The short-term effects of changes in capital regulations in Poland

In this study I attempt to address two questions. First, how to adequately measure capital regulations. Second, once they are adequately measured, what are their effects in Poland. As the starting point, I illustrate the threats of measuring capital regulations by actual regulatory capital ratios. Then, I analyse the effects of the transition to higher actual regulatory capital ratios due to the tightening of capital regulations. As a measure of capital regulations I directly use minimum regulatory capital ratios. I apply Bayesian panel vector autoregressive models and local projections to bank-level data. I find that the tightening of capital regulations lowers bank lending for at least one out of two analysed minimum regulatory capital ratios. This implies that capital regulations are an effective prudential policy tool in Poland. I also attempt to identify whether the effects of changes in capital regulations depend on whether they are tightened or loosened.

JEL codes: E69, E51, G21, C33, C11.

Keywords: capital regulations, bank lending, Bayesian panel vector autoregressive models, panel data, macroprudential policy.

Mariusz Kapuściński

Narodowy Bank Polski and Institute of Economics, Polish Academy of Sciences

mariusz.kapuscinski@nbp.pl

The views expressed in this article are those of the author. I would like to thank Dorota Ścibisz, Ewa Wróbel and the participants of a seminar at NBP for their useful comments to the working paper version of the article. Any errors are mine.

1. Introduction and review of empirical literature

The implementation of the post-crisis banking regulations has been followed by a large number of studies being carried out on their effects. In the case of capital regulations (being the focus of this study), systematic reviews of their results, in the form of meta-analysis, include Malovana et al. (2021), Araujo et al. (2020), and Fidrmuc and Lind (2020). For narrative reviews see, for example, VanHoose (2007) and Kashyap et al. (2010). On the regulations themselves and their aims see Basel Committee on Banking Supervision (2011).

As much as 85% of estimates are based on data on actual, rather than minimum regulatory (capital or liquidity) ratios (Boissay et al., 2019).[1] This matters, for example, because not all changes in actual regulatory capital ratios are driven by capital regulations, while some studies use the former as a measure of the latter. Although understandable in the environment of a small effective number of observations for minimum regulatory ratios, this could lead to a bias, particularly when attempting to establish the transition (or, short-term) effects of capital regulations. Indeed, in models for bank lending, Malovana et al. (2021) find a negative coefficient on minimum regulatory capital ratios, but a positive one on measures of their actual levels, on average.

For Poland, Gajewski and Krzesicki (2017) use measures of domestic prudential policy as control variables in some specifications of univariate bank-level panel data models for bank lending; the study is focused on the effects of foreign prudential policy. One of the measures used, denoted “capital requirements”, is related to minimum regulatory capital ratios. However, it takes a simplified form, of a qualitative variable taking 1 or -1 in periods of prudential policy changes. According to source data (Cerutti et al., 2017), there were only 3 changes in the sample used. The study identifies a negative effect of the tightening of domestic capital requirements, at the 5% significance level.

Bańbuła et al. (2019), among other things, use a vector autoregressive (VAR) model with the tier 1 ratio as one of the endogenous variables. Responses to shocks to the tier 1 ratio, being a

[1] By “regulatory capital ratios”, capital ratios subject to regulations are meant. The wording follows that of Boissay et al. (2019). Malovana et al. (2021) use “regulatory capital ratio” instead of “actual regulatory capital ratio”, which could also be called an “observed” ratio, and “capital requirements” instead of “minimum regulatory capital ratio”.

combination of responses under several Cholesky orderings, are interpreted as transition effects of capital regulations (on the validity of such an identification strategy, see Kilian et al., 2022). Both bank lending and GDP (among other variables) are used as endogenous variables as well, but only responses of GDP are presented, being the focus of the study. The study finds a negative point effect on GDP, though confidence intervals are on both sides of zero. A related study, Serwa and Wdowiński (2017), presents responses of bank lending as well (using a somewhat different measure of capital regulations, set of other endogenous variables and sample), finding a borderline statistically significant, negative effect on bank lending shortly after a capital regulation shock. There is much less evidence of an effect on GDP, though.

The aim of Marcinkowska et al. (2014) is to identify the effects of capital regulations in Poland as well. First, they estimate the parameters of panel data models (bank-level) for rates on loans with the solvency ratio as one of the dependent variables. They find a positive coefficient, with a varying degree of statistical significance, depending on the specification and sample. Second, they use a structural multi-equation model, finding a simulated increase in the solvency ratio to be associated with lower GDP and bank lending, on average.

The common feature of the studies of Dybka et al. (2017), Czaplicki (2021) and Wróbel (2021) is the focus on the impact of changes in capital position on bank lending and, in the first study, on GDP. The first study measures capital position as the difference between the actual and minimum regulatory capital ratio. The second one by the volume of loans that can be made by "using" the difference between the actual and minimum regulatory capital ratio, among other measures. The third study uses a measure based on the Senior loan officer opinion survey. They generally find a more favourable capital position to be associated with higher bank lending and GDP, with the first and the third study applying VAR models to aggregate data, and the second one using univariate panel data models (bank-level).

Kapuściński (2017), and Kapuściński and Stanisławska (2018) use excess capital as one of the regressors in univariate panel data models for bank lending and rates on loans, respectively (focusing, among other things, on the effects of bank balance sheet strength). They find bank-periods with higher excess capital associated with higher bank lending and lower rates on loans; in the latter study, for some loan types and some model specifications.

There are also several studies for Poland using actual regulatory capital ratios (or their proxies) as control variables, or characteristics with respect to which banks are divided into groups, in models for rates on deposits or loans, the volume of loans or lending policy. They include Borsuk and Kostrzewa (2020), Chmielewski (2003), Olszak et al. (2020), Pawłowska et al. (2014), Stanisławska (2014) and Wośko (2015). Borsuk and Kostrzewa (2020), Olszak et al. (2020) and Pawłowska et al. (2014) tend to find positive (less frequently: insignificant) coefficients on capital ratios in models for bank lending. Chmielewski (2003) and Stanisławska (2014) find some evidence on differences in interest rate pass through between banks depending on their capital ratios. Wośko (2015), on the other hand, finds weak, if any, evidence on the effects of capital ratios on bank lending and lending policy.

The aim of this study is to address two questions. First, how to adequately measure capital regulations. Second, once capital regulations are adequately measured, what are their effects in Poland. As the starting point for the choice of a research design, I illustrate the threats of using actual regulatory capital ratios as a proxy for minimum regulatory capital ratios. Then, I directly identify the short-term effects of changes in capital regulations in Poland, using data on minimum regulatory capital ratios as their measure. This is the first study to do so for Poland and one of few for an emerging market economy (with studies for Czechia being a notable exception; see, for example, Kolcunova and Malovana, 2019; Ehrenbergerova et al., 2020) and more general. I focus on the impact on bank lending. Any longer-term effects, likely to be positive in terms of robustness to macroeconomic and financial shocks, are out of the scope of the study. I apply Bayesian vector autoregressive models and local projections, and the fixed effects estimator, to data for either a balanced or an unbalanced panel of banks, respectively. Furthermore, I attempt to identify a non-linearity in the effects of changes in capital regulations, depending on whether they are tightened or loosened.

I find that the tightening of capital regulations lowers bank lending for the minimum regulatory capital ratio allowing for a possibly full dividend pay-out – the first out of two analysed minimum regulatory capital ratios. Evidence for the second analysed measure – minimum regulatory capital ratio associated with macroprudential supervision – is less clear. This implies that capital regulations are an effective prudential policy tool in Poland. I also find that the use of actual regulatory capital ratios as a proxy for minimum regulatory capital ratios can cause a large bias, by not distinguishing between capital regulation shocks and capital shocks. Finally, I find some differences in responses to the tightening and the loosening of capital regulations. However, with different responses of some other variables depending on the regime as well, their interpretation remains ambiguous.

