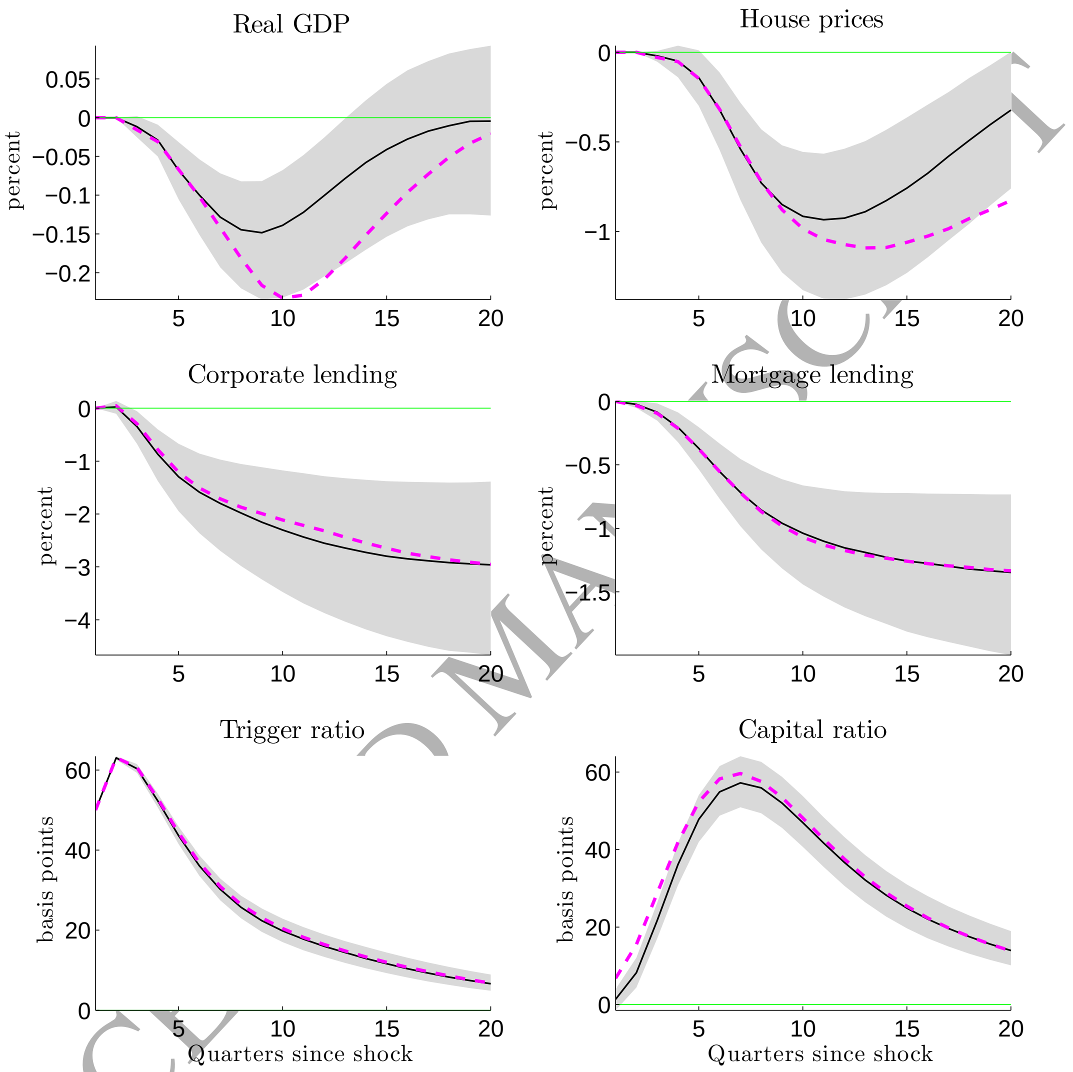


Figure 5. Responses to an unanticipated increase in aggregate capital requirements holding credit spreads constant. *Note*: The panels depict the impulse response functions of selected variables to an orthogonalized shock to the trigger ratio of 50 basis points, with mortgage and corporate bond spreads held constant. – Median response. - - Unrestricted impulse-response function (see Figures 2 and 3). The shaded area represents pointwise 16 to 84 percentile error bands.

