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Effects of Macroprudential Policies on Bank Lending and Credit Risks

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Abstract - I analyse the effects of two macroprudential policy measures implemented in Switzerland: the activation of the countercyclical capital buffer (CCyB) and a cap on the loan-to-value (LTV) ratios. I use a difference-in-differences method to estimate the effects of these measures on risk indicators, such as their LTV and loan-to-income (LTI) ratios and mortgage growth rates.

I find that both the CCyB and the LTV cap led to a reduction in high LTV mortgages. The banks affected by the CCyB also reduced their mortgage growth rates. I do not find any evidence that these measures had unintended consequences on LTI risks or on non-mortgage credit growth.

JEL Classification: E5, G21, G28

Keywords: banks, countercyclical capital buffer, financial stability, loan-to-value ratio, macroprudential policy, mortgages

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1. Introduction

In the aftermath of the last global financial crisis, policymakers recognised the need for macro-prudential policy tools (Arnold et al. 2012, Claessens et al., 2010; Claessens, 2015). Excessive credit cycles have often resulted in financial instabilities with substantial welfare costs. The aim of macroprudential policies is to protect the banking sector and the broader macroeconomy from the destabilising effects of excessive credit growth. In this paper, I provide evidence from two macroprudential measures implemented in Switzerland, namely, a cap on high loan-to-value (LTV) mortgages and a countercyclical capital buffer (CCyB). I find that these measures reduced LTV risks and mortgage growth rates without an increase in risks in other dimensions.

Switzerland is an interesting country to study the effects of macroprudential measures for three reasons. First, it was the first country to activate the CCyB in 2013, which was justified by concerns about imbalances in the domestic mortgage and real estate market (SNB 2014).[1] The CCyB was implemented as a key macroprudential instrument in the Basel III capital regulation (BCBS, 2017). Second, Switzerland is the only country with a *sectoral* CCyB, where the Swiss CCyB targets the mortgage market. A sectoral application of the CCyB has the potential advantages of having a more direct impact on the area of concern, stronger signalling power and smaller effects on the wider economy than the broad-based Basel III CCyB. Such sectoral tools may be useful because lending booms are often confined to a certain sector, such as the mortgage market (BCBS, 2018). Third, Switzerland belongs to the rare number of advanced economies with a large financial sector that implemented an LTV cap in mid-2012, a few months before the CCyB activation in February 2013. Thus, it serves as a case study to analyse the interaction of macroprudential tools.

While both the LTV cap and the sectoral CCyB share a similar objective, namely, to lean against the build-up of systemic risks in the mortgage market, their transmission mechanism is different. The LTV cap targets the LTV distribution of new loans. Mortgage borrowers have to make a down-payment of at least 10% “hard” equity (meaning that the money cannot come from their pension account) when financing a new property. However, it is not a hard cap, i.e., banks can choose to issue mortgages with LTVs over 90% at the cost of holding more capital.

The CCyB is an additional capital requirement on all outstanding mortgage loans, but it does not directly restrict any mortgage characteristics. The CCyB requires banks to build-up capital according to their residential mortgage related risk-weighted assets (RWA). Banks can react to this additional capital requirement in different ways; i.e., they can increase the level of capital (the numerator of the capital ratio) and/or they can shrink their RWA (the denominator of the capital ratio). The former directly increases the loss-absorbing capacity of the banking sector. The latter leans against the build-up of imbalances through two transmission channels. In the first channel, banks reduce mortgage lending because the cost of providing residential mortgages relative to other types of credit increases. In particular, if the banks’ capital situation is tight, imposing additional requirements on already capital constrained banks limits their ability to provide residential mortgages (Drehmann and Gambacorta, 2011). In the second channel, banks reduce their residential mortgage related RWAs by reducing the risks associated with residential mortgage lending. In particular, banks reduce their LTV ratios because they are the key determinants for mortgage related RWAs in the capital regulation.

Because the macroprudential measures were implemented almost contemporaneously and had similar intended effects, disentangling their effects is not straightforward. In this paper, I exploit differences in timing and between banks to estimate the policies' respective effects. I use a difference-in-differences (DiD) estimator and simultaneously include the different measures. The CCyB treatment group consists of capital constrained banks with a high share of residential mortgage related risk-weighted assets. The LTV treatment group consists of banks that issued a substantial share of their new mortgages with an LTV of over 90% before the cap was introduced. The DiD implementation ensures that my results regarding the CCyB are not driven by the LTV cap implemented beforehand. Moreover, I control for bank and time fixed effects to rule out a potential bias due to unobservable bank characteristics or economic conditions.

My analysis exploits a rich bank panel dataset containing various outcome variables of interest. Among them are credit risk indicators (measured by the share of high LTV and high loan-to-income (LTI) mortgages), mortgage growth rates, and the growth rates of other loans. My sample spans 24 quarters (2011Q2-2017Q1) for the LTV and LTI risks and 34 quarters (2008Q4-2017Q1) for the credit growth rates. It includes the 25 largest mortgage banks covering almost 90% of the Swiss mortgage market. These banks report data to the Swiss National Bank through quantitative surveys. I combine the data on their LTV and LTI risks with balance sheet and supervisory data.

My results are that new mortgages with high LTVs were significantly reduced in the groups that were most exposed to the LTV cap and the CCyB, compared to their respective control groups. The measures affected different parts of the upper LTV distribution, as follows: the LTV cap of 90% reduced new mortgages with LTV ratios over 90%; the CCyB reduced new mortgages with LTV ratios between 80% and 90% at the expense of an increase in new mortgages with LTV ratios between 66% and 80%. The banks affected by the CCyB also reduced their mortgage growth rates. These results suggest that the macroprudential measures exhibited the intended effects. The banks chose to comply with the LTV cap instead of issuing mortgages above the cap at the cost of holding more capital (which would have been possible according to regulation but was not desired by regulators). The CCyB contributed to lean against the build-up of risks; i.e., the banks affected by the CCyB reduced mortgage growth and new mortgages with LTV ratios of more than 80% (which receive a higher risk-weight according to the standardised approach of the capital regulation). At the same time, I do not find direct evidence that the measures had any unintended effects. Compared to the banks in the control group, the treated banks did not increase their LTI risks or change non-mortgage lending as a compensation. In the banking system, however, the measures did not prevent the aggregate LTI risks from increasing.

This paper adds to the growing literature on the effectiveness of macroprudential instruments and their interactions. Most of the existing literature focuses on LTV restrictions, which have primarily been implemented in emerging market economies. These studies suggest that LTV caps are effective in reducing mortgage and house price growth (see Akinci and Olmstead-Rumsey, 2018; Allen et al., 2017; Cerutti et al., 2017; Igan and Kang, 2011; Lim et al., 2011; Morgan et al., 2019; and Wong et al., 2011).

My analysis of the LTV cap is most similar to the work of Acharya et al. (2019). Using a difference-in-differences approach, they estimate the effect of LTV and LTI limits on mortgages issued by Irish banks on credit and risk taking. Although these limits were quantitatively important, they affected neither credit volumes nor the LTV or LTI distribution in the aggregate banking system. Instead, the banks reallocated credit to achieve the same risk exposure under the new constraints. For instance, richer households increased their LTV/LTI ratios (within the lending limits) by buying more expensive properties. The Irish case illustrates policymakers' problems with regulatory arbitrage.

For the *sectoral CCyB in Switzerland*, two other studies exist. Basten (2019) – building on Basten and Koch (2015) – uses data from an online platform to examine how the CCyB affects mortgage pricing. He finds that banks with low capital cushions and high mortgage/asset ratios raise their mortgage rates relatively more than their competitors, but he does not find evidence that these banks raise prices more for high LTV loans. Auer and Ongena (2019) study whether the CCyB affects corporate loans that are not subject to the CCyB. They find that banks with high shares of residential mortgage related RWAs over assets report more corporate loans and increase interest rates and commissions for them. They conclude that the sectoral CCyB has caused extra lending in another adjacent sector. When analysing the possible reasons for the differences between my results and the results of the two other studies, I find that the differences in the outcome measurement and treatment definition are relevant, as further discussed in Section 4.2

This paper advances the literature in three respects. First, I disentangle the effects of the LTV cap and the CCyB. In contrast, the other two studies on the Swiss CCyB do not mention the LTV cap that was implemented in July 2012 and became fully binding in December 2012. Given that the CCyB was activated in February 2013 and some banks were affected by both measures, it is important to know how much of the effects should be assigned to each measure. Second, I have a more comprehensive data set. I observe the actual mortgage characteristics of signed mortgage contracts for a longer sample period and a larger sample. Moreover, I estimate the effects for a broader set of outcome variables, i.e., growth rates of mortgages and non-mortgage loans and different credit risk indicators of new mortgage loans. The credit risk indicators are most relevant because credit growth alone may mask important variations in the riskiness of new lending. Third, I refine the measurement of the banks' exposure to the policy. I show that it is crucial to set the additional capital requirement due to the CCyB in proportion to the bank's existing excess capital. Out of two banks with the same mortgage/assets or residential mortgages RWA/asset ratios, the more capital constrained bank is more affected by the CCyB.

The remainder of this paper is structured as follows. The next section provides some background on the economic environment and the different macroprudential measures in Switzerland. Section 3 describes the conceptual framework, the data and the empirical strategy. Section 4 provides the results, while the last section concludes.

