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Abstract

In this study I analyse the effects of the transition to higher actual regulatory capital ratios due to the tightening of capital regulations in Poland. In contrast to earlier studies for this economy, as a measure of capital regulations I directly use minimum regulatory capital ratios. I focus on the impact on bank lending and GDP. I apply Bayesian panel vector autoregressive models to bank-level data. I find that the tightening of capital regulations lowers bank lending and GDP 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 illustrate, as the starting point for the choice of a research design, the threats of not distinguishing capital regulation shocks from capital shocks. Finally, I attempt to identify non-linearities in the effects of changes in capital regulations.

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

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

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 combination of responses under several Cholesky orderings, are interpreted as transition effects of capital regulations. 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),

[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'.

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, finding 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 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 GDP and 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 the fixed effects estimator, to data for a balanced panel of banks. I also illustrate, as the starting point for the choice of a research design, the threats of not distinguishing between capital regulation shocks and capital shocks (for example, by using actual regulatory capital ratios as a proxy for minimum regulatory capital ratios). Furthermore, I attempt to identify non-linearities in the effects of changes in capital regulations, by comparing impulse response functions for groups of banks, divided according to their characteristics.

I find that the tightening of capital regulations lowers bank lending and GDP for the minimum regulatory capital ratio allowing for a 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. Finally, I find some differences in the responses of groups of banks. However, with bank characteristics being correlated, 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 not distinguishing capital regulation shocks from capital shocks. The third section describes research design. In the next two sections there 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 not distinguishing capital regulation shocks from capital shocks, assume the following model:

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

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

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

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 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 loan loss reserves 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 the following misspecifications:

- Omitting the minimum regulatory capital ratio. In the model this would mean inserting equation 3 into equation 2.

$$RCR^a_t = \beta RCR^a_{t-1} + (1-\beta)(RCR^m_{t-1} + \eta^{cr}_t) + \eta^c_t \tag{4}$$

- Modelling the excess capital (defined as the difference between the actual and minimum regulatory capital ratio), rather than the actual and minimum regulatory capital ratio separately. In the model that would mean subtracting equation 3 from equation 2.

$$RCR_t^a - RCR_t^m = \beta(RCR_{t-1}^a - RCR_{t-1}^m) - \beta\eta_t^{cr} + \eta_t^c \tag{5}$$

In both cases there is a combination of capital regulation and capital shocks on the right-hand side of the equations. The consequences of interpreting them as one shock – capital regulation shock – can be studied by simulating data, estimating the parameters of AR 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 and a shock to excess capital, respectively.

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.[3] The magnitude of each impulse 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. A shock to excess capital, containing the difference between capital and capital regulation shocks, with the latter having a smaller weight, has similar effects to a capital shock.

On panel 2 there 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. On 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. The response of bank lending to the shock to excess capital is more similar to the response to the capital regulation shock (in absolute terms).

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. Also, responses to shocks to excess capital approximate responses to capital regulation shocks the better, the slower the adjustment of the actual regulatory capital ratio to

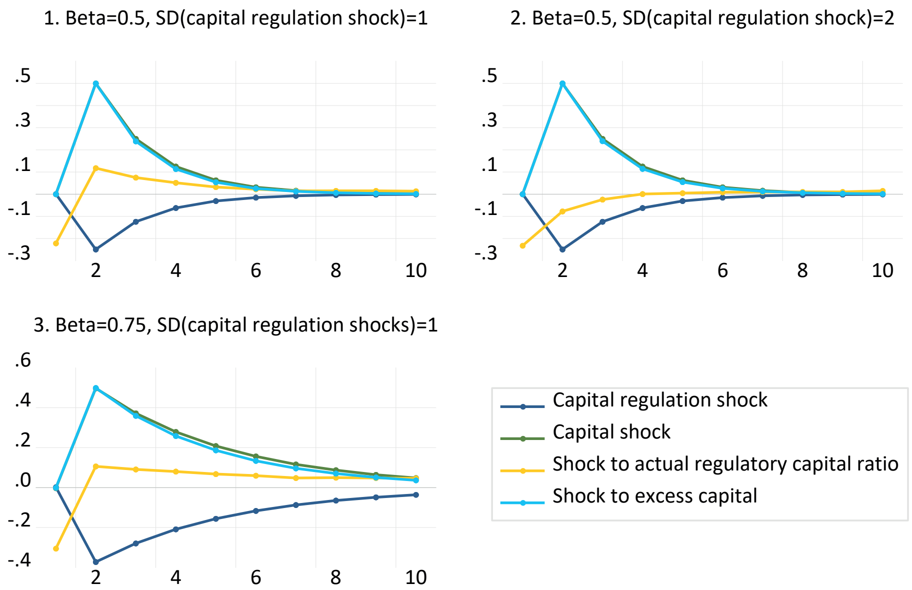
[2] An equivalent way would be to extend equation 1 with a bank lending shock and estimate the parameters of VAR models.

[3] For responses with 95% confidence intervals, see the Online Appendix, available at: https://doi.org/10.6084/m9.figshare.19518859.

the minimal regulatory capital ratio. With a relatively fast adjustment the effect is overestimated.

In short, the use of actual regulatory capital ratios as a proxy for minimum regulatory capital ratios can cause a large bias. This is less so for the modelling of the capital position and assuming the effects of capital regulation shocks to be symmetrical to capital shocks.[4]

Figure 1: Impulse responses of bank lending (median)



[4]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.