The rest of the article is structured as follows. The second section illustrates, by means of a simulation, the threats of measuring capital regulations by actual regulatory capital ratios. The third section describes research design. In the next two sections are results, and sensitivity analysis and extensions. The last section concludes.

2. Capital regulation shocks, capital shocks and shocks to actual regulatory capital ratios – a simulation

In order to illustrate the threats of measuring capital regulations by actual regulatory capital ratios, assume the following model:

$$l_t = \alpha(RCR^a_{t-1} - RCR^m_{t-1}) \quad (1)$$

$$RCR^a_t = \beta RCR^a_{t-1} + (1-\beta)RCR^m_t + \eta^c_t \quad (2)$$

$$RCR^m_t = RCR^m_{t-1} + \eta^{cr}_t \quad (3)$$

where l denotes bank lending, η^c is the capital shock, η^{cr} is the capital regulation shock, RCR^a is an actual regulatory capital ratio, RCR^m is a minimum regulatory capital ratio, α and β are parameters, and t is the period identifier.

Although the model is highly stylised, similar dynamics could result from micro-funded general equilibrium models (see, for example, Jakab and Kumhof, 2018; Benes and Kumhof, 2015; Meh and Moran, 2010; the first two studies feature models with minimum capital adequacy ratios, where a capital regulation shock could be easily introduced; the last study features a capital shock). The model implies that bank lending depends on the difference between the actual and minimum regulatory capital ratio (or, the excess capital). Other things being equal, an increase in the actual regulatory capital ratio increases lending, while an increase in the minimum regulatory capital ratio decreases it. Also, the actual regulatory capital ratio adjusts gradually to changes in its minimum levels. The former (i.e. actual regulatory capital ratio) also depends on factors other than capital regulations, represented by the capital shock. They could include changes in profitability or recapitalisations. The minimum regulatory capital ratio follows a random walk, with its changes driven by the capital regulation shock. Note, the model is not meant to be as realistic as possible. For example, the supervisor could be assumed to follow a more complex macroprudential policy rule. The model is meant to be complex enough for its purpose, as well as to be easy to map on empirical models.

Consider omitting the minimum regulatory capital ratio – a misspecification. In the model this would mean inserting equation 3 into equation 2.

$$RCR_t^a = \beta RCR_{t-1}^a + (1-\beta)(RCR_{t-1}^m + \eta_t^{cr}) + \eta_t^c \quad (4)$$

In this case there is a combination of capital regulation and capital shocks on the right-hand side of the equation. The consequences of interpreting them as one shock – capital regulation shock – can be studied by simulating data, estimating the parameters of AR (autoregressive) models, calculating residuals and regressing simulated bank lending on them in order to compute impulse response functions.[2] Let us denote misspecified capital regulation shocks as a shock to the actual regulatory capital ratio.

Data was simulated 1000 times for 100 periods (i.e. roughly the number available for empirical analysis in this study). α was assumed to be 0.5, β either 0.5 or 0.75. Shocks were drawn from the normal distribution with mean 0, standard deviation 1 for the capital shock and either 1 or 2 for the capital regulation shock.

In figure 1 median responses of bank lending to correctly identified capital regulation and capital shocks, as well as misspecified capital regulation shocks are presented. The magnitude of each impulse

[2] An equivalent way would be to extend equation 1 with a bank lending shock and estimate the parameters of VAR models.

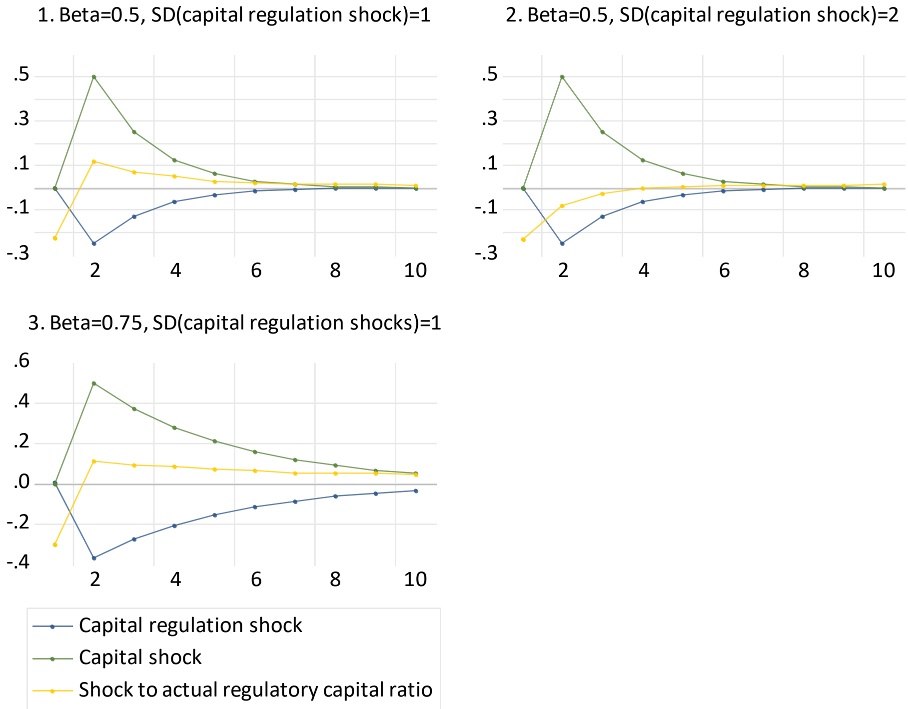
is 1 unit, and the direction is positive. Panel 1 presents responses for β assumed to be 0.5 and the standard deviation of capital and capital regulation shocks to be equal. A capital regulation shock decreases bank lending, while a capital shock increases it, to a larger extent (in absolute terms). A shock to the actual regulatory capital ratio, containing the sum of capital and capital regulation shocks, has an ambiguous effect on bank lending.

In panel 2 are responses for the standard deviation of capital regulation shocks twice as large as for capital shocks. The response of bank lending to the shock to the actual regulatory capital ratio has the same sign as the response to the capital regulation shock, but remains generally different. In panel 3 responses for β assumed to be 0.75 (a slower adjustment of the actual regulatory capital ratio to changes in the minimum regulatory capital ratio) are presented. While more persistent, they are generally similar to responses in panel 1.

The results imply that responses to shocks to the actual regulatory capital ratio correctly identify the sign of responses to capital regulation shocks only for a relatively high variance of the latter. Otherwise the effect is significantly underestimated.

In short, the use of actual regulatory capital ratios as a proxy for minimum regulatory capital ratios can cause a large bias. This is because capital regulation shocks might not get distinguished from capital shocks. The effects of both capital shocks and capital regulation shocks could be correctly identified in a VAR model without a minimum regulatory capital ratio as one of endogenous variables, by using sign restrictions. Such an approach is employed by Budnik et al. (2019). However, in that case, the effect on bank lending has to be imposed. This might not be preferred in studies aiming to establish whether there is any effect in the first place. Another way would be to control for factors other than capital regulations, containing fundamental capital shocks, in the actual regulatory capital ratio – for example, a measure of profitability. Whether this is a viable solution, at least in the case of data for Poland, will be tested in the sensitive analysis in the empirical part of the article.

Figure 1. Impulse responses of bank lending (median)



3. Research design

3.1. Models

In this subsection baseline models are described. In order to identify the effects of capital regulation shocks, and to separate them from capital shocks, I use Bayesian panel vector autoregressive (BPVAR) models. I apply the pooled estimator to within-transformed data, effectively using the fixed effects estimator.[3] For unit *i* it writes as:

$$y_{i,t} = \sum_{k=1}^{p} A_k y_{i,t-k} + Cx_{i,t} + \varepsilon_{i,t} \quad (5)$$

where y denotes a vector of endogenous variables, x is a vector of exogenous variables, A and C are matrices of coefficients and ε is a vector of residuals. t denotes time. $E(\varepsilon_{i,t}\, \varepsilon_{i,t}')$ is time invariant and common to all units, and $E(\varepsilon_{i,t}\, \varepsilon_{j,t}')$ is 0 for $i \neq j$.