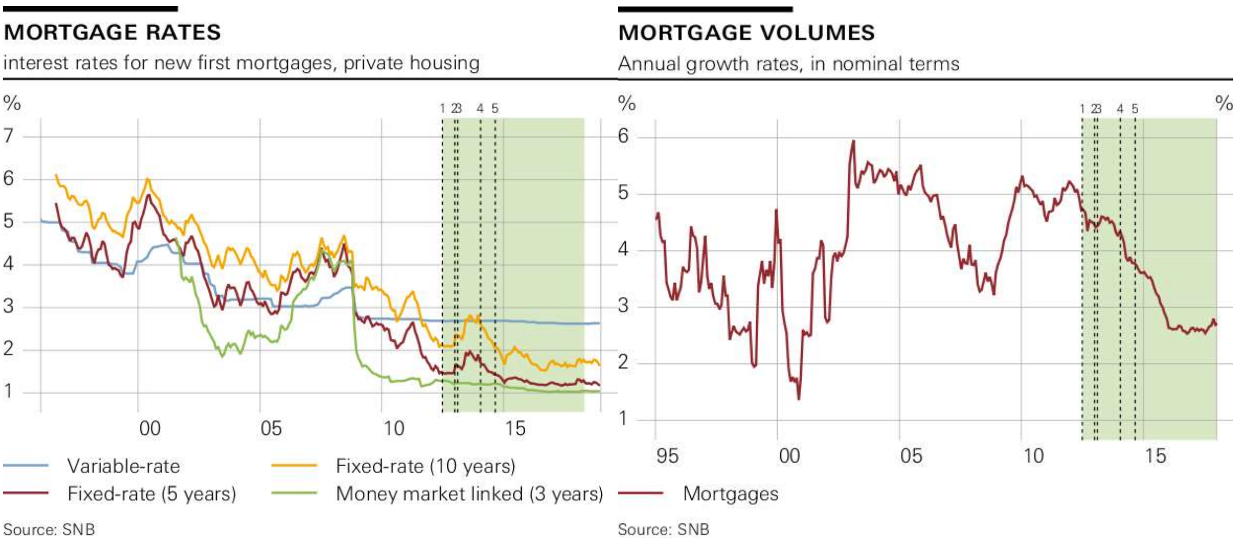
2. Background

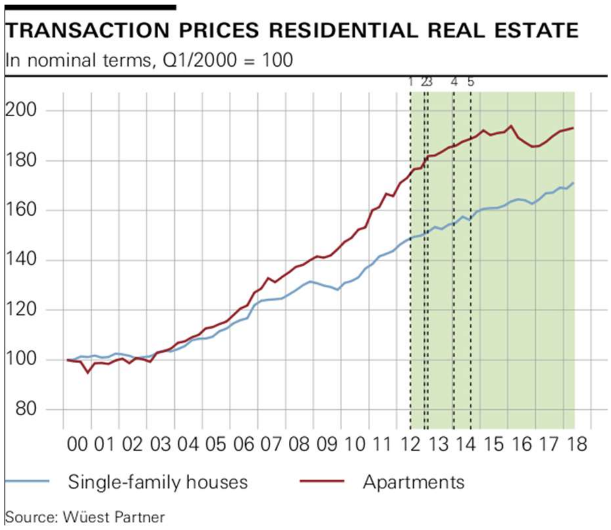
2.1. Economic environment and Swiss banking sector

As a reaction to the last global financial crisis, the Swiss National Bank (SNB) significantly lowered its target range for the three-month Libor in autumn 2008, introducing a prolonged phase of exceptionally low interest rates. Thus, mortgage rates dropped substantially. For instance, the interest rate of a popular mortgage product in Switzerland, namely, a mortgage loan with a fixed interest rate for five years maturity, declined from 3.9% in September 2008 to 1.4% in September 2012 (see first panel in Figure 1).

The persistently low interest rates induced an increase in mortgage lending volumes and real estate prices. The second panel of Figure 1 illustrates the strong increase in mortgage growth from 3.5% in September 2008 to approximately 5% one year later. The third panel shows the rapid rise in residential real estate prices; i.e., the transaction prices for single family houses rose by 15% and those for apartments rose by 28% between September 2008 and September 2012.

Figure 1: Mortgage interest rates, mortgage growth rates, real estate prices





Note: The dotted vertical lines show the dates when I expect the earliest effect of the following measures (discussed in detail in the next section): the first revision of the self-regulation (1), the increased risk-weight for the high LTV loan tranche (2), the activation of the CCyB (3), the increase in the CCyB (4) and the second revision of the self-regulation (5).

The Swiss banking sector is especially exposed to risks in the mortgage sector. Mortgages constitute the most important asset for many Swiss banks; i.e., the average mortgage share is almost 70% for the 23 domestically focused banks in my sample.[2] Moreover, the mortgage risk remains on the banks' balance sheets, as there is no securitisation.[3] Concerned about the accumulation of risks in the mortgage and real estate market, the SNB has issued repeated warnings since 2010.

[2] Beutler, Bichsel, Bruhin and Danton (2015), Dietrich (2016), Vujanovic (2016) and Auer, Ganarin and Towbin (2018) provide a further description of the Swiss banking and housing sector.

[3] Swiss covered bonds serve refinancing purposes, but no risk transfer takes place.

2.2. Macroprudential measures

To address the risks in the mortgage and real estate markets, Swiss policymakers have implemented different macroprudential measures. The most important measures were an LTV cap in 2012 and the activation and increase of a countercyclical capital buffer in 2013 and 2014.

Loan-To-Value ratio cap

In 2012, an LTV cap was introduced within the self-regulation of the Swiss Bankers' Association. A down-payment of at least 10% "hard" equity was required for financing a new residential property. "Hard" equity means own funds without pension savings. Thus, mortgage borrowers were no longer allowed to finance their down-payment by relying exclusively on their pension savings. Using pension savings as part of a down-payment is a widespread practice in Switzerland (Seiler, 2013) and is explicitly supported by the Swiss law.

However, the LTV cap was not a "strict" ban. Because of competition concerns, banks were given the choice between issuing a new mortgage loan (i) with an LTV less than 90% within the normal risk-weighting scheme or (ii) with an LTV above 90% but applying a 100% risk-weight for the entire mortgage loan. The 100% risk-weight is a considerable increase compared to the average risk-weight of approximately 40% for a typical residential mortgage loan.[4] Thus, banks have strong incentives to issue mortgages below the cap if they want to contain their

[4] Under the standardised approach, a mortgage tranche with LTV ratio <66% has a risk-weight of 35%, one with LTV ratio between 66%- 80% has a risk-weight of 75% and one with LTV ratio >80% has a risk-weight of 100%. For illustration purposes, let us consider a loan close to 900,000 CHF for a house with value of 1,000,000 CHF. For a loan of 899,900, the residential RWA is 433,332 (the average risk-weight is 48%), while for a loan of 900,001, the residential RWA is 900,001 (the average risk-weight is 100%).

RWA; nevertheless, other considerations (e.g., winning new customers) could still lead them to issue loans with LTV ratios above 90%.

The LTV cap was announced in June and became effective in July. Banks were given a transition period of five months, i.e., until the end of November 2012, when they could issue mortgages with LTV ratios over 90% without applying a 100% risk-weight. I expect the earliest reaction by banks to be in the third quarter of 2012. This corresponds to the quarter where most of the banks in my sample reported a revision in their guidelines in a qualitative survey.

Countercyclical Capital Buffer

The CCyB has been part of the international macroprudential toolkit since 2016. It requires banks to accumulate capital as imbalances in the credit market develop (Basel III, 2011). Depending on the degree of systemic risk, a countercyclical capital requirement between zero and 2.5% of risk-weighted assets is put in place. It is increased when the systemic risks rise; it is released when the systemic risks materialise or dissipate. According to Basel III, the CCyB is applied on a broad basis, i.e., to total risk-weighted assets (RWA).

In Switzerland, the CCyB targets exposures in the residential mortgage sector, i.e., temporarily raises capital requirements for mortgage loans on residential property. The Swiss authorities chose this sectoral variant of the CCyB (although the broad CCyB has been available to them since 2012), because they were concerned about the risks in the mortgage market. The intended effects of the CCyB are twofold, as follows: (i) to increase the resilience of the banking sector by increasing its loss-absorbing capacity, and (ii) to lean against the build-up of excessive credit growth by limiting the potential for lending, given the current capital available (SNB 2014).[5]

The CCyB activation was announced in February 2013 by the Federal Council. Then, it was set to a level of 1% of the residential mortgage associated risk-weighted position, with a deadline of compliance at end of September 2013. I consider the quarter of the announcement to be the quarter when I expect the earliest effect; i.e., banks will start the lengthy adjustment process immediately. In 2014, the level was increased to 2%. The increase was announced in January 2014, with a compliance deadline at the end of June 2014. In March 2020, the CCyB was released to support banks in their key role as lenders in the coronavirus crisis.