3 Research design

3.1 Models

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.[5] For unit i it writes as:

$$y_{i,t} = \sum_{k=1}^{p} A_k y_{i,t-k} + Cx_{i,t} + \varepsilon_{i,t} \tag{6}$$

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 $i \neq j$.

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 augment models with one exogenous variable (except for the constant) – a dummy variable taking 1 after the introduction of the bank levy. 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, 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-

[5] 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.

sectional homogeneity in coefficients (consistent with the fixed effects estimator), this should significantly improve the quality of estimates.

3.2 Data

I use data for a balanced panel of banks for Poland. After removing branches of credit institutions, not reporting capital in Poland, and one state-owned bank, treated differently than the remaining banks in terms of capital regulations, there were 18 banks with continuous observations for the period from 1997Q1 (or 1997Q2, after first differencing) to 2019Q4.[6] Eventually, the sample coverage of aggregate bank lending ranges from 53% to 75%, with 66% on average (figure 2). The start of the sample marks the first complete quarter of (publicly unavailable) monetary/prudential reporting – the main source of bank-level data used. The sample 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).

Aggregate data are from publicly available sources: GDP from Eurostat, interest rates from Refinitiv, CPI from Statistics Poland and REER from the Bank of International Settlements; CPI and REER are used in sensitivity analysis.

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

Foreign currency loans were adjusted for exchange rate fluctuations, using bank-, period-and loan type-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 lending was adjusted for mergers/acquisitions, using SARMA (seasonal autoregressive moving average) models for loan log-differences with merger/acquisition dummy variables, and then removing the estimated effects of mergers/acquisitions, captured by the dummy variables. Furthermore, bank lending, ROA (return on assets) and the share of impaired loans were seasonally adjusted using the Census X12 method; ROA and the share of impaired loans are used in sensitivity analysis. Finally, bank-level data were winsorised, with the cut-off set at the 2.5th and the 97.5th percentile, except for the actual regulatory capital ratio, for which only observations above the 97.5th percentile were replaced with the 97.5th percentile itself. The minimum regulatory capital ratio was not winsorised. Winsorising was used to limit the influence of outliers.

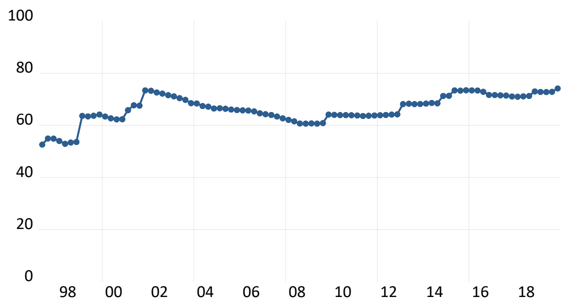
As the measure of interest rate, WIBOR (Warsaw interbank offered rate) 1M was used. Total capital ratio (or, before its introduction, solvency ratio) was used as the measure of the actual regulatory capital ratio. For the minimum regulatory capital

[6] For cooperative banks, only those reporting under monetary statistics directly were considered, and for none of them were there continuous observations for the sample period; this does not concern affiliating banks.

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.[7] 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 full dividend pay-out, according to commercial bank dividend policy, set by KNF – more of a recommendation than legally binding. According to the Author's best knowledge, it was first set for 2009 (Komisja Nadzoru Finansowego, 2009). In the next 2 years KNF recommended, respectively, not to pay out a dividend or to pay it out to the smallest extent (Komisja Nadzoru Finansowego, 2010, 2011). The level of 8% was assumed for 2010-2011, similarly as for the period before 2009. However, using the level of 10% for 2010 and 2011 (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.[8]

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 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.

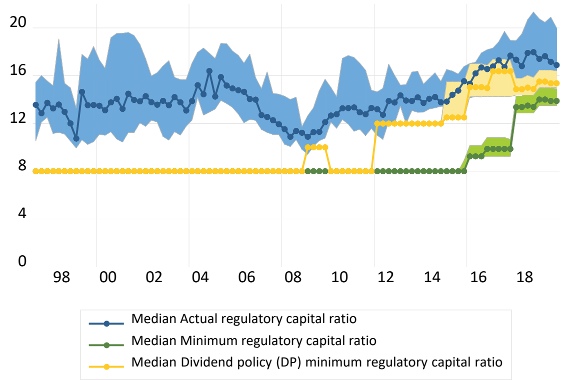
Figure 2: Share of sample in population (volume of loans, %)



[7] In 2015 Q4 a bank-specific add-on related to foreign currency loans was announced. Later, the introduction of respective (aggregate and bank-specific) buffers followed.

[8] 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.

Figure 3: Regulatory capital ratios – actual and minimum (%)



3.3 Sensitivity analysis and extensions

As a sensitivity analysis, I make the following changes:

- replacing the fixed effects estimator with the mean group estimator,
- changing the set of endogenous variables:
  - replacing GDP with the economic sentiment indicator, replacing WIBOR 1M with the policy rate, WIBOR ON or WIBOR 3M,
  - adding CPI, REER, ROA, the share of impaired loans, a measure of liquidity (liquid assets-liabilities ratio) or mean risk weight,
  - removing aggregate variables,
- lowering the number of lags to 2,
- changing hyperparameter values: for the autoregressive coefficient to 0 or 1, for overall tightness to 0.1, for lag decay to 2, for exogenous variable tightness to 100 or infinity,
- taking variables in levels or log-levels (instead of first differences or first log-differences), shortening lag length to 2 quarters at the same time,
- shortening the sample to start in 2009Q1, marking the first change in the first above-mentioned measure of capital regulations.