[3] Having a "fixed *N*, reasonably large *T*" structure of the data, dynamic panel data estimators appeared not to be the optimal solution. In models with a lagged dependent variable as one of the regressors the use of the fixed effects estimator results in a bias. However, taking into account a reasonably large number of observations in the time dimension, any bias should be limited.

I use the BEAR (Bayesian estimation, analysis and regression) toolbox implementation of the pooled estimator of the BPVAR model, which adopts the normal-Wishart identification strategy for the derivation of the posterior (see Dieppe et al., 2016). Following Canova (2007), I assume the following hyperparameter values: for overall tightness – 0.2, for lag decay – 1, for exogenous variable tightness – 10^5. For the autoregressive coefficient I assume 0.8, which may be preferred in the case of variables known to be stationary (Dieppe et al., 2018).

Applying models to quarterly data, 4 lags are used, and shocks are identified using the Cholesky decomposition, with the following ordering (and, more generally, set) of variables: GDP, interest rate, bank lending, minimal regulatory capital ratio and actual regulatory capital ratio (note the mapping on the theoretical model presented in the previous section). I focus on responses to minimum regulatory capital ratio and actual regulatory capital ratio impulses, interpreted as capital regulation and capital shocks, respectively.

I use a panel data framework, as although 91 observations in the time dimension are available in general in baseline models, only 17-44 of them (depending on the measure) comprise the period since the first change in measures of capital regulations. For a given number of coefficients (21 for each equation in this case), with such a small relative number of effective observations, it appears unlikely to obtain high quality estimates exploiting the time dimension only. For example, Ouliaris et al. (2016) suggest the number of parameters to be below the number of observations divided by 3. The additional, cross-section dimension increases the number of effective observations. Assuming cross-sectional homogeneity in coefficients (consistent with the fixed effects estimator), this should significantly improve the quality of estimates.

3.2. Data

In this subsection both data used in baseline models and in the sensitivity analysis are described. In baseline models I use data for a balanced panel of commercial banks for Poland. After removing branches of credit institutions, not reporting capital in Poland, one state-owned bank, treated differently than the remaining banks in terms of capital regulations, and two banks with an activity significantly scaled-down after a portfolio sell-out there were 15 banks with continuous observations for the period from 1997Q1 (or 1997Q2, after first differencing) to 2022Q3. Eventually, the sample coverage of aggregate bank lending in the balanced panel ranges from 54% to 77%, with 62% on average (figure 2). For an unbalanced panel, used in the sensitivity analysis, it increases to 87-95%; 90% on average, with up to 82 banks. The start of the sample marks the first complete quarter of publicly unavailable monetary/prudential reporting – the source of bank-level data used. The sample in baseline models was cut before the quarter containing the first month of the COVID-19 pandemic, as shown to be acceptable for the purpose of parameter estimation by Lenza and Primiceri (2020). A sample ending in 2022Q3 was used in the sensitivity analysis.

Aggregate data are from publicly available sources: GDP from Eurostat and interest rates from GPW Benchmark through Refinitiv.

All variables were induced stationary by first differencing. GDP and bank lending were taken in logarithms first, resulting in log-differences (or quarterly growth rates after multiplying by 100).

Loans to non-financial corporations, households and the local government were taken into account. Foreign currency loans were adjusted for exchange rate fluctuations, using bank- and period-specific weights, so that they (i.e. foreign currency loans) correspond to sample mean exchange rates. Then, they were added to domestic currency loans. Also, bank-level data were winsorised, with the cut-off set at the 0.5th and the 99.5th percentile. The minimum regulatory capital ratio was not winsorised. Winsorising was used to limit the influence of outliers. The winsorising procedure accounted for seasonality by applying different thresholds for each quarter. Furthermore, bank lending was adjusted for mergers/acquisitions, estimating the parameters of AR models for loan log-differences with merger/acquisition dummy variables, and then removing the estimated effects of mergers/acquisitions – if statistically significant – captured by the dummy variables. Finally, bank lending, ROA (return on assets) and the actual regulatory capital ratio were seasonally adjusted using the Census X13 method (the STL decomposition method – seasonal-trend decomposition using locally estimated scatterplot smoothing – for banks with discontinuities); ROA is used in the sensitivity analysis.

As the measure of interest rate, WIBOR (Warsaw interbank offered rate) 1M was used. Total capital ratio (or, before its introduction, solvency ratio) was used as the measure of the actual regulatory capital ratio.

For the minimum regulatory capital ratio, as mentioned, two measures were considered (separately). The first one is the legally binding total capital ratio (solvency ratio), related to the "Act on macroprudential supervision over the financial system and crisis management", marked as just "minimum regulatory capital ratio" on figures in the article. It was first set at the turn of 2015 and 2016.[4] For earlier periods, the level of 8% was assumed (in accordance with the Banking Act, as of before its amendment at the end of 2015).

The second considered minimum regulatory capital ratio is the minimum total capital ratio (solvency ratio) allowing for a possibly full dividend pay-out, according to commercial bank dividend policy, set by KNF. It could be interpreted as more of a recommendation than legally binding. According to the Author's best knowledge, it was first set for 2009 (KNF, 2009), at 10%. In the next 2 years KNF recommended, respectively, not to pay out a dividend or to pay it out to the smallest extent (KNF, 2010; KNF, 2011). The level of 10% was assumed for 2010-2011, while for the period before 2009 assumed was the legally binding minimum solvency ratio. For 2020 and the first half of 2021, when a dividend pay-out was not recommended either, a formula for 2019 was assumed. This was because it appeared more reasonable to assume no change in capital regulations than its loosening when a dividend pay-out was not recommended. In any case, using the legally binding minimum solvency ratio/total capital ratio for 2010, 2011, 2020 and the first half of 2021 (not reported in the article, available on request) did not bring qualitative changes to the results. In the next years there were further changes to this measure.[5]

The actual regulatory capital ratio, as well as the two measures of the minimum regulatory capital ratio, are presented in figure 3. Before the first change in either of the measures of the minimum

[4] In 2015 Q4 the pillar 2 requirement (a bank-specific add-on related to foreign currency loans) was announced. Later, the introduction of respective (aggregate and bank-specific) buffers and the pillar 2 guidance followed.
[5] In earlier studies, when calculating excess capital, Dybka et al. (2017) and Czaplicki (2021) treat 2012 as the first year of the minimum regulatory capital ratio higher than 8%. Kapuściński (2017), and Kapuściński and Stanisławska (2018) use 2009.

regulatory capital ratio there had been visible variability in the actual ratios. This suggests that it was not only driven by capital regulation shocks. Also, there is a tendency for actual regulatory capital ratios to increase, with some lag, together with the tightening of capital regulation. Furthermore, in the whole sample the median of the dividend policy minimum regulatory capital ratio was higher than the median of the legally binding ratio.

Figure 2. Share of sample in population (volume of loans, %)

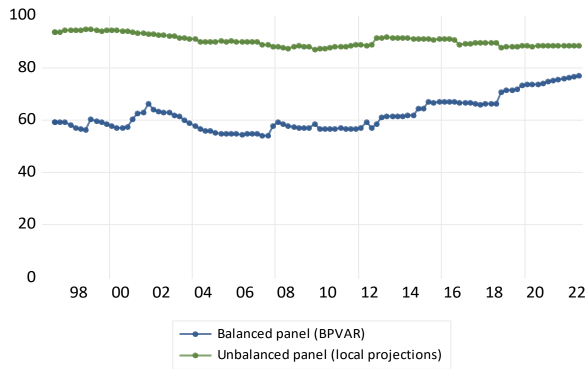
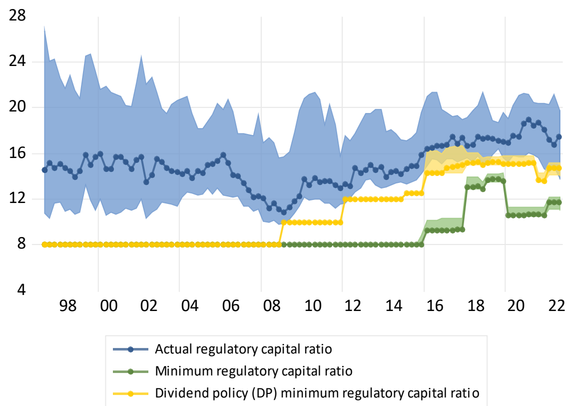


Figure 3. Regulatory capital ratios – actual and minimum (%), unbalanced panel



Note: intervals denote quantiles 0.25 and 0.75.