The CCyB increased the overall capital requirements in the banking system somewhat. For the 25 largest mortgage banks in my sample, the additional required capital due to the CCyB activation was 3% of their total minimum capital requirement. There was considerable heterogeneity among these banks; i.e., the CCyB ratio required capital over the total minimum required capital, which ranged from 1% to 8%, depending on the relative importance of residential mortgage related RWA. Moreover, these banks differed considerably with respect to their capital buffers; i.e., some banks had almost no excess capital relative to regulatory requirements, while many had a capital/RWA ratio more than four percentage points higher than their bank specific target ratio. I will exploit the heterogeneity between the banks to identify the effects of the CCyB.

[5] Risk-sensitive capital requirements may contribute to cyclicality of credit supply; higher capital requirements in booms may contain excessive lending. Jokivuolle et al. (2014) provide another rationale for countercyclical risk-sensitive capital requirements: they may reduce market failure (overinvestment in risky projects) in the presence of adverse selection.

Other policy measures

Swiss policymakers also implemented other policy measures between 2012 and 2014. The most important ones are summarised in Table 1. One policy measure was an increase in the risk-weight for residential mortgage lending; i.e., the risk-weight for the tranche exceeding the 80% LTV ratio was increased from 75% to 100%. Because the higher risk-weight only applies to tranches, i.e., only to the part of the mortgage that actually exceeds the threshold, and because most of the outstanding mortgages have an LTV below the 80% threshold, I expect its effect to be small. Nevertheless, it coincides with the activation of the CCyB and amplifies its impact on the minimum capital requirements.

Table 1: Overview of the policy measures implemented

Quarter (earliest effect)	**Measure**	**Description**	**Intended effect on lending, risk**	**announced**	**effective**
2012Q3	1st revision of the self-regulation	10% "hard" equity down-payment	mortgage growth ↓, LTV↓	1.6.2012	1.7.2012, with a five-month transition period
2013Q1	Increased risk-weight	for loan tranche exceeding LTV of 80%	LTV ↓	1.6.2012	1.1.2013
2013Q1	Activation of the CCyB	1% of residential mortgage related RWA	mortgage growth↓, LTV↓	13.2.2013	30.9.2013
2014Q1	Increase in the CCyB	2% of residential mortgage related RWA	mortgage growth↓, LTV↓	23.1.2014	30.6.2014
2014Q3	2nd revision of the self-regulation	Linear and yearly amortisation requirement	mortgage growth ↓	24.6.2014	1.7.2014, with a five-month transition period

Another policy measure was an amortisation requirement introduced in the revised self-regulation in 2014.[6] New mortgage borrowers were required to amortise their loans to an LTV of 2/3 within the next 15 years, where they should amortise yearly and linearly. Because I do not have the data to assess the effect of this requirement, I only control for it by including a time dummy variable in my estimation.

3. Methods and data

3.1. Conceptual framework

Both the LTV cap and the CCyB share a similar broad objective, namely, to lean against an accumulation of systemic risk in the mortgage market. However, their transmission channels are different. The LTV cap targets the upper part of the LTV distribution of new mortgages. However, banks can choose whether they reduce new mortgages with LTV ratios >90% or issue them at higher capital cost. The CCyB applies to outstanding and new mortgages and directly increases their capital costs. The banks can react to the associated capital costs in different ways. I discuss my hypotheses regarding the effects of the two measures in succession.

Regarding the LTV cap, an effect on the right tail of the LTV distribution is most likely. Banks will reduce new mortgages with LTV ratios >90% at the threshold to avoid much higher costs in terms of capital. However, further away from the threshold, the bank's optimal choice might

[6] An earlier version of the amortisation requirement was implemented in the first revision of the self-regulation, where mortgage borrowers were asked to amortise their loan to an LTV of 2/3 within the next 20 years. However, this version was not binding, because the amortisation amount and schedule were looser than the banks' practice.

be different. For instance, acquiring new customers and expanding profit margins might outweigh the increased capital cost. The Irish example discussed above illustrates the bank's incentive to increase risk taking once an LTV cap is implemented. In addition to the banks' decisions, the borrowing decisions of first time buyers are relevant, because they are the group that is most likely to take out a mortgage with an LTV over 90%. Depending on their time preferences, financially constrained first time buyers can either buy cheaper houses or postpone their purchase until their savings are sufficient.

The CCyB acts as a lending restriction to capital constrained banks, but it does not directly impose any restrictions on mortgage growth or characteristics. Following Gambacorta and Mistrulli (2004), banks hold capital in excess of capital requirements, either because they face capital adjustment costs or they want to convey positive information regarding their economic value. Banks are different with respect to their chosen excess capital; some banks find it optimal to operate close to the regulatory intervention threshold, while others operate far away from it. Some possible reasons for this bank heterogeneity are the differences in equity costs, private costs of bankruptcy or risk aversion.

The activation of or an increase in the CCyB implies a higher regulatory capital requirement and, hence, a shock to the chosen excess capital. Capital constrained banks with a high share of residential mortgages in their portfolio are hit harder by the additional capital requirement. Thus, I expect to observe a reaction by those banks. Other banks hardly need to react given their large amounts of excess capital and the temporary nature of the CCyB requirement.

In principle, banks can choose different strategies to restore their chosen excess capital. They can either increase their capital and/or reduce their RWA. Because issuing equity is costly and lengthy, banks are likely to reduce their RWA. This can be achieved by cutting back the mortgage lending or risks determining the risk-weighting of mortgages.

In addition to reducing their mortgage volumes, I expect banks to reduce their RWA by adjusting their LTV distributions. The reason is that the LTV ratio is a key determinant for mortgage related risk-weighted assets under both the standardised approach (SA) and the internal model based (IRB) approach to capital regulation.[7] As 22 banks in my sample apply the SA, I expect that the LTV thresholds used in this approach (66% and 80%) are relevant (see footnote 4).

However, both measures could also have unintended consequences (Jiménez et al., 2017). They put pressure on the bank's profitability opportunities. To maintain their profitability, the banks might want to increase risks in other dimensions to compensate. In particular, they could increase their LTI risks (which are not regulated under the SA) or switch to riskier types of credit than mortgage loans. For instance, Norwegian and UK banks reacted to an increased capital requirement by originating riskier loans (Juelsrud and Wold, 2018; Uluc and Wieladek, 2018), while Irish banks reacted to LTV and LTI limits at home by increasing their risks abroad (McCann and O'Toole, 2019).

Finally, spillover effects could arise within the banking system from treated to control banks or outside the banking system from regulated banks to other sources of finance, e.g., insurers or pension funds, (Aiyar et al., 2014; Kim et al., 2018) or from regulated banks at home to bank lending abroad (Buch and Goldberg, 2017). The market share of non-bank mortgages accounts for only approximately 5% and has been stable in recent years. Hence, I am more concerned

[7] Besides the effects on LTVs, there could be an effect on LTIs that are a key determinant for banks using the IRB approach. However, only 3 banks in my sample apply the IRB approach. Hence, I am less likely to observe changes in the LTI distribution.

about spillover effects within the banking system than leakages to non-banks. With the exception of the two big banks, the Swiss banks in my sample have little international exposure, thus limiting the scope of spillovers for lending abroad.[8]

3.2. Data and descriptive statistics

My sample consists of the 25 largest mortgage banks in Switzerland. Their mortgages account for almost 90% of the mortgage market. These banks report information on new residential mortgage loans in a quarterly survey conducted by the SNB (2011Q2-2017Q1). New mortgage loans comprise newly granted loans for the construction or purchase of real estate and refinancing of an existing mortgage from another lender. Banks report information on two key risk indicators for owner-occupied residential mortgage loans, i.e., LTV and LTI. The respective numerators are the credit limits of the outstanding mortgage loans. The value in the LTV denominator is the market value of the pledged property.[9] The income in the LTI denominator is the borrower's net employment or pension income.

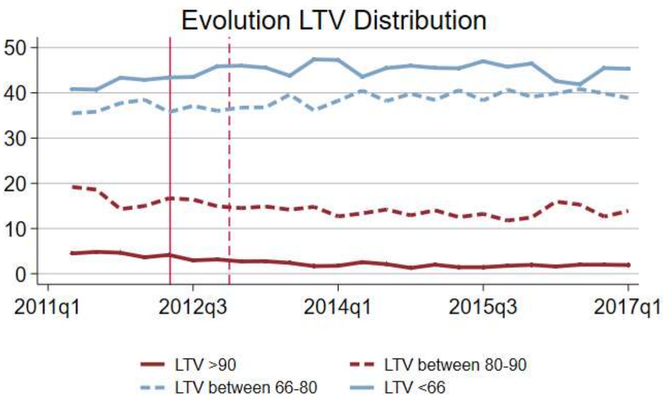
As outcome variables, I examine the shares of new residential mortgages with high LTV ratios because these are the high risk mortgages. For instance, banks that provide many mortgages with LTV ratios of over 90% run the risk of having a high proportion of underwater mortgages in their portfolio should real estate prices fall by 10%. I define additional LTV buckets according to the LTV thresholds under the standardised approach, i.e., 66% and 80% (see footnote 4).

[8] There is also no evidence that foreign policies affect domestic lending (Auer and al., 2017).