As an extension, I attempt to identify non-linearities in the effects of changes in capital regulations. I compare them for groups of banks of equal number. I divide banks with respect to: initial (for 2008Q4) regulatory capital ratio, initial (also for 2008Q4) ROA, the share of loans in assets, the composition of loans (shares of consumer loans, loans for house purchases, and loans to sole proprietors and non-financial corporations), the share of domestic currency loans and size, as measured by the share in aggregate bank lending. Then, I estimate the parameters of BPVAR models using the fixed effects estimator, compute impulse response functions and compare them using the two-sample t-test.

4 Results

This section presents the results of the main analysis. The estimated effects of capital regulation and capital shocks are discussed for the two analysed minimum regulatory capital ratios in turn.

In figures 4-5 median responses to capital regulation and capital impulses are presented, with 95% confidence intervals.[9] In this case, the minimum regulatory capital ratio related to the 'Act on macroprudential supervision...' is used as the measure of capital regulations. 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. After an increase in the minimum regulatory capital ratio there is no statistically significant response of either of the remaining bank-level variables – the actual regulatory capital ratio or bank lending. The response of GDP is of a counterintuitive sign in the horizon it is statistically significant. That is, it is positive. Monetary policy remains passive.

After an increase in the actual regulatory capital ratio exogenous to capital regulations, on the other hand, there is an increase in both bank lending and GDP. Capital regulations remain passive and the response of the measure of monetary policy is negligible.

For results on the effects of capital regulations, as measured by the minimum regulatory capital ratio related to the 'Act on macroprudential supervision...', there appear to be two likely explanations. 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).

Figures 6-7 also present responses to capital regulation and capital impulses. However, here as the measure of capital regulations the dividend policy minimum regulatory capital ratio is used. After a capital regulation shock there is a decrease in bank lending and GDP. The maximum effect on quarterly bank lending growth is -0.40 p.p. (horizon 5). This translates into a maximum effect on annual bank lending growth of -1.30 p.p. (horizon 8) and an effect on the volume of loans after 20 quarters of 2.05%. The effect accumulates from -0.51% after 5 quarters and -1.71% after 10 quarters. The scale of the effect on GDP from bank-level bank panel data models is difficult to interpret.[10]. The response of the actual regulatory capital ratio is statistically insigni-

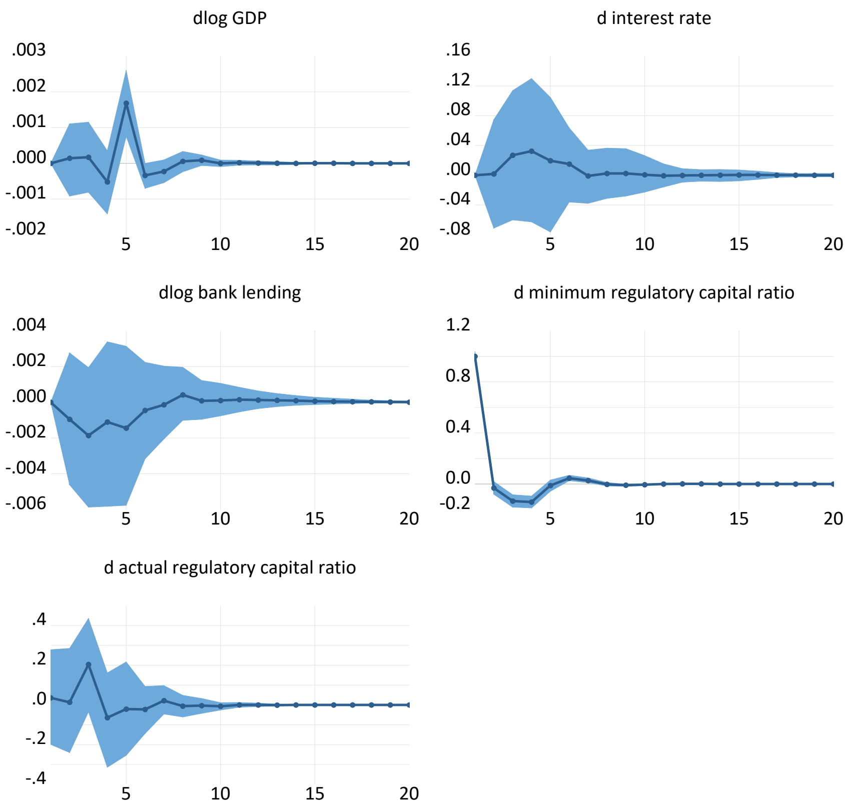
[9] For accumulated impulse responses, see the Online Appendix.

[10] The first issue is the likely heterogeneity of parameters in the GDP equation between banks, related to differences in their size. This is not allowed by the estimator used. However, perhaps surprisingly, this does not appear to affect the estimates of responses of bank-level variables (as reflected in results from sensitivity analysis, where a model without aggregate variables was considered; see next section).

ficant. This could be due to the heterogeneity of responses between banks, however, widening confidence intervals. The median impulse response function is positive. The response of the measure of monetary policy is negligible. It appears to reflect more the environment of changes in capital regulations than monetary-macroprudential policies interactions.

Responses to capital shocks, on the other hand, are qualitatively similar to those based on the model with the first measure of capital regulations – an increase in bank lending and GDP, no (or negligible) response of capital regulations and a negligible response of the measure of monetary policy.

