Capital regulation and the macroeconomy: Empirical evidence and macroprudential policy

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Abstract
This paper studies the macroeconomic effect of changes in microprudential bank capital requirements using confidential regulatory data from the United Kingdom. The central result is that an increase in capital requirements lowers lending to firms and households, reduces aggregate expenditure and raises credit spreads. A financial accelerator effect is found to amplify 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 the 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

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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 U.K. government alone put £37 billion of it into the banking system (HM Treasury, 2009). In this paper we quantify the impact of changes in bank capital regulation on the macroeconomy, study its interactions with monetary policy, and assess post-crisis reforms to the Basel Accords that grant regulators macroprudential powers over minimum capital standards (Basel Committee on Banking Supervision, 2010a). It appears to be the first such study outside the DSGE literature (e.g. Angelini, Neri, and Panetta, 2014; Benes and Kumhof, 2015). 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 been far from 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 specific pockets of economic activity, via lending, progress has otherwise been limited by a lack of suitable instruments.\(^4\)

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\(^1\) In the U.K., 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).
The second difficulty lies in isolating changes in bank capital caused by regulation. In 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 usable 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.

In this paper we claim to go some way towards resolving these problems. We make use of data on the capital requirements set by regulators for individual banks operating in the U.K. Over the 1989-2008 period covered in this study, regulators required banks to hold capital in excess of the time-invariant minimum levels set down in the Basel Accords. They operated a system in which there was variation in capital requirements both over time and across banks. Regulatory requirements were not public information. Moreover, regulatory decisions were not made in response to macroeconomic factors. We use these institutional facts to identify regulation-induced shocks to banking system capital, and to assess the dynamic interactions between the banking system and the macroeconomy, using a standard monetary vector autoregression (VAR) augmented with aggregate banking system and regulatory variables.

Our central finding is that changes in microprudential capital requirements 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 proportion of mortgages in arrears. Systematic monetary policy easing acts to cushion the effect on output, which for a 50 basis point increase in the average required capital ratio is on average roughly 0.2% lower than trend, two-to-three years after the shock. In the absence of a monetary policy response, peak output declines are roughly 50% larger. The finding that a shock that alters the mix of balance sheet liabilities at financial intermediaries has distinct real effects complements 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

Francis and Osborne (2009b) provide a description of the institutional environment, and summarise trends in U.K. 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). I am grateful to Michael Straughan for clarifying these points.
the results of a counterfactual simulation exercise. The exercise is motivated by the absence of direct experience with the tool, and complements model-based approaches. We find that a candidate 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 moderating influence on credit growth and house prices, prior to 2007. A rule that responds to the credit-to-GDP gap, suggested as an indicator for setting the counter cyclical buffer, performs noticeably less well.

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) the goal of isolating the effect of shifts in the supply of bank lending on the economy at large. But whereas the survey responses used in that paper are not specific about the source of the supply shift, in this paper we isolate the effect of capital requirements. In our model, shocks to capital requirements affect bank capital ratios, like the shocks identified by Berrospide and Edge (2010), but have a plausibly exogenous source in regulatory actions.

The econometric estimates we present exploit bank-level variation in required capital ratios, as in Aiyar, Calomiris, and Wieladek (2014), Francis and Osborne (2009a) and Labonne and Lamé (2014), 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. The idea of combining micro and macro information via a Bayesian prior was employed in the context of a DSGE model by Chang, Gomes, and Schorfheide (2002). The 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.

Another approach to incorporating micro information in estimation is to augment a VAR 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 Jimboeann 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

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6A growing literature sets out to estimate fully structural DSGE models that incorporate banks, as in Gerali, Neri, Sessa, and Signoretti (2010) for the euro area; Hirakata, Sudo, and Ueda (2011) for the U.S.; and Villa and Yang (2011) for the U.K. However, each of these studies introduces banking in a different way, making it hard to draw general conclusions.
the type of rotating panel data typically encountered in practice.\footnote{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.}

Our approach to identification is standard in the VAR literature (Rubio-Ramírez, Waggoner, and Zha, 2010, provide a comprehensive exposition), and rests on institutional facts particular to the U.K. 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 U.K. case.

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

## 2 Aggregate and bank-level data

Three categories of information are used in the analysis: aggregate macroeconomic data, aggregate banking data, and micro banking data. Details of the data and its sources can be found in Appendix A. 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 U.K. data, we include average house prices and mortgage spreads. In addition, we include the proportion of households 6 months or more in arrears.\footnote{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 U.K. 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.} 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.

Turning to the banking data, we have an institution-level panel on a number of balance sheet items recorded over some 19 years. The panel is unbalanced and rotating, principally due to multiple merger and takeover events.\footnote{Davies, Richardson, Katinaite, and Manning (2010) detail some history of U.K. banking sector consolidation.} The key variable in the micro data set is the confidential information on how much capital regulators 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’.\(^{10}\) After filtering, the dataset contains 644 observations on 21 U.K. banks, treating pre- and post-merger banks as separate entities.