[9] Banks report the market values of houses according to a standardised definition. Therefore, it should not depend on how conservative a bank is in the valuation process of the house. The banks did not change their market value reporting behaviour during the sample period. In particular, the distribution of the market value did not change for my treatment groups after treatment; i.e., if I run my main regression on selected descriptive statistics of the market value distribution, I do not find any statistically significant effects

The overall LTV distribution was shifted to the left towards lower risks (see Figure 2). In the beginning of the sample period, the share of new mortgages with LTV ratios of more than 90% was approximately 5% for the banks in the sample. It declined to 2% at the end of the sample period. Similarly, the share of new mortgages with LTV ratios between 80% and 90% declined from approximately 20% to 14%. The share of new mortgages with LTV ratios between 66% and 80% increased from 36% to 39%, and the corresponding share with LTV ratios less than 66% increased from 41% to 45%.

Figure 2: Average share of new mortgages by LTV bucket for 25 banks



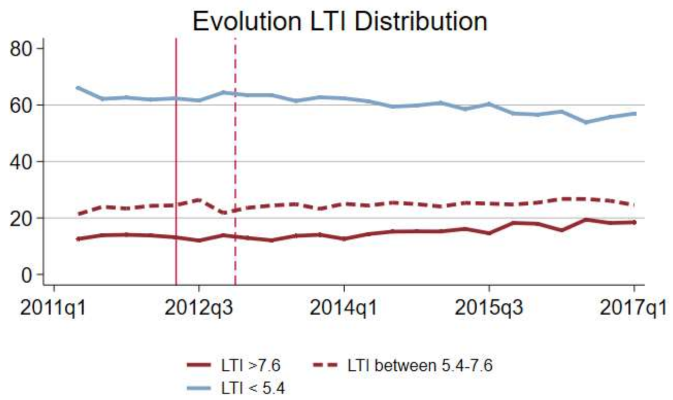
Note: The vertical lines show the dates when I expect the earliest effect of the LTV cap and the activation of the CCyB.

Similarly, I look at high LTI ratios as indicators of affordability risks. I consider an LTI ratio as high if the imputed cost of the mortgage comprising the imputed interest rate (5%) plus the maintenance and amortisation costs (1% each) exceed one-third of the gross wage or pension income. Many Swiss banks claimed to determine the long term affordability of a loan by a similar “golden rule”; the average mortgage rate between 1960 and 2008 was approximately

5%, which corresponds to a situation in which the total mortgage loan is more than 5.4 times higher than the borrower's net income. An LTI ratio of more than 7.6 is considered *very* high (imputed costs are more than one-third of the gross income at an interest rate of 3%).

Figure 3 shows that the shares of new mortgages with very high and high LTI ratios have increased over the sample period, while the shares of mortgages with low LTI ratios have declined. Thus, for the banking system, the affordability risks have increased over the sample period.

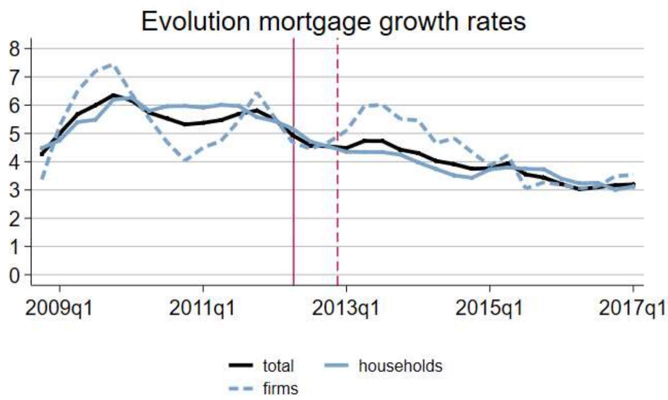
Figure 3: Average share of new mortgages by LTI bucket for 25 banks



Note: The vertical lines show the dates when I expect the earliest effect of the LTV cap and the activation of the CCyB.

Additionally, I am interested in whether the macroprudential measures had an impact on lending volumes. I restrict my sample of lending growth rates to the beginning of the low interest rate phase end-2008. The average growth rates exhibit cyclical pattern and declined over the sample period. Because most mortgages are held by households, the household and total mortgages exhibit similar growth rates. The mortgage growth rates to firms are more volatile. Finally, I look at the yearly growth rate of other (non-mortgage) loans to firms to analyse whether there have been any unintended spillover effects. They also declined over the sample period.

Figure 4: Average credit growth rates for 25 banks



Note: The vertical lines show the dates when I expect the earliest effect of the LTV cap and the activation of the CCyB.

3.3. *Empirical strategy*

My aim is to estimate the causal effects of the policy tools on the outcome variable, for example, the effect of the LTV cap or the CCyB on the LTV distribution. My problem is that I only observe the actual outcome (the LTV distribution after the LTV cap was imposed) but not the counterfactual outcome (the LTV distribution if the LTV cap had not been imposed). Furthermore, the policy intervention was not assigned at random to individual banks. For instance, banks extending high LTV mortgages were more likely to be affected by the LTV cap. Similarly, mortgage specialised and capital constrained banks were more likely to be affected by the CCyB.

The difference-in-differences (DiD) is a popular identification strategy when a policy intervention does not affect every individual at the same time and in the same way. The idea is to compare the average outcomes of two groups before and after the policy intervention, where only one group is affected by the intervention. Two assumptions must be satisfied. First, the average outcomes in both groups do not change in anticipation of later treatment. Second, the average outcomes in both groups are subject to a common time trend that is conditional on the covariates. Under these assumptions, one can estimate the counterfactual outcome in the treatment group by subtracting the changes in the outcomes in the control group.

I estimate the DiD effect in a linear setting. One advantage of the linear specification is that the effects of the different measures can easily be estimated simultaneously. Another advantage is that it allows a treatment variable with a differing "treatment intensity", which will be relevant for the measurement of the CCyB. In particular, I estimate the following equation:

$$y_{it} = \beta_1 LTV * T_{2012Q} \; + \beta_2 CCyB * T_{2013Q} \; + \gamma B + \delta T + \epsilon_{it} \qquad (1)$$

where y_{it} is an outcome variable of interest; *LTV* is a dummy variable indicating whether a bank was affected by the LTV cap; *CCyB* is a dummy variable indicating whether a bank was affected by the activation of the CCyB; the time dummies T_{2012Q3}, T_{2013Q1} are 0 before the quarter when the respective earliest effect is expected, and 1 otherwise; and B and T are vector control variables consisting of bank dummy and time dummy fixed effects. The interactions of the time dummies and the respective measure capture the treatment effects. In robustness checks, I will use continuous treatment intensity variables instead of the dummy specification. For a recent review on the DiD method, see Lechner (2011).

My sample size (N = 25, T = 24) is given by the coverage of the mortgage survey that is used to define the treatment and control groups (see Section 3.3). A small number of N will reduce the statistical power. To obtain the correct inference, I take into account that I have only a few clusters. If banks within the same treatment group are subject to common shocks, their outcomes might be correlated. This correlation in their outcomes might mistakenly be interpreted as the treatment effect. To illustrate this, suppose there was a positive correlation within clusters. Then, each bank would contribute less to the statistical efficiency compared to a case where there were independent observations. This would result in over-rejections of the null hypothesis of no treatment effect. If there are many clusters, a cluster-robust variance estimator in the spirit of White (1980) can be used. If there are only a few clusters, Cameron, Gelbach, and Miller (2008) suggest using bootstrap procedures. When comparing different bootstrap procedures, they find that the wild cluster bootstrap procedure does especially well, which is a cluster generalisation of the wild bootstrap for heteroskedastic models (Wu, 1986). It has been often been applied in cases where the number of clusters was smaller than the 25 (banks, in my case) (see Behncke, 2012; Brown et al., 2009).

Definition of treatment groups

To establish whether a bank was affected by the LTV cap or not, I use a data driven definition. I examine the average share of new mortgages with LTV ratios over 90% before the self-regulation and define the 12 banks where this share was higher than 5% as the treatment group (LTV

=1), because these banks were more likely to be affected by the revision. Table 4 in the Appendix shows the descriptive statistics for the continuous and dummy LTV treatment variables.

To establish whether a bank was affected by the activation of the CCyB, I rely on supervisory capital data. I measure the CCyB treatment intensity as the required CCyB capital relative to the bank's excess capital, i.e., CCyB required capital/(actual capital – target capital). To avoid endogeneity problems, I use the predetermined excess capital end-2012.[10]

For illustration purposes, let us consider four stylised Swiss banks that are different with respect to their excess capital and residential mortgage risk taking. They have 30 CHF bn assets, 21 bn residential mortgages, 15 bn RWA and their regulatory target capital/RWA is 12. The two well capitalised banks have 2.46 bn in actual capital (corresponding to a capital/RWA of 16.4); the two tightly capitalised banks have 1.86 bn in actual capital (corresponding to a capital/RWA of 12.4). Two of the banks have relatively low-risk mortgage portfolios, with residential mortgage RWA (RRWA) values of 7.5 bn each (corresponding to an average risk-weight of 0.36), while two others have riskier mortgages, with RRWA values of 10.5 bn each (corresponding to an average risk-weight of 0.50).