Figure 4: Responses to capital regulation impulse (measure 1)



The second one is the high correlation between estimated structural capital regulation shocks between banks. A simulation based on the model from section 2 extended with a GDP equation (not reported in the article, available on request) shows that the higher the correlation, the more the impulse functions reflect the effects of an aggregate shock, rather than of a bank-level shock. To address these issues, the parameters of a global vector autoregressive model (analogously specified) were estimated; see the Online Appendix. As this model does not exploit the panel structure of the data, confidence intervals were wide. According to the sum of mean responses to a generalised impulse to the minimum regulatory capital ratio of each bank in the sample, the maximum effect on quarterly GDP growth is -0.19 p.p. (horizon 5), translating into a -0.44 p.p. effect on annual GDP growth and an effect of the volume of GDP after 20 quarters of -0.49% (a maximum response of -0.57%, but then a rebound). Dividing by the share of the sample in the population (in terms of the volume of loans), that would be -0.28 p.p., -0.67 p.p. and -0.74% (max -0.86%) for the population, respectively.

Figure 5: Responses to capital impulse (model with measure 1 of capital regulations)

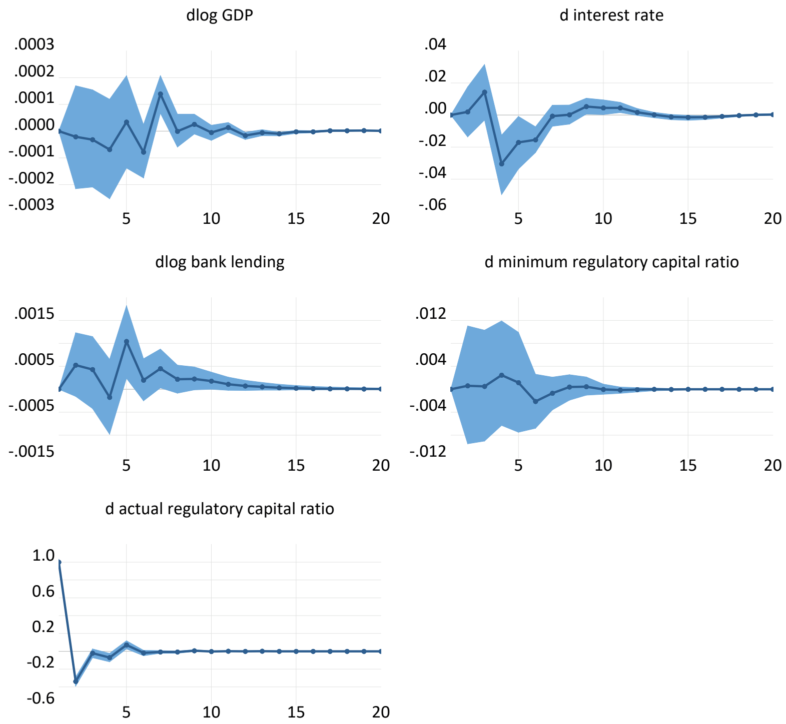


Figure 6: Responses to capital regulation impulse (measure 2)

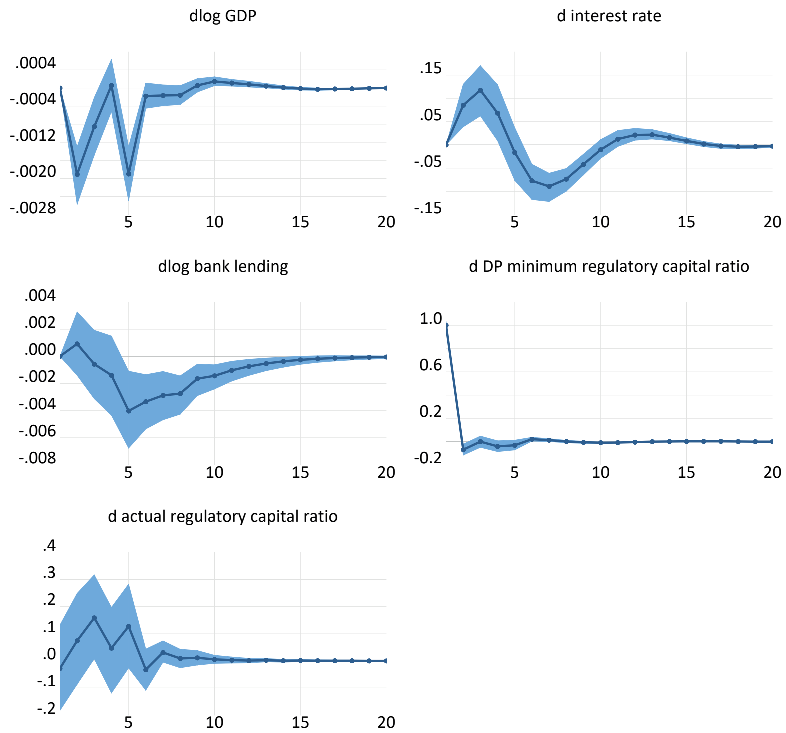
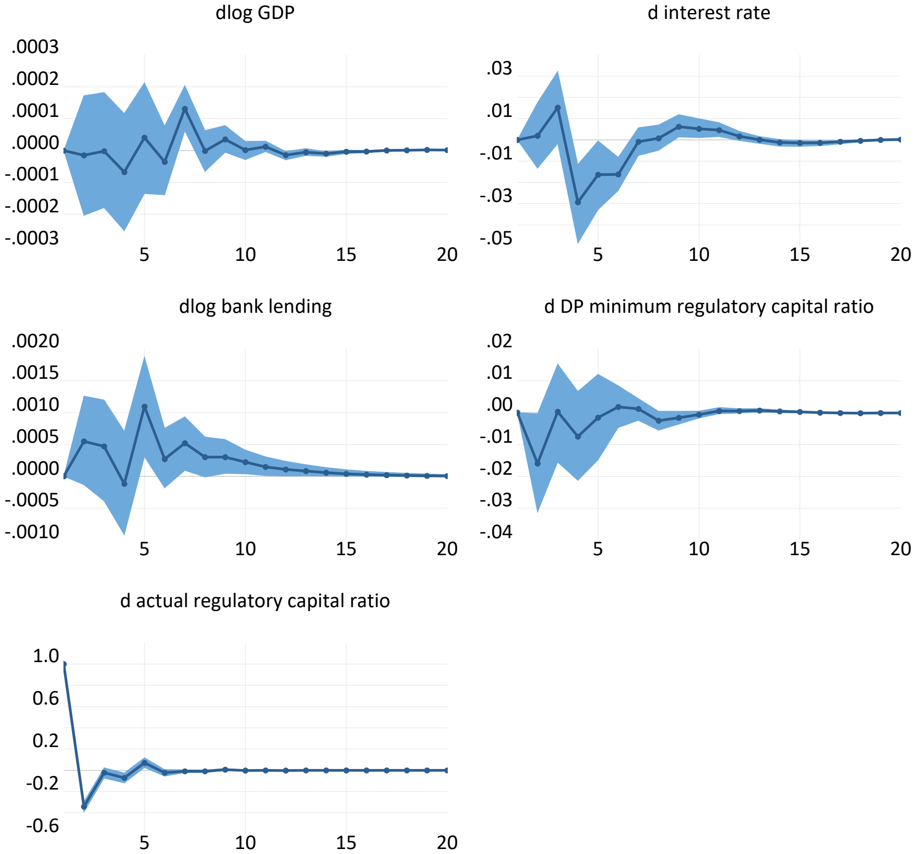