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

\(^{10}\) 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).
to write-offs and other items) scaled by the stock of loans outstanding in the previous quarter.\(^{11}\)

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 aggregate data, plotted in figure 1, naturally inherit the relatively short history of the underlying micro data. The central point of note is that variation in trigger ratios, which affected roughly one in six banks each quarter, 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 change in the system-wide capital requirement was 15 basis points over the full sample. Two further observations are worthy of note: First, the aggregate capital ratio is substantially more variable than capital requirements, as expected; Second, banks are seen to maintain a buffer of capital above minimum requirements to avoid accidentally triggering intervention from regulators, and to preserve future lending capacity (Repullo and Suarez, 2013).

2.1 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 reduced-form relationships between capital and lending. The findings in this section 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).\(^{13}\) However, for completeness we present them here because, as section 3.1 later explains, bank-level estimates inform our priors for how aggregate lending responds to changes in system-wide capital ratios.

Table 1 presents regression results for each of the bank-level variables described above. 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

\(^{11}\) 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 U.K. 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.

\(^{12}\) The results below are near identical when using the unweighted series, so we report only on the weighted series.

\(^{13}\) The results presented here differ slightly in sample period, coverage, and/or the treatment of mergers.
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; 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 very small coefficient.

### 3 The macroeconomic 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

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

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

**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: major banks $N = 21$, 1989:4–2008:3. Further details may be found in Appendix C.
a large number of parameters. 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.

Letting \( y_t \) be a vector containing the \( m = 11 \) aggregate variables listed in section 2, and \( x_t = (y_{t-1}^\top, \ldots, y_{t-p'}^\top, 1)^\top \) be a vector of lag terms, the structural VAR\((p)\) is given by:

\[
y_t^\top A = x_t^\top F + v_t, \quad v_t \sim N(0, I)
\] (1)

where \( A \) summarises the contemporaneous relationships between the elements of \( y_t \), \( v_t \) is a vector of independent stochastic disturbances, and \( F = (F_1^\top, \ldots, F_p^\top, c)^\top \) collects together both the intercept vector \( c \) and the lagged autoregressive matrices. Individual structural equations are read down columns of \( [A^\top; F^\top]^\top \), with variables in rows.

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

\[
a_i \sim N(0, S_i) \quad f_i|a_i \sim N(Ba_i, H_i)
\] (2)

where \( S_i \) and \( H_i \) are symmetric, positive definite matrices defined in Appendix B, and \( B \) is defined in the next section. There we also describe how the task of setting the prior is approached in this study. The robustness checks detailed in section 6 show how prior information affects the main results. The following section explains how the structural relationships in (1) are identified, with alternative identifications laid out again in section 6.

3.1 Prior settings

Our baseline prior settings follow Sims and Zha (1998), and are described in Appendix B. We selectively bring in additional information useful in estimation via the distribution in (2), as we now detail. The parameter \( B \) determines our priors about the reduced form interactions between bank lending, bank capital, and the macroeconomy. The first source of information used to set this parameter is the bank-level data discussed above. It is introduced by setting elements of \( B \) that correspond to aggregate lending and capital variables equal to their counterpart bank-level reduced form point estimates, described in table 1. An appealing aspect of this procedure is that posterior parameter estimates are a function of both micro and macro information, with the expanded information set hopefully leading to improved inference. This Bayesian approach to combining data follows the same rationale as that of Chang, Gomes, and Schorfheide (2002).\(^15\)

\(^14\)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.

\(^15\)The key assumption in Chang, Gomes, and Schorfheide (2002, p. 1502) is that micro and macro data are conditionally independent, given the parameters that are not common to the micro and macro models—the equivalents
The second set of information we bring in is intended to counteract the potential bias arising from the short history at our disposal. The 1989-2008 period covered by the bank-level data encompasses a single business cycle recovery, and a single downturn. Although longer historical aggregate series are available for both macroeconomic and bank lending data, unfortunately there is no consistent capital data to draw on.\textsuperscript{16} The short sample makes statistical detection of the financial cycle; which Drehmann, Borio, and Tsatsaronis (2012) characterize as a ‘medium-term’ cycle, lasting on average around 16 years; problematic. In the U.K. context, failing to capture this medium-term relationship between output and credit tends to overweight an episode of macroeconomic and financial volatility in the early 1990s. This unusual period combined a strong economic recovery with weak bank mortgage lending associated with a major housing bust (on which, see Muellbauer and Murphy, 1997). Formally, we estimate an auxiliary reduced-form VAR in the macroeconomic and bank lending variables on pre-sample data running from 1975:Q1-1989:Q4 under an uninformative prior, and center the elements of $B$ corresponding to the interaction between those variables on the resulting point estimates.\textsuperscript{17}

3.2 Identification

As it stands, the model in (1) embodies no restrictions, aside from the requirement that the $A$ matrix be of full rank, and so is not identified. The remainder of this section details the assumptions made in order to identify the effects of changes in regulatory capital. They are based on features of the institutional practice followed by regulators in the U.K. between 1989 and 2008. The schematic in table 2 details precisely how our identifying assumptions map into the VAR, partitioning $y_t$ into four distinct blocks of variables: macroeconomic (‘M’); bank lending (‘B’); the aggregate bank capital ratio (‘K’); and a policy equation determining how the trigger ratio is set (‘P’).\textsuperscript{18}

The first assumption we make is that in the microprudential policy equation, the trigger ratio depends on banking variables alone. In particular, macroeconomic variables do not to (B.1) and (C.1). This considerably simplifies the analysis, at the cost of making what may be a somewhat crude approximation. However, common practice amongst macro modelers is implicitly to make a stronger independence assumption, which disregards micro data entirely. A relevant exception is Dave, Dressler, and Zhang (2013).