Table 2: CCyB treatment intensity for four stylised Swiss banks (CCyB: 1% of RRWA)

	RRWA in CHF bn	RRWA/ mortgages	Actual capital/ RWA	CCyB required capital in CHF bn	excess capital in CHF bn	CCyB Intensity
well capitalised, low mortgage risk	7.5	0.36	16.4	0.075	0.66	0.11
well capitalised, high mortgage risk	10.5	0.50	16.4	0.105	0.66	0.16
tightly capitalised, low mortgage risk	7.5	0.36	12.4	0.075	0.06	1.25
tightly capitalised, high mortgage risk	10.5	0.50	12.4	0.105	0.06	1.75

10 If I use end-2011 values to define treatment intensity, the results remain qualitatively similar but are less precisely estimated due to the measurement error introduced.

Table 2 shows each bank's CCyB treatment intensity as the ratio of CCyB required capital over excess capital, where the CCyB is set to 1% of the RRWA. This CCyB treatment intensity variable implies a stronger CCyB effect for banks (i) with higher residential mortgage risk and (ii) with smaller excess capital.

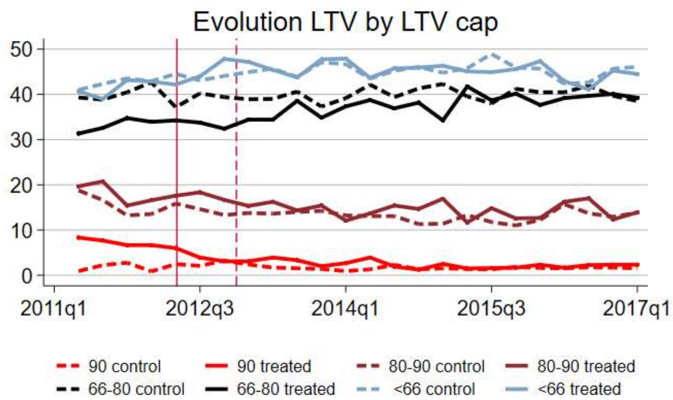
The actual distribution of the CCyB treatment intensity variable is shown in Table 4 in the Appendix. It was 0.12 at the median bank (similar to the numbers for the two well capitalised stylised banks). For the purpose of defining my treatment group, I consider banks to be affected by the CCyB activation if their CCyB treatment intensity is more than 0.4 (corresponding to the 80th percentile). Accordingly, four banks are affected by the CCyB activation. This small number reflects the fact that most of the banks in my sample are well capitalised and have low risk-weights for residential mortgages (the average RRWA/mortgage is 0.34, see Table 4). In robustness checks, I will also consider the continuous treatment intensity and a dummy, where I use the 60th percentile. We also calculate the treatment intensity for the CCyB increase in 2014. However, the treatment intensity variables are highly correlated with each other (coefficient of 0.8) because only one year passed between the activation and the increase. Due to this multicollinearity, I do not have sufficient statistical power to separately identify the effects of the CCyB increase.

Common trends (conditional on covariates)

The identification strategy depends on the assumption that the average outcomes in the treatment and control groups exhibit the same time trends, conditional on the covariates. The covariates should be selected such that they capture all the variables that lead to different time trends but are not influenced by the treatment. In most specifications, we will use bank and time dummy fixed effects to capture all the factors that might lead to different lending behaviours.

Figures 5-7 show the unconditional average outcomes for which I find significant effects, i.e., LTV risks and mortgage growth rates. The solid line shows the average outcomes in the respective treatment, and the dotted line shows the average outcomes in the control group. Figure 5 shows that the average share of mortgages with LTV ratios >90% was approximately 7% for the 12 banks in the LTV treatment group before the LTV cap was announced. For the 13 banks in the control group, the respective share was lower but also relatively flat. Since mid-2012 the share declined in the treatment group and remained stable in the control group. The evolution (and not the level) of the unconditional average outcomes before the treatment started is important. I test this formally by running a regression of the pretreatment interactions on the corresponding shares. With the exception of the second quarter 2012, I find no significant differences in trends across groups in the pretreatment period.

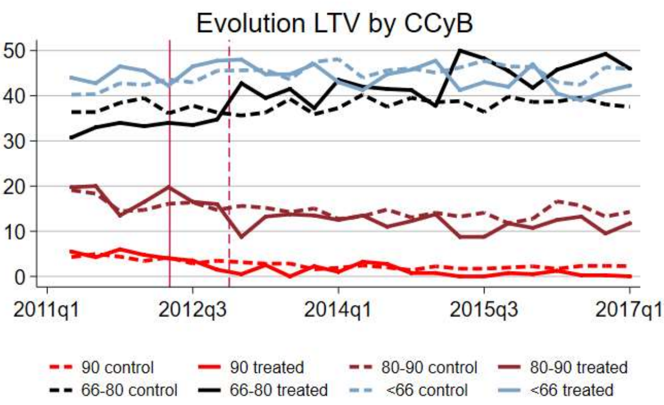
Figure 5: unconditional average LTV outcomes by LTV cap



Note: The vertical lines show the dates when I expect the earliest effect of the LTV cap and the activation of the CCyB.

Figure 6 shows the average LTV outcomes for the four banks in the CCyB treatment and the 21 in the control group. Because three banks in the CCyB treatment group also belong to the LTV treatment group, the evolution of trends is confounded after the LTV cap is announced. Until mid-2012, the LTV shares evolve similarly in both groups. I also test the common trend assumption formally and find no evidence for a violation.

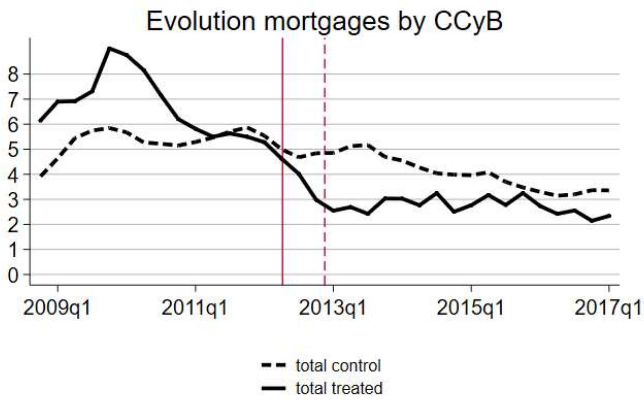
Figure 6: unconditional average LTV outcomes by CCyB activation



Note: The vertical lines show the dates when I expect the earliest effect of the LTV cap and the activation of the CCyB.

Figure 7 shows the average mortgage growth rates in the CCyB treatment and control groups. In the CCyB treatment group, the average mortgage growth increased until end-2009 and fell afterwards. The second decline, starting in mid-2012, is again confounded by the introduction of the LTV cap. The cycle in the CCyB control group until mid-2012 evolves similarly but with a smaller amplitude. Since the cycles are not strictly parallel, it is important to control for co-variates affecting common trends; I include bank and time dummy fixed effects. Moreover, I run placebo treatment regressions.

Figure 7: unconditional average growth rates by CCyB activation



Note: The vertical lines show the dates when I expect the earliest effect of the LTV cap and the activation of the CCyB.

4. Results

4.1. Main results

LTV ratio cap

I find that the LTV cap significantly affects the LTV distribution. The share of mortgages with LTV ratios of more than 90% is 4.4 percentage points smaller for banks in the LTV treatment group compared to their controls after the LTV cap has been implemented (see first row, first column in Table 3). These 12 banks had, on average, shares of mortgages with LTV ratios over 90% of 7.1% before and 2.5% after the implementation of the LTV cap and, hence, experienced a decline of 4.6 percentage points. The 13 banks in the control group experienced an average decline of 0.1 percentage points (from 1.8% to 1.7%). Thus, the introduction of the LTV cap of 90% was effective in reducing high LTVs. Banks chose to reduce LTV risks (as intended by policymakers) instead of maintaining them at the cost of higher capital (which would have been possible according to the regulation). The reduction of high LTV mortgages in the treatment group is material, i.e., two thirds of the initial share.

Except from the upper part, the other parts of the LTV distribution were not affected at conventional significance levels. This suggests that there was no strong substitution from the upper LTV part to a certain bucket in the lower part. In particular, there is no significant increase in the share of mortgages with LTV ratios between 80% and 90%. While some first-time buyers might have switched from the over 90% bucket to the just below 90% bucket, this substitution was not a widespread phenomenon.

I do not find any statistically significant effects on mortgage growth rates. Thus, the LTV cap reduced the risk density of mortgages without contracting mortgage lending. This finding is consistent with two mutually non-exclusive explanations, as follows: (i) first time buyers had sufficient funds to provide the “hard” equity but would have preferred to use their pension savings, and (ii) first-time buyers bought cheaper houses or postponed their house purchase, but banks compensated for their declined demand trough other channels (e.g., increasing renewal loans or renovation loans).[11]

I do not find any other statistically significant effects. The banks did not increase their LTI risks to compensate for the reduced LTV risk. They also did not change other types of credit lending. From a financial stability perspective, one can view this result as positive. The LTV risk density of new mortgages was reduced without any unintended consequences on LTI or other credit risks.

[11] There is some evidence supporting the postponement hypothesis, i.e., the number of credits to mortgage customers with only one house (who are more likely to be first-time buyers) declined, but this effect is only significant at the 10% level.