Figure 7: Responses to capital impulse (model with measure 2 of capital regulations)



5 Sensitivity analysis and extensions

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

Impulse response functions from the sensitivity analysis are presented in the Online Appendix. They focus on the effects of capital regulation shocks, based on the measure of capital regulations which turned out to be statistically significant for bank lending and GDP (i.e. the dividend policy minimum regulatory capital ratio). This also concerns extensions.

Taking into account the above-mentioned difficulties in interpreting the scale of the effect on GDP, let us focus on bank lending responses; that said, the GDP response remains statistically significant and negative in each relevant case. The use of the mean group estimator makes the impulse response function less volatile (a lower peak and a higher trough). However, the difference is small, supporting the use of the more efficient fixed effects estimator. Note, confidence intervals based on mean group estimation are narrower – in fact, extremely narrow – but they take into account the dispersion in bank point impulse responses only.

After replacing GDP with the economic sentiment indicator, the median response of bank lending to capital regulation shocks is weaker, and the upper bound is at, rather than below zero (even though the response of the economic sentiment indicator is statistically significant and negative – but the effect appears later). The use of measures of monetary policy other than WIBOR 1M has negligible effects on median impulse response functions and no effects on statistical significance. Neither does the addition of other endogenous variables. After removing aggregate variables, the effect of capital regulation shocks appears stronger at the beginning and weaker later on, with no effect on statistical significance.

The lowering of the number of lags makes the response statistically insignificant. An effectively similar change – increasing the hyperparameter value for lag length – keeps it significant, but (in terms of the median impulse response function) weaker. This shows the importance of using a sufficient number of lags, rather than invalidating the baseline results. As far as other changes in hyperparameter values are concerned, only decreasing it for overall tightness makes a visible difference (a weaker effect).

After making it comparable, the response from the model using (log-)levels of variables is similar. So is the response from the model based on a shorter sample, which appears to support the use of a longer one, so that a larger number of observations is used to estimate the effects of capital shocks and the bias related to the application of the fixed effects estimator to dynamic panel data models is limited. In both cases, the identified effect (in terms of the median impulse response function) is slightly weaker. The are no consequences for statistical significance. Generally, the sensitivity analysis does not show the results to be driven by choices related to the specification of models (or, in others words, they appear to be robust).

Moving on to extensions, figures 8-16 compare responses to capital regulation impulses between groups of banks, divided according to their characteristics.[11] In the first columns there are responses for banks with a relatively high value of respective characteristics. In the second columns – with a relatively low value. The third columns present the results of the two-sample t-test. In this case bands denote critical values.

In each case there are some statistically significant differences in impulse response functions, at least for some variables and some horizons. For example, the response of bank lending is weaker for banks with a higher initial actual regulatory capital ratio at the beginning, then stronger, and then weaker again. There are also differences in responses of other variables. Everything being endogenous, to some extent this is a cause, and in some cases a result of differences in bank lending responses.

Importantly, for each group the median impulse response function for bank lending and capital regulation shocks is negative; this also holds for GDP. There are differences in statistical significance. However, to some extent this is due to the smaller number of observations than using the entire sample.

Perhaps surprisingly, no systematic differences in GDP responses between small and large banks were found. But this likely results from the issues described in footnote 10.

Unfortunately (for the purpose of this exercise), the bank characteristics are correlated.[12] For example, banks with a higher initial actual regulatory capital ratio are also more profitable and smaller. Therefore, the source of the differences cannot be determined. A strategy controlling for the remaining characteristics would require more data – either a larger number of banks (and, therefore, impulse response functions) or/and the use of models with time-varying parameters. Another strategy could be based on univariate panel data models and interaction terms, with the ability to adequately separate capital regulation shocks from capital shocks at risk, however. This is left for future research.[13]

[11] For accumulated impulse responses and the results of the two-sample t-test based on them, see the Online Appendix.