The ground work for the Basel Accords were laid in the mid-1980s, and included the Basel Committee’s framework for capital measurement which cemented the role of risk weighting assets in capital adequacy assessments, and a bilateral U.S.-U.K. capital adequacy agreement 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 U.K. law in 1990. Prior to 1990, the regulatory treatment of capital, and reported capital ratios, were not on the same basis as afterward.

Other closely related, but arguably more complex, standard methods for combining data with different available sample lengths exist; for example, casting the VAR in state space form with measurement error. Experimentation suggested these alternatives would deliver comparable results.

It is straightforward to check that, under these restrictions, the model is globally identified (Rubio-Ramírez, Waggoner, and Zha, 2010, Theorem 2).
Table 2. Unrestricted and restricted VAR coefficients

<table>
<thead>
<tr>
<th>Impact matrix A</th>
<th>Lag matrix ( F_\ell )</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Variables</strong></td>
<td><strong>M</strong></td>
</tr>
<tr>
<td><strong>M</strong></td>
<td>×</td>
</tr>
<tr>
<td><strong>B</strong></td>
<td>×</td>
</tr>
<tr>
<td><strong>K</strong></td>
<td>×</td>
</tr>
<tr>
<td><strong>P</strong></td>
<td>×</td>
</tr>
</tbody>
</table>

*Note:* Equations in columns, variables in rows. An × indicates position of non-zero coefficient blocks in the \( A \) and \( F_\ell \) 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).

appear. This exclusion restriction reflects in part the fact that under Basel rules, business cycle effects were just one of a panoply of risks not captured by minimum (Pillar 1) standards for supervisors to consider. But of more 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. 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 went in the same direction, as might be expected if supervisors did respond to a common macroeconomic factor. Nevertheless, others have argued that the result, if not the intention, of supervisory actions was to produce counter-cyclical movements in aggregate capital requirements. Robustness checks reported in section 6 indicate that neither relaxing the baseline exclusion restriction in table 2 to allow for systematic feedbacks to prudential policy, nor strengthening it to make the trigger exogenous, has much

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19. 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), so giving little concrete guidance on where and how to account for the business cycle.

20. 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.

21. Aiyar, Calomiris, and Wieladek (2014) argue 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 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 capital ratios by U.K. lending share, the correlation is 0.40 (s.d. 0.10), which although still large is significantly below the Aiyar, Calomiris, and Wieladek figure.
impact on the conclusions we draw.

Our second substantive assumption is that the transmission mechanism linking actual and required bank capital ratios to the wider economy is via bank lending alone. For capital requirements, this assumption follows from the fact 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 a bank’s response to such a change naturally could be. Accordingly, we exclude the trigger ratio from the macroeconomic block of the VAR (see also Peek, Rosengren, and Tootell, 2003), so that no macroeconomic variable is able to respond directly to it, either within the quarter or with a delay. By contrast, actual capital ratios respond immediately to changes in the trigger ratio, and loan quantities and proxies for the cost of credit respond with a one period 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).

Perhaps the most substantive aspect of the exclusion restrictions 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 policy. As a check, we examined the official record of Monetary Policy Committee meetings. 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. This is understandable given that the only instance of modest banking instability that the U.K. 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 macroeconomic impact of microprudential regulation

4.1 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 we consider is an unanticipated increase in the trigger ratio, the minimum capital to risk-weighted asset ratio required by bank regula-

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22Official 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.’
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 bands represent pointwise 16 to 84 percentile error bands.

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 5). Figure 2 shows the responses of banking system variables, along with pointwise 68% error bands. The initial impact of the shock falls on the aggregate capital ratio. As can be seen, 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 rebuilt fairly rapidly, and has returned to its baseline value within two years.

The effect of capital movements on bank lending is rapid and significant. Secured household lending is close to 0.5% lower, relative to trend, in a little over a year, and non-financial corporate lending is more than 1% lower. The estimated responses we observe are consistent with
regulatory capital requirements being a binding constraint on aggregate bank lending. Loan growth stops falling roughly coincident with the return of the aggregate buffer to its pre-shock level. Because household secured and corporate categories attract high risk weights—50% and 100% respectively under Basel I—lower loan growth entails a higher risk-based capital ratio, other things equal.

Figure 3 summarises the responses of variables in the macroeconomic block. 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). Prices remain broadly flat for two years, whereafter they undergo a noticeable decline, bringing about a systematic easing in monetary policy. 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.

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 sample of U.K. 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

23Binding 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’.
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 bands represent pointwise 16 to 84 percentile error bands.
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.