Table 3 Main coefficients of interest

	with LTV				with LTI			Mortgage growth			Other credit
	>90%	80%-90%	66%-80%	<66%	>7.6	5.4-7.6	<5.4	total	private	firms	firms
LTV*T2012Q3	-4.37***	-0.27	2.35	2.28	-0.69	-1.61	2.29	-0.79	-0.59	-0.81	0.12
	(0.00)	(0.89)	(0.24)	(0.13)	(0.72)	(0.37)	(0.49)	(0.28)	(0.48)	(0.59)	(0.97)
CCyB* T2013Q1	-0.44	-3.50**	8.88***	-4.95*	-3.54	-0.08	3.61	-2.04***	-2.53**	-1.29	1.64
	(0.45)	(0.04)	(0.00)	(0.08)	(0.32)	(0.86)	(0.32)	(0.00)	(0.02)	(0.13)	(0.33)
Observations	600	600	600	600	575	575	575	850	850	850	850
R-squared	0.55	0.57	0.44	0.67	0.61	0.53	0.71	0.60	0.62	0.50	0.38

Notes: This table shows the DiD regression results for 11 dependent variables: the share of new mortgages with LTV ratios >90%, LTV ratios between 80% and 90%, 66% and 80% and LTV ratios <66%, the share of new mortgages with LTI ratios (>7.6), LTI ratios between 5.4-7.6, and LTI ratios <5.4, yearly growth rates of domestic mortgages, mortgages to households and firms and other credit growth to firms. *LTV* and *CCyB* measure whether a bank was in the LTV or CCyB treatment group. The respective DiD effects are the interactions of these variables with the relevant time dummies (2012Q3 and 2013Q1). Further controls include bank and quarter time fixed effects. P-values (in parentheses) are determined by a wild cluster bootstrap (clustered with respect to each bank) with ***,**,* denoting significance at the 1%, 5% and 10% levels, respectively.

CCyB activation

The CCyB treatment effects are shown in the second row of Table 3. The LTV distribution was significantly affected, as follows: mortgages with LTVs between 80% and 90% declined, mortgages with LTVs between 66% and 80% increased and mortgages with LTVs smaller than 66% declined.[12] Thus, the LTV distribution was compressed, which resulted in more probability mass in the middle. Quantitatively, and from a risk perspective most important, though, is that high (80%-90% and >80%) LTV bucket was reduced.

[12] I find similar effects if I consider different LTV buckets, i.e., over 80% (instead of 80%-90%), 70%-80% (instead of 66%-80%).

Regarding the 80%-90% LTV bucket, the banks in the CCyB treatment group had an average share of 17.4% before and 11.7% after the CCyB activation. Thus, their unconditional average share declined by 5.7 percentage points. Controlling for confounding factors, this results in a 3.5 percentage point decline (see second row, second column in Table 3). This effect is sizeable; it corresponds to a fifth of their initial share. For banks in the CCyB control group, this share only declined slightly (from 16.2% to 14.1% before and after 2013Q1), as Figure 6 illustrates. The banks in the CCyB treatment group extended fewer mortgages with LTV ratios of more than 80%, because they have higher risk-weights under the standardised approach of capital regulation.

This reduction occurred at the expense of an increase of the mortgage share, with LTV ratios between 66% and 80%. The coefficient of 8.8 corresponds to an increase of approximately one quarter of the initial share in the CCyB treatment group. In the control group, this share did not change after the CCyB activation was announced.

There is also evidence that the banks in the CCyB treatment group slightly reduced the share of mortgages with LTV ratios under 66%. However, this effect is only significant at the 10% level and not in every robustness check, which suggests that the increase of the 66%-80% LTV bucket stems from the reductions of both the 80%-90% LTV and the <66% LTV buckets.

Moreover, the banks in the CCyB group reduced their mortgage growth more than the banks in the control group. The total mortgage growth, household and firm mortgage growth declined by approximately two percentage points. Again, these reductions are substantial because they correspond to more than 30% of their average growth rates before the CCyB activation. However, the effects on the growth rates are less significant than the effects on the LTV distribution.

Moreover, there is effect heterogeneity (which I will discuss in the next section), i.e., not every bank in the CCyB treatment group reduced its mortgage growth rate.

I do not find any evidence that banks in the CCyB group increase their LTI risks or changed their other credit lending. From a financial stability perspective, I consider my results as positive; i.e., the CCyB contributed to reducing high LTV risks and – somewhat weaker – mortgage growth without resulting in unintended consequences regarding other risks.

4.2. Robustness checks

Treatment intensity

In a first robustness check, I analyse whether the treatment intensity matters for the reaction to the treatment. In particular, banks with higher treatment intensity might react more strongly to the treatment. To test this presumption, I use the continuous CCyB treatment intensity variable instead of a dummy in one specification, i.e., the ratio of CCyB required capital over excess capital before the CCyB activation was announced. Similarly, I use a continuous LTV treatment intensity, i.e., the average share of mortgages with LTV ratios >90% before the LTV cap was announced.

Table 5 in the Appendix shows that the effects of the LTV cap on the LTV distribution are barely affected by continuous treatment definition; again, I only find a significant effect for the over 90% LTV bucket. For the CCyB treatment intensity, I find similar effects on the LTV distribution, i.e., a reduction in the 80%-90% LTV bucket and an increase of the 66%-80% LTV bucket. In addition, the effect on the over 90% LTV bucket becomes significant. The continuous treatment indicator discriminates between both measures less sharply compared to the dummy indicator.

The effects of the CCyB treatment intensity variable on mortgage growth rates, though, are not statistically significant at conventional levels. One explanation is that there was no proportional relationship between the CCyB treatment intensity and a reduction in growth rates. Indeed, I find evidence for effect heterogeneity, which I will discuss next.

CCyB effect heterogeneity

Among the four banks in the CCyB treatment group, one bank materially exceeds the others with respect to its treatment intensity. In this robustness check, I estimate separate CCyB effects for this bank and define the CCyB treatment group as consisting of the other three banks. Table 6 in the Appendix illustrates the effect heterogeneity among the treated banks. For the three CCyB treatment banks, I find similar effects as in my main specification, i.e., the 80%- 90% LTV bucket is reduced, the 66%-80% LTV bucket is increased and the below 66% bucket is reduced. I also find significant effects on mortgage growth rates. The bank B with the highest treatment intensity reduced the 80%-90% bucket at the expense of an increase of the LTV under the 66% bucket. However, it did not significantly change its mortgage growth rates. I consider this as evidence that all the CCyB banks reduced their RWA by reducing high LTV mortgages, but not every CCyB bank reduced its mortgage growth rates.

Broader CCyB treatment definition

In my main specification, the CCyB treatment group consists of only four banks. This reflects the fact that many Swiss banks are well capitalised or residential RWAs are small compared to their overall RWA. However, drawing general conclusions if there are only a few banks affected by the CCyB may be a concern. I check whether my results would also hold if the CCyB treatment group is enlarged. More banks in the treatment group might increase the statistical precision if the additional banks are truly affected by the treatment. However, increasing the size of the treatment group can also result in bias if the additional banks belong to the control group. In my main specification, I use the 80th quantile (0.4) of the CCyB intensity variable to determine whether a bank is in the CCyB treatment group. In this robustness check, I use the 60th quantile (0.21). According to this definition, there are six banks in the CCyB treatment group. Table 7 in the Appendix shows the corresponding results. The estimates for the LTV cap are barely affected. For the CCyB treatment, I find that the 80%-90% LTV bucket was reduced at the expense of an increase of the 66%-80% bucket. Again, I find that CCyB treated banks reduced their mortgage growth rates. The coefficients are smaller and less significant than in my main specification. Adding banks with a lower treatment intensity to the treatment group reduces the size of the CCyB effects.

Comparison to other studies

In this section, I analyse why some of my findings differ from those in the existing work on the CCyB in Switzerland. Specifically, Auer and Ongena (2019) find that the CCyB activation resulted in an increase in commercial loans, while I find insignificant effects on non-mortgage lending to firms. In Basten (2019), there is no significant effect of the CCyB on the mortgage

LTV distribution, which is in contrast to the significant effects in this paper. My analysis differs in three respects from these studies, as follows: (i) the sample size and period, (ii) the measurement of the outcome variable and (iii) the definition of the CCyB treatment group.

Regarding the work by Auer and Ongena (2019), the differences in the sample size and period do not drive the different results; if I run my main regression with the 20 banks of the Auer-Ongena sample and/or restrict the sample period to be the same period as theirs (2012Q3-2013Q4), I find similar (non-)results to those reported in Table 3. Instead, the different effects on commercial loans are likely driven by how this outcome variable is measured; even if I use the Auer-Ongena treatment definition and their sample, I still find insignificant effects. This paper relies on the change in the stock of commercial loans reported in the balance sheet. Auer-Ongena measure the flow of new lending based on loan-level data. While preferable in principle, the latter is potentially problematic in the Swiss context due to measurement issues.[13]

Moreover, there are differences in the treatment definition; Auer-Ongena use the ratio of residential mortgage RWA over domestic assets to determine the CCyB exposure, without taking into account the bank's excess capital. This leads to the differences illustrated in Table 2; i.e., the well capitalised, high mortgage risk bank (second row) would be in my control but their treatment group, whereas the tightly capitalised, low mortgage risk bank (third row) would be in my treatment but their control group. While there is a positive rank correlation of 0.55 between my CCyB treatment intensity variable and theirs, using their definition would "re-classify" 13 banks in my sample from treatment to control or vice-versa. Given these differences, it is no surprise that the effects of the RRWA/asset ratio are not similar to the CCyB treatment

[13] In particular, the available dataset does not reliably distinguish between a true new loan and a change in conditions of an existing loan (see also Schelling and Towbin, 2019).

effects, i.e., most of the effects are insignificant. My interpretation is that the banks with higher residential RWA/asset ratios did not behave differently compared to banks with lower ratios, but this ratio is not a good proxy for being affected by the CCyB activation.