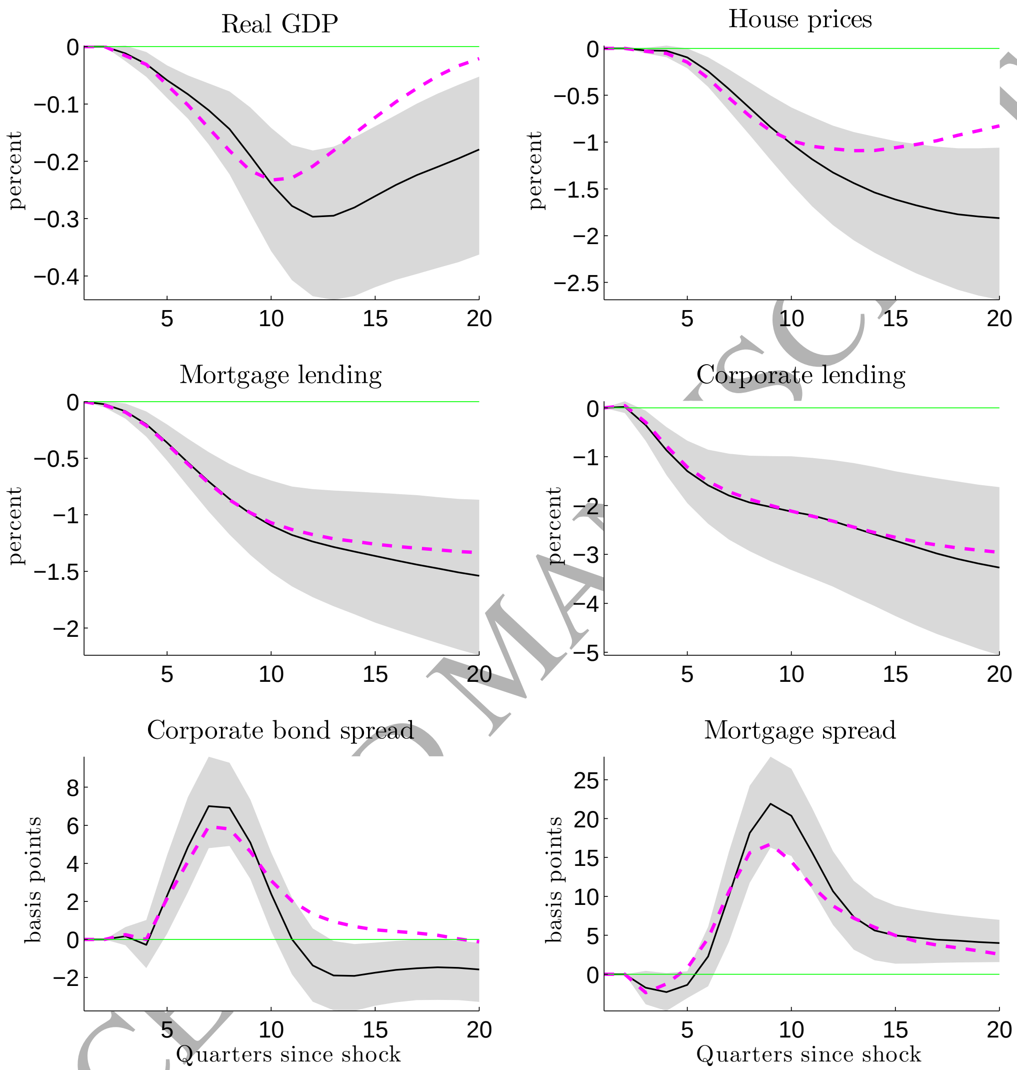


Figure 6. Responses to an unanticipated increase in aggregate capital requirements holding policy interest rates fixed. *Note*: The panels depict the impulse response functions of selected variables to an orthogonalized shock to the trigger ratio of 50 basis points, with the short term nominal interest rate held fixed. – Median response. - - Unrestricted impulse-response function (see Figures 2 and 3). The shaded area represents pointwise 16 to 84 percentile error bands.

disturbance, as in the classic financial accelerator mechanism. Both firm-side and household-side financial accelerator effects appear to be important, as emphasised in Iacoviello (2005) for example.