4.2 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). 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 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 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

\[\text{These experiments are not intended to assess the plausibility of the implied restrictions, or to pose a counter-factual change in the structure of the economy (for which, see section 5). Rather, they are intended to highlight the role played by the dynamic responses of particular variables.}\]

\[\text{Perhaps surprisingly, this was also found to be the case when all macroeconomic variables were held constant. However, it does not follow that banking variables are not impacted by other, macroeconomic shocks.}\]
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 been zero. – Median path. The shaded bands represent 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 bands represent pointwise 16 to 84 percentile error bands.
Figure 6. Responses to an unanticipated increase in aggregate capital requirements holding policy rate 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 bands represent pointwise 16 to 84 percentile error bands.
Iacoviello and Minetti, 2008; Walentin, 2014).

Our second scenario involves holding the policy interest rate constant. The prolonged period that advanced economies, including the U.K., 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 around 50% larger, compared to the baseline, when monetary policy rates are constant.

5 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 (CCB), to address these macroprudential concerns. The CCB, 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 has so far been only limited applications of the CCB, but also interesting because the CCB is not prone to some of the leakages associated with other prudential tools. 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.

Tarullo (2013) notes that the CCB was also included in the implementation of Basel III by U.S. authorities in summer 2013, but that there too its possible use is not planned for several years hence.

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.
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.

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 a consensus view on what constitutes a correctly specified and fully structural model that can accommodate macroprudential policy analysis is not currently in evidence in the profession, although several variants of candidate DSGE models for this purpose have already been mentioned. In the meantime, some indication of the effects of the countercyclical buffer can contribute to the formulation of policy.

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.

5.1 Feedback on the credit gap

A useful benchmark exercise is to examine how policy would be set if it mechanically followed the aggregate private sector credit-to-GDP gap set out by the Basel Committee on
Figure 7. Simulated paths for macroeconomic and banking variables under a counterfactual macroprudential rule responding to the credit-to-GDP gap. Note: Solid black line – median path under counterfactual rule; solid pink line – data. The shaded bands represent pointwise 16 to 84 percentile error bands.
Figure 8. 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 bands represent pointwise 16 to 84 percentile error bands.
Table 3. Estimated policy reaction function

<table>
<thead>
<tr>
<th>Variable</th>
<th>posterior mode</th>
<th>HPD interval</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>min</td>
</tr>
<tr>
<td>trigger</td>
<td>8.84</td>
<td>7.67</td>
</tr>
<tr>
<td>mortgage lending (-1)</td>
<td>-1.08</td>
<td>-4.54</td>
</tr>
<tr>
<td>corporate lending (-1)</td>
<td>-0.84</td>
<td>-1.43</td>
</tr>
<tr>
<td>trigger (-1)</td>
<td>11.2</td>
<td>9.71</td>
</tr>
<tr>
<td>capital ratio (-1)</td>
<td>0.17</td>
<td>0.14</td>
</tr>
<tr>
<td>mortgage lending (-2)</td>
<td>2.86</td>
<td>2.39</td>
</tr>
<tr>
<td>corporate lending (-2)</td>
<td>0.70</td>
<td>0.60</td>
</tr>
<tr>
<td>trigger (-2)</td>
<td>-3.36</td>
<td>-3.84</td>
</tr>
<tr>
<td>capital ratio (-2)</td>
<td>0.11</td>
<td>0.09</td>
</tr>
</tbody>
</table>

Table shows posterior estimates of $(a_i^T, f_i^T)$ for $i=P$, the equation for trigger, under the baseline identification scheme. The first column indicates the mode of the marginal posterior densities. The min and max columns show the lower and upper bounds of the 100(1 - α)% highest probability density set, respectively, with α = 0.1.

Banking Supervision (2010b). The credit gap is intended as a common reference point, against which judgemental decisions on precise instrument settings will be made. The feedback rule used in the simulation has the trigger ratio depend on the moving average of current and lagged credit gaps:

$$\text{trig}_t = \theta_{\text{gap}} \frac{1}{3} (\text{credgap}_t + \text{credgap}_{t-1} + \text{credgap}_{t-2}) + \hat{\beta}' \text{w}_t + \nu_{t}^{\text{trig}}$$ (3)

where $\text{w}_t$ contains the lending and capital variables from the estimated prudential policy equation, and $\hat{\beta}$ are their estimated coefficients, see table 3. The reason for including this term is so that when $\theta_{\text{gap}} = 0$, every simulated path coincides exactly with the realized path in the data. Because we consider only linear rules, our exercise does not map precisely into the settings for the capital buffer recommended by the BCBS. Nevertheless, the exercise can illustrate the general behaviour of the macroeconomy under the scheme.

The simulation reported in figure 7 has $\theta_{\text{gap}}$ set to 1/8, 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. It is apparent that, under the counterfactual policy, capital requirements would have been raised substantially until 1999, relative to what was actually observed, and would then have fall back. This pattern mirrors movements in the credit gap. A result of these movements is that the simulated path for the actual capital ratio also lies materially above its

\[^{28}\text{The credit gap is the difference between the ratio of a broad measure of credit to GDP, and a one-sided HP filtered estimate of its trend. The baseline model is re-estimated to include this variable within the macro block.}\]

\[^{29}\text{Variation in the buffer beyond this limit are possible, but need not be reciprocated in other jurisdictions.}\]
observed path. By the end of the sample period in 2008, the system-wide tier 1 capital ratio is close to 14%, about 3 percentage points above what was observed.

