Five Essays on Bank Regulation

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Contents

List of Figures

List of Tables

Introduction

The story of the financial crisis of 2007/2008 is also a story of bank regulation. Commentators from academia and policy institutions have identified an inappropriate regulation of banks and capital markets as one of the main factors that contributed to the transformation of the U.S. subprime crisis into the global financial crisis with all its devastating consequences. Clearly, the regulation of banks and capital markets is one of the most important issues in today's post-crisis world. The present work contains five essays that contribute to the literature on bank regulation. The first three chapters deal with the effects of model-based, risk-weighted capital regulation as specified in the Basel II/Basel III regulatory framework. In Chapter 4, we examine how political factors affect bailout decisions in the German savings bank sector. Chapter 5 uses a panel of 26 countries and investigates how the removal of entry barriers for foreign banks affects economic outcomes, and how it interacts with the efficiency of the domestic banking sector at the time of liberalization.

CHAPTER 1.[1] A major innovation of the Basel II framework was the introduction of model-based capital regulation. For the first time, large banks were allowed to use their internal risk models in order to determine capital charges for credit risk. In this way—the hope was—a better alignment between capital charges and actual asset risk could be achieved, which would lead to a better allocation of resources and reduced incentives for regulatory arbitrage. However, even before its implementation several aspects of the new approach were heavily criticized. One of the main criticisms was that model-based regulation would exacerbate the pro-cyclicality of the financial system: As risk estimates are responsive to economic conditions, they are

[1]This chapter is based on joint work with Rainer Haselmann and Paul Wachtel.

likely to increase in a downturn, which means that capital requirements for credit risk will increase when economic conditions deteriorate. To the extent that banks are unable or unwilling to raise new equity, they will be forced to deleverage, for example by cutting back lending activities. As this could mean a restriction in firms' access to funds, the initial downturn might be exacerbated.

In this chapter, we empirically examine how the introduction of asset-specific, risk-weighted capital charges affected banks' lending behavior and firms' access to funds in a recession. Specifically, we exploit the gradual introduction of the Basel II internal ratings-based approach (IRB) by large German banks in order to test whether model-based capital regulation has exacerbated the pro-cyclicality of the financial system. While German banks started to introduce the IRB approach in early 2007, it was not feasible for them to transfer all their assets to the new approach at the same time. In September 2008, when the collapse of the investment bank Lehman Brothers exogenously increased credit risk in the German economy, banks introducing IRB had transferred only a portion of their loan portfolios to the new approach. Exploiting this within-bank variation in the regulatory approach, and the fact that many firms borrow from several IRB banks at the same time, we are able to test whether, in response to the Lehman collapse, loans under IRB—for which capital charges are responsive to economic conditions—were adjusted in a different way compared with loans under the traditional approach, for which capital charges do not respond to economic conditions. Importantly, this setup allows us to control for both bank-level and firm-level heterogeneity. We find that loans to the same firm decline by about 3.5 percent more when the loan is part of an IRB portfolio as compared with a portfolio using the traditional regulatory approach. Since banks tend to reduce especially large IRB credit exposures during the recession, firms relying on IRB loans experience an even stronger reduction in aggregate borrowing (5 to 10 percent larger) as compared with firms relying on loans under the traditional approach. Overall, the findings in this chapter confirm the claim that model-based capital regulation has exacerbated the pro-cyclicality of the financial system. Although Basel III includes several tools that are meant to address this issue (see Chapter 2), it continues to rely on model-based regulation, leaving the basic mechanism behind our findings unchanged.

CHAPTER 2.[2] Following the financial crisis of 2007/2008, the regulator acknowledged pro-cyclical features of the Basel II framework and implemented several tools to mitigate this problem. As one such tool, the Basel III framework includes a countercyclical capital buffer (CCB) that aims to increase the resilience of the banking sector by absorbing shocks arising from financial and economic stress. The idea behind the CCB is simple: Banks should build up additional capital buffers in times of excessive credit growth, which can then be released when economic conditions deteriorate. In this context, a key task for the regulator is to determine whether credit growth is excessive in the sense that there is a build-up of vulnerabilities in the banking sector that could potentially lead to a crisis. If this is the case, the CCB should be activated, which would on the one hand slow down excessive credit growth and smooth the credit cycle, and, on the other hand, increase the resilience of the banking sector.

This chapter was written in close collaboration with policy makers during a traineeship at the European Central Bank (ECB). Importantly, it does not aim to evaluate whether a CCB is able to adequately address the problem of pro-cyclicality documented in Chapter 1. Rather, we develop a tool for the detection of vulnerabilities in the banking sector that is meant to guide policy makers' decisions on the setting of CCB rates, a multivariate early warning model relying on private credit variables and other macro-financial and banking sector indicators. For this, we use data for 23 EU member states covering the period between 1982 and 2012. We find that, in addition to credit variables, other domestic and global financial factors such as equity and house prices as well as banking sector variables help to predict vulnerable states of the economy in EU member states. The models we analyze demonstrate good out-of-sample predictive power, signaling the Swedish and Finnish banking crises of the early 1990s at least six quarters in advance. Based on these findings, we suggest that policy makers take a broad approach when deciding on CCB rates. What remains to be shown is to what extent the CCB is able to address the inherent pro-cyclicality of model-based capital regulation.

CHAPTER 3.[3] Apart from its inherent problem of pro-cyclicality, Basel II-type

[2]This chapter is based on joint work with Carsten Detken, Tuomas Peltonen, and Willem Schudel. It has been published in the ECB Working Paper Series (No. 1604).

[3]This chapter is based on joined work with Rainer Haselmann and Vikrant Vig.

model-based capital regulation has been criticized for being much too complex and intransparent. In particular, as banks have to estimate tens of thousands of parameters in order to determine risk-weighted assets, it has become almost impossible for regulators to keep track of all these estimations. As a measure of riskiness, risk-weighted capital ratios have come under pressure: An increasing number of investors prefers to rely on traditional, unweighted capital ratios when assessing the solvency of a bank. The trust in regulatory risk weights is deteriorating, which raises the question whether model-based capital regulation has failed to meet its objective of creating a safer and more efficient banking system.

In this chapter, we examine how the Basel II reform affected lending and financial stability. Using data from the German credit register, and employing a difference-in-difference identification strategy, we empirically investigate how the introduction of model-based capital regulation affected the quantity and the composition of bank lending. We find that, following the reform, banks that introduced the internal ratings-based (IRB) approach increased their lending relative to banks that remained under the traditional approach, as the move to IRB was associated with a considerable reduction in capital requirements for credit risk. Moreover, loans under IRB exhibit a higher sensitivity to model-based PDs as compared with loans under the traditional approach. Interestingly, however, we find that—for IRB loans—risk models systematically underpredict actual default rates by about 0.5 to 1 percentage points. There is no such systematic prediction error in PDs for loans under the traditional approach. Our findings suggest that, counter to the stated objectives, model-based risk weights have weakened the link between PDs and actual defaults. We conclude that the reform has failed to meet the objective of a better alignment between capital charges and actual asset risk.

CHAPTER 4.[4] The year 2014 will bring a historic change for the regulation of banks in the European Union, as the ECB takes over the supervision of the largest and most significant banks from national supervisors. Among other things, this change creates a larger distance between banks and regulators. On the one hand, this may mean a loss of knowledge, if one believes that national supervisors are closer to local banks and hence have a better understanding of their business models. On the

[4]This chapter is based on joined work with Rainer Haselmann, Thomas Kick, and Vikrant Vig.

other hand, previous experiences have shown that national regulators often refrain from tough regulatory actions as they fear a competitive disadvantage for "their" banks as compared with banks from other countries. Hence, greater distance may actually lead to better supervision.

In this chapter, we contribute to the debate on the optimal proximity between banks and politicians or regulators. Specifically, we investigate how political factors affect public bailout policies in the German savings bank sector. German savings banks are interconnected by a state level association that operates a safety net for these banks. In case of distress, this association injects funds or restructures the respective bank. Alternatively, if politicians want to avoid a formal distress case and a potential restructuring of the bank, they can use taxpayers' money to support the distressed bank. As they often function as a chairman of the savings bank—hence exerting significant control over the bank—they could have an incentive to do so if political circumstances allow it. For a sample of 148 distress events, we find that indeed politicians' interests and ideology have a significant impact on their decision to bail out distressed banks. The probability that a politician injects taxpayers' money into a distressed bank is 30 percent lower in the year before an election as compared with the years after an election. High competition in the electoral process reduces the probability of a public bailout by 15 percent. We also show that ideology affects bailout decisions: Capital injections are 17 percent less likely if the politician is a member of the German conservative party (CDU). Further, politicians tend to refrain from capital injections if their community is highly indebted. Banks that are bailed out by politicians experience less restructuring and perform worse in the years following the event compared with banks that are bailed out by the savings bank association. Moreover, we do not observe a better macroeconomic performance of counties in which the bank distress event was resolved by the owner as compared with the association. The fact that bailout decisions are often driven by personal interests of the politicians involved provides an argument for a larger distance between banks and politicians or regulators that decide on bailouts. Hence, our results provide support for the move towards a unified banking supervision in the European Union.

CHAPTER 5.[5] In many countries, the banking sector is one of the most heavily

[5]This chapter is based on joined work with Rainer Haselmann, Amit Seru, and Vikrant Vig.

regulated industries, due to its importance for an efficient allocation of resources and overall economic growth and stability. In particular, many governments tried to exert a certain amount of control over their financial systems, for example by imposing ceilings on interest rates or capital flows, by owning or micromanaging large parts of the banking system, or by restricting entry to the financial sector, especially for foreign banks (see Beim and Calomiris 2001). The late 20^{th} and the early 21^{st} century, however, witnessed a move away from such financial repression, as the International Monetary Fund (IMF) and the World Bank—as part of the so-called Washington Consensus—promoted financial liberalization in many member states. Whether this liberalization was actually beneficial for the countries is still subject to considerable debate.

In this chapter, we look at banking sector liberalization in 26 countries and investigate how the removal of entry barriers for foreign banks affected economic outcomes. We argue that the nature of the financial structure (supply of financing) impacts a country's industry structure through its influence on the allocation of credit to firms and industries. We exploit the variation in the efficiency of the domestic banking sector at the time of liberalization to identify large changes in the nature of the supply of financing in an economy due to the entry of foreign banks. Foreign—relatively arm's length—capital largely crowds out domestic lending in markets with relatively inefficient banks after liberalization. In contrast, there is an increase in the aggregate supply of credit in countries with relatively efficient domestic banks following such an event. We use this changed mix of financing across economies and show that the nature of the supply of financing significantly impacts the allocation of credit. There is a higher growth rate and lower growth volatility for industry sectors in markets with relatively more efficient domestic banks following liberalization. These results are driven by more credit flowing to industries that are reliant on external financing and more credit flowing to smaller firms. In contrast, industry growth is lower and growth volatility is higher in countries with relatively inefficient domestic banks following liberalization. Particularly small firms are harmed in these countries. Thus, the timing of liberalization of credit markets interacts with the efficiency of the incumbent domestic banking sector, and the changed nature of the supply of financing it induces has implications on the allocation of credit and economic growth.

Chapter 1

Pro-Cyclical Capital Regulation and Lending

1.1 Introduction

The design of banks' capital charges has long been one of the most important and controversial issues in discussions of bank regulation.[1] Prior to the financial crisis, much of the effort to improve regulation was concentrated on the microprudential goal of a better alignment of capital charges with banks' actual asset risk. Although this idea was already present in the Basel I agreement of 1988, Basel II went a step further by introducing the concept of internal ratings-based (IRB) capital requirements. Under the IRB approach, the amount of capital a bank has to hold for a given loan is a function of the model-based, estimated risk of that loan. Many of the world's larger banks are now using their own rating models to determine capital charges for individual credit risks.[2]

There is an argument that linking capital charges to asset risk may exacerbate business cycle fluctuations (see Daníelsson et al. 2001, Kashyap and Stein 2004, Repullo and Suarez 2012). Specifically, capital requirements will increase in a downturn if measures of asset risk are responsive to economic conditions, while at the same

[1]See Peltzman (1970), Koehn and Santomero (1980), Kim and Santomero (1988), Blum and Hellwig (1995), Diamond and Rajan (2000, 2001), Morrison and White (2005), or Acharya (2009). An early review of the literature is provided by Bhattacharya, Boot, and Thakor (1998).

[2]Over 100 countries have implemented the agreement, with more than half using the more advanced methodology for individual credit risks (see Financial Stability Institute 2010).

time bank capital is likely to be eroded by losses. Capital constrained banks that are unable or unwilling to raise new equity in bad times will be forced to deleverage by cutting back lending activities, hence exacerbating the initial downturn.[3] In this paper, we causally identify the effect of asset-specific, risk-based capital charges on banks' lending behavior and firms' aggregate borrowing around the financial crisis in Germany. Hence, we estimate the magnitude of the pro-cyclical effects of model-based capital regulation on lending during a downturn.

While the pro-cyclicality of Basel II has been widely discussed in the academic as well as in the policy literature,[4] three issues beset empirical identification of the effects on lending. First, the assessment of asset-specific risk and the lending decision of a bank are endogenous. If a bank increases lending to a firm, the firm's leverage increases, and this will increase the model-based estimation of credit risk. Thus, the relationship between bank lending decisions and firm credit risk may suffer from reverse causality. Second, economic downturns are likely to affect both a firm's loan demand and the evaluation of its credit risk by banks. Therefore, it is essential to disentangle a shock to a firm's loan demand from a potential loan supply shock. Third, economic downturns are likely to have a differential impact on banks. Thus, it is difficult to determine whether a change in bank lending is driven by the pro-cyclicality of capital regulation or the way the bank is affected by the recession shock. The latter concern is an important identification challenge, since larger German banks introduced the IRB approach while most smaller banks continue to use the traditional standard approach (SA) to determine capital charges.[5] If large banks are affected differently by a downturn, as compared with small banks, it is difficult to disentangle the effect of capital regulation on lending from other bank-specific factors.

[3] Admati et al. (2012) show that even if raising capital is possible, bank shareholders are likely to prefer reducing assets over raising new capital in order to fulfill regulatory requirements.

[4] See Borio, Furfine, and Lowe (2001), Lowe (2002), Goodhart, Hofman, and Segoviano (2004), Gordy and Howells (2006), Rochet (2008), or Repullo, Saurina, and Trucharte (2010). Brunnermeier (2009) and Hellwig (2009) discuss how pro-cyclical features of the regulation contributed to the financial crisis.

[5] In the SA capital requirements do not depend on asset risk or economic conditions and are constant over time (see Section 1.2.1 for details). Exceptions are cases where borrowers have external credit ratings, as the SA allows for the use of these ratings to determine capital requirements. However, the German market for corporate bonds is very small; hence, very few companies have an external rating. We exclude a small number of SA loans to these companies to ensure that all loans under the SA in our sample are subject to a fixed capital charge.

We overcome all these identification issues by exploiting the institutional arrangements surrounding the introduction of the Basel II Accords in Germany in 2007 (see Bundesbank 2006 for details) and the richness of the data from the German credit registry. Specifically, once Basel II was introduced, banks started to use their own internal risk models to determine the regulatory capital for their loan portfolios (IRB banks) or remained under the old regime (SA banks). For IRB institutions, the regulator separately certified the internal model for each loan portfolio within the bank, before the IRB approach could be used to determine capital charges. Since this certification process took several years, IRB banks had only a portion of their loan portfolios transferred to the IRB approach at the time of the Lehman collapse in September 2008. Hence, they were using the new IRB approach to determine capital charges for some loan portfolios and the traditional SA for other portfolios when the financial crisis occurred.

We take advantage of this variation of the regulatory approach within IRB banks to identify the effect of pro-cyclical capital regulation on lending. While the crisis event resulted in an unexpected increase in credit risk in Germany, it had an impact on the capital charges of the IRB loan portfolios only.[6] The capital charges on SA loan portfolios within IRB banks were not affected by this event. German firms usually borrow from more than one bank and, as it turns out, many firms have relationships with banks that are using different regulatory approaches to determine capital charges. Thus, we are able to examine the effect of the regulatory approach holding constant the firm-specific determinants of loan demand. On the supply side, the gradual introduction of IRB meant that many firms had loans from large (IRB) banks that were in some instances subject to the IRB approach to determine capital charges and in other instances using the SA. By comparing the relative change in lending to firms that take a loan from at least two different IRB institutions—one where the particular loan is in a business segment that is using the IRB approach and another where the loan is in a business segment that is still using the SA—we can systematically control for bank heterogeneity.[7]

[6] The average probability of default (PD) in our sample increased by 3.5 percent over the crisis period. Correspondingly—as the PD is a key factor in the determination of capital charges under the IRB approach—capital requirements rose by 0.54 percentage points on average.

[7] The identification strategy to isolate loan supply shocks from firm demand shocks by focusing on borrowing by a given firm from different banks is based on Khwaja and Mian (2008) and has

Our identification strategy provides us an unbiased estimate of the pro-cyclicality effect as long as there is no relationship between the order in which IRB banks shifted their loan portfolios toward the new regulatory approach (IRB) and the banks' decision to adjust these loans in response to a crisis. There are two potential determinants of the order in which loan portfolios are shifted toward IRB within banks. First, the regulator requires that the bank has a sufficient amount of data to calibrate a meaningful credit risk model for a certain portfolio before it is shifted to IRB (i.e., banks have to first transfer business segments where they are relatively active). Second, if they were free to choose, banks would have an incentive to shift the least risky portfolios to the new approach first (since the reduction in capital charges is the highest for these portfolios). We find that less risky loans as well as loans in business segments where the bank is more active are adjusted less over the crisis. This means that any bias would work against finding a significant impact of the regulatory approach. Moreover, banks had to announce the order in which loan portfolios would be transferred toward IRB years before the outbreak of the financial crisis.[8] Hence, they were unable to react to the crisis by changing the order of portfolios that are moved to the new approach.

We find that capital regulation has a strong and economically meaningful impact on the cyclicality of lending. Loans to the same firm by different IRB banks are reduced by 3.7 percent more over the crisis event when internal ratings (IRB) instead of fixed risk weights (SA) are used to determine capital charges. These findings are robust to the inclusion of bank and firm fixed effects in first differenced data. Further, there is no difference in the adjustment of loans using the SA provided from IRB banks or loans from SA banks to the same firm. Both of the above results illustrate that our findings are not driven by bank heterogeneity.

We are also able to identify the effect of the Basel II capital regulations on the pro-cyclicality of the aggregate supply of loans to firms. That is, we examine whether the adoption of the IRB approach makes it more difficult for firms to borrow from any source. On the one hand, it could be that a firm with IRB loans that were reduced

been applied by Jiménez et al. (2013a).

[8]Banks and the regulator had to agree on an implementation plan that specified an order according to which loan portfolios were transferred to IRB (see Bundesbank 2005). German banks that introduced the new approach submitted these plans to the regulator in 2006. Note that no individual loans could be shifted and that there could be no reversal of this choice.

during the crisis can offset the effect by increasing its borrowing from banks using the standard approach. On the other hand, if banks tend to ration especially large loans, the magnitude of the pro-cyclical effect on aggregate firm borrowing could be even larger. If this is the case, then the new capital regulations have important and, perhaps, undesirable macroeconomic implications.

The effects on aggregate firm borrowing are difficult to identify because there is only one observation per firm (borrowing from all of its banking relationships).[9] To surmount the problem, we restrict the sample to firms that have loans from IRB banks where some loans are under IRB to determine capital charges while others are still under the SA. We show that aggregate loan supply to a firm is reduced more during the crisis when the share of its loans from IRB institutions subject to IRB capital charges is greater. Specifically, we find that a firm that borrowed only with IRB loans experienced a reduction in total loans that is about 5 to 10 percent larger than the reduction for a firm that borrowed only with loans under the SA. During economic downturns, it seems to be difficult for firms to offset reductions in lending from one bank by increasing borrowing from other banks. We find only weak evidence that firms that had more IRB loans experienced greater increases in capital costs. This suggests that IRB banks adjusted loan quantities rather than loan conditions as a reaction to the crisis.