It is noteworthy that Figure 5 shows bank lending and bank capital variables responding similarly to the baseline case, indicating that feedbacks from spreads to the banking system are relatively weak. Feedbacks appear to be most important within the banking system itself. For example, if corporate lending is held constant, the responses of secured lending, spreads, house prices and real expenditure are all muted; if mortgage lending is held constant, the transmission to the real economy is close to nil, indicating the central role played by housing (see in particular Iacoviello and Minetti, 2008; Walentin, 2014).

Our second scenario involves holding the policy interest rate constant. The prolonged period that advanced economies, including the UK, have spent at the zero nominal interest rate bound since 2009 naturally raises the question of how tighter regulation might play out when monetary policy is constrained. Figure 6 shows that the constraint on monetary policy leads to amplified responses to tighter prudential policy. The main effects fall on the housing market. House prices decline by around 2% four years out versus 1%, and arrears (not shown) also rise strongly. Around 5bps are added to mortgage spreads, likely the result of the higher credit risk associated with rising arrears, and the impact on mortgage lending is modestly negative. From a stabilisation perspective, the most significant finding is that the decline in aggregate expenditure in response to tighter prudential policy is both larger—at about 0.3%, compared to the baseline of 0.2%—and more protracted when monetary policy rates are constant.

6 A macroprudential counterfactual

It is now widely recognized that pre-2008 bank regulation was excessively focused on individual institutions, and failed to act on build-ups of system-wide risk. The macroprudential approach to regulation explicitly takes into account trends in the financial sector that pose such risks, in particular rapid growth in aggregate bank credit (for an overview, see Hanson, Kashyap, and Stein, 2011). Basel III introduces a new regulatory tool, the counter cyclical buffer (CCyB), to address these macroprudential concerns. The CCyB, which applies to all banks, is a variable requirement on the common equity ratio. It is one of the macroprudential tools given to national regulatory authorities in recent EU-wide legislation, known as Capital Regulation Directive or CRD IV, to be phased in from 2016 in Europe. An important question for policymakers is the extent to which changes to the required countercyclical buffer will lead to changes first in banking system capital ratios, and second in aggregate credit growth and wider economic conditions. Answering these questions is hard because there have so far been only limited applications of the CCyB, but also interesting because the CCyB is not prone to some of the leakages associated with other prudential tools.[28] The previous sections have shown how variation in microprudential capital requirements led to variation in banking system capital ratios that exerted some influence on the macroeconomy, and so it is tempting to try to extrapolate from the old regime in the hope of learning something about the new one.

In order to provide some indicative evidence on the effect of a countercyclical macroprudential capital requirement, the remainder of this section reports on the results of a counterfactual simulation exercise employing the model developed above. The basic idea is straightforward. We use the VAR to recover the time series of structural shocks that hit the economy over the sample period. Then taking the proposed macroprudential policy instrument to be the trigger ratio, we modify the corresponding equation in the VAR to introduce some counterfactual feedback from financial conditions (to be specified) to system wide bank capital requirements. We then ask how the paths followed by the endogenous variables of the system change when the model is simulated using the same exogenous structural shocks as the driving force, but with the counterfactual equation setting the aggregate required capital ratio.[29]

The principal objection to the counterfactual analysis just described is that it falls foul of the Lucas (1976) critique, as it takes the remaining structural relations in the VAR to be invariant to the introduction of the macroprudential policy. If private agents do take changes to bank regulation into account when forming expectations of future policy, the results may be in error. However, there are reasons to proceed, albeit with some care. In the specific context of risk-based capital regulation, which was itself a novel policy tool in 1990, it is not clear that agents would have been capable of formulating an estimate of what the 'usual' policy response would be; deviations from the estimated rule, particularly over the early part of the sample, are so unlikely to cause Lucas-type concerns. Moreover, as will be made clear below, the simulated impact of macroprudential policy on macroeconomic variables is for the most part rather modest.

In weighing the merits of this exercise, it is important to recognize that the treatment of banking in DSGE models remains quite stylized, and that a consensus view on the specification of a fully structural model for macroprudential analysis is not currently in evidence in the profession. The discussion below provides a qualitative comparison between one candidate model, due to Angelini, Neri, and Panetta (2014), and our own, which indicates that the two approaches lead to similar conclusions.