The counterparts to higher capital ratios would have been consistently lower mortgage and corporate lending, and higher mortgage spreads, which make house prices (not shown) a shade lower than otherwise. The simulation reveals an apparent drawback with (3). 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.\(^\text{30}\) Exactly this concern was raised by Repullo and Saurina (2012) on the basis of simple correlation analysis for a sample of developed economies. The rule considered next appears to have better stabilisation properties.

5.2 Feedback on housing

The second counterfactual policy we construct focuses on housing finance. It is parameterized so that the trigger is raised when house prices are accelerating, and when spreads are falling:

\[
\text{trig}_t = \theta_h \Delta^2 \ln \text{house}_{t-1} - \theta_{spr} \left( \text{spr}_{t-1} - \frac{1}{2} \left[ \text{spr}_{t-2} + \text{spr}_{t-2} \right] \right) + \beta' w_t + \nu_{trig}^t
\]

where again \(\beta' w_t\) is the estimated systematic component. In the simulation, \(\theta_h\) is set to 3/4, and \(\theta_{spr}\) is set to 1/5.

The effects of the simulated macroprudential policy are shown in figure 8. The most noticeable impact falls upon on the policy instrument itself: 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 stabilized the housing market somewhat. 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 trigger ratio 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 been 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.

\(^{30}\)Although not considered here, a better macroprudential instrument to deploy at this time might have been a sectoral capital requirement targeted on mortgage lending.
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. It is 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, coordinated 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.

5.3 Discussion

There are of course reasons to doubt that counter cyclical macroprudential policy will have precisely the effects outlined above. A number of differences between the microprudential and macroprudential regimes are of relevance. First, the Basel counter cyclical buffer 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 CCB regime.

Second, as section 3.2 details, changes to microprudential requirements were private information, and set bank-by-bank. By contrast, the CCB is public information and applies to the banking system as a whole. But it is not clear how much this matters much for macroeconomic outcomes under the maintained assumption that bank capital requirements directly matter only for banks: in neither case is private information about individual banks being released, which could affect their ability to raise capital.

In sum, even if the conditions in place under the period studied here were thought to be particularly propitious for the results we presented, and are thought of as an upper bound on the effectiveness of counter cyclical macroprudential policy, it seems very likely that the conclusion that policy can build banking system resilience by raising actual capital ratios, with little impact on aggregate expenditure, will remain intact.

31There may be variation in the required CCB across banks, however, because their total CCB is the weighted average of the buffers in force in each of the jurisdictions they have exposures to.

32One reason why banks might find it costly to raise their equity ratio without cutting loans is the debt overhang problem. If investors know that regulators have identified weaknesses in the bank, they will be reluctant to supply funds that may partially bail out existing debt holders. However, when regulatory actions can only be inferred indirectly, or when they apply to the entire system, the debt overhang problem is reduced.
6 Robustness

This section reports on three sets of sensitivity analyses. We vary, in turn: the weight placed on bank-level information on the relationship between capital and capital requirements and lending, and on pre-1990 data; the restrictions imposed upon the microprudential policy equation; and the sample period used in estimation.

6.1 Sensitivity to priors

This section reports on the results of using several variants of our baseline prior in which the weight placed on bank-level information used in estimation is selectively altered. The exercise involves varying the hyperparameter controlling the prior tightness on the banking block of the model. At one extreme, a ‘tight’ prior results in posterior estimates that put most weight on micro data. At the other extreme, a ‘loose’ prior results in posterior estimates that put most weight on the 1989-2008 aggregate data. A fully uninformative or ‘diffuse’ prior produces rather poorly determined estimates, due to the large number of parameters in the model.

The sensitivity of our results to settings for the micro prior are shown in figure 9. The qualitative shapes of the responses are broadly similar to those of the baseline model (solid line). With ‘tight’ prior settings, the responses are pulled towards to those estimated from micro data. The declines in mortgage and corporate lending growth are slightly lower than in the base case, leading to smaller declines in aggregate real expenditure and house prices. With ‘loose’ prior settings the quantitative results are markedly different. The main effect of reducing the weight on bank-level information is to make the median responses of lending growth to an innovation in trigger larger, and in the case of mortgage lending, more volatile. The main cause appears to be the very persistent responses in the trigger and capital ratios; indeed, the capital ratio ‘overshoots’ the rise in trigger somewhat, leading to persistently higher capital buffers. This possibility seems somewhat implausible, and mainly reflects the lower precision of the estimates: error bands (not shown) on all responses widen considerably, supporting the notion that useful information on the response of lending to changes in capital is contained in the micro data.

6.2 Sensitivity to identifying assumptions

In section 3.2 the reasoning behind our baseline identification scheme was laid out. In this section, we test the sensitivity of the results presented above to adopting two polar alternative identifications for the policy equation. The first alternative is to adopt a standard recursive assumption, with the trigger ratio ordered second-to-last (so that actual capital is allowed to respond to regulation within period), see table 4. Under this scheme, policy can respond to

\[ \lambda_i \] in Appendix B.  