3.3. Sensitivity analysis and extensions

As a sensitivity analysis, I make the following changes:

- omitting the minimum regulatory capital ratio, preceding the actual regulatory capital ratio with ROA in the Cholesky decomposition and interpreting an impulse associated with the actual regulatory capital ratio as the capital regulation shock,

- estimating impulse responses using local projections (see Jordà, 2005) and data for an unbalanced panel of banks, assuming the same relationship structure as in baseline BPVAR specifications,
- lengthening the sample to 2022Q3, both for baseline BPVAR specifications and local projections.[6]

The first part of the sensitivity analysis was to test whether the effects of capital regulations could be reliably identified without using the minimum regulatory capital ratio as one of the variables in the model. Instead, the impulse to the actual regulatory capital ratio orthogonal to ROA – as well as to GDP and the interest rate – was interpreted as the capital regulation shock. Bank profits are the basic source of bank capital accumulation and could contain fundamental capital shocks.

The second part of the sensitivity analysis was to establish whether baseline results were not distorted by some form of sample selection bias. For example, banks dropping out of the sample could have been affected disproportionately by capital regulations. In that case, results based on the balanced panel could be underestimated. Local projections allow for an unbalanced panel. They produce the same (population) impulse responses as VAR-type models, up to the horizon equal to the VAR-type model lag length (Plagborg-Møller and Wolf, 2021). For longer horizons they might differ, but, as will be seen, any differences might be difficult to interpret. Impulse responses based on BPVAR models will turn out to be more well-behaved.

As far as the third, final sensitivity check is concerned, it was to establish whether the use of observations since the start of the COVID-19 pandemic distorts the estimates (given the increased volatility), or rather sharpens them (given the changes in prudential policy at the time).

[6] In the working paper version of the article more basic sensitivity checks were carried out. They included: replacing the fixed effects estimator with the mean group estimator, changing the set of endogenous variables, halving the number of lags, changing hyperparameter values, taking variables in levels or log-levels and shortening the sample to start in 2009Q1, marking the first change in one of the analysed measures of capital regulations. The results turned out to be robust to reasonable changes in the specification and are not presented in this version of the article.

As an extension, I attempt to identify a non-linearity in the effects of changes in capital regulations, depending on whether they are tightened or loosened. In other words, whether the loosening of capital regulations resembles “pushing on a string”, as in the case of monetary policy in some economies (see, for example, Angrist et al., 2018); on evidence for a set of macroprudential policy instruments in a panel of economies, see Cerutti et al. (2017). This could easily be tested within the local projections framework. The interaction between a measure of capital regulations and a dummy variable, taking 1 when capital regulations are loosened and 0 otherwise, was added as one of the regressors, together with the dummy variable itself. The statistical significance of the former was tested, and separate impulse responses for capital regulation tightening and loosening were computed.

4. Results

This section presents the results of the main analysis. The estimated effects of capital regulation and capital shocks are discussed in turn, for the two analysed minimum regulatory capital ratios. Both shocks are normalised to be of one unit (i.e. a one percentage point increase in the minimum or the actual regulatory capital ratio). This also concerns the rest of the results.

In figure 4 median responses to the capital regulation impulse are presented, with 95% confidence intervals. After a capital regulation shock there is a decrease in bank lending, both when the minimum regulatory capital ratio related to the “Act on macroprudential supervision…” is used as the measure of capital regulations and when the dividend policy minimum regulatory capital ratio is. Impulse responses are statistically significant at least for some horizons (in the former case, the effect is rather borderline statistically significant, though). In the former case, the maximum effect on quarterly bank lending growth is -0.46 p.p. (horizon 4). This translates into a maximum effect on annual bank lending growth of -1.49 p.p. (horizon 6) and an effect on the volume of loans after 20 quarters of -2.09%. The effect accumulates from -1.47% after 5 quarters and -2.16% after 10 quarters. In the latter case, the maximum effect on quarterly bank lending growth is -0.41 p.p. (horizon 6). This translates into a maximum effect on annual bank lending growth of -1.41 p.p. (horizon 8) and an effect on the volume of loans after 20 quarters of -2.58%. The effect accumulates from –0.89% after 5 quarters and -2.29% after 10 quarters.

There appear to be two likely explanations for the only borderline statistically significant results on the effects of capital regulations, as measured by the minimum regulatory capital ratio related to the "Act on macroprudential supervision…". The first one is that the number of effective observations remains too low to obtain narrow confidence intervals; the median impulse response function of bank lending is, intuitively, negative. The second explanation is that banks adjusted to the tightening of this measure of capital regulations in advance, by complying with the dividend policy minimum regulatory capital ratio (tightened earlier and more restrictive on average).

In both cases, the response of the actual regulatory capital ratio is statistically insignificant. This could be due to the heterogeneity of responses between banks, however, widening confidence intervals. The median impulse response function is positive. As far as the response of GDP is concerned, when the minimum regulatory capital ratio related to the "Act on macroprudential supervision…" is used as the measure of capital regulations, it is of a counterintuitive sign in the horizon it is statistically significant. That is, it is positive. When as the measure of capital regulations the dividend policy minimum regulatory capital is used, there is a decrease in GDP. The scale of the effect on GDP from bank-level bank panel data models is difficult to interpret. Monetary policy either remains passive or its response is negligible. It appears to reflect more the environment of changes in capital regulations than monetary-macroprudential policies interactions.

Figure 5 presents responses to the capital impulse. They are qualitatively and quantitatively similar for both variants of the model. After an increase in the actual regulatory capital ratio exogenous to capital regulations, initially the response of bank lending switches signs, to stabilize above zero and cumulate to positive values. There is also an increase in GDP. Capital regulations remain passive, and the response of the measure of monetary policy is negligible. These results are consistent with capital regulation shocks and capital shocks affecting bank lending in the opposite direction.