[12] For correlation matrices, see the Online Appendix.

[13] Another sensitivity analysis/extension carried out was to optimize the start of the sample so that data for the largest number of banks with continuous observations could be used, within reasonable bounds. Starting in 2004Q2 (or 2004Q3, after first differencing) brought another 9 cross-sections. Supplementing the original 18 cross-sections with them did not bring qualitative changes to the results. Also, other bank groupings were considered: according to the liquidity position, the share of capital funding, the share of deposit funding, the share of market-based funding, ownership (foreign banks and remaining, public banks and remaining) and being affiliating, listed, specialised or cooperative, or not. These results are not reported in the article, but are available on request.

Figure 8: Responses to capital regulation impulse (measure 2)

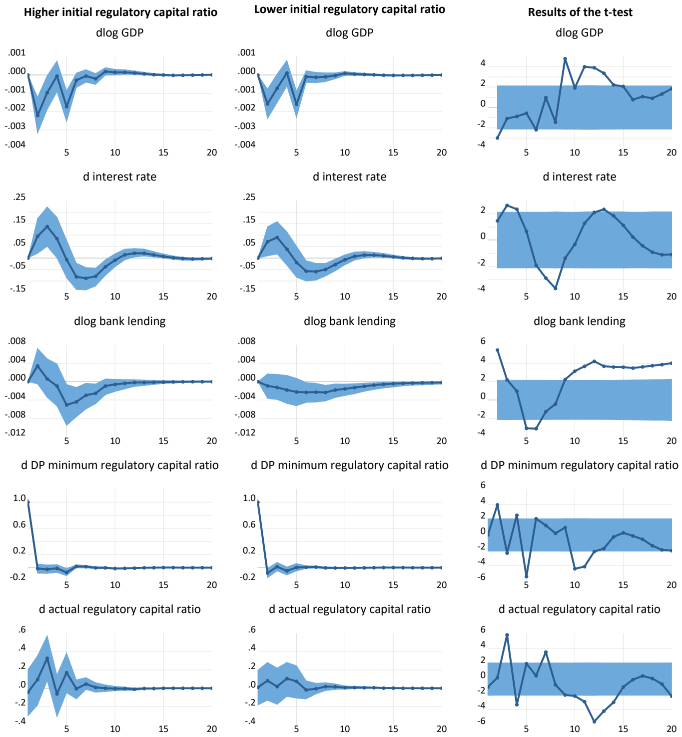


Figure 9: Responses to capital regulation impulse (measure 2)

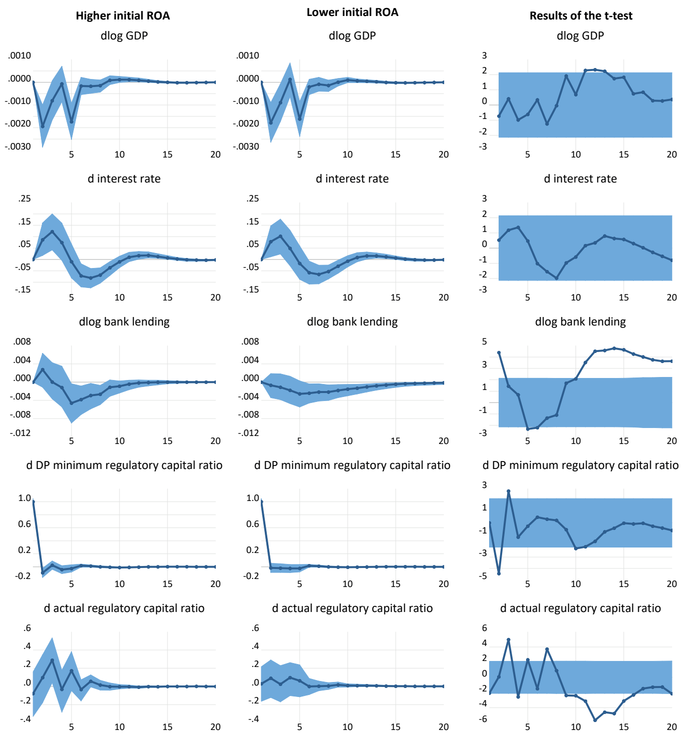


Figure 10: Responses to capital regulation impulse (measure 2)

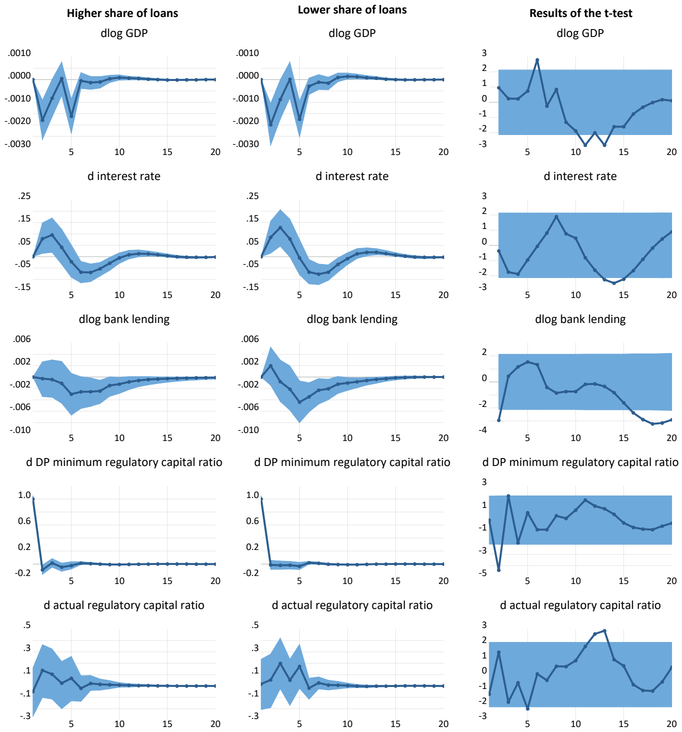


Figure 11: Responses to capital regulation impulse (measure 2)