[28]For example, raising sectoral capital requirements sectoral risk weights may drive lending activity out of one sector and into another; and higher Pillar 2 requirements at one regulated institution may drive lending activity to another. The issue of leakages to foreign branches (Aiyar, Calomiris, and Wieladek, 2014) and to non-bank lenders remain common to prudential policy measures in general, however.

[29]We do not know the precise form policy on countercyclical macroprudential capital buffers will take in practice, but for the purposes of this exercise we rule out threshold effects, non-linearities, and reaction to indicators other than those included in the model as it stands (e.g. the results of banking system stress tests such as the Federal Reserve's SCAP). In other words, we limit the scope of the counterfactual macroprudential policies we consider to those taking the form of a linear feedback rule on macroeconomic and financial variables.

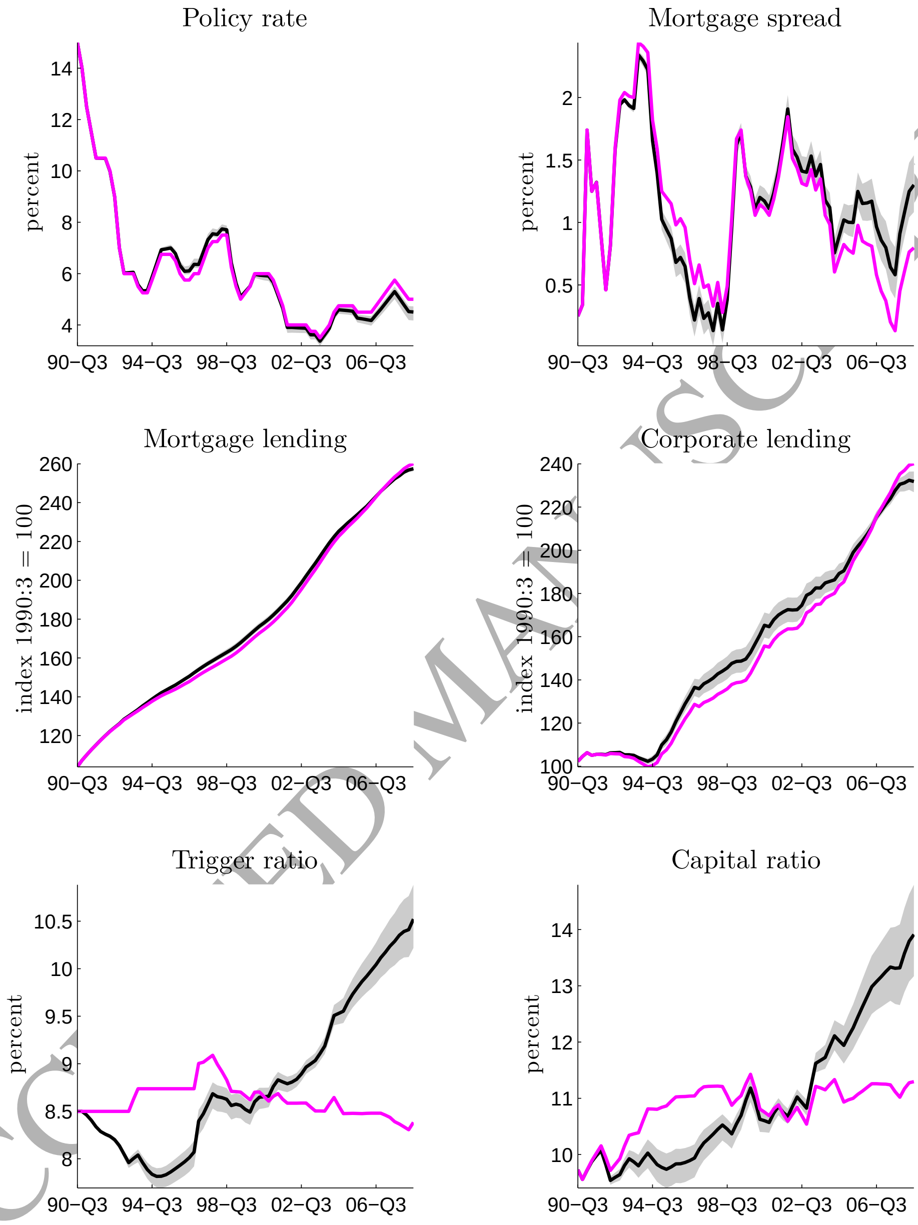


Figure 7. Simulated paths for macroeconomic and banking variables under a counterfactual macroprudential rule responding to house price acceleration. *Note*: Solid black line – median path under counterfactual rule; solid pink line – data. The shaded area represents pointwise 16 to 84 percentile error bands.