Figure 4. Responses to capital regulation impulse

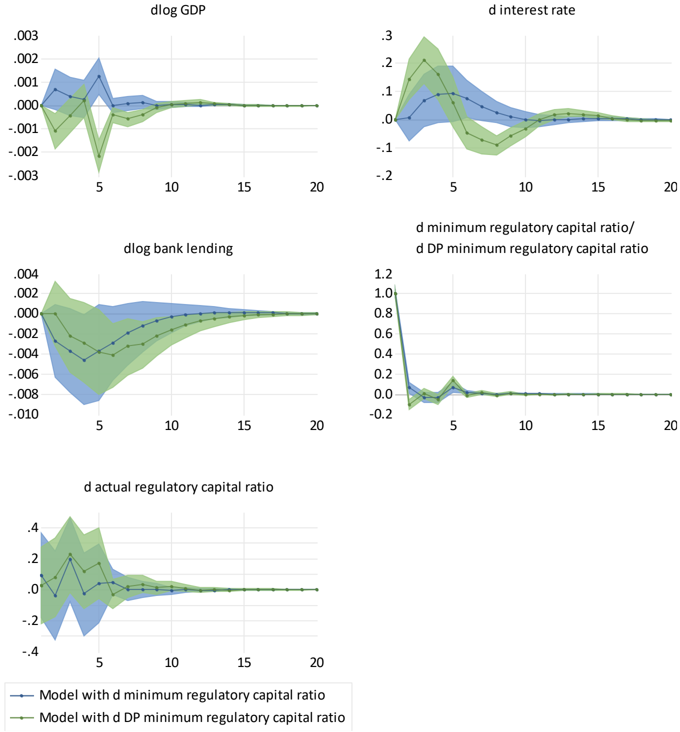
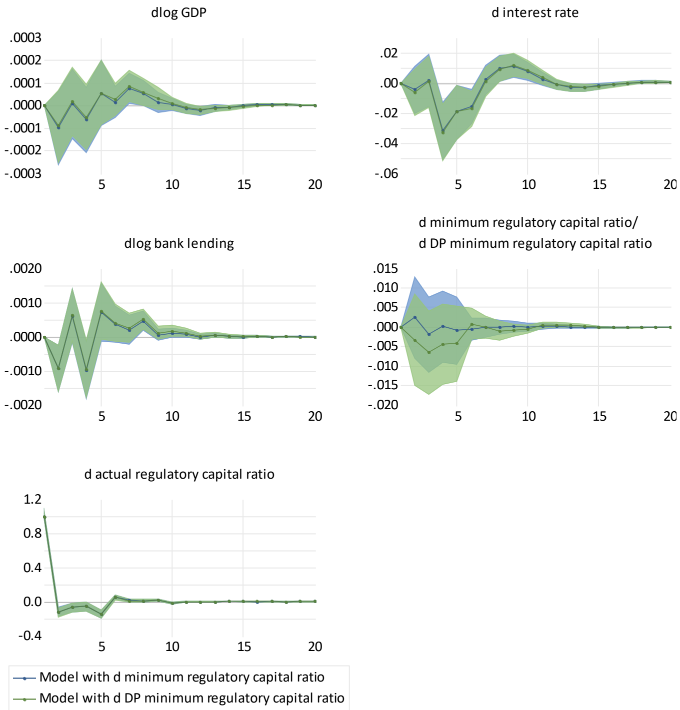


Figure 5. Responses to capital impulse



5. Sensitivity analysis and extensions

This section discusses the results of the sensitivity analysis. Then, it turns to presenting the results of extensions.

In figure 6 responses to the shock to the actual regulatory capital ratio based on the model omitting a minimum regulatory capital ratio, but containing ROA, are presented. They are both qualitatively and quantitatively similar to responses to the capital shock based on models with the minimum regulatory capital ratio, and without ROA. This suggests that it is unlikely that responses in figure 6 could be interpreted as being to capital regulation impulses. In order to adequately identify their effects, at least together with the Cholesky decomposition (rather than sign restrictions) and at least in the case of data for Poland, the use of the minimum regulatory capital ratio as one of the variables appears to be required.

Figures 7-8 present responses to capital regulation and capital impulses from local projections and the larger, unbalanced panel of banks. For horizons up to the BPVAR lag length (i.e. 4 quarters after the shock, marked as horizon 5) they are qualitatively similar to those based on baseline models. One difference is that responses to the capital regulation impulse are more firmly statistically significant for both minimum regulatory capital ratios. Also, the response of the actual regulatory capital ratio becomes borderline statistically significant for one measure of capital regulations. As far as responses to the capital impulse are concerned, the initial sign-switching in the impulse response function of bank lending disappears. For horizons after the BPVAR lag length, on the one hand, in the case of capital regulation impulses, responses become more rich. On the other hand, with non-negligible responses of aggregate variables long after the shock (in terms of point estimates), this longer perspective is difficult to interpret, appearing to reflect the environment of changes in minimum regulatory capital ratios. Baseline impulse responses are more well-behaved.

In figures 9-12 responses to capital regulation and capital impulses based on BPVAR models and local projections are presented, from the longer sample, ending in 2022Q3. For the capital shock, responses based on both BPVAR models and local projections are qualitatively similar to those from the shorter sample. As far as the capital regulation shock is concerned, on the other hand, in BPVAR models responses of bank lending cease to be statistically significant. In local projections, where the share of pandemic and post-pandemic sample in total observations is smaller given the larger number of banks, they largely remain statistically significant, but smaller in terms of point estimates. The upper bounds of the confidence intervals are closer to 0 (in many cases crossing it). This could be due to the increased volatility in the sample since the start of the COVID-19 pandemic.

Moving on to the extensions, figures 13-16 present results of the analysis of the non-linearity in the effects of capital regulations, depending on whether they are tightened or loosened. Figures 13-14 are based on local projections estimated on the shorter sample (i.e. ending in 2019Q4), while figures 15-16 – on the longer sample (i.e. ending in 2022Q3). In figures 13 and 15 are *p*-values for the test with the null hypothesis of no non-linearity, for respective variables and respective variants of the model (differing in terms of the measure of capital regulations). Figures 14 and 16 present non-linear impulse responses, separate for the tightening and the loosening of capital regulations, for each variable and for each variant of the model.

Focusing on the early horizons of responses to capital regulation shocks, the effect on bank lending is more evident to be non-linear in the longer sample. In the shorter sample the non-linearity also appears to have been present, but more so for the effects of the dividend policy minimum regulatory capital ratio. However, there is also evidence on non-linear responses of some other variables – most notably the interest rate. This makes the results difficult to interpret. In any case, the point estimates of the effect of the tightening of capital regulations are negative, while responses for the loosening – around zero. This result needs to be treated with caution, however, also due to the dominance of increases in the measures of capital regulation in the sample.

Figure 6. Responses to impulse to actual regulatory capital ratio – BPVAR without minimum regulatory capital ratio, with ROA (sample ending in 2019Q4)

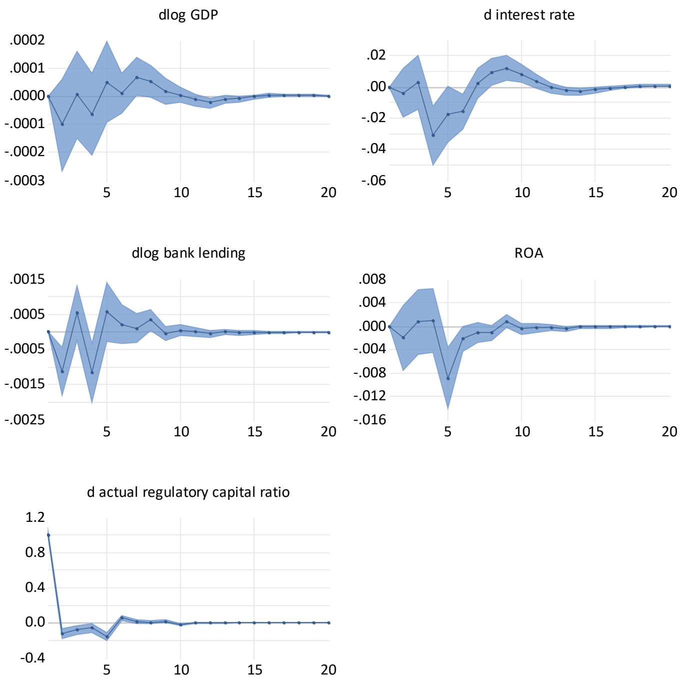


Figure 7. Responses to capital regulation impulse – local projections, sample ending in 2019Q4