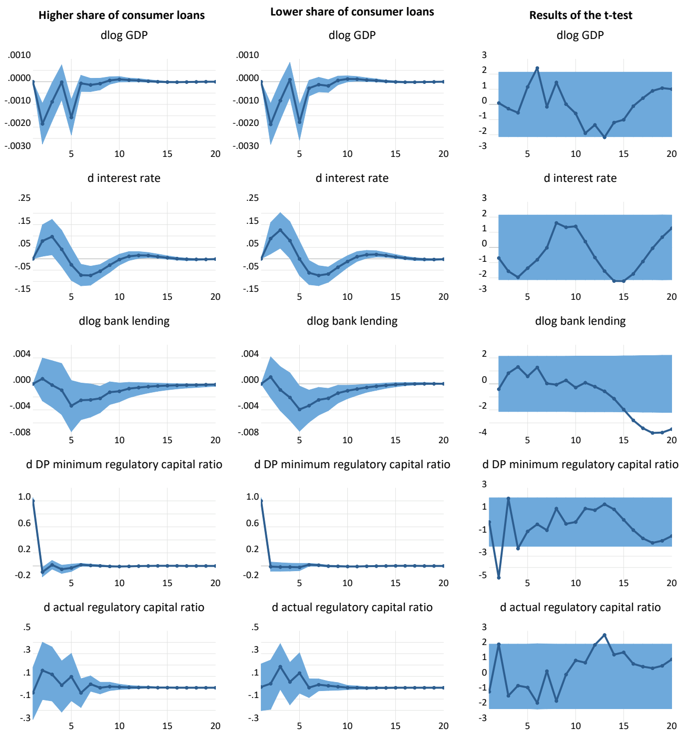


Figure 12: Responses to capital regulation impulse (measure 2)

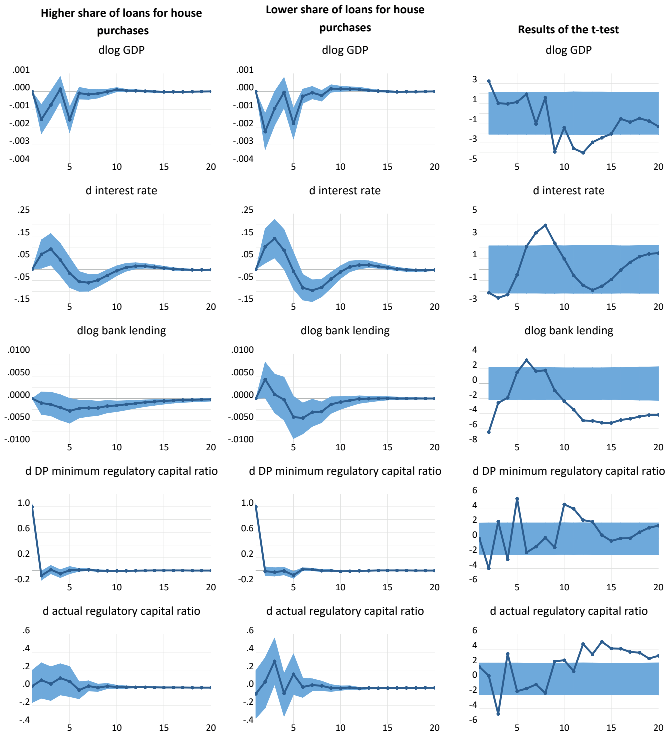


Figure 13: Responses to capital regulation impulse (measure 2)

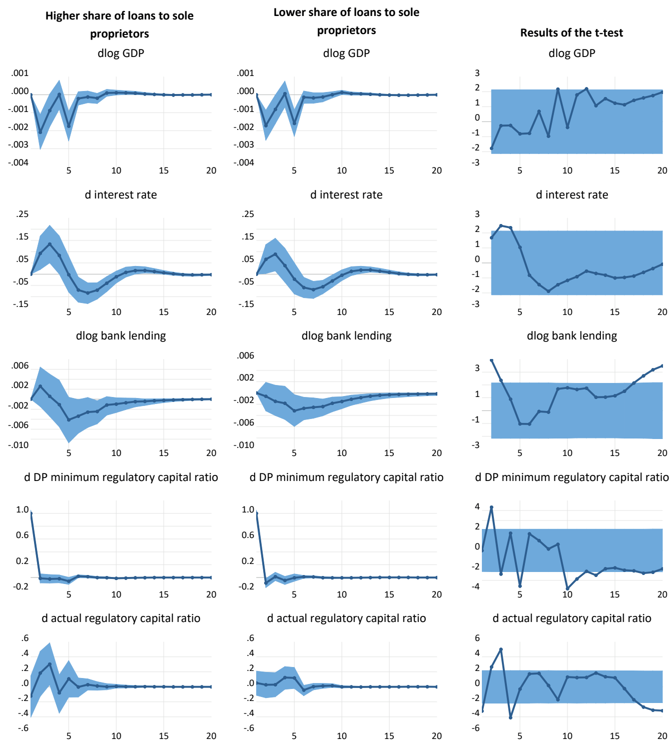


Figure 14: Responses to capital regulation impulse (measure 2)