Exploiting the cross-sectional heterogeneity of bank capital ratios before the crisis allows us to further nail down the channel through which capital regulation affects lending. IRB banks with a low equity ratio had a small buffer to absorb increases in capital charges induced by an increase in credit risk. Therefore, the IRB effect documented above should be particularly pronounced for these banks. We find that—among IRB banks—those institutions with a lower than median initial capital ratio prior to the crisis reduce their IRB loans by 2.9 percent more, relative to those with a higher than median capital ratio.

In additional tests we find that IRB banks reduce loans to which they have a large exposure relatively more. In particular, IRB banks reduce the IRB loans to which they have a larger than median exposure by 9.7 percent more than their smaller IRB loans. They also reduce loans more to those firms that experienced a higher

[9]This means that it is not possible to use firm fixed effects to hold firm demand constant.

deterioration of model-based credit risk estimates during the crisis. In both instances this supports our claim that banks had to deleverage in order to fulfill higher capital requirements. They do so by reducing particularly those loans for which they can save the most in required capital: I.e., larger and riskier loans.

Our paper is the first to provide these direct empirical estimates of how the pro-cyclicality inherent in the model-based approach to capital regulation affects the supply of loans to firms. Previous studies used numerical simulations on hypothetical or real-world portfolios (Carling et al. 2002, Corcóstegui et al. 2002, Lowe and Segoviano 2002, Kashyap and Stein 2004, Saurina and Trucharte 2007, Francis and Osborne 2009, Andersen 2011) or analyzed the overall effect of business cycle fluctuations on banks' capital buffers (Ayuso, Pérez, and Saurina 2004, Lindquist 2004, Jokipii and Milne 2008). While these studies find that the bank capital buffers fluctuate counter-cyclically, they cannot causally quantify how pro-cyclical capital regulation affects the supply of loans to firms. There are two recent papers that examine the effect of changes in capital requirements on bank lending. First, and most closely related to our own paper, Jiménez et al. (2013b) examine the effect of dynamic provisioning rules on bank lending in Spain. Exploiting variation across banks, they show that lowering capital requirements when economic conditions deteriorate helps banks to maintain their supply of credit. Our paper uses within-bank variation to examine the effect of risk-based capital regulation on lending in the context of a shock to credit risk. Second, Aiyar, Calomiris, and Wiedalek (2012) exploit a policy experiment in the United Kingdom and show that regulated banks, as compared with non-regulated banks, reduce lending in response to tighter capital requirements. Our loan-level data allow us to more directly address issues of firm-level and bank-level heterogeneity.

Our findings are in line with theoretical evidence on the pro-cyclicality of risk-based capital regulation, such as the dynamic equilibrium model of Repullo and Suarez (2012), which shows that increasing capital charges in a downturn can lead to a severe reduction in the supply of credit. Earlier, Thakor (1996) argued that small increases in risk-based capital requirements lead to large reductions in aggregate lending.[10] Also, policy analysts have argued that the Basel II model-based approach

[10]Berger and Udell (1994) provide empirical evidence that the introduction of Basel I exacerbated

would increase the pro-cyclicality of bank capital (e.g., Borio, Furfine, and Lowe 2001, Goodhart, Hofman, and Segoviano 2004, and Gordy and Howells 2006).

Our paper also relates to the broader literature analyzing the impact of banks' liquidity or capital shocks on loan supply (Bernanke 1983, Bernanke, Lown, and Friedman 1991, Kashyap and Stein 2000). Peek and Rosengren (1995a,b) and Gambacorta and Mistrulli (2004) find evidence to support the concern that low-capitalized banks are forced to cut their loan supply during a recession. Peek and Rosengren (1997, 2000) go a step further by showing that capital shocks to Japanese banks in the 1990s induced them to cut back lending in the United States and that the resulting loan supply shock negatively affected real economic activity. For the recent crisis, Ivashina and Scharfstein (2010), Puri, Rocholl, and Steffen (2011), Iyer et al. (2013), Kahle and Stulz (2013), and Paravisini et al. (2013) document a credit crunch. Our paper combines these different strands of the literature by showing that a tightening of capital requirements caused by pro-cyclical regulation affected lending in Germany after the Lehman collapse and that this had severe consequences for firms' overall access to funds.

Our findings illustrate how microprudential and macroprudential goals of banking sector regulation might conflict with one another.[11] On the one hand, the reduction in lending we document is due to capital charges that are based on improved evaluation of credit risk. In terms of safety of the individual bank, it might make sense to extend fewer loans when economic conditions deteriorate. Following this logic, Repullo and Suarez (2012) suggest that the business cycle side effects of Basel II may have a payoff in the long-term solvency of the banking system. On the other hand, as banks simultaneously restrain their lending, firms' access to funds becomes restricted, and such restriction might negatively affect firm-level investment and exacerbate the cyclical shock. In order to evaluate the welfare effects of pro-cyclical capital regulation one would have to evaluate both its impact on the long-term safety of the banking sector and its effect on credit supply in economic downturns. While we cannot make a statement on the former, our findings help to quantify the latter.

a credit crunch in the United States by inducing banks to shift into assets with lower capital charges.

[11] See Galati and Moessner (2012) for a survey of the literature on macroprudential regulation. Recent contributions from the academic side include Kashyap, Rajan, and Stein (2008), Brunnermeier et al. (2009), Hellwig (2010), Hanson, Kashyap, and Stein (2011), and Acharya et al. (2012).

Basel III tries to account for both sides of the trade-off described above: While it continues to rely on risk-based regulation and the incentives such regulation provides for banks, macroprudential policy instruments like the countercyclical capital buffer were introduced with the explicit goal of smoothing credit supply over the cycle. Our conjectures are provided in the conclusion (Section 1.6). There, we question whether a countercyclical capital buffer would have been useful in the presence of a severe unexpected shock to credit risk such as the one analyzed here.

The remainder of the paper is structured as follows: In Section 1.2 we describe our data set and explain both the structure of the Basel regulations and the German institutional framework. Section 1.3 develops our empirical framework and explains our identification method. Section 1.4 presents our main results. Further robustness checks are in Section 1.5. The last section concludes and discusses the implications of the results.

1.2 Institutional background and data

In this section we outline the framework governing the determination of capital charges. We begin with an explanation of the relevant aspects of the Basel II agreement and the arrangements for its introduction in Germany. We then describe our underlying data set and present descriptive statistics.

1.2.1 Introduction of risk-weighted capital charges

The original Basel agreement (Basel I) introduced risk-based capital charges for the first time in 1988. First, bank assets were assigned to several risk groups (referred to as buckets) that received different risk weights. Second, regulatory capital requirements were defined in terms of risk-weighted assets, which were calculated as the total amount of each asset multiplied by its risk weight. For example, AAA-rated sovereign debt had a risk weight of 0 percent (i.e., no regulatory capital was required for such holdings), while all corporate loans had the same risk weight, 100 percent (Basel Committee on Banking Supervision 1988). A drawback of this regulatory framework was that banks had an incentive to hold the riskiest assets within each risk group, as these provided the highest yield while being subject to the same capital charges as less risky assets in the same bucket.[12] Therefore, an important motive for the introduction of Basel II capital standards was the wish of regulators to establish a stronger link between capital charges and the actual risk of each asset.

Basel II assigns an individual risk weight to each loan so that the capital charge reflects the underlying risk of the loan. Minimum capital requirements form the basis of the first of three pillars of the regulatory framework and allow banks to choose among two broad methodologies to calculate their capital charges for credit risk (Basel Committee on Banking Supervision 2006).[13] First, the standard approach (SA) is similar to the old Basel I framework and automatically assigns a risk weight of 100 percent to corporate loans if the borrower has no external rating. If a firm's debt is rated by an external agency, the rating can be used to determine capital charges for loans to the firm. In Germany, the corporate bond market is extremely small, and therefore only very few firms have external bond ratings. We exclude from our sample SA loans to firms with ratings.[14] Therefore, all SA loans considered in our analysis are automatically assigned a risk weight of 100 percent independent of the riskiness of the loan.

Second, if banks fulfill certain conditions and disclosure requirements they can opt for the internal ratings-based (IRB) approach that relies on the banks' own risk estimates to determine risk weights for assets.[15] IRB requires the estimation of four parameters to determine the risk weight of a loan: The probability of default (PD), the loss given default, exposure at default, and the effective maturity of the loan. The higher the estimate for any of these parameters, the higher the risk weight that is attributed to the loan. In the advanced internal ratings-based approach, the bank provides estimates for all of them, while in the basic internal ratings-based approach the bank estimates the only PD, and standard values are assumed for the others.[16]

[12] The effects of Basel I on bank behavior are analyzed in Basel Committee on Banking Supervision (1999).

[13] Minimum capital requirements under Basel II are designed to address credit risk, operational risk, and market risk. The other two pillars of Basel II are a better supervisory review and a stronger focus on market discipline.

[14] Only 149 firms in our sample have an external bond rating. These firms constitute 0.1 percent of all firms in our sample, or 2.1 percent of the firms used in our main identification test (Test 3, see Section 1.3).

[15] See Solvabilitätsverordnung (2006), §§ 56-59 for the requirements that banks have to fulfill to be eligible for IRB.

[16] We do not distinguish between the advanced and basic internal ratings-based approaches in our empirical analysis, because the risk weight depends on the loan's PD in both cases.

No matter whether a bank applies SA or IRB, the Basel agreement requires that aggregate capital charges must be no lower than 8 percent of risk-weighted assets.

Since the organizational efforts as well as the administrative expenses for the introduction of the new approach are high, the main determinant of a bank's decision to opt for IRB is its size. Large banks have the ability to distribute the administrative costs over a larger number of loans. Moreover, banks are incentivized to become IRB institutions by the fact that capital requirements are substantially lower under IRB than under SA (Basel Committee on Banking Supervision 2006, p. 12).[17] Since banks that decide to become IRB institutions may have an incentive to report low PDs for their loan portfolios in order to economize on regulatory capital charges, the introduction of IRB is closely monitored by the regulator. The regulator requires that the PDs used for the determination of capital charges are the same as the ones that the bank uses in order to determine loan conditions (e.g., the interest rate). Thus, if a bank were to consistently report smaller PDs in order to save on regulatory capital, the bank would eventually hurt itself by mispricing its loans.

For our analysis the crucial difference between the two approaches is that capital charges are endogenous to credit risk with IRB but not with SA. For loans under SA, risk weights are determined when the loan is made and do not change. For loans under IRB, the risk weights are determined by the PD, which can change as the firm's underlying condition changes. The internal risk models used by German banks estimate PDs at each point in time rather than taking an average over the business cycle. Thus, during an economic downturn PDs are likely to increase, implying higher capital charges if the bank is using IRB. The pro-cyclicality of capital charges under Basel II is one of its most controversial features. In this paper we analyze the effects of pro-cyclical capital charges on banks' lending behavior. The analysis depends on our ability to distinguish the effect of pro-cyclical capital charges from other determinants of lending behavior. Our identification approach exploits the gradual introduction of IRB by German banks.

The introduction of IRB is a highly regulated process that is laid out in the Solv-

[17] At the beginning of our sample period, the mean risk weight for loans from IRB banks was 43.7 percent: I.e., a loan with a face value of €100 increased risk-weighted assets by only €43.70 on average (see Panel B of Table 1.1), while under Basel I the same loan increased risk-weighted assets by €100. Thus, IRB institutions experienced a substantial reduction in required capital for corporate lending following the reform.

abilitätsverordnung (2006), §56. In order to deal with the operational complexity of introducing new rating models, banks do not apply the new approach to all loans at once; rather, they agree on a gradual implementation plan with the regulator.[18] The phased roll-out of IRB means that during the transition, which typically lasts for several years, banks will have both IRB and SA loans in their portfolios. Furthermore, the regulator requires banks to introduce IRB not for individual loans but for the entire loan portfolio of a given business unit that can be evaluated with a given internal rating model. Once a rating model has been put in place, the capital charges for all loans in the business unit arising from existing or new customers are determined with the consistent use over time of the same rating model.[19] Thus, loans in certain business units or asset classes have to be shifted all at once, so that it is not possible for the bank to strategically shift individual loans from one approach to the other.

The implementation plan specifies an order according to which IRB is applied to the different business units (loan portfolios) within the bank. The regulator requires banks to start by implementing IRB for those business units that have sufficient data on past loan performance available to calibrate a PD model. Consequently, banks have started with loan portfolios in industry segments where they are relatively active. Further, the bank and the regulator agree on the implementation plan for business units and the timing of the roll-out years before the actual introduction. The German banks that adopted IRB had submitted their implementation plans to the regulator in 2006. Hence, they were not able to react to the financial crisis by changing the order of loan portfolios that were transferred to IRB or by applying the standard approach to IRB portfolios after loan PDs deteriorated. At the outset of our data sample in 2008Q1 the phase-in of business units using IRB was underway. Thus, capital charges for IRB banks were determined by the internal ratings-based approach for some parts of the loan portfolio and by the standard approach for

[18]See Solvabilitätsverordnung (2006), §§ 64-67 for details on the implementation plan. Banks adopting the new approach must show on application that at least 50 percent of their risk-weighted assets will be calculated on the basis of IRB (entrance threshold). Furthermore, the implementation plan has to specify how the bank will achieve 80 percent IRB coverage within two and half years after the introduction (regulatory reference point) and 92 percent IRB coverage five years after the introduction (exit threshold).

[19]See Solvabilitätsverordnung (2006) §57,3.

other parts. We exploit this within-bank variation for our identification strategy as explained in Section 1.3.

1.2.2 Data and descriptive statistics

Our principal source of data is the German credit registry compiled by the Deutsche Bundesbank. As part of its supervisory role, the central bank collects data each quarter on all outstanding loans of at least € 1.5 million. The data set includes information on the lender's and the borrower's identity, the amount of the loan outstanding, the regulatory approach used by the bank, the probability of default (PD), and the risk-weighted assets corresponding to the respective loan.[20] We combine these data with annual information from bank balance sheets obtained from the Bundesbank's BAKIS database.

Our sample includes 1,825 commercial banks, state banks and cooperative banks.[21] We restrict the analysis to those commercial loans for which we are able to determine the regulatory approach used at the beginning of our sample period in 2008.[22] We consider a loan to be an IRB loan if the bank adopted the approach for the loan in either the first or second quarter of 2008.[23] To control for potential differences between IRB banks and SA banks that might have an impact on lending, we separate our sample into those banks using the internal ratings-based approach during our sample period and those banks not using it. As can be seen in Panel A of Table 1.1, there are 1,784 SA banks and 41 IRB banks in our sample. On average, IRB banks had adopted the new approach for 62 percent of their loans at the onset of our sample period in early 2008 (*Share IRB*). As expected, IRB banks are much larger and have lower equity ratios than SA banks. Regarding profitability, mea-

[20] The loan registry does not report additional information about loan terms such as the interest rate and maturity of the loan.

[21] We exclude loans from finance companies, stock brokerage firms, and other special purpose institutions.

[22] Although Basel II in Germany was introduced in January 2007, detailed information on the regulatory approach applied to a certain loan as well as PD estimates that we need for our analysis became available to the regulator only in 2008.

[23] As the implementation period for the internal ratings-based approach may last for up to five years it is possible that certain loans that we classify as SA loans are switched to IRB at a later point during our sample period. The opposite case, however, cannot occur since IRB banks are not allowed to switch IRB loans back to SA. Loans switched to IRB at later point in time would—if anything—prevent us from finding a significant impact of the regulatory approach on lending as they simply add noise.

sured by ROA, there are no substantial differences between the two groups. There are relatively more commercial banks among the group of IRB banks, while most cooperative banks continue to use the standard approach. These differences between SA and IRB banks pose potential problems for identification as the two groups might have been affected differently by the crisis event (e.g., owing to different degrees of internationalization or differences in capitalization). Our estimation strategy allows us to systematically address these important identification issues.

Descriptive statistics for the loan data are shown in Panel B of Table 1.1. Overall, our sample contains 182,966 loans to 107,025 distinct firms for the period from the first quarter of 2008 through the third quarter of 2011. The size of the average loan in our sample is € 16.1 million. Although there are many more SA banks than IRB banks, the total number of loans extended is approximately the same for each group as IRB banks have many more loans on average. Of the 182,966 loans, 49.5 percent are granted by IRB banks and 33.6 percent are subject to the internal ratings-based approach. There are more loans from IRB banks than loans that are subject to the internal ratings-based approach because IRB banks had not yet shifted all their loan portfolios to the new approach at the onset of our sample period (see Section 1.2.1).[24]

As noted earlier, our empirical approach will examine lending behavior in the context of a specific crisis event, the Lehman failure in September 2008. That is, our variable of interest will be the difference between (the log of) average lending in the post-crisis and pre-crisis periods. Average loan balances fell by almost 4 percent over the crisis period. The average PD reported by banks to the regulator before the crisis was 4.1 percent and increased to 7.8 percent over the crisis.[25] For IRB loans an increase in PD translates into an increase in the risk-weighted assets (RWAs) of the loan (i.e. the loan amount multiplied with its risk weight). The average ratio of RWAs to loans was 43.7 percent before the crisis but increased by 6.7 percent over the crisis. Hence, an increase in PD results in a disproportionally large increase in the ratio

[24]Specifically, our sample contains 90,500 loans from IRB banks. Of these loans, 61,417 are subject to IRB, while the remaining 29,083 are still subject to the standard approach.

[25]According to Basel Committee on Banking Supervision (2006), PD estimates should reflect the probability of a default event for the loan over the next 12 months. Note that we have information on changes in PD for 64,880 of the 182,966 loans in our sample. These are more than the 61,417 IRB loans in our sample, as the regulator asks IRB institutions to report PDs also for SA loans in cases where they estimated PDs for internal purposes.