6.1 Feedback on housing

The counterfactual policy we construct is based on housing finance, which is a particular focus for macroprudential policymakers in the UK due to the size of banks' exposures to the mortgage market. It is parameterized so that the trigger is raised when house prices are accelerating, and when spreads are falling:

$$\text{trig}_t = \theta^{\text{hp}}\Delta^2 \ln \text{housep}_t - \theta^{\text{spr}}\left(\text{spr}_t - \frac{1}{2}\left[\text{spr}_{t-1} + \text{spr}_{t-2}\right]\right) + \hat{\boldsymbol{\theta}}'\mathbf{w}_t + \nu_t^{\text{trig}} \tag{3}$$

where $\hat{\boldsymbol{\theta}}'\mathbf{w}_t$ is the estimated feedback on banking variables in the policy rule. In the simulation, θ^{hp} is set to 3/4 and θ^{spr} is set to 1/5, which ensures that the range of variation in the counterfactual capital requirement is broadly in line with the 2.5% limit laid down in Basel III.[30]

The effects of the simulated macroprudential policy are shown in Figure 7. The most noticeable impact falls upon on the policy instrument itself, and on capital ratios: the trigger ratio is lower throughout the 1990s, as policy attempts to ease conditions in the mortgage market. The trigger ratio would have been around 50 basis points lower than the historical ratio during this period. Simulated capital ratios would also have been somewhat lower as a result. The remaining simulated paths are not, in most cases, radically different from those that were actually observed.

There are indications that this alternative policy rule would have contributed towards stabilizing the housing market. Under the simulation, mortgage lending is higher through the mid-1990s, and mortgage spreads are 20 basis points or so lower. House prices (not shown) are marginally higher in this period. The picture alters as we move into the 2000s. Now the counterfactual capital requirement is higher than the observed one, as are capital ratios. This would have tended to depress mortgage lending growth, so that by the mid-2000s the stock of mortgage loans would have converged towards, and eventually dipped below, the level actually observed. Spreads would also have been higher under the counterfactual policy, and house prices lower, over this latter period.

Throughout the simulation, there is barely any impact on growth in GDP (not shown). A key reason for this is the endogenous response of monetary policy. As can be seen from the figure, the counterfactual monetary policy would have been marginally tighter through the period in the 1990s when the counterfactual macroprudential policy was easier; and it would have been marginally looser through the mid-2000s, when macroprudential policy was tighter.[31] It is

[30] Variation in the buffer beyond this limit are possible, but need not be reciprocated in other jurisdictions.

[31] The apparent coordination between monetary and macroprudential policies arises because the latter has an effect on the macroeconomic variables to which monetary policy primarily responds. But a formal analysis of policy coordination, such as that in Angelini, Neri, and Panetta (2014) for example, is beyond the scope of this analysis. As a practical matter, it is noteworthy that Angelini, Neri, and Panetta report paths for capital requirements, capital ratios, and lending that are virtually identical whether coordination is present or not.

noteworthy that the model predicts no contradiction in this particular mix of policies, a caveat being that it does not account for a possible 'risk-taking' channel of monetary policy (Borio and Zhu, 2012). In these simulations, offsetting monetary policy action is able to stimulate the broad economy at the same time that macroprudential policy damps down mortgage lending and raises bank capital ratios.

6.2 Alternative rules

As a robustness check, we examined how a policy feeding back on the aggregate private sector credit-to-GDP gap set out by the Basel Committee on Banking Supervision (2010b) would have performed.[32] The counterparts to higher capital ratios would have been consistently lower mortgage and corporate lending, and higher mortgage spreads. These patterns are consistent with capital requirements having a relatively large effect on lending, and a relatively modest effect on GDP. The simulation reveals an apparent drawback with the credit gap indicator: By raising capital requirements during the deleveraging phase of the credit cycle, when the credit gap was still high but lending growth was falling, the counterfactual rule acts to amplify the decline in credit. The effect was particularly pronounced for corporate lending, which effectively suffers 'collateral damage' from mortgage market deleveraging.[33] Exactly this concern was raised by Repullo and Saurina (2012) on the basis of simple correlation analysis for a sample of developed economies.