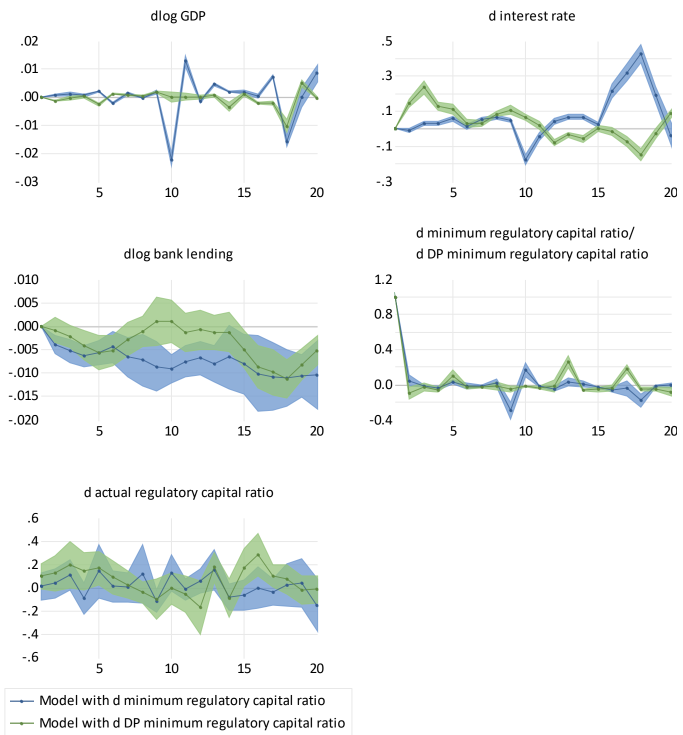


Figure 8. Responses to capital impulse – local projections, sample ending in 2019Q4

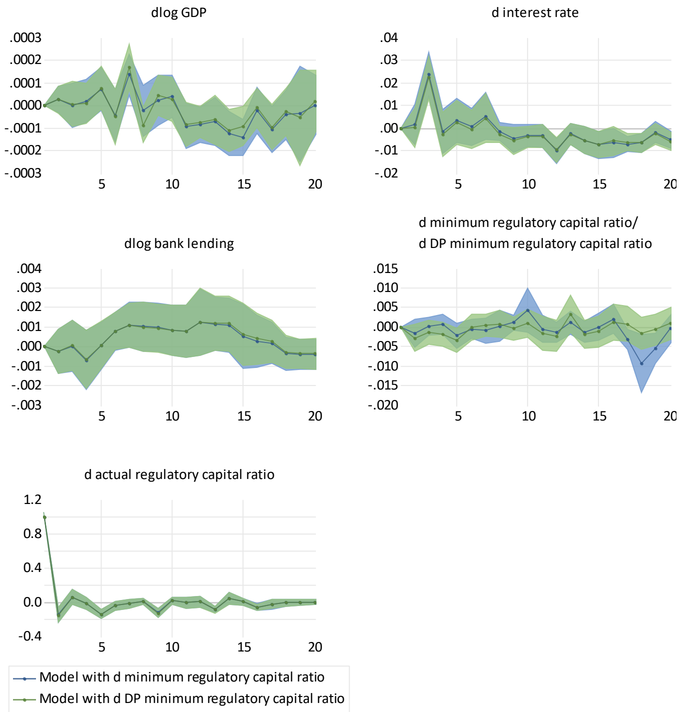


Figure 9. Responses to capital regulation impulse – BPVAR, sample ending in 2022Q3

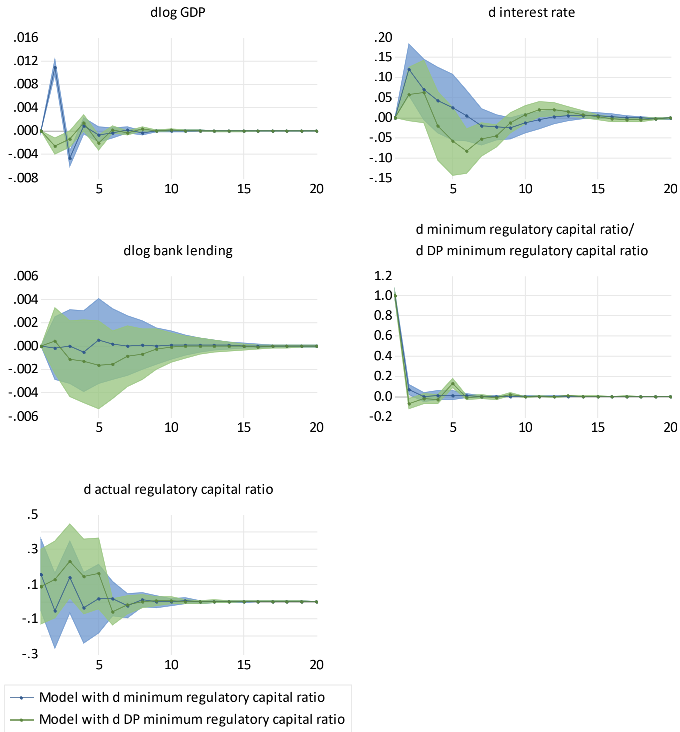


Figure 10. Responses to capital impulse – BPVAR, sample ending in 2022Q3

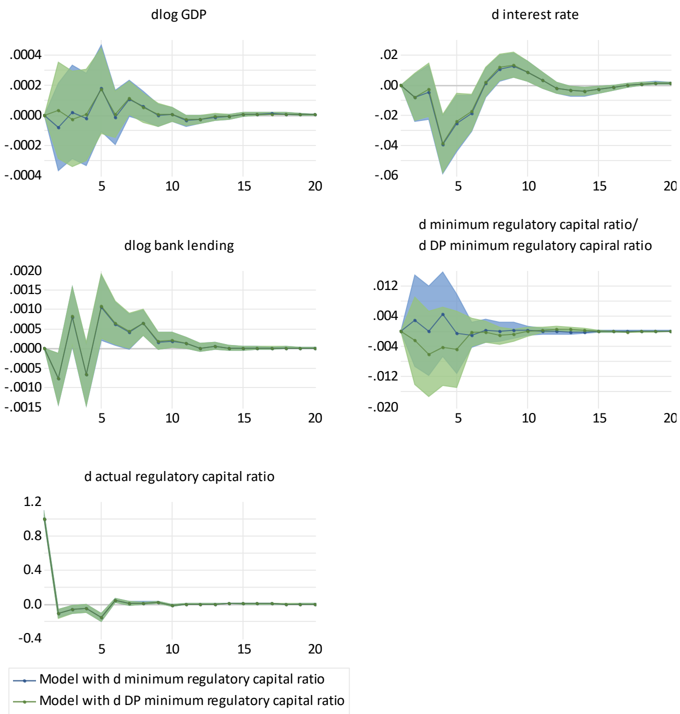


Figure 11. Responses to capital regulation impulse – local projections, sample ending in 2022Q3

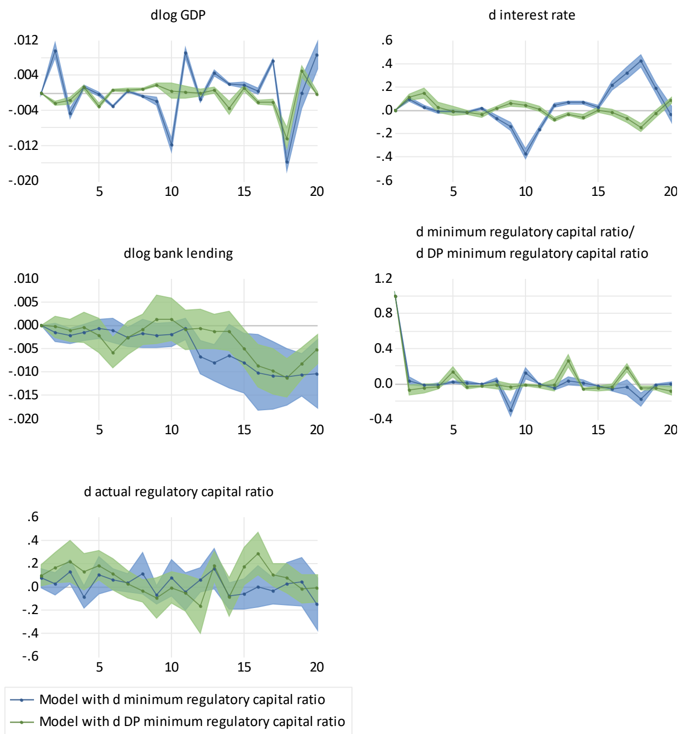


Figure 12. Responses to capital impulse – local projections, sample ending in 2022Q3