Table 1.1: Summary statistics

Panel A: Bank-level variables	1,784 SA banks		41 IRB banks	
	Mean	S. D.	Mean	S. D.
Share IRB	0	0	0.620	0.371
Total assets in € mn (pre-event)	1,080	2,580	138,000	307,000
Bank equity ratio (pre-event)	0.067	0.051	0.046	0.029
Bank ROA (pre-event)	0.006	0.012	0.006	0.010
Bank Type				
... Commercial	8.7%	—	58.5%	—
... State	24.4%	—	31.7%	—
... Cooperative	66.8%	—	9.8%	—

Panel B: Loan-level variables	Obs.	Mean	S.D.
Loan size in € mn (pre-event)	182,966	16.1	38.4
D(IRB bank)	182,966	0.495	0.500
D(IRB loan)	182,966	0.336	0.472
Change in log lending	182,966	-0.038	0.456
PD (pre-event)	64,880	0.041	0.160
Change in PD	64,880	0.037	0.145
RWAs/loans (pre-event)	53,278	0.437	0.448
Change in RWAs/loans	53,278	0.067	0.461

Panel C: Firm-level variables	Firms	Mean	S.D.
Firm assets in € mn (pre-event)	7,778	153.4	347.9
Firm ROA (pre-event)	7,778	0.063	0.093
Firm leverage ratio (pre-event)	7,778	0.133	0.141
Firm PD (pre-event)	7,136	0.016	0.022
Total firm loans in € mn (pre-event)	107,025	22.7	67.5
Change in log of total firm loans	107,025	–0.078	0.399
Firm capital cost (pre-event)	4,977	0.0829	0.0712
Change in firm capital cost	4,977	–0.0011	0.0201

Panel D: Identifying observations	Test 1	Test 2	Test 3
Firms	20,740	10,496	7,167
Observations	93,370	49,492	27,620
a) ... of which from SA bank	44,423	35,852	
... of which from IRB bank	48,947	13,640	
b) ... of which SA loans			9,226
... of which IRB loans			18,394

Panel A shows descriptive statistics for the groups of SA and IRB banks. An IRB bank is defined as a bank that uses the internal ratings-based approach for some loans during our sample period, whereas an SA bank is defined as a bank that uses the Basel II standard approach in all its lending relationships. Panel B shows summary statistics for all loans of commercial, state, and cooperative banks for which we are able to determine the regulatory approach used at the beginning of our sample period. Panel C shows summary statistics for the matched sample of 7,778 firms for which we are able to obtain firm balance sheet information. Moreover, it includes information on aggregate loans of the 107,025 firms in our sample. Panel D shows the number of identifying observations in our three main tests. Test 1 requires that the firm has at least one loan from an SA bank and at least one loan from an IRB bank or at least two loans from distinct IRB banks, a condition that holds for 20,740 distinct firms with 93,370 loans. Test 2 requires that the firm has an SA loan from both an SA bank and an IRB bank or from two distinct IRB banks. Test 3 requires that the firm has both an SA loan and an IRB loan from an IRB bank (see Section 1.3 for details).

of RWAs to loans. Regulatory capital requirements are 8 percent of risk-weighted assets; accordingly, capital charges for IRB loans increased by 0.54 percentage points on average (6.7 percent × 8 percent). By definition, capital charges for SA loans are not affected by changes in default probabilities.

We also match our loan data with accounting information for German firms from the Bureau van Dijk's Amadeus database to obtain more detailed firm-level information. The Bundesbank credit register and the Amadeus accounting information were hand-matched by company name and location. Matches were made for 7,778 firms. Descriptive statistics for firms in the matched sample are provided in Panel C of Table 1.1. The average firm in the matched sample is rather large, with total assets of € 153.4 million. Further control variables are the firm's pre-event profitability (measured by its ROA) and leverage (defined as total debt over total assets). We use a credit risk model developed by Förstemann (2011) that applies firm balance sheet information in order to calculate firm-specific PDs that are similar to estimates obtained from Moody's RiskCalc model.[26] In Section 1.4.2, we investigate how aggregate firm loans change over the crisis. Total firm loans were € 22.7 million prior to the crisis and declined by 7.8 percent on average following the event.[27] Remarkably, the decline in total firm loans is about twice the size of the decline in the average loan. Following the crisis event banks reduce particularly those loans to which they have a large exposure. Finally, firm capital costs are defined as aggregate interest expenses over total loans. The overall interest rate for the average firm was about 8.3 percent in early 2008 and did not change much over the crisis, although the standard deviation of 2 percent for the change variable suggests that there was some variation across firms.[28]

[26]See Förstemann (2011) for details. Estimates from the credit risk model are smaller than loan-specific PDs reported by banks on average, as the credit risk models rely exclusively on accounting information.

[27]We calculate total firm loans by aggregating all the firm's loans in our sample.

[28]As our capital cost measure is a rather crude approximation we exclude implausible observations, in particular those observations where the absolute change in capital costs over the crisis was greater than 5 percentage points. Results in the empirical section do not depend on the choice off the cutoff point and are robust to using a higher cutoff.

1.2.3 Graphical analysis of the impact of the financial crisis on banks' capital charges

Before we present our methodology for identifying changes in loan supply, we provide a graphical analysis of the impact of the financial crisis on banks' capital charges for IRB loans. Figure 1.1 shows that a slowdown in German GDP growth began before 2007. However, a severe contraction followed the Lehman shock in 2008Q3, resulting in negative GDP growth rates until 2010Q1. We are interested in the impact of this severe real shock on bank capital charges on IRB loans.

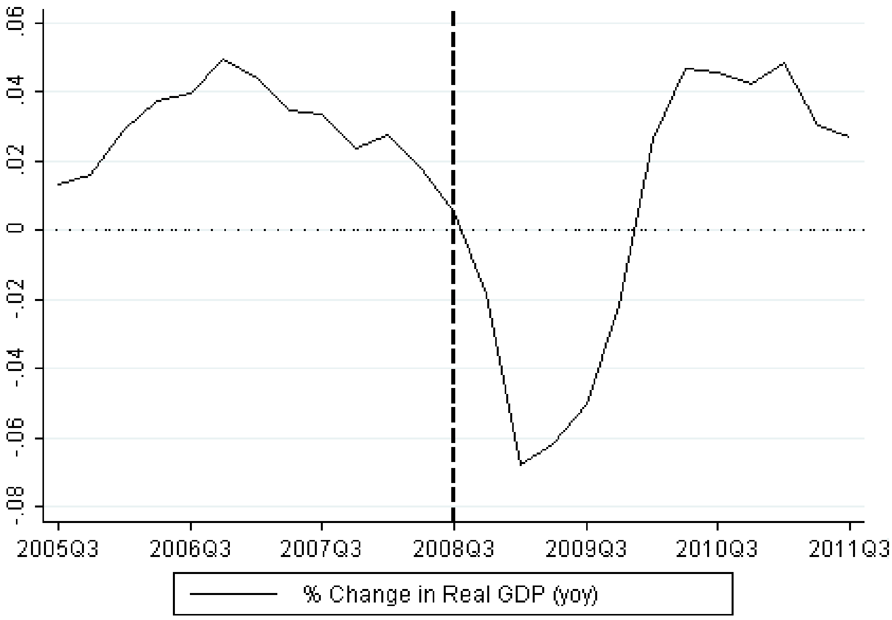

Figure 1.1: The crisis shock and the German economy.

This figure shows the year-over-year growth rate of GDP in Germany. The dashed vertical line indicates the crisis event in September 2008. (Source: Data from the German Federal Statistical Office.)

As documented in the descriptive statistics above, the crisis slowdown was associated with a rise in capital charges per euro lent for IRB loans. The average ratio of RWAs to loans rose by about 6.7 percent for the IRB loans which translates into an increase in capital charges of 0.54 percentage points. Panel A of Figure 1.2 illustrates how aggregate IRB loans and the associated RWAs evolved during the crisis event.[29]

[29] Aggregate RWAs are calculated as the sum of the outstanding loan amounts multiplied by their respective risk weights. Recall that our sample includes all lending relationships that existed in the second quarter of 2008, so relationships that originated after the crisis event are not included

Total RWAs are relatively constant throughout the period (they rise slightly after the crisis event until the second quarter of 2009 and decrease slightly thereafter). In contrast, the aggregate volume of IRB loans drops sharply after the crisis event as banks reduce their IRB lending exposure. This observation is consistent with the increase in the risk weight for the average loan documented above. The right graph shows the ratio of total RWAs to the total amount of loans; the increase in risk weights is also present on aggregate. The ratio increases sharply until the second quarter of 2009. Subsequently, it declines for about a year and then levels off.[30]

The figure shows that banks have to hold more capital for the same amount of IRB loans following the crisis event. This pattern illustrates the pro-cyclical effect of capital charges: During a recession the bank has to reduce its lending in order to keep capital charges constant. The subsequent drop in the ratio of RWAs to loans can most likely be explained by adjustments in banks' loan portfolios: As banks were forced to deleverage in order to fulfill capital requirements, they reduced particularly those loans whose risk weights increased most over the crisis. In order to provide evidence for this interpretation, we show the evolution of total RWAs under the assumption that banks do not adjust the quantity of their loan portfolios. To do so, we calculate a hypothetical series of RWAs by multiplying the observed risk weight for each loan in each period by the loan amount in 2008Q3, and then aggregate these amounts in each quarter. Since we cannot observe risk weights for loans that were canceled or matured before the end of our sample period, we consider only loans that existed throughout the entire sample period.[31] The results of this exercise are shown in Panel B of Figure 1.2. The right graph shows the ratio of the hypothetical RWAs series to the total amount of loans in 2008Q3. Its development over time illustrates that if banks had not adjusted their IRB loan portfolios following the crisis shock, the RWAs/loans ratio would have continued to rise throughout the period.

Figure 1.2 offers strong evidence of a pro-cyclical effect of risk-weighted capital charges on banks' loan supply. To rule out the possibility that this effect is driven by banks' heterogeneity or changes in firms' loan demand, we will introduce our

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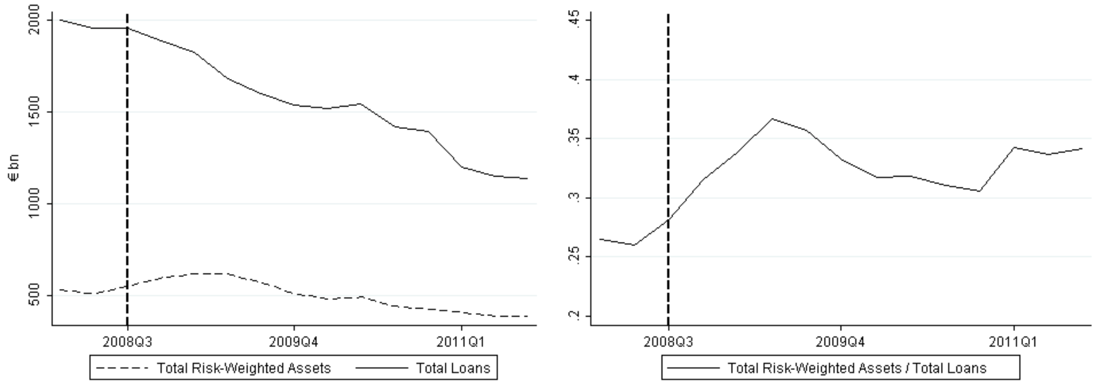
in the aggregate series.

[30]The ratio of aggregate RWAs to aggregate loans is somewhat lower than the average ratio of RWAs to loans (see Table 1.1), as larger loans tend to have lower risk weights.

[31]Note that the exclusion of loans that were canceled or not rolled over by banks is likely to bias against finding an increase in the RWAs/loans ratio.

identification methodology in Section 1.3.

Panel A



Panel B

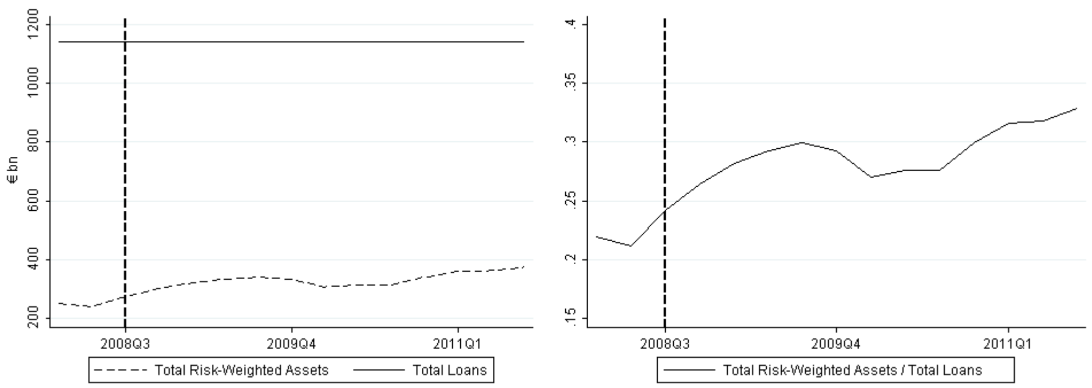


Figure 1.2: Total risk-weighted loans and total loans.

Panel A shows how total risk-weighted loans and total loans evolve over time. The series include only those lending relationships that existed prior to the crisis event; i.e., we do not include lending relationships that were originated after the crisis shock. The left graph shows the development of total loans and total risk-weighted assets for these loans. The right graph depicts the ratio of total risk-weighted assets to total loans. Panel B shows how total risk-weighted assets and total loans would have evolved over time for a constant portfolio of loans. We include all loans that exist throughout the entire sample period and calculate the risk weight for each loan at each point in time. We then calculate hypothetical risk-weighted loans in a given period by multiplying the loan amount of 2008Q3 with the risk weight for the respective period. In a final step we aggregate the calculated risk weighted loans in each period and divide it by the (constant) amount of total loans in 2008Q3 to obtain the ratio of total risk-weighted loans to total loans for a constant portfolio of loans. The left graph shows the aggregate series. The right graph shows the ratio between the two.

1.3 Methodology

1.3.1 Identifying changes in loan supply

Our identification strategy exploits the gradual introduction of IRB as described in Section 1.2.1. Loans in our sample fall into one of the following three groups (see Figure 1.3). First, all loans provided by SA banks remain under the standard approach. Thus, the required capital charges of these loans do not depend on their credit risk.[32] Second, loans by IRB banks can be subject to IRB if the loan is part of a portfolio that had been moved to the new approach at the onset of our sample period. Third, loans by IRB banks that had not yet been moved to IRB remain under the standard approach. The distinction between these three classes of loans provides the foundation of our identification strategy.

We start by examining changes in lending by SA and IRB banks in the context of the crisis event. Following Khwaja and Mian (2008), we consider how lending by IRB banks changed in comparison with lending by SA banks to the same firm (Test 1). The within-firm comparison is important because firm-specific loan demand is likely to be affected by the event. We define a variable *Share IRB* that is equal to the percentage share of all loans of the bank that are subject to the IRB approach (i.e., it takes the value zero for SA banks). Alternatively, we use a dummy variable to indicate whether or not an institution has opted for IRB. Thus, Test 1 is based on firms that have at least two loans—one loan from an SA bank and one loan from an IRB bank, or two loans from distinct IRB banks.[33] Formally, we estimate:

$$\Delta\log(\text{loans})_{ij} = \alpha_i + \beta \times \text{Share IRB}_j + X'_{ij}\gamma + \epsilon_{ij} \tag{1.1}$$

The dependent variable is the change over the crisis event in the logarithm of loans from bank j to firm i. In order to avoid problems of serial correlation we collapse our quarterly data into single pre- and post-event time periods by taking time-series averages of loans (Bertrand, Duflo, and Mullainathan 2004).[34] Thus, there is

[32] As stated in Section 1.2, there are no SA loans with an external rating in our sample.

[33] Our sample contains 20,740 firms that have at least one loan from an SA bank and one loan from an IRB bank or two loans from distinct IRB banks. Overall, these firms have 93,370 loans, which separate into 44,423 SA loans and 48,947 IRB loans (Table 1.1, Panel D).

[34] We could also estimate Equation (1.1) without time-collapsing the data if we replace the firm

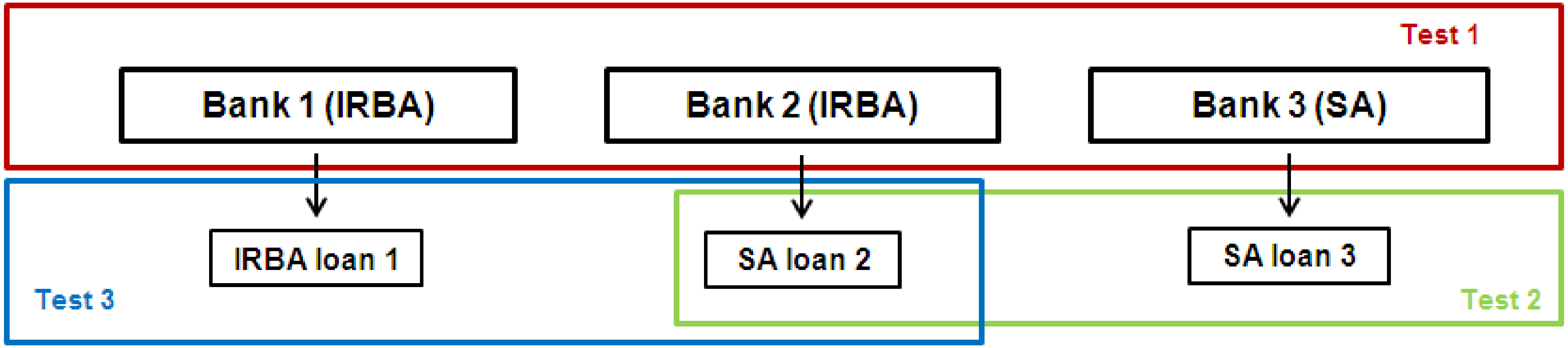


Figure 1.3: Institutional setup and identification.

This figure illustrates how we use multiple bank relationships of the same firm for identification in the empirical analysis. Suppose a firm has three loans: One IRB loan from an IRB bank, one SA loan from an IRB bank, and one SA loan from an SA bank. In Test 1 we include all loans to firms that have at least one loan from an SA bank and one loan from an IRB bank or at least two loans from distinct IRB banks; hence all the firm's loans would be included. Test 2 includes only SA loans and investigates whether there is a difference between SA loans from SA banks and SA loans from IRB banks. Hence, SA loan 2 and SA loan 3 would be included in this specification. Finally, in Test 3 we use only loans from IRB banks and test whether these banks—for the same firm—reduce their IRB loans more than their SA loans. In the example, this test would use IRB loan 1 and SA loan 2 for identification of the coefficients.

one observation per firm-bank relationship. The equation includes firm fixed effects α_i. In particular this means that identification of our coefficient of interest—β, the coefficient on the share of IRB loans within a bank—comes only from variation within the same firm. Our test shows whether the same firm, borrowing from two different banks, experiences a larger decline in lending from banks that use IRB for a larger share of their loans. Control variables X_{ij} include pre-event size, capitalization and profitability of the bank, a set of dummy variables indicating the bank's type, and the share of bank j's loans in firm i's two-digit SIC industry sector. To account for potential correlation among changes in loans from the same bank we cluster standard errors at the bank level in all our regressions.

Interpreting β from Equation (1.1) as the impact of credit risk-specific capital charges on lending behavior might be problematic if banks that provide more IRB loans differ systematically from banks that provide more (or only) SA loans. We have shown already that IRB institutions tend to be systematically larger, have lower capital ratios, and are more likely to be privately owned (Table 1.1, Panel A). Thus, our main concern is that IRB banks were also internationally more active and therefore more affected by the global crisis. Clearly, if IRB institutions generally experienced a larger crisis shock than SA institutions, this could explain why banks with larger shares of IRB loans reduced their lending significantly more than banks with lower shares.[35] However, as we explained before IRB banks did not introduce the IRB approach for all loans at the same time. Consequently, not all their loan portfolios were subject to potentially higher capital charges following the recession. To address concerns regarding the banks' heterogeneity, we exploit this feature by introducing two additional tests.

First, we test whether IRB banks' SA loans and SA institutions' loans are affected differently by the crisis event. Neither the SA institutions' capital charges nor those for IRB banks' SA loans are affected by an increase in firms' credit risk. Thus, by comparing the lending reaction of SA banks' SA loans with IRB banks' SA loans, we can test whether the IRB effect estimated in Test 1 is driven by bank heterogeneity.

fixed effects with firms times quarter fixed effects. Results from this specification are qualitatively very similar.

[35]Equation (1.1) includes bank size, capital ratio, and ownership as control variables and hence also directly controls for the influence of these variables on banks' lending behavior.

For Test 2 we use a subsample restricted to firms that obtain at least two SA loans from separate institutions that differ in the share of IRB loans they hold in their aggregate loan portfolio (see Figure 1.3 for an illustration).[36] We then estimate Equation (1.1) for this subsample of loans.[37] If we find β to be close to zero in Test 2, we conclude that the treatment group and the control group are not systematically different from each other and that the effect identified in Test 1 is indeed due to the choice of the regulatory approach rather than the characteristics of the banks.