6.3 Discussion

It is instructive to compare our counterfactual exercise with that from the estimated DSGE model in Angelini, Neri, and Panetta (2014), which does not fall foul of Lucas critique concerns. Their results indicate that, in response to a business cycle shock, output and inflation follow near-identical paths whether or not a CCyB instrument is active. The main impact of the macroprudential instrument is on lending spreads, loan volumes, and the capital ratio itself. In this respect, the DSGE counterfactual leads to rather similar conclusions to those from the VAR approach. An interesting point of difference is that those authors also report a somewhat less active monetary policy response to shocks, whereas in our exercise the monetary policy rule is assumed unchanged; but the predictions from the VAR and DSGE approaches are not obviously at variance.

There remain of course reasons to doubt that counter cyclical macroprudential policy will have precisely the effects outlined above. A relevant difference between the microprudential

[32]The credit gap is the difference between the ratio of a broad measure of credit to GDP, and a one-sided HP filtered estimate of its trend. The baseline model is re-estimated to include this variable within the macro block.

[33]Although not considered here, a better macroprudential instrument to deploy at this time might have been a sectoral capital requirement targeted on mortgage lending.

and macroprudential regimes is that the Basel CCyB does not form a 'hard floor' for banks' capital ratios. Breaches of the combined required capital ratio will lead to restrictions on payouts to equity holders, rather than regulatory action. However, there is evidence that banks are very reluctant to reduce payouts even in the face of substantial losses (Acharya, Gujral, Kulkarni, and Shin, 2011). Recent studies support the idea that such reluctance is due to an underlying risk-shifting motive (Onali, 2014). It therefore seems probable that banks will avoid breaching their combined capital requirement under the CCyB regime.

Overall, the simulated effects of macroprudential policy on the macroeconomy are small, while their effects on banking lending and loan spreads are modest. The main payback to macroprudential policy appears to lie in the higher capital ratios that banks maintain at the end of the simulation, compared to those that were observed.

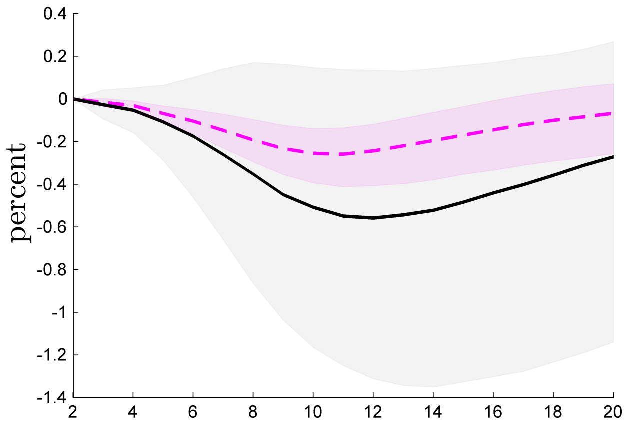
7 Robustness

This section reports on two sets of sensitivity analyses. In the first, we present the results of using a variant of our baseline prior in which the weight placed on bank-level information in estimation is reduced. Our presumption thus far has been that the additional information present for variables in the banking block should dictate that it has a relatively higher weight, compared to the aggregate banking data. A 'looser' prior results in posterior estimates that put more weight on the 1990-2008 aggregate data, and less weight on micro data.[34] The response of real GDP to a trigger ratio shock under the looser prior is shown in Figure 8(a) (solid line). The baseline response is also shown (dash line). The median decline in GDP is somewhat larger than in the baseline case. Underlying this is an on average larger, but much more uncertain, response of lending to changes in the trigger ratio. However, the baseline output response lies within the wider error bands generated in the loose prior case. We therefore conclude that from the perspective of the looser prior, the baseline responses remain plausible, and that some shrinkage towards bank-level estimates is helpful in sharpening estimates of the link between capital and lending.