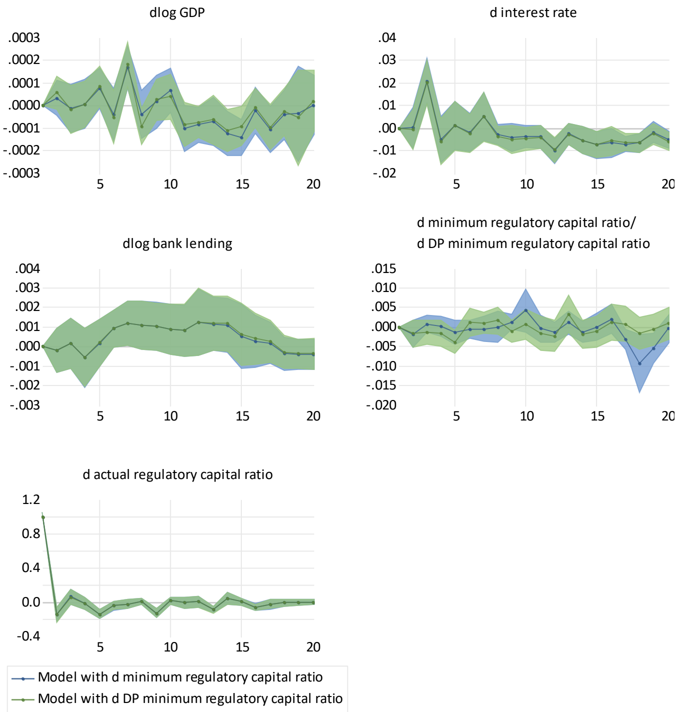


Figure 13. *P*-values for non-linearity, sample ending in 2019Q4 (local projections)

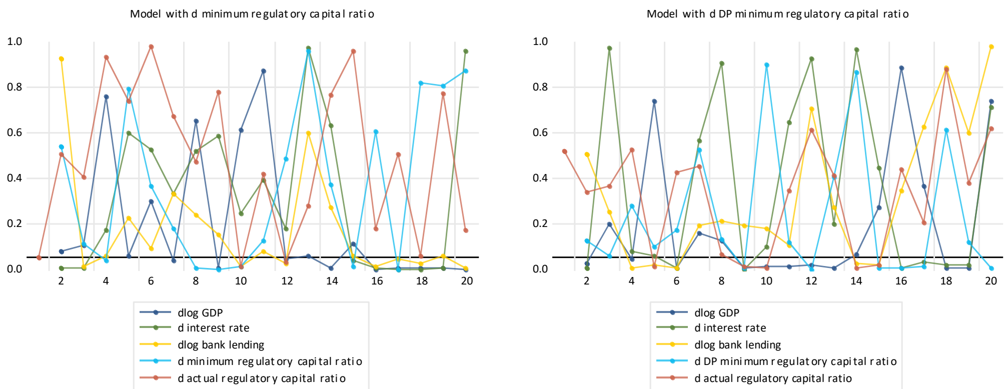


Figure 14. Non-linear responses to capital regulation impulse, sample ending in 2019Q4 (local projections)

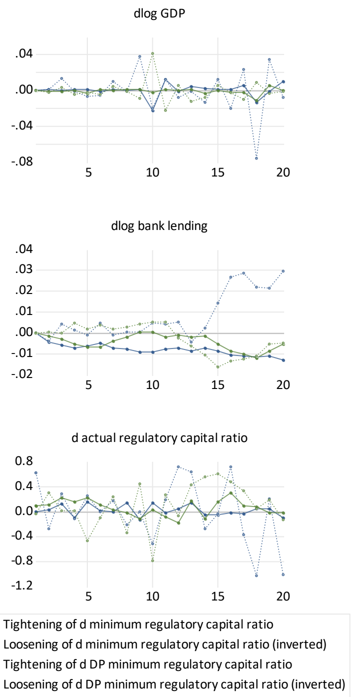


Figure 15. *P*-values for non-linearity, sample ending in 2022Q3 (local projections)

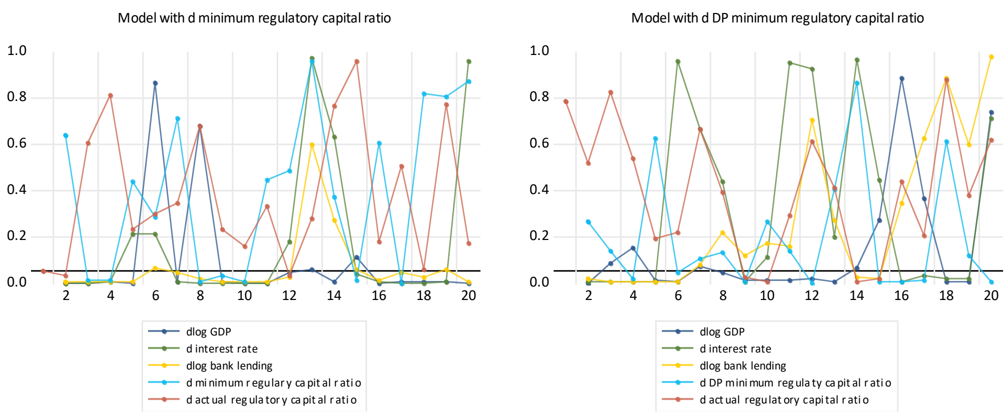
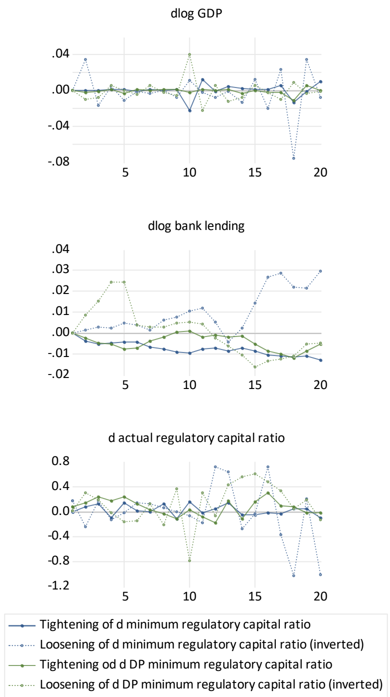


Figure 16. Non-linear responses to capital regulation impulse, sample ending in 2022Q3 (local projections)



6. Conclusion

The study is the first to directly estimate the short-term effects of changes in capital regulations in Poland, measuring them by minimum regulatory capital ratios (rather than by actual ones or indirectly, by excess capital). For the minimum regulatory capital ratio allowing for a full dividend pay-out, a negative effect of the tightening of capital regulations on bank lending was found. Evidence for another analysed measure – the minimum regulatory capital ratio associated with macroprudential supervision – was less clear. It was also illustrated, as the starting point for the choice of a research design, that the use of actual regulatory capital ratios as a proxy for minimum regulatory capital ratios can cause a large bias. Ambiguous evidence on differences in the effects of changes in capital regulations, depending on whether they are tightened or loosened, was found.

As far as policy implications are concerned, the results confirm that capital regulations are an effective tool in limiting excessive bank lending (in aggregate) in Poland. After 20 quarters, every 1 p.p. increase in the minimum regulatory capital ratio results in the volume of loans being lower by 2.09-

2.58% on average. The effect accumulates from between -0.89 and -1.47% after 5 quarters and bet\ween -2.16 and -2.29% after 10 quarters.

The study provides evidence against using actual regulatory capital ratios as a measure of capital regulations. Future research, as far as capital regulations are concerned, could focus on identifying differences in the responses of different types of loans. Also, the responses of other variables (for example, rates on loans or dividend pay-out ratios) could be further explored. Furthermore, the effects of other prudential tools could be attempted to be identified by using bank-level panel data for Poland (loan-to-value or debt-to-income, for example).