Second, we can test for the IRB effect within the group of IRB banks only and thereby systematically control for bank heterogeneity. For Test 3 (see also Figure 1.3) we restrict the sample to firms that borrow from more than one IRB bank. In particular, we require that the firm has at least one IRB loan and at least one SA loan from different IRB institutions.[38] Formally, we estimate:

$$\Delta\log(\text{loans})_{ij} = \alpha_i + \alpha_j + \delta \times \text{D(IRB loan)}_{ij} + X'_{ij}\gamma + \epsilon_{ij} \tag{1.2}$$

where $D(IRB\ loan)$ is a dummy variable that takes the value 1 if the loan is subject to the IRB approach. In contrast to Tests 1 and 2 we include bank fixed effects α_j in addition to firm fixed effects α_i to systematically control for bank heterogeneity. This means that identification in Equation (1.2) is based on within-bank variation (compare with Jiménez et al. 2013a). The test shows whether the same firm—borrowing from two different IRB banks—experiences a larger decline in lending for loans that use the IRB as compared with the standard approach

1.3.2 Selection of IRB portfolios

Test 3 provides us with an unbiased estimate of δ as long as the choice of the loan portfolios whose capital charges are determined by IRB within IRB banks at the

[36] In our sample 10,496 firms have at least one SA loan from an SA bank and at least one SA loan from an IRB bank or two SA loans from distinct IRB banks. These firms have a total of 49,492 SA loans, of which 35,852 are from SA banks and 13,640 are from IRB banks (Table 1.1, Panel D).

[37] Again, we use the dummy variable $D(IRB\ bank)$ instead of *Share IRB* in an alternative specification. Essentially, this means that we examine the relative change in lending to firms that have at least one SA loan from an SA bank and another SA loan from an IRB bank.

[38] In our sample 7,159 firms have at least one IRB and one SA loan from two different IRB institutions. These firms have a total of 27,620 loans: 9,226 SA loans and 18,394 IRB loans (Table 1.1, Panel D).

outset of our sample period is not endogenous to a bank's decision to adjust these loans in a different way from other loans as a response to a crisis. We rule out concerns that this results in an absolute upward bias of δ, for two reasons. First, if the regulator allows a bank to decide on the order in which loan portfolios are shifted toward IRB, the bank would have an incentive to select the least risky portfolios first (the least risky portfolios would yield the lowest average PDs, and for these portfolios the reduction in capital charges due to the shift to IRB would be the greatest). We observe, however, that banks tend to reduce the outstanding amount of the least risky portfolios relatively less over a crisis. Thus, this selection concern would rather cause a bias against finding an IRB effect in Test 3. Second, the regulator requires banks to start with loan portfolios for which they have sufficient data in order to calibrate meaningful PD models (see Section 1.2.1). In doing so, the regulator respects the structure of banks' internal loan portfolios (which are generally based on industry classifications).[39] Consequently, at IRB banks, the classification of a given loan as IRB or SA at the beginning of our sample period depends on whether or not the loan is in a business segment where the bank is relatively active. Thus, our results would be biased if banks reduced loan portfolios in segments where they are more active relatively more over the crisis. In the latter empirical analysis we will show that this is not the case; if anything, in business segments where they are more active, banks reduce portfolios relatively less over the crisis. To sum up, any bias resulting from the selection of loan portfolios within IRB banks would be against finding an IRB effect in Test 3.

We can substantiate our argument by empirically testing for the determinants of the IRB/SA loan classification within IRB banks at the onset of our sample period. In Table 1.2, we show estimates of a probit model to examine the role of loan-specific as well as portfolio-specific determinants of the observed IRB/SA classification. The dependent variable is a dummy variable equal to 1 if the loan is subject to IRB. To measure a bank's activity in a given business segment, we sum the bank's loans to firms within the respective two-digit SIC industry sector and divide them by all loans in our sample to firms within this industry sector (*Loan share*). Column 1 shows that *Loan share* is an important determinant for the IRB/SA classification

[39] See Bundesbank (2005) for details.

Table 1.2: Classification of IRB/SA loans in 2008Q1

	Dependent variable: D(IRB loan)				
	(1)	(2)	(3)	(4)	(5)
Loan share	1.899***				3.645**
	(0.474)				(1.790)
Portfolio PD		–1.194			–6.713
		(2.671)			(6.465)
Log loans (pre-event)			0.072		0.048
			(0.053)		(0.038)
Log firm assets (pre-event)				0.081	0.067
				(0.064)	(0.060)
Firm ROA (pre-event)				0.631	0.607
				(0.409)	(0.403)
Firm Leverage (pre-event)				–0.080	–0.080
				(0.109)	(0.089)
Firm PD (pre-event)				0.485	0.758
				(1.929)	(1.908)
Bank dummies	YES	YES	YES	YES	YES
Observations	87,725	87,725	87,725	10,405	10,405
Pseudo R-squared	0.343	0.340	0.343	0.573	0.575

The table shows results for simple probit regressions. The dependent variable is equal to 1 if the loan from bank j to firm i is subject to IRB and equal to zero if the loan is subject to the standard approach. The regressions include only loans from IRB banks. Robust standard errors adjusted for clustering at the bank level are reported in parentheses. Note: * indicates statistical significance at the 10% level, ** at the 5% level and *** at the 1% level.

within an IRB bank at the outset of our sample. In line with the argument from above, loan portfolios in segments where the bank is more active were shifted first toward IRB within IRB institutions. In order to measure the risk of a given loan portfolio, we calculate the average firm-specific PD in each two-digit SIC industry sector (*Portfolio PD*). While the average risk of a loan portfolio enters the probit model negatively, the coefficient is not significant. In columns 3 and 4 we test for loan-specific determinants of the IRB/SA classification. Neither the loan size nor the firm balance sheet variables, such as firm size, ROA, leverage, or PD, significantly affect the observed IRB/SA selection within IRB banks. When we include all explanatory variables at the same time (column 5), *Loan share* is the only significant determinant for the classification of loan portfolios. Therefore we will include this variable in our main empirical tests, in order to observe whether the selection may account for a bias.

1.4 Empirical results

1.4.1 Loan-specific risk weights and lending

In this section we present results for Tests 1 to 3 described above. As noted, the dependent variable for these tests is the change in the log of lending over the crisis period. Specifically, we collapse all available quarterly data for a given loan into pre- and post-crisis event periods and look at the difference. All regressions are estimated with firm fixed effects to control for unobserved heterogeneity in the firm's demand for credit. Results for the three tests are shown in Table 1.3.

We start by estimating Equation (1.1) to see whether, over the crisis, a given firm borrowing from different banks experienced a larger reduction in loans from banks with a high fraction of IRB loans in their portfolios (Test 1).[40] Column 1 of Table 1.3 indicates that the larger the share of IRB loans within a bank, the more the bank reduces its lending relative to a bank with a lower share of IRB loans to the same firm. Since firm-specific credit demand shocks get absorbed by the firm fixed effects, the coefficient reflects differences in banks' credit supply. It is likely that banks were affected differently by the financial crisis, and therefore we add bank-level control variables in the estimates shown in column 2. The coefficient for $Share\ IRB$ is smaller but still significant. We can directly address potential selection concerns resulting from the order in which loan portfolios were shifted toward IRB by including the relative size of a loan portfolio (*Loan share*) as a control variable. The coefficient on *Loan share* is positive but not significant. If anything, in response to the crisis event banks tend to adjust loans and hence loan portfolios less drastically in business segments where they are relatively active. Thus, the classification of loans to IRB portfolios would bias our estimates against finding a significant effect of the choice of the regulatory approach on changes in lending following the crisis.

In column 3 we replace the variable *Share IRB* with *D(IRB bank)*, a dummy variable that takes the value 1 if a bank decided to become an IRB institution and zero otherwise. Thus, we consider the relative change in lending for a firm that has at least one lending relationship with an SA institution and one with an IRB institution.

[40] Technically, a loan is included as long as the firm is borrowing from two or more institutions where the share of IRB loans differs (see Section 1.3).

Table 1.3: Lending and regulatory approach

	Test 1			Test 2			Test 3				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Exit	Exit
Share IRB	–0.081***	–0.053**		–0.054**	–0.043						
	(0.020)	(0.021)		(0.022)	(0.038)						
D(IRB bank)			–0.032**			–0.030					
			(0.016)			(0.023)					
D(IRB loan)							–0.039***	–0.040**	–0.021*	0.044	0.027
							(0.012)	(0.016)	(0.011)	(0.035)	(0.028)
Loan share		0.133	0.111		0.390**	0.374*		0.145	0.078	0.163	–0.005
		(0.091)	(0.089)		(0.193)	(0.197)		(0.143)	(0.089)	(0.167)	(0.348)
Log bank assets (pre-event)		–0.011**	–0.011**		–0.007	–0.007		–0.018		–0.042***	
		(0.005)	(0.005)		(0.006)	(0.006)		(0.015)		(0.013)	
Bank equity ratio (pre-event)		–0.273	–0.171		–0.179	–0.157		0.540		–0.018	
		(0.376)	(0.397)		(0.433)	(0.435)		(1.264)		(1.195)	
Bank ROA (pre-event)		–0.003	–0.003		0.016	0.016		–0.107**		0.047	
		(0.017)	(0.016)		(0.011)	(0.011)		(0.052)		(0.080)	
D(state bank)		0.007	0.010		0.034*	0.037*		–0.018		–0.069*	
		(0.022)	(0.022)		(0.019)	(0.019)		(0.045)		(0.038)	
D(cooperative bank)		0.009	0.014		0.030	0.033*		–0.038		–0.097**	
		(0.017)	(0.017)		(0.018)	(0.018)		(0.026)		(0.039)	
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Bank FE	NO	NO	NO	NO	NO	NO	NO	NO	YES	NO	YES
Observations	93,370	93,370	93,370	49,492	49,492	49,492	27,620	27,620	27,620	27,620	27,620
R-squared	0.27	0.27	0.27	0.25	0.25	0.25	0.30	0.30	0.30	0.44	0.44

The table shows the relationship between the decline in loan size during the recent financial crisis and the regulatory approach used by the bank. We take the crisis shock in late 2008 as an event and collapse all quarterly data for a given loan into a single pre- and post-crisis period. Data are restricted to (a) loans that are larger than € 1.5 million (b) loans from commercial, state, or cooperative banks that are subject to the Basel II capital regulation (c) loans that have an observation in both the pre- and the post-crisis period. The dependent variable in columns 1 to 9 is the difference in log(loans) between the pre- and the post-crisis period. The dependent variable in columns 10 and 11 is a dummy that is equal to 1 if a loan that existed in the second quarter of 2008 ceased to exist following the crisis shock. In columns 1 to 3 we use observations for firms that have at least one loan from an SA bank and one loan from an IRB bank or loans from at least two distinct IRB banks and test whether banks with larger shares of IRB loans reduce lending to the same firm more over the crisis (Test 1). In columns 4 to 6 the sample is restricted to SA loans and includes only firms that have at least one SA loan from an SA bank and at least one SA loan from an IRB bank or SA loans from at least two distinct IRB banks (Test 2). Finally, columns 7 to 11 include only loans from IRB banks and only firms that have at least one SA loan and at least one IRB loan from an IRB bank (Test 3). All regressions include firm fixed effects in order to control for unobserved heterogeneity in firms' demand for credit. Columns 9 and 11 additionally include bank fixed effects to control for unobserved heterogeneity across banks. Robust standard errors adjusted for clustering at the bank level are reported in parentheses. Note: * indicates statistical significance at the 10% level, ** at the 5% level and *** at the 1% level.

Table 1.4: Lending and regulatory approach—OLS

	Test 1			Test 2			Test 3				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Δlog(loans)	Exit	Exit
Share IRB	–0.058***	–0.030*		–0.030	–0.034						
	(0.020)	(0.017)		(0.034)	(0.026)						
D(IRB bank)			–0.025*			–0.024					
			(0.014)			(0.015)					
D(IRB loan)							–0.050***	–0.025**	–0.025*	0.069*	0.028
							(0.016)	(0.010)	(0.013)	(0.040)	(0.040)
Loan share	0.198	–0.224	–0.231	0.354	0.224	0.205	0.211	–0.295	–0.312	0.569**	0.432
	(0.196)	(0.200)	(0.198)	(0.225)	(0.216)	(0.201)	(0.253)	(0.229)	(0.236)	(0.218)	(0.296)
Log bank assets (pre-event)	–0.010**	–0.004	–0.003	–0.010***	–0.005**	–0.005**	–0.016	0.001		0.002	
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.011)	(0.007)		(0.013)	
Bank equity ratio (pre-event)	–0.296	–0.341	–0.330	–0.246	–0.371	–0.350	0.471	0.025		0.796	
	(0.301)	(0.239)	(0.244)	(0.355)	(0.228)	(0.220)	(0.812)	(0.541)		(1.283)	
Bank ROA (pre-event)	–0.000	0.007	0.007	0.012	0.005	0.005	–0.104**	–0.007		–0.016	
	(0.015)	(0.009)	(0.009)	(0.011)	(0.008)	(0.008)	(0.038)	(0.026)		(0.114)	
D(state bank)	0.014	0.007	0.009	0.030*	0.026**	0.028**	–0.024	–0.008		–0.110**	
	(0.020)	(0.016)	(0.016)	(0.016)	(0.011)	(0.011)	(0.037)	(0.023)		(0.043)	
D(cooperative bank)	0.011	0.018	0.022	0.023	0.034***	0.037***	–0.035	–0.003		–0.134**	
	(0.018)	(0.014)	(0.015)	(0.017)	(0.012)	(0.012)	(0.023)	(0.017)		(0.058)	
Constant	0.213**	0.074	0.053	0.193***	0.092*	0.080	0.394	–0.048		0.694*	
	(0.104)	(0.078)	(0.075)	(0.073)	(0.056)	(0.058)	(0.317)	(0.190)		(0.403)	
Bank FE	NO	NO	NO	NO	NO	NO	NO	NO	YES	NO	YES
Observations	93,370	182,966	182,966	49,492	121,549	121,549	27,620	90,500	90,500	90,500	90,500
R-squared	0.005	0.006	0.006	0.003	0.003	0.003	0.003	0.003	0.008	0.038	0.059

The table shows the same tests as in Table 1.3, using OLS instead of the FE estimation. As before, we collapse our sample into single pre- and post-intervention time periods and use the change in log(loans) for a bank-firm relationship as the dependent variable. Columns 1, 4 and 7 restrict the sample to firms with multiple lending relationships as in Table 1.3, so that we are able to compare the coefficients from the FE estimation with the coefficient from the OLS regression. As OLS does not rely on identification within the same firm, we include also firms with only one lending relationships (or with only IRB loans or only SA loans) in columns 2 and 3, 5 and 6 and 8 to 11. In all specifications we control for bank size, capitalization, profitability and type as well as the bank's market share within the firm's two-digit SIC sector. Columns 9 and 11 additionally include bank fixed effects. Robust standard errors adjusted for clustering at the bank level are reported in parentheses. Note: * indicates statistical significance at the 10% level, ** at the 5% level and *** at the 1% level.

Again, we find a significant relative reduction in lending from IRB compared with SA institutions. The IRB effect is also economically meaningful: If we apply the estimate from column 3, IRB banks reduce their lending by 3.1 percent more than banks that have only SA loans.[41] These findings indicate that tailoring capital requirements to the risk of each individual asset based on an internal rating system increases the pro-cyclicality of lending.

Next, we turn to Test 2, where our sample includes SA loans made by both SA and IRB institutions. There are 10,496 firms that obtain SA loans from at least two different banks where the share of IRB loans differs.[42] Test 2 will show us whether IRB banks reduce their lending uniformly or differentiate between IRB and SA loans. If the regulatory approach chosen for a specific loan was the driving force behind our findings for Test 1, the estimate of β in Equation (1.1) for this sample of SA loans should be zero. If banks that introduced IRB were more severely hit by the financial crisis and this outcome drove our findings for Test 1, we should find a negative significant coefficient.

Results for Test 2 are shown in Table 1.3, columns 4 to 6. The share of IRB loans is significant when we do not control for bank characteristics (column 4). This implies that IRB institutions reduced their lending over the crisis more than SA institutions. However, this difference is driven by observable bank characteristics that are correlated with the choice of becoming an SA or an IRB institution. Once we control for these characteristics, the *Share IRB* coefficient is not significantly different from zero (column 5). This finding is also robust to using a dummy variable indicating an IRB institution instead of the variable *Share IRB* (column 6). As before, firm fixed effects absorb any firm-specific credit demand shocks and ensure that we are comparing changes in lending to a given firm. The comparison of Tests 1 and 2 supports our claim that it is indeed the regulatory approach that is responsible for the stronger reduction of loans from IRB banks.

To remove any remaining concern that our findings are driven by bank heterogeneity, we identify the effect of risk-specific capital charges on lending within IRB banks. Identification within IRB banks is possible, as these institutions had not yet

[41] The effect is equal to $exp(\beta) - 1$ (Halvorsen and Palmquist 1980).

[42] These are all firms with at least two SA loans that are not only from SA institutions.

switched all their loan portfolios to IRB before the crisis. Thus, the IRB banks have loans under IRB as well as loans under SA. Further, there are firms that have an IRB loan from one bank and an SA loan from another IRB bank. For Test 3, we restrict the sample to loans from IRB banks and investigate whether these banks reduced their IRB loans more than their SA loans during our sample period. Identification in the regression with firm fixed effects requires that the firm has at least one SA loan from an IRB bank and at least one IRB loan from an IRB bank. Our sample contains 7,159 firms with 27,620 loans that fulfill this condition (Table 1.1, Panel D). Results for Test 3 are reported in columns 7 to 9 of Table 1.3 and show a significantly negative coefficient for the IRB loan dummy. The effect is robust to the inclusion of bank-level control variables in column 8 and also economically significant: Within the same firm, loans for which IRB is used are reduced by about 3.9 percent more than loans for which the SA approach is used. Since Test 3 is on the loan level, we can systematically account for bank heterogeneity by including bank fixed effects (column 9). We still find a significant effect of the regulatory approach on changes in lending to the same firm.[43] This provides evidence that the regulatory approach used for a certain loan has a strong and economically meaningful influence on the extent to which the loan was "crunched" during the recent crisis. Increases in risk weights during economic downturns force capital-constrained banks that use the internal ratings-based approach to deleverage in order to fulfill their capital requirements (recall Figure 1.2). Results for Test 3 indicate that they do so by reducing the very assets that caused the increase in capital requirements: Loans that are subject to IRB. Loans that are subject to the standard approach, on the other hand, are relatively less affected.

Our findings so far focus on changes in the loan volume of existing lending relationships, which we call the intensive margin. We can also test whether IRB banks are more likely to end an existing relationship entirely if the loan is subject to IRB as compared with SA, the extensive margin. In columns 10 and 11 of Table 1.3 the dependent variable is a dummy variable equal to 1 for loans that existed in the second

[43] The estimation strategy in column 9 is similar to the one developed by Jiménez et al. (2013a): While they use quarterly data and include time × firm and time × bank fixed effects, we collapse our data on the time dimension and therefore include firm and bank fixed effects (see Khwaja and Mian 2008).

quarter of 2008 but that ceased to exist at some point following the crisis shock. The coefficient for $D(IRB\ loan)$ is positive, but insignificant, indicating that the effect of the regulatory approach is less pronounced on the extensive as compared with the intensive margin of lending. This is consistent with a finding by Jiménez et al. (2013b), who argue that the somewhat moderated effect on the extensive margin is due to a time lag, since lending relationships end only when all loans—including those with a longer maturity—are fully repaid.