The second sensitivity check we perform relates to the identification of the policy equation. We re-estimate the VAR using a standard contemporaneously recursive scheme, with the trigger ratio ordered second-to-last (see Table 3). Under this scheme, policy can respond to macroeconomic variables contemporaneously and at lags. The estimated response of policy and the associated lending responses are similar to the baseline, and as a result the shape and magnitude of the real GDP response remains very close to the base case as well (dash line), as

[34]The exercise involves varying the hyperparameter controlling the prior tightness on the banking block of the model (the parameter λ_2^j in Appendix B), while holding constant the coefficients in the policy equation such that the trigger ratio in Figure 8(a) behaves as in Figure 2. In this way, we avoid confounding differences in the policy path with differences in the impact of policy on lending.

(a)



(b)

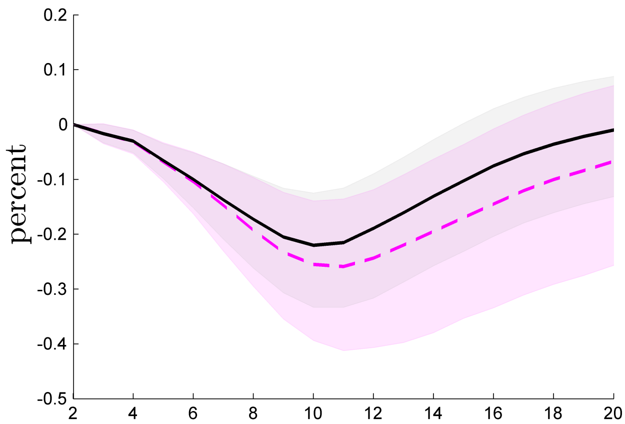


Figure 8. Panel (a): The effect on the response of real GDP to an unanticipated increase in aggregate capital requirements of putting lower weight on bank-level information (–).

Panel (b): The effect on the response of real GDP to an unanticipated increase in aggregate capital requirements of allowing policy to feed back on macroeconomic variables (–).

Note: – – Median real GDP response in the baseline model. Shaded areas represent pointwise 16 to 84 percentile error bands.

Table 3. An alternative identification scheme

Identification I: Policy responds to all macroeconomic variables

Impact matrix $\mathbf{A}$

Variables	M	B	P	K
M	×	×	×	×
B		×	×	×
P			×	×
K				×

Lag matrix $\mathbf{F}_\ell$

Variables	M	B	P	K
M	×	×	×	×
B	×	×	×	×
P		×	×	×
K		×	×	×

Note: An × indicates position of non-zero coefficient blocks in the $\mathbf{A}$ and $\mathbf{F}_\ell$, $\ell = 1, \ldots, p$ matrices in equation (1). M – macroeconomic variables; B – bank lending variables; K – system-wide capital ratio; P – prudential policy variable (trigger ratio).

Figure 8(b) (solid line) indicates. The result offers good additional evidence in favour of the argument in Section 4.4, in that the macroeconomy appears to have had a minor direct effect on prudential policy (although its indirect effect, via banking variables, is more significant). In sum, our results appear reassuringly robust to variations in our set of baseline assumptions.

8 Conclusion

This paper has demonstrated that variation in microprudential capital requirements at individual banks, when aggregated, caused changes in aggregate credit supply, aggregate expenditure, and asset prices under the Basel I and II regimes in the UK. An increase in the required capital ratio is estimated to have had persistent and negative effects on overall household and corporate lending growth, consistent with the existence of binding regulatory constraints on banks at the system level. Lower credit growth was found to exert downward pressure on GDP, with wider corporate bond and mortgage spreads acting to amplify the initial impulse through a financial accelerator channel. These results add to the growing literature on the real effects of financial disturbances.

The paper also offered a counterfactual analysis of the type of macroprudential capital tool introduced under Basel III. Simulations of the structural VAR model developed in the paper indicated that a macroprudential rule that mechanically tracked the credit-to-GDP gap, an indicator proposed by the Basel Committee, would have produced greater fluctuations in credit than a rule that reacted to house price acceleration and mortgage spreads. The analysis we presented complements a burgeoning literature that uses DSGE models to simulate the effects of macroprudential policy. Indeed, our results appear encouragingly consistent with the findings of at least one such study (Angelini, Neri, and Panetta, 2014).

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