Comparing my results to Basten (2019), differences in the sample size and period do not drive differences in statistical significance; i.e., if I run my main regression with the 7 banks that are in both samples and/or restrict the sample period to be the same as his (2012Q3-2013Q3), the estimated effects on the LTV distribution and mortgage growth rates remain statistically significant.[14] Another possible driver is that the approach used to measure the CCyB exposure in my work is different than that used in Basten's work. However, using his approach results in a similar treatment group.[15] Hence, using his treatment definition does not materially affect the results in my sample. Instead, the difference in statistical significance could be driven by the precision with which the LTV ratio is measured: I observe the LTV distribution once the mortgage contract has been signed, while Basten (2019) observes LTVs at the time of the borrower request on an online platform.

Other covariates

In a next robustness test, I consider different covariates instead of bank and time dummy fixed effects. If I can observe all covariates that lead to different trends in the outcome variables, I

[14] There is one exception; i.e., the effect on new mortgages with LTV ratios between 80%-90% in the specification with 7 banks is no longer significant.

[15] Basten assumes that banks with below median capital cushions and above median mortgage/asset ratio are affected by the CCyB. Although his definition is different from mine, the treatment groups happen to be similar. Three of my four treated banks would also be in his treatment group. This overlap in treatment groups depends on a specific feature in his sample, i.e., banks have smaller capital cushions. Because their median value corresponds to the 20th percentile of my sample, it is informative for identifying tightly capitalised banks.

can increase the degrees of freedom by including them instead of fixed effects dummies in the regression. The covariates should be selected such that they capture all variables that lead to different time trends but are not influenced by the treatment. Depending on their risk profile, banks might respond differently to shocks to their mortgage portfolios. Hence, I control for variables affecting the evolution of mortgages and their risks (house price growth, interest rates, unemployment rate). Moreover, I control for the bank individual risk taking before the measures were introduced (e.g., average mortgage growth rate in the past years). It is more likely that the banks in the treatment and control groups experience similar time trends if they are more alike before the measures were implemented. Table 8 in the Appendix shows that the coefficients are very similar compared to my main specification. For the LTV cap, I find significant effects on the over 90% LTV bucket and for the CCyB activation on the 80%-90% and 66%-80% LTV buckets and on mortgage growth rates.

Short-, medium- and long-term effects

Next, I analyse how the treatment effects evolve over time. I estimate the short-, medium- and long-term effects of each treatment to see whether the banks react immediately and persistently. I consider six quarters after treatment as the short-term, seven to 12 quarters as the medium-term and everything afterwards as the long-term period. Table 9 in the Appendix shows the regression results. For every time period considered, I find a similar picture as in my main regression. Overall, the treatment effects are remarkably stable, suggesting a steady adjustment. The effects of the LTV cap on the over 90% LTV bucket are significant and the absolute coefficients slightly increase over time (from 4 to 4.7). The effects of the CCyB on the LTV distribution also increase over time, suggesting stronger effects in the long-term. The effects of the

CCyB on mortgage growth rates, however, decrease over time. CCyB affected banks adjust their mortgage growth immediately after the activation, but no long term effects are found.

Placebo treatment

In a final robustness check, I test the validity of the common trend assumption by estimating the effects of placebo treatments. In a pretreatment sample, I should not find any significant effects in the treatment group in the periods in which nothing happened. Because I can observe the LTV and LTI risks for only four quarters before the banks' reaction to the LTV cap, I can only examine the placebo treatment test for the mortgage growth rates. Table 10 in the Appendix shows that (with one exception)[16] I do not find significant effects in the CCyB treatment group assuming different placebo treatments between end-2009 to end-2010.

5. Conclusion

From a financial stability perspective, my results are encouraging. Both macroprudential measures contributed to lean against a further accumulation of mortgage risks without any apparent unintended consequences. They significantly reduced the LTV risks of residential mortgages by affecting different parts of the LTV distribution. The banks in the LTV treatment group reduced the share of new mortgages with LTV ratios >90% (and hence complied with the LTV cap). The banks in the CCyB treatment group reduced their mortgage share with LTV ratios >80% (that receive a higher risk-weight according to the standardised approach of capital

[16] The 2010Q4 placebo treatment is only significant at the 10% level for total mortgage growth, which might be a type I error.

regulation). Moreover, some of the banks affected by the CCyB reduced their mortgage RWAs by reducing their mortgage growth rates.

The size of the effect on the treated banks is substantial. Compared to the pretreatment period, treated banks considerably reduced high LTV mortgages and mortgage growth rates. The banks affected by the LTV cap reduced their over 90% LTV bucket by 4.4 percentage points. Similarly, the banks affected by the CCyB activation reduced the between 80%-90% LTV bucket by 3.5 percentage points. Some of them also reduced their mortgage rates by 2 percentage points.

Finding significant effects on the bank level is a first condition for the effectiveness of macroprudential policies in the banking system. A second condition is that the reduction in risk-taking by treated banks passes through the banking system. For a pass through on the banking system, it is not only relevant how *strong* the reaction by the treated banks was but also how *many* and *which* banks were affected by the treatment. Almost half of the banks in my sample were affected by the LTV cap but only 4 by the CCyB activation. The latter reflects the fact that banks in Switzerland are well capitalised and that the sectoral CCyB increased capital requirements only slightly. Hence, my CCyB estimates have the same direction, but a smaller magnitude compared to the simulation for the UK banking system by Noss and Toffano (2016). The descriptive statistics for the 25 banks in my sample show that both the over 90% and 80%-90% LTV buckets were reduced in the banking system.

To the extent that banks in the treatment and control groups reacted in the same way to the macroprudential policies implemented, this is not reflected in my estimates. The DiD only identifies effects that are different between banks and not those similar to all banks. Similarly, I do not capture any effects on the aggregate mortgage demand that macroprudential policies might have. This might be particularly important for the "communication" channel. The Swiss National Bank has issued repeated warnings about the risks in the domestic mortgage and real estate markets (see Financial Stability Report 2010-2019, various press releases and speeches). To the extent that all banks or mortgage borrowers reacted to the warnings by being more careful, I underestimate the potential benefits of the macroprudential measures. Indeed, I observe a decline in mortgage growth but can only assign a fraction of it to the activation of the CCyB.

In the banking system, the reduction in LTV risks was accompanied by an increase in LTI risks. On a microeconometric level, the banks in the LTV or CCyB treatment group did not increase their LTI risks to compensate for the reduced LTV risks more than the banks in the respective control group. However, with the exception of the CCyB treatment group, all the other bank groups increased their LTI risks over the sample period. The increase in LTI risk was certainly not caused by the macroprudential policies, but it was also not prevented.

I underestimate the potential benefits of the CCyB in two respects. First, I do not analyse the effect of the CCyB on the resilience in the banking system. Increased capital buffers enhance the survival probability of banks during banking crises (Berger and Bouwman, 2013). Higher capital might also be associated with greater lending because better capitalised banks face lower funding costs (Gambacorta and Shin, 2018). However, I cannot disentangle how much of the increase in capital can be contributed to the CCyB because many changes to the capital regulation were implemented prior to the CCyB, as well as contemporaneously. Second, because the CCyB has just recently been released, its effectiveness in supporting credit in downturns cannot be analysed yet.

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Appendix

Table 4: Descriptive statistics

	N	Mean	Std. Dev.	p10	p50	p90
outcome variables						
share of new mortgages with LTV>90%	600	2.55	3.09	0	1	7
LTV between 80%-90%	600	14.52	6.63	5	14	23
LTV between 70%-80%	600	30.41	6.71	22	30	38
LTI very high (>7.6)	575	14.88	9.42	5	13	28
LTI high (between 5.4 and 7.6)	575	24.59	7.26	14	25	33
mortgage growth rate, total	850	4.61	2.41	1.63	4.58	7.62
mortgage growth rate, households	850	4.61	2.75	1.63	4.25	7.89
mortgage growth rate, firms	850	4.81	5.08	-0.78	4.40	10.80
other credit growth rate, firms	850	4.09	5.92	-2.81	3.86	11.34
treatment variables						
$\mathbf{LTV_{continuous}}$: Average share of new mortgages with LTV>90% before 2012Q3	25	4.33	3.18	0	4.23	8.69
LTV: affected by the LTV cap	25	0.48	0.50	0	0	1
$\mathbf{CCyB_{continuous}}$: CCyB required capital/excess capital	25	0.37	0.92	0.05	0.12	0.62
CCyB: affected by the CCyB activation	25	0.16	0.37	0	0	1
$CCyB_{increase}$: affected by the CCyB increase	25	0.24	0.43	0	0	1
$RRWA/assets_{2012Q4}$ (used in Auer and Ongena)	25	0.20	0.07	0.06	0.22	0.29
controls: macroeconomic variables						
growth rate house prices	24	3.41	3.59	-1.95	3.19	8.20
growth rate rent	24	0.01	0.00	0.00	0.01	0.01
real GDP growth	24	1.35	1.30	-0.53	1.33	2.75
mortgage rate (5 year fix)	24	1.52	0.29	1.21	1.47	1.90
unemployment rate	24	3.09	0.28	2.66	3.09	3.46
three month LIBOR	24	-0.26	0.39	-0.78	0.01	0.09
controls: bank individual variables at 2010Q4						
average mortgage growth rate (2008-2012)	25	5.57	2.88	2.61	5.29	9.57
residential mortgages/assets	25	54.15	16.50	27.29	57.46	72.02
residential mortgages in million Swiss Franc	25	22.63	31.28	5.03	11.51	70.86
risk density residential mortgages	25	34.35	9.76	13.3	38.2	39.8
capital/assets	25	7.22	1.73	5.2	7	9.6
capital/rwa	25	14.11	2.65	10.7	13.3	18