In Table 1.4 we replicate regressions from Table 1.3 using an ordinary least squares (OLS) specification without firm fixed effects. The main advantage of reporting OLS results is that it allows us to include firms that have only one bank relationship. We begin with OLS estimates that use only those firms that were included in the regressions in Table 1.3 to make our findings comparable (columns 1, 4, and 7). The coefficients remain relatively stable and are comparable to the coefficients from the fixed effects regressions in Table 1.3. Thus, firm loan demand does not appear to differ for firms borrowing from IRB or SA banks (or for IRB or SA loans within IRB banks). We next include single-relationship firms and firms that borrow only from SA institutions in columns 2, 5, and 8 of the table. For Test 1, the coefficient for the IRB variable (*Share IRB*) remains significant but increases from –0.058 to –0.030. The effect of the regulatory approach seems to be less pronounced for single-bank firms. Single-relationship firms are less able to replace IRB loans if lending terms deteriorate.[44] Results for Test 3 point in the same direction: The coefficient for the IRB loan dummy increases from –0.050 in column 7 to –0.025 in column 8. For Test 2, the OLS specification does not find a significant difference between banks using the standard approach and banks using the IRB approach and hence confirms the finding from the fixed effects estimation. Finally, if we use *Exit* instead of the change in lending as a dependent variable, the coefficient for the IRB loan dummy is weakly significant in column 10, but becomes insignificant when bank fixed effects are added in column 11. Overall, the impression from Table 1.3 that the effect of the regulatory approach is moderated on the extensive margin is confirmed in Table 1.4.

[44]In Section 1.4.2 we will investigate whether firms' loan costs actually increased over the crisis.

1.4.2 Capital regulation and firms' overall access to funds

In the previous section we showed that—compared to SA loans—IRB loans were reduced relatively more over the crisis and that this effect was independent of any firm or bank characteristics. It is a priori unclear how this result would affect firms' overall access to funds. On the one hand, the OLS results with all firms showed that the reduction in the quantity of IRB loans is smaller for firms with a single banking relationship. That is, firms with multiple relationships may have some ability to compensate with other lending relationships (i.e., increase lending with SA loans when banks adjust their IRB loans). On the other hand, if larger loans are reduced relatively more, the magnitude of the documented pro-cyclical effect could be considerably larger at the aggregate firm level as compared with the individual loan level. In this section we turn our attention directly to the overall access to funds by firms. Did firms relying on IRB loans experience a stronger reduction in loan supply? The question is central for evaluating the real economic effects of capital charges based on credit-specific risks. In this section we examine how the regulatory approach used for loans affects the aggregate supply of loans to a firm and the average borrowing costs for the firm.

Importantly, firms that borrow (mostly) from IRB or SA banks might also differ in their loan demand over the crisis (e.g., owing to size differences). As we examine the change in firms' total outstanding loans during the crisis period, we have only one observation per firm and hence cannot include firm fixed effects. The variation in the IRB/SA loan classification within IRB banks during this period allows us to directly address this issue. Specifically, we restrict the sample to firms that have both SA loans and IRB loans from IRB banks and define a variable *Share (IRB-IRB loans)* as the share of a firm's loans from IRB banks that are subject to the internal ratings-based approach. Firms in this subsample should be relatively similar, and this additional variable allows us to investigate whether—among these firms—those with a larger share of IRB loans experience a greater reduction in aggregate loans over the crisis.[45]

[45] Aggregate loans also include loans from SA banks that are not taken into account for the definition of the variable *Share (IRB-IRB loans).* However, loans from IRB banks account for 85.4 percent of total loans for the average firm in the restricted sample. The remainder of 14.6 percent loans from SA banks simply adds noise and thus prevents us from finding a significant impact

Using the matched firm sample for which we have balance sheet information, we are able to estimate the following firm-level equation:

$$\Delta\log(\text{total firm loans})_i = \beta \times \text{Share (IRB-IRB loans)}_i + X_i'\gamma + \epsilon_i \tag{1.3}$$

The dependent variable is the difference in the logarithm of a firm's total loans over the crisis. As in the loan-level regressions we collapse our data into single pre-event and post-event time periods by taking time averages of a firm's total loans. The coefficient of interest is β, which shows how *Share (IRB-IRB loans)* , the share of a firm's loans from IRB banks that are subject to the internal ratings-based approach, affects the change in the firm's total loans over the crisis. Firm-level control variables include the logarithm of the firm's pre-shock total assets, the firm's pre-shock ROA, and the firm's pre-shock leverage defined as the ratio of total debt to total assets. Additionally, we control for differences across a firm's lenders by including weighted averages of the lending banks' characteristics (i.e., pre-shock logarithm of assets, equity ratio, and ROA).[46] Finally, standard errors in the firm-level regressions are clustered by firms' main lender.

Total loans for the average firm declined by 7.8 percent over the crisis event (Table 1.1, Panel C). The reduction on the firm level is larger than the reduction for the average loan (–3.8 percent; Table 1.1, Panel B), suggesting that larger loans are reduced relatively more over the crisis.[47] We proceed by estimating *Share (IRB loans)*, the effect of the firm-level share of IRB loans on aggregate loans outstanding for all 107,025 sample firms (column 1 of Table 5.8).[48] Firms receiving larger shares of IRB loans prior to the crisis experienced larger reductions in total borrowing following the recession. The coefficient remains highly significant and even increases in magnitude if we include variables that control for bank-level characteristics of a firm's lenders (column 2). Further, the effect is robust to the inclusion of firm-level control variables in the matched sample of 7,778 firms (columns 3 and 4).

of the share of a firm's loans from IRB banks that are subject to IRB on changes in total firm loans.

[46] We use the amount that the firm borrows from a certain bank divided by the total loans of the firm prior to the crisis as a weight to calculate firm-level bank characteristics.

[47] See Section 1.5.2 for a detailed examination of this issue.

[48] This sample includes single-relationship firms as well as firms that have only SA loans or only IRB loans. For these firms, *Share (IRB loans)* is equal to zero or 1, respectively.

Importantly, the impact of the regulatory classification of a firm's loans on changes in the firm's aggregate loans is economically meaningful. For example, the coefficient in column 2 implies that a firm borrowing only IRB loans experienced a reduction in total loans that is on average 5.4 percent larger than the reduction for a firm that borrowed only SA loans. This magnitude increases to 10.7 percent in the matched sample where we control for firm level characteristics (column 4). Note that this magnitude is larger than the previously identified effect on the loan level (between 2.1 and 4.9 percent for Test 3). The reason for this is that large IRB loans are reduced the most following the crisis event (see Section 1.5.2 for details). A stronger reduction of larger IRB loans relative to SA loans results in a disproportionally large reduction of a firm's total loans if these loans are mostly classified as IRB.

To address potential differences in loan demand by firms that borrow from SA and IRB institutions, we restrict the sample to firms that have both SA and IRB loans from IRB banks.[49] Reducing the sample in this way allows us to mitigate concerns regarding firm demand, since the classification of each loan depends on the bank-specific implementation plan as outlined in Section 1.2. Estimating Equation (1.3), we identify a significant impact of the share of a firm's loans from IRB banks that are subject to IRB on changes in firms' aggregate lending (Table 5.8, column 5). Again, the result is robust to the inclusion of weighted bank-level characteristics as well as firm-level characteristics in a matched sample (columns 6 to 8). In order to assess the economic magnitude of the coefficient we compare the firms at the 25^{th} and the 75^{th} percentile of the distribution for *Share (IRB-IRB loans)* .[50] Based on the coefficient in column 6, the firm that has relatively more IRB loans experiences a reduction in total loans that is 4.5 percent larger than the reduction for the firm that has relatively more SA loans. It is worth noting that this result provides strong evidence for a causal effect of the regulatory classification of loans within IRB banks on aggregate firm loans following the crisis. While we consider only firms that borrow from different IRB banks in this test, we can identify a significant difference in aggregate firm borrowing that depends on how many of the firm's loans had already

[49]Note that these are precisely the 7,159 firms that we use for identification in Test 3 (Section 1.4.1).

[50]The firm at the 25^{th} percentile has 42.6 percent IRB loans while the firm at the 75^{th} percentile has 81.7 percent IRB loans. The variable takes values between but excluding zero and one as we require that firms in this test have both SA loans and IRB loans from IRB banks.

Table 1.5: Firm-level outcomes

	Dependent variable: Δ log(total firm loans)								Dependent variable: Δ capital cost			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Share (IRB loans)	–0.051***	–0.056***	–0.086***	–0.113***					0.0020***	0.0017		
	(0.015)	(0.018)	(0.024)	(0.029)					(0.0008)	(0.0011)		
Share (IRB-IRB loans)					–0.099*	–0.117**	–0.097**	–0.078*			0.0011	0.0015
					(0.057)	(0.050)	(0.043)	(0.040)			(0.0062)	(0.0061)
Log bank assets (pre-event)		0.006***		0.003*		0.003**		–0.003		–0.0001		–0.0001
		(0.001)		(0.002)		(0.001)		(0.003)		(0.0001)		(0.0004)
Bank equity ratio (pre-event)		0.641*		–0.334		–1.509*		–0.537		–0.0380		0.2400
		(0.335)		(0.510)		(0.913)		(2.001)		(0.0452)		(0.2697)
Bank ROA (pre-event)		–0.005		–0.019		–0.054		0.022		–0.0017		–0.0067
		(0.016)		(0.024)		(0.074)		(0.088)		(0.0019)		(0.0114)
Log firm assets (pre-event)			–0.008	–0.008			0.038***	0.036***	0.0006*	0.0003	–0.0027	–0.0025
			(0.007)	(0.007)			(0.009)	(0.009)	(0.0004)	(0.0003)	(0.0019)	(0.0019)
Firm ROA (pre-event)			–0.062	–0.070			–0.134	–0.128	–0.0013	–0.0015	0.0312*	0.0319*
			(0.059)	(0.059)			(0.138)	(0.138)	(0.0034)	(0.0034)	(0.0183)	(0.0180)
Firm leverage ratio (pre-event)			–0.102***	–0.109***			–0.230**	–0.188	–0.0075***	–0.0062***	–0.0065	–0.0096
			(0.035)	(0.035)			(0.115)	(0.114)	(0.0022)	(0.0021)	(0.0115)	(0.0111)
Constant	–0.060***	–0.214***	0.020	–0.021	–0.118***	–0.125***	–0.545***	–0.475***	–0.0072*	0.0006	0.0366*	0.0308
	(0.004)	(0.041)	(0.071)	(0.083)	(0.031)	(0.044)	(0.101)	(0.100)	(0.0040)	(0.0034)	(0.0216)	(0.0227)
Observations	107,025	107,025	7,778	7,778	7,159	7,159	1,575	1,575	4,977	4,977	1,273	1,273
R-squared	0.003	0.006	0.011	0.012	0.003	0.006	0.026	0.028	0.006	0.010	0.0044	0.0055

This table reports firm-level results for the full sample of 107,025 firms and for a matched sample that includes up to 7,778 firms. As before we collapse our data into single pre- and post-crisis time periods. The dependent variable is the change in the logarithm of a firm's total amount of loans in columns 1 to 8 and the change in a firm's capital cost (defined as interest paid over total loans) in columns 9 to 12. The variable *Share (IRB Loans)* gives the share of a firm's loans that are subject to IRB. The variable *Share (IRB-IRB loans)* gives the share of a firm's loans received from IRB banks that are subject to IRB. For tests that include this variable the sample is restricted to firms that have at least one IRB loan and at least one SA loan from an IRB bank: I.e., firms for which the variable *Share (IRB-IRB loans)* takes values unequal to 0 or 1. We gradually include firm-level and weighted bank-level control variables. Robust standard errors adjusted for clustering at the main bank level are reported in parentheses. Note: * indicates statistical significance at the 10% level, ** at the 5% level and *** at the 1% level.

been transferred into IRB portfolios before the crisis event. This suggest that firms are unable to compensate for the reduction in IRB lending by switching to SA loans.

Finally, we investigate whether the regulatory classification of loans had an impact only on lending quantities or also on the price of lending. Since the documented increase in risk-weighted assets induced an increase of the capital charges for a given euro lent out, banks might also react by increasing the interest rate charged on IRB loans. While we cannot do this analysis on the loan level (since the German credit register does not report interest rates), we can link changes in the ratio of aggregate interest expense to loans from firms' balance sheets and income statements to their share of IRB loans before the crisis. Regression results are presented in columns 9 to 12 of Table 5.8. Column 9 shows that firms obtaining a larger share of IRB loans experience a greater increase in capital cost over the crisis. This effect, however, vanishes once we include (weighted) bank-level controls in column 10. In columns 11 and 12 we reduce the sample to firms that have both SA and IRB loans from IRB banks in order to account for potential differences in firm demand (see above). The coefficient for the IRB variable in the reduced sample, *Share (IRB-IRB loans)*, is insignificant in both regressions. Overall, we find only weak evidence that firms that obtain more IRB loans experience greater increases in capital costs over the crisis. Our results suggest that banks mostly reacted to the crisis event by adjusting the amounts of IRB loans outstanding in order to deleverage and fulfill regulatory requirements.

1.5 Further evidence: The impact of bank, loan, and firm characteristics

In Section 1.4.1 we showed that after the crisis shock banks reduced lending on IRB loans more than on SA loans. We argued that the underlying cause of this finding is that IRB loans require banks to increase their regulatory capital if PDs increase during a recession, whereas changes in the PD of SA loans have no effect on banks' regulatory capital requirements. Thus, we expect a stronger reaction to the crisis event (a) by banks with a low capital ratio before the recession; (b) for large IRB loans that have a substantial impact on capital charges when their PD deteriorates; and (c) for loans whose PD increases relatively more than others. In this section we present evidence for these three relationships.

1.5.1 The lending reaction of IRB banks: The role of bank equity

Hellwig (2010) argues that banks reduced buffers over minimum capital requirements to a bare minimum in an attempt to "economize on equity" prior to the crisis and that there was only limited scope for raising additional equity during the crisis. Thus, we would expect that banks that have lower capital ratios and are hence closer to the regulatory minimum will react more when capital requirements increase because of the crisis. We test this by creating a dummy variable that separates the IRB banks into those with a lower than median and those with a higher than median pre-shock equity ratio and estimate the following equation:

$$\begin{aligned} \Delta\log(\text{loans})_{ij} &= \beta_1 \times \text{Share IRB}_j + \beta_2 \times \text{D(low equity)}_j \\ &+ \beta_3 \times \text{Share IRB}_j \times \text{D(low equity)}_j + X_j'\gamma + \epsilon_{ij} \end{aligned} \tag{1.4}$$

In an alternative specification we include an interaction between banks' initial equity ratio and the variable $Share\ IRB$ instead of the interaction with the dummy variable. In principle, it would be possible to include firm fixed effects in this equation to control for firm-specific credit demand shocks as before. However, such a specification requires that each firm has at least four banking relationships in order to identify the coefficients.[51] As the results from OLS and fixed effects regressions in Section 1.4.1 are very similar, we estimate Equation (1.4) with OLS.

Results are reported in Table 1.6. The significantly negative coefficient for the interaction term in column 1 indicates that the effect of the regulatory approach is

[51] For example, if the firm had only two loans, one from an IRB bank with low equity according to the dummy variable, and one SA bank with high equity, it would be impossible to say whether a potential difference in the change in lending of these banks was due to the regulatory approach or the capitalization of the banks. In order to clearly identify the effects, one would require the firm to have at least one loan from an IRB bank with low capital, one loan from an IRB bank with high capital, one loan from an SA bank with low capital and one loan from an SA bank with high capital.

Table 1.6: Bank capitalization, regulatory approach, and lending

	Dependent variable: Δ log(loans)					
	Test 1		Test 2		Test 3	
	(1)	(2)	(3)	(4)	(5)	(6)
Share IRB × D(low equity)	–0.207**		–0.064			
	(0.081)		(0.102)			
Share IRB × bank equity ratio (pre-event)		1.897*		–1.048		
		(1.121)		(1.368)		
D(IRB loan) × D(low equity)					–0.046**	
					(0.020)	
D(IRB loan) × bank equity ratio (pre-event)						2.117*
						(1.057)
Share IRB	–0.016	–0.104**	–0.009	0.016		
	(0.014)	(0.045)	(0.023)	(0.047)		
D(IRB loan)					–0.017	–0.102***
					(0.012)	(0.030)
Constant	0.132**	0.096	0.071	0.062	0.141	0.121
	(0.065)	(0.080)	(0.064)	(0.063)	(0.162)	(0.181)
Bank controls	YES	YES	YES	YES	YES	YES
Observations	182,966	182,966	121,549	121,549	90,500	90,500
R-squared	0.006	0.005	0.002	0.002	0.004	0.003

These regressions examine the impact of the regulatory approach on the change in lending for banks of differing levels of capitalization. As before, our quarterly data are collapsed into single pre- and post-crisis time periods and the dependent variable is the change in log(loans) for a bank-firm relationship. We include all loans in columns 1 and 2 (Test 1), only SA loans in columns 3 and 4 (Test 2), and only loans from IRB banks in columns 5 and 6 (Test 3). The *Dummy(low equity)* has the value 1 for banks with equity ratios below the median and zero otherwise. It is interacted with the share of IRB loans within the bank in columns 1 and 3 and with the IRB loan dummy in column 5. Alternatively, we include an interaction between the bank's initial equity ratio and the respective IRB variable in columns 2, 4, and 6. Bank control variables are the same as in previous tables. All variables that are included in interactions terms are also included on their own. Robust standard errors adjusted for clustering at the bank level are reported in parentheses. Note: * indicates statistical significance at the 10% level, ** at the 5% level and *** at the 1% level.

more pronounced for those IRB banks that had below median capital ratios prior to the crisis. Similarly, the positive coefficient for the interaction in column 2 shows that the negative effect of a higher share of IRB loans within the bank is mitigated by a higher pre-shock capitalization of the bank. This is consistent with our argument since banks with a high share of IRB loans and low capitalization are likely to experience larger increases in risk-weighted capital requirements and to react more strongly. Again, Test 2 and Test 3 show that this result is driven by the regulatory approach. While we do not find significant differences across banks in the sample of SA loans (columns 3 and 4), columns 5 and 6 show that among IRB banks the IRB loans from banks with relatively little equity are reduced the most over the crisis. The higher the initial capitalization of the bank, the less pronounced the relatively stronger reduction of IRB loans as compared to SA loans.

1.5.2 The lending reaction of IRB banks: The role of loan size

Next, we investigate whether the size of a loan has an influence on how it is affected during the crisis. For example, increases in risk weights for larger loans result in larger increases in required capital and hence banks are likely to respond more. For this purpose we calculate the bank's exposure to each loan by dividing the loan amount prior to the crisis by the bank's pre-shock total assets. An identical increase in risk weights would translate into a larger increase in required capital for loans to which the bank has a higher exposure. The same argument is true if we take the absolute size of the loan instead of the relative exposure of the bank as a criterion. We then generate two dummy variables taking the value 1 if the exposure of the bank to a certain loan is larger than the median of our sample or if the loan is larger than the median loan in our sample, and zero otherwise.

We replace the equity variables in Equation (1.4) with the exposure and loan size dummies and show the results in Table 1.7. Column 1 shows that banks reduced loans relatively more over the crisis when they had a large exposure prior to the crisis, and that this is particularly true for banks with a high share of IRB loans that are vulnerable to increases in risk weights. The same result is obtained if we use loan size instead of relative exposure as shown in column 2: Banks with higher shares of IRB loans reduce larger loans relatively more. Also, columns 3 and 4 show that among SA loans relatively larger loans are reduced relatively more. However, as before there is no significant difference between banks with different shares of IRB loans. Finally, columns 5 and 6 report results for Test 3. Among the sample of loans from IRB banks, loans to which the bank has a higher exposure are reduced relatively more, and this is especially true of IRB loans for which increases in risk weights translate into higher capital requirements for the bank.