27
Figure 9. The effect on impulse-response functions of bank-level information. Note: The panels depict the median estimated impulse response functions of selected variables to an orthogonalized shock to the trigger ratio of 50 basis points, when the micro prior given in section 3.1 is applied according to: – baseline settings; - - a ‘loose’ setting; --- a ‘tight’ setting. (Appendix B gives details of these settings.) The shaded bands represent pointwise 16 to 84 percentile error bands under the baseline prior.
Figure 10. The effect on impulse-response functions of different identifying assumptions. Note: The panels depict the estimated impulse response functions of selected variables to an orthogonalized shock to the trigger ratio of 50 basis points. – Median response under baseline identification. – Median response when trigger responds to all macro and lending variables. · Median response when trigger is exogenous. The shaded bands represent pointwise 16 to 84 percentile error bands under the baseline identification.
Table 4. Alternative identification

<table>
<thead>
<tr>
<th>Impact matrix $A$</th>
<th>Lag matrix $F_{\ell}$</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Variables</strong></td>
<td><strong>M</strong></td>
</tr>
<tr>
<td><strong>M</strong></td>
<td>×</td>
</tr>
<tr>
<td><strong>B</strong></td>
<td>×</td>
</tr>
<tr>
<td><strong>K</strong></td>
<td>×</td>
</tr>
<tr>
<td><strong>P</strong></td>
<td>×</td>
</tr>
</tbody>
</table>

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

macroeconomic variables contemporaneously and at lags. The second alternative is to have the trigger ratio depend only on its own lags. Prudential policy is then set independently from any aggregate variable (although not from idiosyncratic bank-level factors, which appear as shocks to the trigger ratio).

Figure 10 plots the median responses of selected variables under the baseline identification, along with pointwise error bands, and compares them with the median responses under the two alternative identifications. The qualitative shape of the responses is again similar to the base case. The size of the responses in output, lending and house prices is also broadly similar, although when policy is allowed to respond to macroeconomic variables the adverse effects of the shock are somewhat ameliorated. When the trigger is treated as exogenous, the effects of the policy shock on credit and the wider macroeconomy are somewhat more pronounced.

The sensitivity of our results to the specification of the policy equation turns out not to be due to significant feedback effects from macroeconomic variables to the trigger ratio. The $100 \times (1 - \alpha)\%$ highest probability density credible set for the parameters in the policy equation contains zero for every variable aside from the trigger and capital ratios for $\alpha = 0.1$. Moreover, the response of the trigger ratio to a policy shock is unaltered when the responses of non-capital variables are closed off, indicating that any feedback from those sources is also quantitatively insignificant. Rather, it is mainly a result of changing estimates of the autoregressive component of the trigger ratio. In the bank-level estimates, the largest root rises from 0.68, in the baseline case, to 0.94 when the trigger depends only on its own lags. It seems likely that the coefficients in the latter case are biased when lagged capital ratios, which are correlated with the trigger ratio, are excluded from the trigger equation.

6.3 Sensitivity to estimation period

A final concern is that regulatory actions in the early part of the estimation sample may be driving the results. Between the first quarter of 1990 and the last quarter of 1994, there were
only three quarters in which there were changes in aggregate capital requirements (see figure 2). The trigger ratio increased from 8% to 8¾%. The magnitude and timing of these changes might cause us to attribute too much variation in lending to capital requirements. As a check, we re-estimated the model excluding these movements by starting the estimation in 1994:3 (the last increase in capital requirements during this period is in 1993:4, and the model contains two lags). As might be expected, the error bands for GDP and house prices on this shorter sample are significantly wider than on the full sample. Most noticeable, though, are the differences in the household mortgage and corporate lending responses: Mortgage lending begins to recover starting 2-3 years after the shock, unlike in the full sample, but corporate lending is 6-7% lower at the same horizon. The peak decline in GDP is about 0.1% lower. Taken together, these results are consistent with the banking system having been more sensitive to the housing market, and less sensitive to business conditions, in the early 1990s than subsequently.

7 Conclusions

This paper has demonstrated that variation in microprudential capital requirements at individual banks, when aggregated, caused changes in aggregate expenditure, asset prices and credit supply under the Basel I and II regimes in the U.K. An increase in the required capital ratio was estimated to have persistent and negative effects on household and corporate lending growth, consistent with the existence of binding regulatory constraints 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. The 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. Of course, a full analysis of such tools requires the development of a suitable DSGE model. A good model should be capable of reproducing the main features of the empirical behaviour described here (see Iacoviello, 2015, for a candidate model geared towards reproducing the type of facts presented here, for example).

A caveat that future research aimed at informing counter cyclical macroprudential policy should address is that there are conditions under which the regulatory constraint on banks is slack. Changes in counter-cyclical macroprudential buffers may then have little effect (at least, not through the direct channels operative during the period studied here). That suggests that

34The changes affected six separate banks, and involved a single change (or sequence of changes) in each case.
modeling non-linear effects, as in Mittnik and Semmler (2013), may be of importance.

References


BARNETT, A., AND R. THOMAS (2014): “Has weak lending and activity in the UK been driven by credit supply shocks?,” Manchester School, 82(S1), 60–89.