Table 5: treatment intensity (LTV continuous and CCyB continuous)

	with LTV				with LTI			mortgage			Other credit
	>90%	80%-90%	66%-80%	<66%	>7.6	5.4-7.6	<5.4	total	private	firms	firms
$LTVconT_{2012Q3}$	-0.72***	-0.15	0.50	0.38	-0.38	-0.37	0.60	-0.16	-0.23	-0.00	0.15
	(0.00)	(0.72)	(0.24)	(0.11)	(0.28)	(0.37)	(0.24)	(0.29)	(0.16)	(0.98)	(0.47)
$CCyBconT_{2013Q1}$	-0.36***	-1.69***	2.13***	-0.08	-0.05	-0.97	1.22	-0.40	-0.43	-0.30	0.12
	(0.00)	(0.00)	(0.00)	(0.74)	(0.92)	(0.33)	(0.36)	(0.14)	(0.41)	(0.19)	(0.63)
Observations	600	600	600	600	575	575	575	850	850	850	850
R-squared	0.56	0.58	0.41	0.66	0.61	0.54	0.71	0.58	0.61	0.50	0.38

Notes: See note below Table 3. Instead of CCyB activation and LTV dummies, the CCyB and LTV treatment intensity variable are used.

Table 6: Effect heterogeneity for the bank with the highest treatment intensity

	with LTV				with LTI			mortgage			Other credit
	>90%	80%-90%	66%-80%	<66%	>7.6	5.4-7.6	<5.4	total	private	firms	firms
$LTV*T_{2012Q3}$	-4.37***	-0.07	2.41	2.04	-0.88	-1.30	2.18	-0.84	-0.66	-0.83	0.18
	(0.00)	(0.90)	(0.24)	(0.15)	(0.67)	(0.45)	(0.54)	(0.27)	(0.49)	(0.60)	(0.94)
$CCyB*T_{2013Q1}$	-0.45	-2.22*	9.24***	-6.56***	-4.73	1.81	2.91	-2.39***	-3.00**	-1.47*	2.06
	(0.51)	(0.07)	(0.00)	(0.00)	(0.30)	(0.36)	(0.51)	(0.03)	(0.01)	(0.09)	(0.36)
$B*T_{2013Q1}$	0.06	-5.28***	-1.48	6.70***	4.91	-7.81***	2.90	1.44***	1.95***	0.75	-1.73
	(0.86)	(0.00)	(0.46)	(0.00)	(0.27)	(0.02)	(0.52)	(0.00)	(0.00)	(0.19)	(0.47)
Observations	600	600	600	600	575	575	575	850	850	850	850
R-squared	0.55	0.58	0.44	0.67	0.61	0.54	0.71	0.60	0.62	0.50	0.38

Notes: See note below Table 3. Instead of CCyB activation, the dummy consists of three banks, and B is a dummy for the bank with the highest CCyB treatment intensity.

Table 7: CCyB treatment with 6 instead of 4 banks

	with LTV				with LTI			mort-gage			Other credit
	>90%	80%-90%	66%-80%	<66%	>7.6	5.4-7.6	<5.4	total	private	firms	firms
LTV*T2012Q3	-4.42***	0.20	1.75	2.47	-0.49	-1.51	2.00	-0.68	-0.39	-0.83	-0.17
	(0.00)	(0.9)	(0.37)	(0.13)	(0.81)	(0.42)	(0.57)	(0.43)	(0.68)	(0.62)	(0.91)
CCyB* T2013Q1	-0.04	-3.30*	6.49***	-3.15	-2.43	-0.35	2.77	-1.41*	-1.92**	-0.59	1.77
	(0.63)	(0.09)	(0.00)	(0.14)	(0.34)	(0.86)	(0.37)	(0.09)	(0.04)	(0.59)	(0.28
Observations	600	600	600	600	575	575	575	850	850	850	850
R-squared	0.55	0.57	0.42	0.66	0.61	0.53	0.71	0.59	0.63	0.51	0.38

Notes: See note below Table 3. Instead of the CCyB activation dummy with four banks, six banks are in the CCyB treatment group.

Table 8 other covariates instead of bank and time dummies

	with LTV				with LTI			mort-gage			Other credit
	>90%	80%-90%	66%-80%	<66%	>7.6	5.4-7.6	<5.4	total	private	firms	firms
LTV*T2012Q3	-4.37***	-0.27	2.35	2.28	-0.70	-1.65	2.35	-0.79	-0.59	-0.81	0.12
	(0.00)	(0.82)	(0.24)	(0.13)	(0.71)	(0.35)	(0.47)	(0.28)	(0.48)	(0.60)	(0.95)
CCyB* T2013Q1	-0.44	-3.50*	8.88***	-4.95**	-3.54	-0.09	3.63	-2.04***	-2.53**	-1.29	1.64
	(0.45)	(0.06)	(0.00)	(0.02)	(0.32)	(0.82)	(0.32)	(0.00)	(0.02)	(0.13)	(0.33)
Observations	600	600	600	600	575	575	575	850	850	850	850
R-squared	0.41	0.18	0.37	0.34	0.36	0.19	0.32	0.37	0.44	0.31	0.24

Notes: See note below Table 3. Instead of bank and time dummy fixed effects, other covariates are used.

Table 9: Short-, medium and long-term effects

	with LTV				with LTI			Mortgage growth			Other credit
	>90%	80%-90%	66%-80%	<66%	>7.6	5.4-7.6	<5.4	total	private	firms	firms
LTV*T_{short}	-4.03***	0.07	0.98	2.97	-1.18	-0.70	1.88	-0.89	-0.74	-0.10	0.63
	(0.01)	(0.97)	(0.62)	(0.14)	(0.67)	(0.70)	(0.66)	(0.26)	(0.37)	(0.98)	(0.74)
LTV*T_{medium}	-4.36***	-0.32	2.12	2.56	-0.88	-1.89	2.78	-0.82	-0.68	-0.62	1.41
	(0.01)	(0.88)	(0.32)	(0.13)	(0.71)	(0.31)	(0.43)	(0.30)	(0.46)	(0.66)	(0.43)
LTV*T_{long}	-4.70***	-0.53	3.81*	1.42	-0.07	-2.18	2.25	-0.67	-0.37	-1.59	-1.45
	(0.01)	(0.81)	(0.09)	(0.38)	(0.93)	(0.32)	(0.52)	(0.44)	(0.70)	(0.38)	(0.39)
CCyB*T_{short}	-0.18	-2.97	7.24***	-4.09**	-3.70	1.28	2.43	-2.76***	-3.06***	-2.82**	-1.63
	(0.71)	(0.18)	(0.01)	(0.04)	(0.32)	(0.56)	(0.40)	(0.00)	(0.01)	(0.02)	(0.20)
CCyB*T_{medium}	-0.21	-3.51**	8.76***	-5.04**	-1.80	-0.27	2.06	-1.68***	-2.30**	-0.41	1.67
	(0.77)	(0.04)	(0.01)	(0.03)	(0.59)	(0.89)	(0.58)	(0.00)	(0.04)	(0.73)	(0.28)
CCyB*T_{long}	-0.86	-3.96**	10.36***	-5.54**	-5.64	-1.08	6.72	-1.63*	-2.21**	-0.33	5.68
	(0.24)	(0.05)	(0.01)	(0.05)	(0.19)	(0.69)	(0.23)	(0.09)	(0.04)	(0.75)	(0.22)
Observations	600	600	600	600	575	575	575	850	850	850	850
R-squared	0.55	0.57	0.45	0.67	0.61	0.54	0.71	0.60	0.62	0.50	0.40

Notes: See note below Table 3. Six quarters after treatment is considered as short-term, seven to 12 quarters as medium-term and everything afterwards as long-term.

Table 10 placebo treatment effects in the pretreatment sample

	total	households	firms
CCyB* T_{2009Q4}	0.47	0.89	2.02
	(0.32)	(0.63)	(0.46)
CCyB* T_{2010Q1}	0.13	-0.24	2.56
	(0.68)	(0.88)	(0.34)
CCyB* T_{2010Q2}	-0.26	-0.92	2.41
	(0.50)	(0.34)	(0.39)
CCyB* T_{2010Q3}	-0.79	-0.77	1.00
	(0.20)	(0.41)	(0.74)
CCyB* T_{2010Q4}	-1.20*	-0.79	-0.51
	(0.10)	(0.36)	(0.78)

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