1.5.3 The lending reaction of IRB banks: The role of firm risk

In this section we investigate the influence of firm risk on bank lending. So far we have shown that firms' PDs increase during a recession and therefore capital requirements

Table 1.7: Loan cross-section

	Dependent variable: Δ log(loans)					
	Test 1		Test 2		Test 3	
	(1)	(2)	(3)	(4)	(5)	(6)
Share IRB × D(high exposure)	–0.133***		–0.045			
	(0.039)		(0.044)			
Share IRB × D(large loan)		–0.180***		–0.050		
		(0.061)		(0.044)		
D(IRB loan) × D(high exposure)					–0.115**	
					(0.045)	
D(IRB loan) × D(large loan)						–0.133***
						(0.045)
Share IRB	–0.012	0.086**	–0.022	0.018		
	(0.021)	(0.039)	(0.024)	(0.034)		
D(IRB loan)					–0.013	0.066**
					(0.011)	(0.027)
D(high exposure)	–0.143***		–0.113***		–0.181***	
	(0.018)		(0.017)		(0.042)	
D(large loan)		–0.111***		–0.114***		–0.150***
		(0.009)		(0.008)		(0.018)
Constant	0.818***	0.020	0.585***	0.070	1.142***	0.121
	(0.093)	(0.063)	(0.060)	(0.045)	(0.226)	(0.163)
Bank Controls	YES	YES	YES	YES	YES	YES
Observations	182,966	182,966	121,549	121,549	90,500	90,500
R-squared	0.020	0.043	0.009	0.025	0.023	0.049

These regressions examine whether the impact of the regulatory approach differs across loan characteristics. As before, our quarterly data are collapsed into single pre- and post-crisis time periods and the dependent variable is the change in log(loans) for a bank-firm relationship. We include all loans in columns 1 and 2 (Test 1), only SA loans in columns 3 and 4 (Test 2) and only loans from IRB banks in columns 5 and 6 (Test 3). We calculate a bank's exposure to a certain loan by dividing its amount by the bank's total assets prior to the crisis and separate our sample into those loans where the exposure of the bank is lower than median and those where it is higher than median. The resulting dummy variable is interacted with the share of IRB loans within the bank in columns 1 and 3 and with the IRB loan dummy in column 5. Alternatively, we generate a dummy that takes the value 1 for loans that are larger than the median loan in our sample and interact it with the respective IRB variables in columns 2, 4, and 6. Robust standard errors adjusted for clustering at the bank level are reported in parentheses. Note: * indicates statistical significance at the 10% level, ** at the 5% level and *** at the 1% level.

increase for IRB loan portfolios. For the empirical analysis we assumed that increased PDs affect all IRB loans uniformly. However, a recession hits firms heterogeneously. The capital requirements would increase most for those firms whose PD increases the most. Relating changes in the PD to changes in lending is, however, problematic. A bank's lending decision has a direct impact on firms' leverage, and this again is a key determinant of a firm's PD. Therefore we need to find a firm characteristic that is likely to predict future changes in PD but is not directly affected by the banks' lending decision.

Our measure of a firm's likelihood of experiencing an increase in its PD is its pre-shock profitability, measured by its ROA in 2007/2008. Current profitability is an important predictor of the firm's future PD, and it will be observed by the bank's analysts. We are able to obtain data on firm ROA from a matched firm sample with a total of 17,332 loans to 4,906 firms that borrow from at least two banks with a different share of IRB loans.

In Panel A of Table 1.8 we start with estimates of Equation (1.1) for the matched firm sample, which comprises about 20 percent of the original sample. The coefficient for the IRB variable is –0.045, which is very close to the estimate with the full sample (Table 1.3, column 2), indicating that our matched subsample is representative. Firm fixed effects are included and absorb firm-specific shocks to credit demand as before. We are able to include firm fixed effects because we are making comparisons across firms and not across banks or across loans. We are therefore able to split our sample into firms of different riskiness. We divide the sample into firms with a lower than median ROA and firms with a higher than median ROA and run the same regression on each subsample. As expected, the coefficient for the IRB variable is smaller for firms that are relatively less profitable prior to the crisis (column 2). It is larger and insignificant for the more profitable firms, as shown in column 3. Similar results are obtained if we use the loan-level instead of the bank-level IRB variable. Within the same firm, IRB loans are reduced more than SA loans, especially if the firm has a relatively low ROA prior to the crisis.[52]

In Panel B of Table 1.8 we split the sample according to firms that experienced a negative change and those that experienced a positive change in PD during the crisis. As discussed above, this test is likely to suffer from endogeneity bias. Nevertheless, to demonstrate the plausibility of our categorization by pre-crisis ROA in Panel A, we also show results based on the actual change in PD. Since banks do not report PDs together with SA loans, we need to generate model-based PDs. We therefore use a credit risk model developed by Förstemann (2011) that applies firm balance sheet

[52]Unfortunately, there are not enough firms in the matched sample that have at least one SA loan and one IRB loan from an IRB bank, which would be the pre-condition for Test 3. Similarly, there are not enough firms that have SA loans from both SA banks and IRB banks in the matched sample. To circumvent this problem columns 4 to 6 also include SA loans from SA banks and test whether the regulatory approach used for a certain loan has an impact on how the loan is affected by the crisis. Bank-level control variables are included in all regressions to account for systematic differences across banks.

Table 1.8: Firm cross-section

Panel A: Firm ROA						
	Dependent variable: Δ log(loans)					
	Test 1			Test 3		
	(1)	(2)	(3)	(4)	(5)	(6)
	All firms	Low ROA	High ROA	All firms	Low ROA	High ROA
Share IRB	–0.045**	–0.063**	–0.022			
	(0.023)	(0.030)	(0.026)			
D(IRB loan)				–0.032**	–0.043**	–0.018
				(0.013)	(0.020)	(0.019)
Bank controls	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Observations	17,332	8,718	8,614	14,460	7,130	7,330
R-squared	0.362	0.361	0.364	0.324	0.321	0.329

Panel B: Change in firm PD						
	Dependent variable: Δ log(loans)					
	Test 1			Test 3		
	(1)	(2)	(3)	(4)	(5)	(6)
	All firms	PD up	PD down	All firms	PD up	PD down
Share IRB	–0.046**	–0.062***	–0.031			
	(0.022)	(0.024)	(0.028)			
D(IRB loan)				–0.033**	–0.041***	–0.028
				(0.013)	(0.015)	(0.018)
Bank controls	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Observations	16,154	8,018	8,136	13,445	6,646	6,799
R-squared	0.369	0.389	0.380	0.330	0.348	0.343

This table reports results for a matched sample of up to 17,332 loans to 4,906 firms for which we are able to obtain balance sheet information and that borrow from at least one SA bank and one IRB bank or at least two distinct IRB banks (Test 1) or that have at least one SA loan and one IRB loan (Test 3). As before, our quarterly data are collapsed into single pre- and post-crisis time periods and the dependent variable is the change in log(loans) for a bank-firm relationship. All regressions include firm fixed effects and the bank-level control variables from above. In Panel A, we separate our sample of firms into those with a lower than median pre-shock ROA and those with a higher than median pre-shock ROA and estimate Equation (1.1) (columns 1 to 3) and Equation (1.2) (Test 3) on each subsample. Similarly, Panel B divides our sample into firms for which the probability of default increased over the crisis and firms for which it decreased and runs separate regressions on each subsample. Robust standard errors adjusted for clustering at the bank level are reported in parentheses. Note: * indicates statistical significance at the 10% level, ** at the 5% level and *** at the 1% level.

information in order to calculate firm-specific PDs.[53] As in the ROA regressions, we split the sample into firms where the PD increased over the crisis and firms where it decreased.

The results in Panel B are very similar to the results in Panel A: While banks with a higher share of IRB loans reduce lending to the same firm relatively more, this result seems to be driven by firms for which the PD increased over the crisis

[53]See Section 1.2.2 for details.

(column 2). The coefficient for firms with a decrease in PD is smaller in absolute terms and insignificant. A similar result is obtained for the loan-level IRB variable in columns 4 to 6.

In summary, our results show that the effect of a reduction of lending by IRB banks or a reduction of IRB loans is especially pronounced for IRB institutions with a low level of equity. Furthermore, IRB banks tend to ration loans that are large and also loans to firms that are likely to experience an increase in PD. These findings provide further support for our claim and underline the transmission channel through which the introduction of internal ratings affects banks' loan supply.

1.6 Conclusion and discussion

In this paper we overcome complex identification issues and estimate how capital regulation based on individual asset risk affects bank lending. We employ the gradual introduction of Basel II in Germany as a laboratory and use the recession shock following the collapse of Lehman Brothers in September 2008 as an event that increased credit risk in the German market for corporate loans. Our main finding is that the pro-cyclicality of capital charges based on individual asset risk has a significant effect on the lending behavior of banks as well as a considerable effect on firms' aggregate ability to borrow. For a given firm, loans by different IRB banks are reduced by 3.7 percent more when internal ratings (IRB) instead of fixed risk weights (SA) are used to determine capital charges. Since banks tend to ration large IRB loan exposures relatively more, the effect is even stronger on the aggregate firm level: Firms that had only IRB loans prior to the crisis experienced a reduction in total loans that was 5 to 10 percent larger than the reduction for firms that had only SA loans.

Our findings have important policy implications for the design of bank capital regulation. The new Basel III framework includes measures that are meant to address the problem of pro-cyclicality. Most important, Basel III introduces a countercyclical capital buffer that requires banks to build up additional capital reserves in times of excessive credit growth which can be used to satisfy capital requirements when economic conditions deteriorate. Our findings could be interpreted as justification for such a measure. However, countercyclical capital buffers reduce pro-cyclicality only if the supervisor has sufficient foresight about future economic conditions.[54] By definition, the regulator cannot anticipate unexpected shocks to credit risk (e.g., shocks that originate abroad such as the one analyzed in this paper) and therefore cannot always pre-empt such shocks by setting buffer rates accordingly. Furthermore, Basel III introduces capital conservation buffers. While these buffers are not per se anti-cyclical, they are meant to address the problem of pro-cyclicality by reducing the pressure on banks to deleverage when economic conditions deteriorate. However, they also do not solve the basic problem of pro-cyclical capital requirements, as their release has severe consequences for banks, and markets might not accept lower capital ratios when economic conditions deteriorate.[55] One solution to the problem of pro-cyclicality would be the introduction of a simple leverage ratio. While this would solve the problem of pro-cyclicality, the link between capital charges and actual asset risk would vanish.[56]

Asset-risk-specific capital regulation, the most important feature of both Basel II and Basel III, has an inherent problem of pro-cyclicality. Our results show for the first time that pro-cyclical capital charges under the model-based approach affect both bank lending and firms' access to funds. Moreover, in our view the measures introduced in the Basel III framework are insufficient to fully address this problem. To make a final judgment regarding the efficiency of risk-based capital requirements requires further research on the costs and benefits of this regulatory approach.

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[54]In particular, the buffer works only if the credit-to-GDP ratio (or any other measure the supervisor might use in order to define "excessive credit growth") is a sufficient statistic for future economic conditions.

[55]Capital conservation buffers are buffers on top of the minimum capital requirements. Once the bank's capital ratio is below the sum of the minimum requirement and the capital conservation buffer, it faces restrictions on dividend payments. These restrictions remain in place until the buffer is replenished.

[56]In Europe, a leverage ratio will tentatively be introduced in 2017. However, its currently discussed level, 3 percent of total (unweighted) assets, is rather low, so that it would serve only as a backstop against excessive leverage. Risk-based capital requirements would remain the binding ones in most cases.

Chapter 2

Setting Countercyclical Capital Buffers Based on Early Warning Models: Would it Work?

2.1 Introduction

Being faced with the longest and most severe financial crisis in decades, policy makers around the globe have actively searched for policy tools which could help to prevent or at least reduce the intensity of future financial crises. A tool that is an integral part of the Basel III regulations and the EU Capital Requirements Directive (CRD IV) is the countercyclical capital buffer (CCB), which has been proposed by the Basel Committee on Banking Supervision (BCBS) at the Bank of International Settlements (BIS).

The CCB aims to increase the resilience of the banking system in case of a financial crisis by ensuring that banks set aside capital in times of *"aggregate growth in credit [...] associated with a build-up of systemic risk"*, which can be *"drawn down during stressed periods"* (EU 2013). In order to promote international consistency in setting CCB rates, the BCBS has developed a methodology based on the ratio of aggregate credit to GDP (Basel Committee on Banking Supervision 2010). The CRD IV, while acknowledging the importance of credit growth and the credit-to-GDP ratio, specifies that buffer rates should also account for *"other variables relevant to* *the risks to financial stability"* (EU 2013).[1] This provides the motivation for this paper: We assess the usefulness of credit and other macro-financial variables for the prediction of banking sector vulnerabilities in a multivariate framework, hence enabling a more informed decision on the setting of CCB rates.

The BCBS guidelines are based on an analysis that uses a sample of 26 countries from all over the world, for which the credit-to-GDP gap (defined as the deviation of the credit-to-GDP ratio from its long-term trend) performs as the best single indicator in terms of signaling a coming financial crisis. However, from the evidence presented by the BCBS it is not clear whether the credit-to-GDP gap provides a warning signal that is early enough to account for the 12 months implementation period for raising the capital buffers specified in the CRD IV regulation.[2] In other words, the credit gap may be an early warning indicator that is not early enough for policy implementation purposes.[3] Moreover, the guidelines (or the work by Drehmann, Borio, and Tsatsaronis 2011) do not directly compare the predictive power of the credit-to-GDP gap to that of other potentially relevant variables related to risks to financial stability (as stated in the CRD IV) in a *multivariate framework*. Acknowledging the potentially very large implications that this policy has for the international banking sector, our paper aims to address these non-trivial omissions.

The main findings of the paper are the following: First, we find that global variables and especially global credit variables are strong predictors of macro-financial

[1]In particular, the CRD IV specifies that the deviation of the credit-to-GDP ratio from its long-term trend should serve as *"a common starting point for decisions on buffer rates by the relevant national authorities, but should not give rise to an automatic buffer setting or bind the designated authority. The buffer shall reflect, in a meaningful way, the credit cycle and the risks due to excess credit growth in the Member State and shall duly take into account specificities of the national economy"* (EU 2013).

[2]According to Article 126(6) of the CRD IV, *"when a designated authority sets the countercyclical buffer rate above zero for the first time, or when thereafter a designated authority increases the prevailing countercyclical buffer rate setting, it shall also decide the date from which the institutions must apply that increased buffer for the purposes of calculating their institution specific countercyclical capital buffer. That date may be no later than 12 months after the date when the increased buffer setting is announced [in accordance with paragraph 8]. If the date is less than 12 months after the increased buffer setting is announced, that shorter deadline for application shall be justified by exceptional circumstances"*.

[3]Several other potential shortcomings of the credit-to-GDP gap have been discussed in the literature. For example, Edge and Meisenzahl (2011) argue that gap measures are sensitive to the exact specification of the trending variable, in particular with regards to end-of-sample estimates of the credit-to-GDP ratio. For other critical views on the reliability or suitability of the credit-to-GDP gap in the context of the CCB, see for example Repullo and Saurina (2011) and Seidler and Gersl (2011).

vulnerability, providing good signals when used as single variables and demonstrating consistent and significant effects in multivariate logit models. Domestic credit-to-GDP also affects the probability of being in a vulnerable state, even though the effect is clearly smaller than that of global credit variables. However, despite the importance of credit variables, we also find evidence suggesting that other variables play a salient role in predicting vulnerable states of the economy.[4] For example, domestic house price growth and global equity growth are positively associated with macro-financial vulnerabilities. Moreover, we find that banking sector variables exert significant effects: Strong banking sector profitability may incur excessive risk-taking, leading to increased vulnerability, while a high banking sector capitalization decreases the probability of entering a vulnerable state. This result is potentially important for policy makers involved in setting the CCB, as it reinforces the notion that higher CCB rates and bank capital ratios overall reduce the likelihood of financial vulnerability. As such, our findings suggest that even though credit variables are near-essential in early warning models, other macro-financial and banking sector variables are important covariates to control for and to improve the predictive power of these models. Moreover, as a validation of our analysis, we find a good out-of-sample performance of the models in predicting the vulnerable states preceding the financial crises in Finland and Sweden in the early 1990s as well as those in Italy and the U.K. in the mid-1990s.

This paper contributes to the literature in the following ways: First, we apply state-of-the-art modeling techniques from the early warning system (EWS) literature to see whether they could be useful for decisions on countercyclical capital buffers in EU countries. In line with the forthcoming legislation for the CCB, the models are calibrated so that they aim to predict a vulnerable state of the economy (or banking sector), i.e., a build-up of system-wide risk that, with a suitable trigger, could turn into a banking crisis. In practice, we analyze the out-of-sample predictive abilities of a variety of models for those states of the economy that have preceded earlier banking

[4]See also Drehmann and Juselius (2013), who find that the debt service ratio performs well as a supplementary early warning indicator to credit variables for horizons up to two years prior to banking crises. The authors also find that the debt service ratio prior to economic slumps is related to the size of subsequent output losses. Moreover, Hahm, Shin, and Shin (2013) find that growth of banks' non-core liabilities is an indicator for a lending boom and a source of vulnerability to a crisis.

crises by twelve to seven quarters. This would, hopefully, allow a timely build-up of the CCB. Following the methodological approach of Frankel and Rose (1996) and Demirgüç-Kunt and Detragiache (1998), the analysis is conducted in a multivariate logit model framework using data for 23 EU Member States spanning over the period from 1982Q2 to 2012Q3, where we complement the credit variables with several domestic macro-financial and banking sector variables, and following e.g. Frankel and Rose (1996) and Lo Duca and Peltonen (2013), also include global variables in our models in order to account for potential spillover effects. In a similar fashion as Alessi and Detken (2011), Lo Duca and Peltonen (2013) and Sarlin (2013), the models are evaluated using a framework that takes into account a policy makers' preferences between type I (missing a crisis) and type II errors (false alarms of crises). Moreover, the paper focuses exclusively on EU countries, including the largest possible sample (limited by data availability) instead of focusing on a few large economies as is common in the literature.

Second, given the importance of the credit variables in the CRD IV regulatory framework, we use the same BIS database on credit as the BCBS and evaluate their salience in predicting vulnerable states of the economy, both in univariate and multivariate frameworks. Hence, we build on the work of Drehmann, Borio, and Tsatsaronis (2011), who use a univariate signal extraction methodology (see also Kaminsky, Lizondo, and Reinhart 1998) to find that the credit-to-GDP gap provides the best early warning signals for the build-up of capital buffers. Finally, we employ different definitions of banking crises (Babecky et al. 2012; Laeven and Valencia 2008, 2012; Reinhart and Rogoff 2008, 2011) as well as several other variations of the analysis to assess the robustness of the main results.

The remainder of the paper is organized as follows: We present our data set in Section 2.2 and introduce the methodology in Section 2.3. Estimation results and robustness analysis are presented in Section 2.4, while Section 2.5 is reserved for our concluding remarks.

2.2 Data

This section introduces the data used for our study. We begin with the identification of vulnerable states, i.e., the dependent variable in the study, based on banking crises in the European Union. We then proceed by introducing the independent variables used in the empirical analysis. Finally, we present some descriptive statistics on the development of key variables around banking sector crises in the sample countries.