References

Araujo, J., M. Patnam, A. Popescu, F. Valencia, and W. Yao (2020), Effects of Macroprudential Policy: Evidence from Over 6,000 Estimates, IMF Working Papers 20/67, International Monetary Fund.

Bańbuła, P., A. Kotuła, A. Paluch, M. Pipień, and P. Wdowiński (2019), Optimal level of capital in the Polish banking sector. NBP Working Papers 312, Narodowy Bank Polski.

Basel Committee on Banking Supervision (2011), Basel III: A global regulatory framework for more resilient banks and banking systems, Basel Committee on Banking Supervision.

Benes, J. and M. Kumhof (2015), Risky bank lending and countercyclical capital buffers, Journal of Economic Dynamics and Control 58, 58–80.

Boissay, F., C. Cantu, S. Claessens, and A. Villegas (2019), Impact of financial regulations: insights from an online repository of studies, BIS Quarterly Review March 2019, Bank for International Settlements.

Borsuk, M. and K. Kostrzewa (2020), Miary ryzyka systemowego dla Polski. Jak ryzyko systemowe wpływa na akcję kredytowa banków?, Bank i Kredyt 51 (3), 211–238.

Budnik, K., M. Affinito, G. Barbic, S. B. Hadj, E. Chretien, H. Dewachter, C. I. Gonzalez, J. Hu, L. Jantunen, R. Jimborean, O. Manninen, R. Martinho, J. Mencia, E. Mousarri, L. Narusevicius, G. Nicoletti, M. O'Grady, S. Ozsahin, A. R. Pereira, The benefits and costs of adjusting bank capitalisation: evidence from euro area countries, Working Paper Series 2261, European Central Bank.

Canova, F. (2007), Methods for Applied Macroeconomic Research, Princeton University Press.

Cerutti, E., R. Correa, E. Fiorentino, and E. Segalla (2017), Changes in Prudential Policy Instruments – A New Cross-Country Database, International Journal of Central Banking 13 (1), 477–503.

Chmielewski, T. (2003), Interest rate pass-through in the Polish banking sector and bank-specific financial disturbances, MPRA Paper 5133, Munich University Library.

Czaplicki, M. (2021), Measuring the restrictiveness of (macro)prudential policy: the case of bank capital regulation in Poland, Journal of Banking Regulation 23, 322–338.

Dieppe, A., R. Legrand, and B. van Roye (2016), The BEAR toolbox, Working Paper Series 1934, European Central Bank.

Dieppe, A., R. Legrand, and B. van Roye (2018), The Bayesian Estimation, Analysis and Regression (BEAR) Toolbox, Technical Guide.

Dybka, P., B. Olesiński, P. Pękala, and A. Torój (2017), To SVAR or to SVEC? On the transmission of capital buffer shocks to the real economy, Bank i Kredyt 48 (2), 119–148.

Ehrenbergerova, D., M. Hodula, and Z. Rakovska (2020), Does Capital-Based Regulation Affect Bank Pricing Policy? Working Paper Series 5/2020, Czech National Bank.

Fidrmuc, J. and R. Lind (2020), Macroeconomic impact of Basel III: Evidence from a meta-analysis, Journal of Banking & Finance 112.

Gajewski, K. and O. Krzesicki (2017), International Banking and Cross-Border Effects of Regulation: Lessons from Poland, International Journal of Central Banking 13 (1), 315–340.

J. Rivera-Rozo, C. Trikoupis, F. Venditti, and S. Velasco (2019), The benefits and costs of adjusting bank capitalisation: evidence from euro area countries, Working Paper Series 2261, European Central Bank.

Jakab, Z. and M. Kumhof (2018), Banks are not intermediaries of loanable funds – facts, theory and evidence, Staff Working Papers 761, Bank of England.

Jordà Ò. (2005), Estimation and Inference of Impulse Responses by Local Projections, American Economic Review 95(1), 161-182.

Kapuściński, M. (2017), The Role of Bank Balance Sheets in Monetary Policy Transmission: Evidence from Poland, Eastern European Economics 55 (1), 50–69.

Kapuściński, M. and E. Stanisławska (2018), Measuring bank funding costs in the analysis of interest rate pass-through: Evidence from Poland, Economic Modelling 70, 288–300.

Kashyap, A., J. Stein, and S. Hanson (2010), An Analysis of the Impact of 'Substantially Heightened' Capital Requirements on Large Financial Institutions, Booth School of Business, University of Chicago, mimeo.

Kilian L., M.D. Plante, A.W. Richter (2022), Macroeconomic Responses to Uncertainty Shocks: The Perils of Recursive Orderings, CEPR Discussion Paper 17698, Centre for Economic Policy Research.

Kolcunova, D. and S. Malovana (2019), The Effect of Higher Capital Requirements on Bank Lending: The Capital Surplus Matters, Working Paper Series 2/2019, Czech National Bank.

Komisja Nadzoru Finansowego (2009), Raport o sytuacji banków w 2008 roku, Komisja Nadzoru Finansowego.

Komisja Nadzoru Finansowego (2010), Raport o sytuacji banków w 2009 roku, Komisja Nadzoru Finansowego.

Komisja Nadzoru Finansowego (2011), Raport o sytuacji banków w 2010 roku, Komisja Nadzoru Finansowego.

Lenza, M. and G. Primiceri (2020), How to estimate a VAR after March 2020, Working Paper Series 2461, European Central Bank.

Malovana, S., M. Hodula, J. Bajzik, and Z. Gric (2021), A Tale of Different Capital Ratios: How to Correctly Assess the Impact of Capital Regulation on Lending, Working Paper Series 8/2021, Czech National Bank.

Marcinkowska, M., P. Wdowiński, S. Flejterski, S. Bukowski, and M. Zygierewicz (2014), Wplyw regulacji sektora bankowego na wzrost gospodarczy – wnioski dla Polski, Materiały i Studia 305, Narodowy Bank Polski.

Meh, C. and K. Moran (2010), The role of bank capital in the propagation of shocks, Journal of Economic Dynamics and Control 34 (3), 555–576.

Olszak, M., I. Kowalska, P. Chodnicka-Jaworska, and F. Switała (2020), Do cyclicality of loan-loss provisions and income smoothing matter for the capital crunch – the case of commercial banks in Poland, Bank i Kredyt 51 (4), 383–436.

Ouliaris, S., A. Pagan, and J. Restrepo (2016), Quantitative Macroeconomic Modeling with Structural Vector Autoregressions – An EViews Implementation, IHS Global.

Pawłowska, M., D. Serwa, and S. Zajączkowski (2014), International transmission of liquidity shocks between parent banks and their affiliates: the host country perspective, NBP Working Papers 172, Narodowy Bank Polski.

Plagborg-Møller M. and C. K. Wolf (2021), Local Projections and VARs Estimate the Same Impulse Responses, Econometrics, 89(2), 955-980.

Serwa, D. and P. Wdowiński (2017), Modeling Macro-Financial Linkages: Combined Impulse Response Functions in SVAR Models, Central European Journal of Economic Modelling and Econometrics 9 (4), 323–357.

Stanisławska, E. (2014), Interest rate pass-through in Poland. Evidence from individual bank data, NBP Working Papers 179, Narodowy Bank Polski.

VanHoose, D. (2007), Theories of bank behavior under capital regulation, Journal of Banking & Finance 31 (12), 3680–3697.

Wośko, Z. (2015), Modelling credit growth in commercial banks with the use of data from Senior Loan Officers Opinion Survey, NBP Working Papers 210, Narodowy Bank Polski.

Wróbel, E. (2021), Shocks to bank capital position: Do they matter for lending to firms and how they are channelled? Evidence from Senior Loan Officer Opinion Survey for Poland, NBP Working Papers 336, Narodowy Bank Polski.