Additional material for ‘Capital regulation and macroeconomic activity’
by
Roland Meeks

A Data sources

<table>
<thead>
<tr>
<th>Series</th>
<th>Sample</th>
<th>Source</th>
<th>Notes</th>
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<tbody>
<tr>
<td>Log real GDP</td>
<td>1975:1-2008:3</td>
<td>ONS</td>
<td></td>
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<tr>
<td>Log CPI</td>
<td>1975:1-2008:3</td>
<td>ONS</td>
<td></td>
</tr>
<tr>
<td>Official Bank rate</td>
<td>1975:1-2008:3</td>
<td>Bank of England</td>
<td>Bankstats, Table G</td>
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<tr>
<td>Log house prices</td>
<td>1975:1-2008:3</td>
<td>ONS</td>
<td>Mix adjusted, all dwellings</td>
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<tr>
<td>Log arrears</td>
<td>1975:1-2008:3</td>
<td>Council of Mortgage Lenders, ONS</td>
<td>% of outstanding mortgages &gt; 6 months in arrears</td>
</tr>
<tr>
<td>Credit gap(^a)</td>
<td>1975:1-2008:3</td>
<td>Bank of England</td>
<td>FPC countercyclical buffer guide</td>
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<tr>
<td>Mortgage spread</td>
<td>1975:1-2008:3</td>
<td>Council of Mortgage Lenders, Oxford Ec.</td>
<td>Average, all floating rate mortgages, over Bank rate</td>
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<tr>
<td>Corp. bond spread</td>
<td>1975:1-2008:3</td>
<td>Global Financial Data</td>
<td>Average investment-grade yield over 10 year gilts</td>
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</table>

Aggregate (a) lending and capital

<table>
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<tr>
<th></th>
<th>Sample</th>
<th>Source</th>
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<tr>
<td>Secured lending(^a) (a)</td>
<td>1975:1-2008:3</td>
<td>Bank of England</td>
<td>Bankstats, Table A</td>
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<tr>
<td>PNFC lending(^bc) (a)</td>
<td>1975:1-2008:3</td>
<td>Bank of England</td>
<td>Bankstats, Table A</td>
</tr>
<tr>
<td>Trigger ratio (a)</td>
<td>1989:4-2008:3</td>
<td>FSA/BoE</td>
<td>From Trigger ratio (b)</td>
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<tr>
<td>Capital ratio (a)</td>
<td>1989:4-2008:3</td>
<td>FSA/BoE</td>
<td>From Capital ratio (b)</td>
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</table>

Bank-level (b) lending and capital

<table>
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<th>Source</th>
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<tbody>
<tr>
<td>Secured lending (b)</td>
<td>1989:4-2008:3</td>
<td>FSA/BoE</td>
<td>Reporting form BE</td>
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<tr>
<td>PNFC(^c) lending (b)</td>
<td>1989:4-2008:3</td>
<td>FSA/BoE</td>
<td>Reporting form BE</td>
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<tr>
<td>Trigger ratio (b)</td>
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<tr>
<td>Capital ratio (b)</td>
<td>1989:4-2008:3</td>
<td>FSA/BoE</td>
<td>Reporting form BSD3</td>
</tr>
</tbody>
</table>

\(^a\)Not included in baseline model.
\(^b\)Adjusted for securitisations and loan transfers.
\(^c\)Private Non-Financial Corporations.
B The Sims and Zha prior

Following Sims and Zha (1998), the prior distribution of the parameters is specified in terms of a marginal prior \( p(a) \) and a conditional prior \( p(f|a) \) (lowercase letters understood to stand for the corresponding vectorized uppercase matrices). Both distributions are normal, and independent across equations. Their prior means are given in (2). Beliefs about the structural parameters \( F \) are derived from priors over the reduced form behaviour of the time series; to see this, note that the reduced form VAR is:

\[
y_t^T = x_t^T B + u_t^T, \quad u_t \sim N(0, \Sigma_u)
\]  

(B.1)

where \( B = FA^{-1}, u_t^T = v_t^T A^{-1} \) and \( \Sigma = E[u_t u_t^T] \). As \( F = BA \), conditional on \( B \) the prior (2) has mean \( f = (I \otimes B) a \) with independence across structural equations. Sims and Zha impose a Litterman-type belief that \( y_t \) follows a multivariate random walk, in which case \( B = I \).

The unrestricted prior covariance and conditional covariance matrices are set in a standard way, with the sole complication that we allow for two distinct blocks of variables, along the lines of table 2: macroeconomic (M) and bank related (B, K, P). The standard deviation of elements in \( a_i \) is given by

\[
\frac{\lambda_1}{\sigma_i}
\]

and the conditional standard deviation of elements in \( f_i \) is given by:

\[
\frac{\lambda_1 \lambda_2^{j}}{\sigma_i \ell^{\lambda_3}}, \quad j = M \text{ or } B, K, P.
\]

In each case the scale factor \( \sigma_i \) is an estimate of the standard deviation of the residuals from a \( p \)th order univariate autoregression in the \( i \)th variable. The hyperparameters can be understood as follows:

- \( \lambda_1 \) sets the overall tightness of prior beliefs; it is set to 0.1.
- \( \lambda_2 \) sets the tightness of prior beliefs around the dynamics implied by \( B \); the baseline setting is 0.05 for both blocks. Under the ‘loose’ prior, it is set to 0.20, and under the ‘tight’ prior to 0.001.
- \( \lambda_3 \) controls the rate at which prior variance shrinks with lag length \( \ell \); it is set at 2.
- \( \lambda_4 \) controls the conditional standard deviation of the intercept, set to \( \lambda_0^{(p)} \lambda_4 \), with \( \lambda_4 \) a large number.