2.2.1 Definition of vulnerable states

The paper develops an early warning model that attempts to predict vulnerable states of the economy from which—given a suitable trigger—banking crises could emerge. Thus, we are not trying to predict banking crises per se, even though we need to identify these crises in order to determine the vulnerable states. Specifically, we define a vulnerable state as the period twelve to seven quarters before the onset of a banking crisis. The time horizon accounts for the CCB announcement period of twelve months that is specified in the CRD IV (EU 2013, Art. 126, 6), and for a time lag required to impose such a policy. At the same time, extending the horizon too far into the past may weaken the link between observed variation in the independent variables and the onset of banking crises. To account for this, we provide a number of alternative time horizons in the robustness section.

In order to identify banking crises, we use the dataset which has been compiled by Babecky et al. (2012) as part of a data collection exercise by the European System of Central Banks (ESCB) Heads of Research Group (labeled as HoR database hereafter). This quarterly database contains information on banking crises in EU countries between 1970Q1 and 2012Q4.[5] The crisis index takes a value of 1 when a banking crisis occurred in a given quarter (and a value of 0 when no crisis occurred). The HoR database aggregates information on banking crises from "several influential papers", including (in alphabetical order): Caprio and Klingebiel (2003); Detragiache and Spilimbergo (2001); Kaminsky (2006); Kaminsky and Reinhart (1999); Laeven and Valencia (2008, 2010, 2012); Reinhart and Rogoff (2008, 2011); and Yeyati and Panizza (2011). The crisis indices from these papers have subsequently been cross-

[5] Croatia, which joined the EU on 1 July 2013, has not yet been included in the database.

checked with the ESCB Heads of Research before inclusion into the database. A list of the banking crisis dates for our sample countries based on this dataset is provided in Panel A of Table 2.1. In the robustness section, we test the robustness of the results by regressing the benchmark model on banking crisis data provided by Laeven and Valencia (2012) and Reinhart and Rogoff (2011).

We set the dependent variable to 1 between (and including) twelve to seven quarters prior to a banking crisis as identified by the ESCB HoR database and to 0 for all other quarters in the data. In order to overcome crisis and post-crisis bias (see e.g. Bussière and Fratzscher 2006), we omit all country quarters which either witnessed a banking crisis or which fall within six quarters after a banking crisis.

2.2.2 Macro-financial and banking sector variables

The panel dataset used in the analysis contains quarterly macro-financial and banking sector data spanning over 1982Q2-2012Q3 for 23 EU member states. The data is sourced through Haver Analytics and originally comes from the BIS, Eurostat, IMF, ECB, and OECD.[6] Panel A of Table 2.1 provides an overview of the data availability for our main variables, while Panel B summarizes the variables included in our study.

Following Drehmann, Borio, and Tsatsaronis (2011), we first include variables measuring the supply of credit to the private sector. We use the "long series on total credit and domestic bank credit to the private non-financial sector" compiled by the BIS. This data includes "borrowing from non-financial corporations, households and non-profit institutions serving households. [...] In terms of lenders, the new total credit series aims to capture all sources independent of the country of origin or type of lender [...] [while] the coverage of financial instruments includes loans and debt securities such as bonds and securitised loans" (see Dembiermont, Drehmann, and

[6] In particular, the individual series stem from the following original sources: Data on total credit to the private non-financial sector is obtained from the BIS and—for those countries where BIS data is not available—from Eurostat. Information on nominal GDP growth and inflation rates comes from the IMF's International Financial Statistics (IFS). Data on stock prices is obtained from the OECD, while data on house prices is provided by the BIS. Banking sector variables are obtained from two sources: The OECD provides relatively long series on banking sector capitalization and profitability on an annual basis that we use in the empirical analysis. Additionally, for illustrative purposes, we use a shorter series of banking sector capitalization in Figure 2.3 that is available on a quarterly basis and which is obtained from the ECB's Balance Sheet Items (BSI) statistics. Finally, quarterly data on the 10-year government bond yield and the 3-months interbank lending rate (money market rate) are obtained from the OECD.

Table 2.1: Data availability and descriptive statistics

Panel A: Data availability	Credit Variables	Other Variables	HoR Banking Crises
Austria	1982Q1-2012Q3	1986Q4-2012Q3	2008Q4
Belgium	1982Q2-2012Q3	1982Q1-2012Q3	2008Q3-2008Q4
Czech Republic	1994q2-2012Q2	—	1998Q1-2002Q2
Denmark	1982Q2-2012Q3	1992Q2-2012Q3	1987Q1-1993Q4, 2008Q3-ongoing
Estonia	2005Q1-2012Q2	2005Q2-2012Q2	–
Finland	1982Q2-2012Q3	1987Q2-2012Q3	1991Q1-1995Q4
France	1982Q2-2012Q3	1992Q2-2012Q3	1994Q1-1995Q4, 2008Q1-2009Q4
Germany	1982Q2-2012Q2	1991Q2-2011Q4	2008Q1-2008Q4
Greece	2003Q1-2012Q2	2003Q1-2012Q2	2008Q1-ongoing
Hungary	1997Q1-2012Q3	2002Q1-2012Q2	2008Q3-2009Q2
Ireland	1999Q1-2012Q3	1999Q1-2010Q4	2008Q1-ongoing
Italy	1982Q2-2012Q3	1990Q3-2012Q2	1994Q1-1995Q4
Lithuania	2005Q1-2012Q2	2005Q1-2012Q2	2009Q1-2009Q4
Luxembourg	2004Q2-2012Q3	2004Q2-2010Q4	2008Q2-ongoing
Malta	2006Q2-2012Q2	—	–
Netherlands	1982Q2-2012Q2	1982Q1-2011Q4	2008Q1-2008Q4
Poland	1997Q1-2012Q3	2003Q1-2012Q3	–
Portugal	1982Q2-2011Q4	1998Q2-2011Q4	–
Slovakia	2005Q2-2012Q2	—	–
Slovenia	2005Q3-2012Q2	—	–
Spain	1982Q2-2012Q3	1995Q2-2012Q3	1982Q2-1985Q3
Sweden	1982Q2-2012Q3	1986Q2-2012Q3	1990Q3-1993Q4, 2008Q3-2008Q4
United Kingdom	1982Q2-2012Q3	1988Q2-2012Q2	1991Q1-1995Q2, 2007Q1-2007Q4

Panel B: Descriptive statistics	Obs	Mean	Std. Dev.	Min	Max
Dom. Credit Growth (qoq)	1220	0.0228	0.0196	-0.0318	0.0989
Dom. Credit Growth (yoy)	1220	0.0926	0.0662	-0.0690	0.3579
Dom. Credit Gap	1220	0.1149	0.1186	-0.1570	0.4550
Dom. Credit Growth (4q MA)	1220	0.0232	0.0166	-0.0173	0.0897
Dom. Credit Growth (6q MA)	1220	0.0232	0.0154	-0.0122	0.0813
Dom. Credit Growth (8q MA)	1220	0.0233	0.0150	-0.0099	0.0805
Dom. Credit to GDP Ratio	1220	1.2756	0.4259	0.4426	2.4829
Dom. Credit to GDP Gap	1220	0.0346	0.0796	-0.1788	0.3249
Dom. Credit Growth - GDP Growth	1220	0.0081	0.0171	-0.0508	0.0715
Glo. Credit Growth (qoq)	1220	0.0152	0.0086	-0.0048	0.0335
Glo. Credit Growth (yoy)	1220	0.0614	0.0289	-0.0113	0.1095
Glo. Credit Gap	1220	0.0597	0.0431	-0.0101	0.1593
Glo. Credit Growth (4q MA)	1220	0.0154	0.0071	-0.0028	0.0274
Glo. Credit Growth (6q MA)	1220	0.0156	0.0069	-0.0021	0.0280
Glo. Credit Growth (8q MA)	1220	0.0158	0.0065	0.0005	0.0274
Glo. Credit to GDP Ratio	1220	0.7557	0.1193	0.5778	0.9933
Glo. Credit to GDP Gap	1220	0.0158	0.0285	-0.0420	0.0676
Glo. Credit Growth - Glo. GDP Growth	1220	0.0022	0.0235	-0.0492	0.0671
GDP Growth	919	0.0123	0.0088	-0.0232	0.0437
Inflation	919	0.0242	0.0166	-0.0108	0.1078
Equity Price Growth	919	0.0240	0.1199	-0.3759	0.3051
House Price Growth	919	0.0172	0.0289	-0.0735	0.1204
Banking Sector Capitalization	756	0.0507	0.0161	0.0238	0.1088
Banking Sector Profitability	756	0.0066	0.0040	-0.0142	0.0292
Gov. Bond Yield	862	0.0575	0.0237	0.0220	0.1385
Money Market Rate	862	0.0460	0.0292	0.0010	0.1643
Global GDP Growth	919	0.0117	0.0229	-0.0585	0.0616
Global Equity Price Growth	919	0.0135	0.0675	-0.3344	0.1122
Global House Price Growth	919	0.0066	0.0162	-0.0389	0.0539

Panel A shows the availability of credit and other variables as well as the crisis dates for the 23 countries in our sample. Credit variables are obtained from the BIS database for total credit to the private non-financial sector (see Dembiermont, Drehmann, and Muksakunratana 2013) and from Eurostat for those countries where the BIS data is not available. Other macro-financial and banking sector variables are obtained from various sources, including the BIS, IMF, and OECD. The crisis definitions are from the ESCB Heads of Research database described in Babecky et al. (2012). Panel B shows descriptive statistics for the credit as well as the other variables. Credit variables are available for a longer period of time in most countries, which is why the number of observations is larger for them.

Muksakunratana 2013 for a description of the database). To our knowledge, the BIS credit series offers the broadest definition of credit provision to the private sector, while having been adjusted for data gaps and structural breaks.

We include four different measurements of credit in our models, accounting for credit growth and leverage, both at the domestic and at the global level. Credit growth is entered as a percentage (annual growth), while leverage is measured by the deviation of the credit-to-GDP ratio (using nominal GDP data) from its long-term backward-looking trend (using a backward-looking Hodrick-Prescott filter with a smoothing parameter λ of 400,000) as proposed by the Basel Committee on Banking Supervision (2010) Consultative Document.[7] Global credit variables have been computed using a GDP-weighted average of the variable in question for several countries, including the United States, Japan, Canada, and all European countries which are in this study (see also Alessi and Detken 2011). In addition, we include four sets of interaction terms in the same fashion as Lo Duca and Peltonen (2013), namely the product of the domestic variables, the product of the global variables and that between the domestic and the global credit variables. The results using different variations of the credit variables are discussed in Section 2.4.[8]

In order to test the importance of credit variables in a comparative fashion as well as to analyze the potential importance of other factors, we include a number of additional variables in our study. These variables are available for fewer observations than the credit variables, which is why the number of observations in the full model differs from the number of observations in models that include only credit variables.[9] Variables are selected based on the existing literature and on data availability. In or-

[7]Recommendations in the BCBS Consultative Document are based on a paper by Borio et al. (2010), who find that trends calculated with a λ of 400,000 perform well in picking up the long-term development of private credit. In particular, a λ of 400,000 is consistent with the assumption of credit cycles being four times longer than business cycles if one follows a rule developed by Ravn and Uhlig (2002), which states that the optimal λ of 1,600 for quarterly data should be adjusted by the fourth power of the observation frequency ratio (i.e., if credit cycles are four times longer than business cycles, λ should be equal to $4^4 \times 1,600 \approx 400,000$).

[8]In Section 2.4.1 we evaluate how individual credit variables perform in the prediction of banking sector vulnerabilities. In this section, we look at several other transformations of the credit variables, including the credit gap (defined as the deviation of private credit from its long-term trend), the credit-to-GDP ratio, several credit growth moving averages, and a variable defined as the difference between credit growth and nominal GDP growth. We evaluate all these variables on the domestic as well as on the global level.

[9]Estimating credit models on the reduced sample yields results that are very similar to the ones for the full sample.

der to account for the macroeconomic environment and monetary stance, we include nominal GDP growth (domestic and global) and CPI inflation rates. Furthermore, following Reinhart and Rogoff (2008), we include data on equity and residential house prices, both domestically and globally (using the same methodology to calculate the global variables as in the case of the credit variables), focusing on annual growth rates. Finally, to control for banking sector profitability and solvency, we include aggregate bank capitalization (calculated by the ratio of equity over total assets) and aggregate banking sector profitability (defined as net income before tax as a percentage of total assets), which has e.g. been suggested by Barrell et al. (2010).

As we are estimating binary choice models using panel data, non-stationarity of independent variables could be an issue (Park and Phillips 2000). We perform panel unit root tests suggested by Im, Pesaran, and Shin (2003) as well as univariate unit root tests developed by Dickey and Fuller (1979) in order to analyze the time series properties of the variables of interest. In the panel unit root test, the null hypothesis that all cross-sections contain unit roots can be rejected at least at the 10 percent level for all series except for the credit-to-GDP gap and global credit growth.[10] We complement the panel unit root analysis by using the Dickey and Fuller (1979) test country-by-country, and can reject the null hypothesis of a unit root for the credit-to-GDP gap at least at the 10 percent level for all countries except for Estonia, Lithuania and Greece. Furthermore, in the country-by-country unit root tests, we can reject the null hypothesis for the global credit-to-GDP gap at least at the 10 percent level for all countries except for Estonia and Lithuania, while for global credit growth the null hypothesis can be rejected for all countries. This implies that sample periods for individual countries seem to affect unit root test results. Overall, the transformations done to the original variables, the results from the unit root tests and general economic theory make us confident that we have addressed potential non-stationarity concerns for the variables of interest.

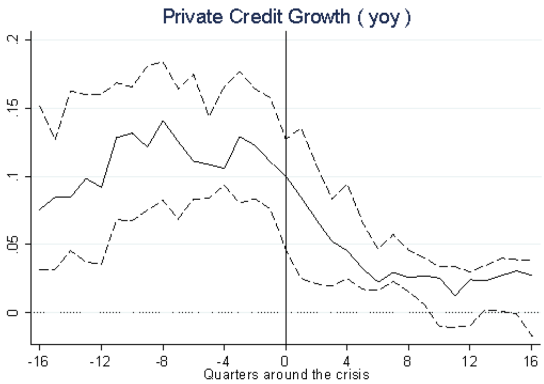
[10] Alternatively, we perform a Fisher-type test by running a Dickey and Fuller (1979) test by cross-section and then combining the p-values from these tests to produce an overall test statistic. The null hypothesis that all cross-sections contain unit roots can be rejected at least at the 10 percent level for all series except for the credit-to-GDP gap and the global credit-to-GDP gap.

2.2.3 Development of key variables

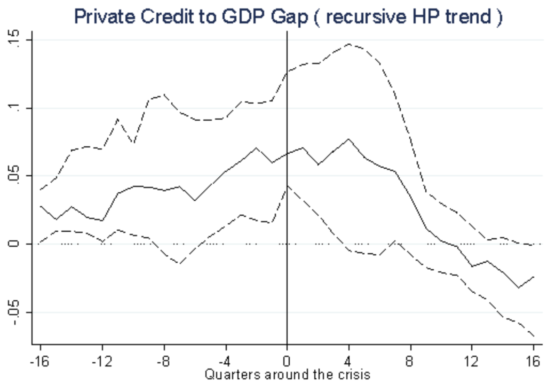
Before entering the discussion of the main results, we shortly present some descriptive statistics, which provide the context of our main argument of moving beyond credit variables when predicting macro-financial vulnerabilities. Figure 2.1 presents the average development of the six main variables of interest over time before and after the onset of a banking crisis. For the purpose of predicting crises, one would hope to find an indicator variable that (on average) peaks (or bottoms out, or at least changes direction) a number of quarters before a crisis, so that it can be used as a signal. In the current case of predicting a vulnerable state of the economy which precedes a potential banking crisis, we would be interested in variables that change direction a bit earlier before the onset of a crisis (i.e., two to three years before the crisis), so that policy makers can use this time to increase the resilience of banks.

In this context, we observe that among the six variables depicted here, the credit-to-GDP gap shows one of the least clear pictures in terms of signaling a coming crisis. On average, the credit gap increases slowly prior to a banking crisis and only starts falling about one year into the crisis. Yet, this does not need to be a very surprising development, as this variable is a ratio and therefore requires the numerator to grow more slowly (or decrease faster) than the denominator in order for the variable to decrease in value. The BCBS itself concedes that the credit-to-GDP trend may not capture turning points well (Basel Committee on Banking Supervision 2010). Consequently, the ratio will not fall unless credit falls faster than GDP, something which is not at all certain during banking crises. Still, it shows that purely from a descriptive perspective, any signal derived from the credit gap needs to come from the level of this variable (i.e., a threshold value), not from changes in its development.

Unlike the credit gap, credit growth (as depicted in % year-on-year growth) does appear to hit a peak about two years before the onset of a banking crisis, even though its fall only becomes clear during the last pre-crisis year. A similar development can be observed in nominal GDP growth and equity price growth figures. These variables do peak before a crisis (on average), but the signal that a crisis is coming only becomes evident shortly before the crisis happens. This makes it difficult, at least from a descriptive point of view, to extract any strong signal from these a)



b)



c)

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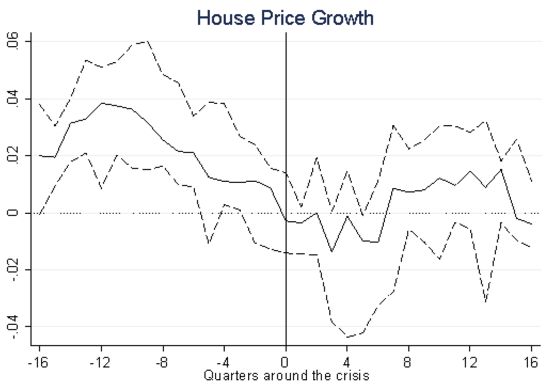

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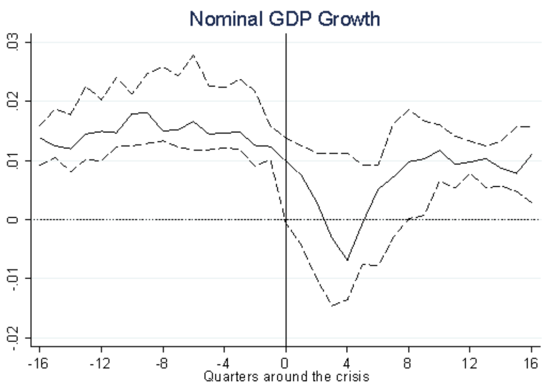


f)

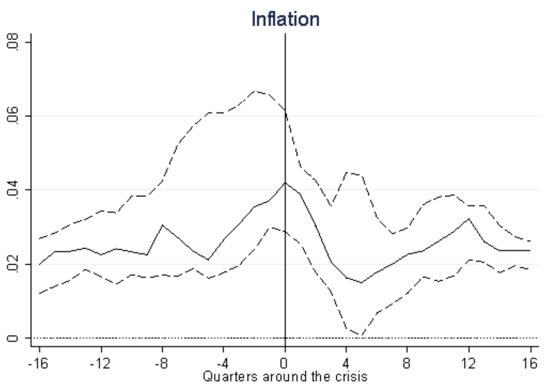


Figure 2.1: Development of key variables around banking crises

The figure depicts the development of selected key variables around banking crises within the sample countries. The start date of a banking crisis is indicated by the vertical line, while the solid line shows the development in the median country and the dashed lines represent the countries at the 25th and the 75th percentile, respectively.

variables. By some margin, residential house price growth outperforms the other domestic variables in terms of signaling 'power' in this descriptive exercise. In our sample, the growth rate of residential house prices tends to peak about 3 years before a crisis happens on average, starting a clear descent (although prices are still rising) that lasts into the crisis where growth stalls. Based on this evidence, we would conclude that residential house prices would be a useful tool (at least much more useful than the other variables shown here) for decisions on the CCB, as it passes the early warning requirement (one year of implementation plus one or two quarters of publication lag) with verve. So, at least from a descriptive standpoint, it is clear that it makes sense to gauge the developments of different macro-financial variables to predict or signal coming crises. Whether this result holds in a more rigorous comparative (multivariate) framework will be discussed in the subsequent analysis.