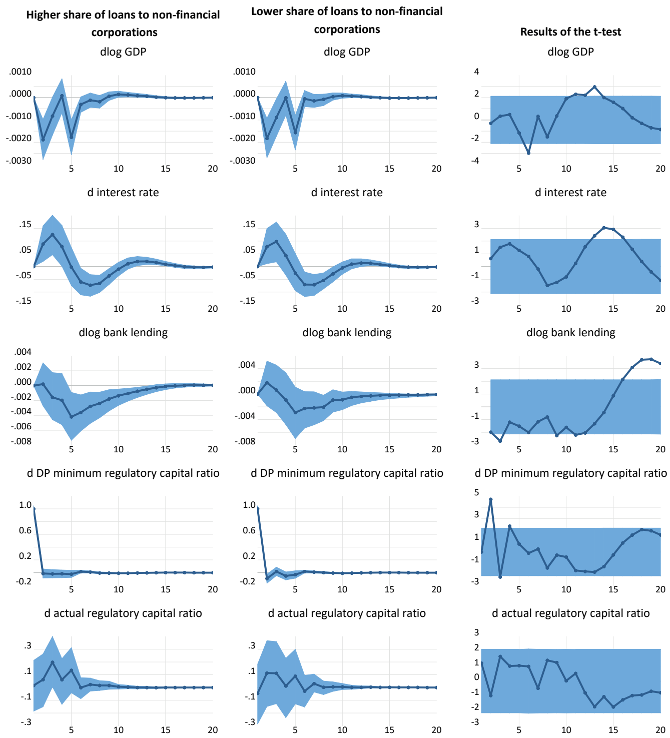


Figure 15: Responses to capital regulation impulse (measure 2)

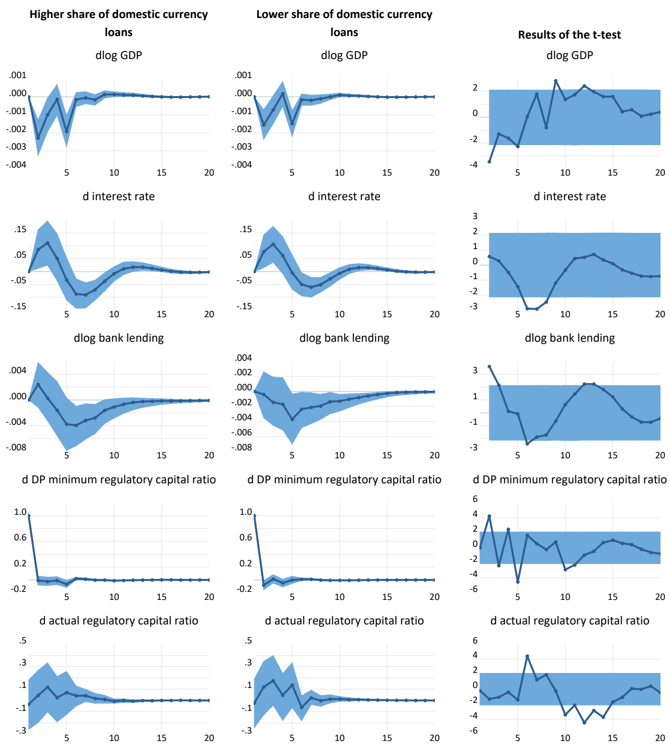
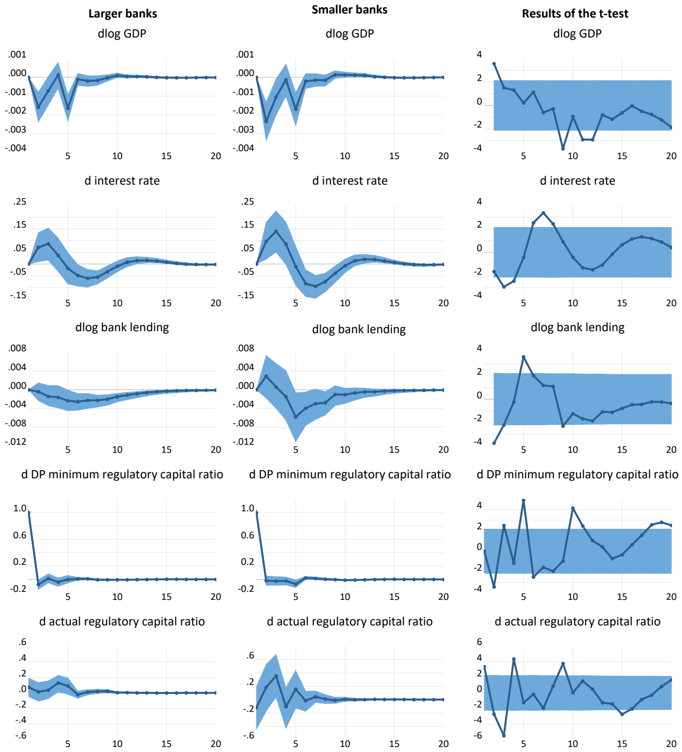


Figure 16: Responses to capital regulation impulse (measure 2)



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 and GDP 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. Some differences in the responses of groups of banks were found. However, with bank characteristics being correlated, their interpretation remains ambiguous.

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 2.05% on average, with spillovers to GDP. The effect accumulates from -0.51% after 5 quarters and -1.71% 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 non-linearities could be further explored, as well as the responses of other variables (for example, rates on loans or dividend pay-out ratios). 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).

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