The derived prior under the restrictions in table 2 is given by Waggoner and Zha (2003, eq. 10).
Letting \( y_{it} \) be a vector of micro-level bank lending and capital variables, we formulate a dynamic two-way error component model as:

\[
y_{it}^\top = x_{it}^\top B + z_{i,t-1}^\top \Phi + \varphi_i + \lambda_t + \epsilon_{it}^\top
\]  

(C.1)

where: 

- \( x_{it} = (y_{i,t-1}^\top, \ldots, y_{i,t-p}^\top)^\top \); \( \varphi_i \) represents an unobserved individual fixed effect; \( \lambda_t \) is a time fixed effect that captures the common macroeconomic and seasonal factors; \( \epsilon_{it} \) is an i.i.d. bank-specific error term, assumed independent of the other error components; and \( z_{it} \) is a vector of bank-specific controls.\(^{35}\)

The parameters in \( B = (B_1^\top, \ldots, B_p^\top)^\top \) capture the reduced form dynamics of capital and lending variables, which are the micro analogues of the reduced form parameters in the bank lending and capital blocks of (B.1). This specification typical of those commonly adopted to model balance sheet dynamics in the banking literature (see Hancock, Laing, and Wilcox, 1995, for example).\(^{36}\)

There are three main dimensions to the robustness exercise we carried out. First, we checked how the microeconometric estimates presented in Table 1 changed when alternative estimators and alternative sample sizes were used. The two estimators we consider are Within Group (WG) and Generalised Method of Moments (GMM). The WG approach eliminates the individual fixed effect by transforming variables into deviations-from-time mean form. The GMM approach does the same by time differencing the variables. In the presence of a lagged dependent variable, the GMM approach induces correlation between the explanatory variables and the error term which requires instrumentation. The WG and GMM approaches are equivalent only when the number of time series \( T = 2 \). When \( T > N \), the literature often finds the WG estimator performs well.

A second dimension of robustness concerns whether a broad (subject to filtering that removes banks with insufficient data, or very small absolute amounts of lending to households and firms) sample of banks are used in estimation, or a sub-sample corresponding to the current major U.K. banks and their pre-merger antecedents. The broad sample adds some 380 observations. The final dimension of robustness concerns the controls for common time effects. We consider three possibilities: (a) No time controls; (b) Time fixed effects (the baseline case); (c) Macroeconomic covariates.

\(^{35}\)The common set of control variables are: size (total assets); the loan-to-deposit ratio; the provision ratio; the Basel risk-asset ratio; and capital quality (the ratio of tier 1 to total regulatory capital); see table 1.

\(^{36}\)Our modeling work stops short of the detailed treatment of balance sheet components in Hancock, Laing, and Wilcox, in part due to lack of data and also to keep the number of endogenous variables in the aggregate analysis manageable. These authors examine the dynamic effect of a shock to bank capital on several categories of loans, on securities, and on equity capital and other liabilities in a panel VAR, but the study does not make the link from bank credit to real activity.
In each of the figures that follow, 48 separate estimates are shown in all. Each column of the figures corresponds to one of the four estimator/sample combinations described above. Within columns, the coefficients on each of the explanatory variable are shown with symbols, with coefficients on the same variable joined by a line for clarity. Each estimator/sample/variable coefficient is plotted for the three types of time control (a)-(c) described in the preceding paragraph. We focus on the equations for household secured and corporate lending growth.

Figure (C.1) gives information on the secured lending equation. The signs of the coefficients are robust across all specifications; the magnitudes of the coefficients are also very similar. In all specifications, secured lending growth (squares) is strongly positively autocorrelated; lending growth depends positively on capital, and negatively on capital requirements and corporate lending growth. The coefficients on capital requirements implies a long-run multiplier of between 0.15 up to 0.7 on lending growth (for full sample GMM). Overall, the results raise little cause for concern over robustness.

Figure (C.2) gives information on the private non-financial corporate lending equation. The main feature is that capital requirements exert a consistently large and negative influence on lending growth. In three out of four cases the estimates lie in a range between about -1.5 and -2.5, but in with the WG estimator on the fall sample this drops to about -0.5 (the implied long-run multiplier is then similar to the upper end of the secured lending range). The effect of actual capital ratios on lending growth is not robustly estimated on the full sample either, although it is usually positive. On the other hand, the major banks sub-sample used in the paper is robust to estimation method and to the presence or absence of time controls.
Figure C.1. Robustness of microeconometric estimates of the household secured lending equation. Note: The point estimate of the sum of lag coefficients on each lagged endogenous variable in the secured lending equation of (C.1) is plotted for two estimators (Within Group and Generalised Method of Moments) and two sample sizes across the four columns. Within each column, estimates under three alternative sets of controls (labeled a, b and c) are shown as symbols connected by a line.

Figure C.2. Robustness of microeconometric estimates of the corporate lending equation. See note to figure C.1.