2.3 Methodology

In this section we introduce the methodology used in the empirical analysis. We start by introducing the logistic regressions used in our multivariate framework. Thereafter, we explain how we evaluate individual indicators' and model predictions' usefulness for policy makers.

2.3.1 Multivariate models

In order to assess the predictive abilities of credit, macro-financial and banking sector variables in a multivariate framework, we estimate logistic regressions of the following form:

$$Prob(y_{it} = 1) = \frac{e^{\alpha_i + X'_{it}\beta}}{1 + e^{\alpha_i + X'_{it}\beta}} \tag{2.1}$$

where $Prob(y_{it} = 1)$ denotes the probability that country i is in a vulnerable state where a banking crisis could occur seven to twelve quarters ahead of quarter t. As described in Section 2.2.1, we set the dependent variable to 1 twelve to seven quarters before the onset of a banking crisis in the respective country and to 0 otherwise. As independent variables, the vector X_{it} includes credit and macro-financial variables on the domestic and on the global level as well as domestic banking sector variables (see

Section 2.2.2 for a precise definition of the variables). The estimations also include a set of country dummy variables α_i in order to account for unobserved heterogeneity at the country level (country fixed effects).[11,12] Finally, we use robust standard errors clustered at the quarterly level in order to account for potential correlation in the error terms that might arise from the fact that global variables are identical across countries in a given quarter.[13]

The analysis is conducted as much as possible in a real-time fashion, meaning that only information that is available at a particular point in time is used. As such, all de-trended variables have been calculated using backward trends, thereby only using information that was available up to that point. Furthermore, the explanatory variables have been lagged by one quarter, also to account for endogeneity bias through simultaneity. We are well aware that this simple procedure cannot crowd out all endogeneity-related bias, but we note that the dependent variable itself is an early warning variable. The time horizon for which this variable is equal to 1 has been chosen in the context of our exercise and has not been exogenously determined. Therefore, we consider endogeneity to be a somewhat smaller problem in this study. Nevertheless, we have tested our models for different specifications of the dependent variable, both in terms of the pre-crisis period chosen (12-1/20-13 quarters before the onset of a crisis) and the definition and data source of banking crises in the robustness section.

[11] There is an argument for omitting these dummies from the estimations as they automatically exclude all countries without a crisis from the estimation, hence introducing selection bias (see e.g. Demirgüç-Kunt and Detragiache 1998 and Davis and Karim 2008). However, not including them also induces bias, namely omitted variable bias caused by unit effects. As it is unlikely that financial crises are homogeneously caused by identical factors (see also Candelon, Urbain, and Van den Berg 2008) and as a Hausman test indicates unit heterogeneity, we have decided to include unit dummy variables in our estimations. Results for pooled models (without country dummies) are available upon request. Coefficients in these models are of course different, but carry the same sign in virtually all models that we have estimated.

[12] In principle, we could have included time dummies in addition to country dummies in order to account for heterogeneity in crisis probabilities over time. However, we decided against the inclusion of these dummies for two reasons: First, only quarters where at least one country experiences a banking crisis could be used for identification in such a specification. As our sample includes many quarters where none of the countries experienced a crisis the inclusion of time dummies would significantly reduce our sample size. Second, the focus in our paper is on the prediction of future banking crises. Specifically, we aim to develop an early warning model that policy makers can use for the detection of vulnerabilities in the banking sector. While time dummies might improve the ex post fit of a model, they are of little use for out-of-sample forecasting since they are not known ex ante (see e.g. Schularick and Taylor 2012).

[13] Alternatively, we cluster standard errors at the country level, which results in smaller estimates in particular for the global variables.

2.3.2 Model evaluation

Banking crises are (thankfully) rare events in the sense that most EU countries have encountered none or only one over the past two decades. Still, when they occur, banking crises tend to be very costly, both directly through bailouts and fiscal interventions and indirectly through the loss of economic output that oftentimes (particularly in systemic banking crises) tends to follow these crises. Thus, policy makers have a clear incentive to be able to detect early enough potential signs of vulnerabilities that might precede banking crises in order to take measures to prevent further building up of vulnerabilities or to strengthen the resilience of the banking sector. Yet, at the same time, policy makers may not want to be signaling crises when in fact they do not happen afterwards. Doing so may (a) reduce the credibility of their signals, weakening decision-making and damaging their reputation, and (b) needlessly incur costs on the banking sector, endangering credit supply. As a consequence, policy makers also have an incentive to avoid false alarms, i.e., they do not want to issue warnings when a crisis is not imminent. As pointed out by Alessi and Detken (2011), an evaluation framework for an early warning model needs to take into account policy makers' relative aversion with respect to type I errors (not issuing a signal when a crisis is imminent) and type II errors (issuing a signal when no crisis is imminent).

The evaluation approach in this paper is based on the so-called 'signaling approach' that was originally developed by Kaminsky, Lizondo, and Reinhart (1998), and extended by Demirgüç-Kunt and Detragiache (2000), Alessi and Detken (2011), Lo Duca and Peltonen (2013) and Sarlin (2013). In this framework, an indicator issues a warning signal whenever its value in a certain period exceeds a threshold τ, defined by a percentile of the indicator's country-specific distribution. Similarly, a multivariate probability model issues a warning signal whenever the predicted probability from this model exceeds a threshold $\tau \in [0, 1]$, again defined as a percentile of the country-specific distribution of predicted probabilities. In this way, individual variables and model predictions for each observation j are transformed into binary predictions P_j that are equal to 1 if the respective thresholds are exceeded for this observation and 0 otherwise. Predictive abilities of the variables and the models can then be evaluated by comparing the signals issued by the respective variable or model to the actual outcome C_j for each observation.[14] Each observation can be allocated to one of the quadrants in the contingency matrix depicted in Table 2.2: A period with a signal by a specific indicator can either be followed by a banking crisis twelve to seven quarters ahead (TP) or not (FP). Similarly, a period without a signal can be followed by a banking crisis twelve to seven quarters ahead (FN) or not (TN). Importantly, the number of observations classified into each category depends on the threshold τ.

Table 2.2: Contingency matrix

		Actual class C_j	
		1	0
Predicted class P_j	1	*True positive (TP)*	*False positive (FP)*
	0	*False negative (FN)*	*True negative (TN)*

The table shows the relationship between model prediction and actual outcomes. Observations are classified into those where the indicator issues a warning that is indeed followed by a banking crises twelve to seven quarters ahead (TP), those where the indicator issues a warning that is not followed by a crisis (FP), those where the indicator issues no warning and there is no crises seven to twelve quarters ahead (TN), and those where the indicator issues no warning although there is a crisis coming (FN).

In order to obtain the optimal threshold τ one needs to take the policy maker's preferences vis-à-vis type I errors (missing a crisis, $T_1(\tau) = FN/(TP + FN) \in [0, 1]$) and type II errors (issuing a false alarm, $T_2(\tau) = FP/(FP + TN) \in [0, 1]$) into account. This can be done by defining a loss function that depends on the two types of errors as well as the policy maker's relative preference for either type. The optimal threshold is then the one that minimizes the loss function. Taking into account the relative frequencies of crises $P_1 = P(C_j = 1)$ and tranquil periods $P_2 = P(C_j = 0)$, the loss function is defined as follows:[15]

$$L(\mu, \tau) = \mu P_1 T_1(\tau) + (1 - \mu) P_2 T_2(\tau), \tag{2.2}$$

[14] C_j is equal to 1 if the country experiences a banking sector crisis twelve to seven quarters ahead of the respective period and 0 otherwise.

[15] As pointed out by Sarlin (2013), policy makers should be concerned about the absolute number of misclassification rather than the share of misclassifications in relation to class size (i.e., unweighted type I and type II errors). Therefore, a failure to account for the relative frequency of crisis episodes and tranquil periods—as in previous studies—results in a bias on the weighting of type I and type II errors in the loss function.

where $\mu \in [0, 1]$ denotes the policy maker's relative preference between type I and type II errors. A μ larger than 0.5 indicates that the policy maker is more averse against missing a crisis than against issuing a false alarm, which—in particular following the recent financial crisis—is a realistic assumption in our view.

Using the loss function $L(\mu, \tau)$, the usefulness of a model can be defined in two ways. First, following the idea of Alessi and Detken (2011) and as in Sarlin (2013), the absolute usefulness is defined as:

$$U_a = min(\mu P_1, (1 - \mu)P_2) - L(\mu, \tau). \tag{2.3}$$

Note that U_a computes the extent to which having the model is better than having no model. This is because a policy maker can always achieve a loss of $min(\mu P_1, (1-\mu)P_2)$ by either always issuing a signal (in which case $T_1(\tau) = 0$) or never issuing a signal (in which case $T_2(\tau) = 0$). The fact that P_1 is significantly smaller than P_2 in our sample (i.e., there are relatively few vulnerable states preceding banking crises) implies that, in order to achieve a high usefulness of the model, a policy maker needs to be more concerned about the detection of vulnerable states potentially preceding banking crises in comparison to the avoidance of false alarms.[16] Otherwise, with a suboptimal performing model, it would easily pay off for the policy maker to never issue a signal given the distribution of vulnerable states and tranquil periods (see Sarlin 2013 for a detailed discussion of this issue).

A second measure, the relative usefulness U_r, is computed as follows (see Sarlin 2013):

$$U_r = \frac{U_a}{min(\mu P_1, (1 - \mu)P_2)} \tag{2.4}$$

The relative usefulness U_r reports U_a as a percentage of the usefulness that a policy maker would gain from a perfectly performing model.[17] The relative usefulness is our preferred performance indicator as it allows the comparison of models for policy makers with different values for the preference parameter μ.

In addition to assessing the relative and absolute usefulness of a model, we also

[16] The share of observations that is followed by a banking crisis twelve to seven quarters ahead—P_1—is approximately equal to 10 % in our sample.

[17] A perfectly performing indicator would achieve $T_1 = T_2 = 0$, implying $L = 0$. Consequently, U_a would reduce to $min(\mu P_1, (1 - \mu)P_2)$.

employ receiver operating characteristics (ROC) curves and the area under the ROC curve (AUROC) as these are also viable measures for comparing performance of early warning models. The ROC curve shows the trade-off between the benefits and costs of a certain threshold τ. When two models are compared, the better model has a higher benefit (TP rate (TPR) on the vertical axis) at the same cost (FP rate (FPR) on the horizontal axis).[18] Thus, as each FP rate is associated with a threshold, the measure shows performance over all thresholds.[19] In this paper, the size of the AUROC is computed using trapezoidal approximations. The AUROC measures the probability that a randomly chosen vulnerable state is ranked higher than a tranquil period. A perfect ranking has an AUROC equal to 1, whereas a coin toss has an expected AUROC of 0.5.

2.4 Empirical results

In this section we present the empirical results. We first explore the usefulness of credit variables for the identification of vulnerable states of the banking sector, and proceed by extending the framework to a multivariate model including other macro-financial and banking sector indicators. Thereafter, we evaluate the out-of-sample performance of the estimated models and—finally—present some robustness checks.

2.4.1 Estimation and evaluation

As the CRD IV regulations emphasize the role of credit variables for setting the countercyclical capital buffer rate—in particular the role of credit growth and the credit-to-GDP gap—we start by evaluating the usefulness of these variables for the

[18]The TPR (also called sensitivity) gives the ratio of periods where the model correctly issues a warning to all periods where a warning should have been issued, formally $TPR = TP/(TP+FN)$. The FPR (also called specificity) gives the ratio of periods where the model wrongly issues a signal to all periods where no signal should have been issued, formally $FPR = FP/(FP+TN)$. An ideal model would achieve a TPR of one (no missed crises) and a FPR of zero (no false alarms).

[19]The measure can also be interpreted as showing the performance over all preference parameters μ of the policy maker: The lower the threshold τ, the more aggressive is the policy maker in making crisis calls as almost all signals are above the threshold. Hence, a low τ corresponds to a policy maker with a strong aversion against type I errors, i.e., a policy maker with a strong preference for correctly calling all crises. Equivalently, the larger the threshold τ the more conservative is the policy maker in making crisis calls. Therefore, a high τ corresponds to a policy maker with a strong aversion against type II errors, i.e., a policy with a strong preference for the avoidance of false alarms.

identification of vulnerable states within the EU banking sector.

Individual indicators

First, we evaluate the usefulness of domestic credit variables by using a simple signaling approach. Using a preference parameter of μ equal to 0.9, Panel A of Table 2.3 reports the optimal threshold for several credit variable indicators.[20] Given the optimal threshold, the table also shows the number of observations in each quadrant of the matrix depicted in Table 2.2, the percentage of type 1 and type 2 errors, as well as several performance measures, such as the absolute and the relative usefulness, the adjusted noise-to-signal (aNtS) ratio[21], the percentage of vulnerable states correctly predicted by the indicator (% Predicted), the probability of a vulnerable state conditional on a signal being issued (Cond Prob) and the difference between the conditional and the unconditional probability of a vulnerable state (Diff Prob).

Among the domestic indicators, indeed, the credit-to-GDP gap performs best in the sense that it generates the highest relative usefulness.[22] This indicator issues a signal whenever the credit-to-GDP gap is above the 40^{th} percentile of its country-specific distribution and achieves 25.6 % of the usefulness a policy maker would gain from a perfectly performing model. The indicator correctly calls 81.3 % of the vulnerable states and displays an adjusted noise-to-signal ratio of 0.678. Conditional on a signal being issued, the probability of a vulnerable state is 16.8 %, which is 4.7 % higher than the unconditional probability of a vulnerable state in our sample. Other variables that perform relatively well are annual credit growth, the credit-to-

[20] A preference parameter of μ equal to 0.9 indicates a strong preference for the detection of crises by the policy maker. In our view this is a reasonable assumption as the current crisis illustrated once more that financial crises often translate into large costs for the economy. As Sarlin (2013) points out, using a μ equal to 0.9 and simultaneously taking into account the unconditional probability of a crisis (which is about 10 % in our sample) is equivalent to using a μ equal to 0.5 without adjusting for the unconditional probabilities (as in Alessi and Detken 2011 or Lo Duca and Peltonen 2013). Results for different values of the preference parameter are available upon request.

[21] The aNtS ratio is the ratio of false signals measured as a proportion of quarters where false signals could have been issued to good signals as a proportion of quarters where good signals could have been issued, or $(FP/(FP + TN))/(TP/(TP + FN))$. A lower aNtS ratio indicates better predictive abilities of the model.

[22] This is consistent with findings by Drehmann, Borio, and Tsatsaronis (2011) for a different set of countries and seems to support the approach taken in the CRD IV regulation. However, the main argument of our paper will be that performance of the individual indicators can be improved if they are combined in a multivariate approach. Moreover, also global variables are useful for the identification of vulnerabilities in the banking sector.

Table 2.3: Evaluation of individual indicators

	μ	Threshold	TP	FP	TN	FN	T_1	T_2	Absolute Usefulness	Relative Usefulness	aNtS Ratio	% Predicted	Cond Prob	Diff Prob
Panel A: Domestic Variables														
Dom. Credit to GDP Gap	0.9	40	100	497	404	23	0.187	0.552	0.023	0.256	0.678	0.813	0.168	0.047
Dom. Credit Growth (yoy)	0.9	58	85	399	502	38	0.309	0.443	0.022	0.240	0.641	0.691	0.176	0.056
Dom. Credit to GDP Ratio	0.9	69	51	169	732	72	0.585	0.188	0.019	0.211	0.452	0.415	0.232	0.112
Dom. Credit Gap	0.9	37	104	577	324	19	0.154	0.640	0.018	0.201	0.757	0.846	0.153	0.033
Dom. Credit Growth (4q MA)	0.9	48	93	500	401	30	0.244	0.555	0.017	0.194	0.734	0.756	0.157	0.037
Dom. Credit Growth (6q MA)	0.9	61	72	364	537	51	0.415	0.404	0.015	0.170	0.690	0.585	0.165	0.045
Dom. Credit Growth (qoq)	0.9	46	92	530	371	31	0.252	0.588	0.014	0.153	0.786	0.748	0.148	0.028
Dom. Credit Growth - GDP Growth	0.9	54	70	409	492	53	0.431	0.454	0.009	0.103	0.798	0.569	0.146	0.026
Dom. Credit Growth (8q MA)	0.9	66	57	314	587	66	0.537	0.349	0.009	0.100	0.752	0.463	0.154	0.034
Panel B: Global Variables														
Glo. Credit Gap	0.9	45	113	427	474	10	0.081	0.474	0.040	0.443	0.516	0.919	0.209	0.089
Glo. Credit Growth (qoq)	0.9	60	100	357	544	23	0.187	0.396	0.037	0.412	0.487	0.813	0.219	0.099
Glo. Credit Growth (yoy)	0.9	57	101	365	536	22	0.179	0.405	0.037	0.411	0.493	0.821	0.217	0.097
Glo. Credit Growth (4q MA)	0.9	49	109	448	453	14	0.114	0.497	0.035	0.386	0.561	0.886	0.196	0.076
Glo. Credit Growth (6q MA)	0.9	46	110	467	434	13	0.106	0.518	0.033	0.373	0.580	0.894	0.191	0.071
Glo. Credit Growth (8q MA)	0.9	41	109	509	392	14	0.114	0.565	0.029	0.318	0.637	0.886	0.176	0.056
Glo. Credit to GDP Ratio	0.9	75	44	100	801	79	0.642	0.111	0.021	0.229	0.310	0.358	0.306	0.185
Glo. Credit to GDP Gap	0.9	37	105	571	330	18	0.146	0.634	0.019	0.216	0.742	0.854	0.155	0.035
Glo. Credit Growth - Glo. GDP Growth	0.9	83	46	161	740	77	0.626	0.179	0.016	0.178	0.478	0.374	0.222	0.102

The table shows results for the evaluation of individual indicator variables using the signaling approach (see Section 2.3.2 for a detailed description). The preference parameter of $\mu = 0.9$ indicates that policy makers have a strong preference for the detection of crises (i.e., avoiding type I errors) as compared to the avoidance of false alarms (i.e., type II errors). The optimal threshold is calculated as the one that maximizes the relative usefulness and gives the percentile of the country-specific distribution at which the respective indicator issues a warning. The columns of the table report the number of observations: Where the indicator issues a warning that is indeed followed by a banking crises seven to twelve quarters ahead (TP); where the indicator issues a warning that is not followed by a crisis (FP); where the indicator issues no warning and there is no crises seven to twelve quarters ahead (TN); and where the indicator issues no warning although there is a crisis coming (FN). Furthermore, the table reports the fraction of type I errors $T_1 = FN/(TP + FN)$, the fraction of type II errors $T_2 = FP/(FP + TN)$, the absolute and the relative usefulness (see Section 2.3.2 for details), the adjusted noise-to-signal ratio (i.e., the ratio of false signals measured as a proportion of months where false signals could have been issued to good signals as a proportion of months where good signals could have been issued, or $(FP/(FP + TN))/(TP/(TP + FN))$), the probability of a crisis conditional on a signal being issued (Cond Prob) and the difference between the conditional and the unconditional probability of a crisis (Diff Prob). The domestic and the global variables are ranked in terms of relative usefulness, respectively.

GDP ratio and the credit gap (defined as the deviation of the stock of credit from its long term trend, see Section 2.2.2).

Interestingly, global variables seem to outperform domestic variables in terms of usefulness. Panel B of Table 2.3 shows that these indicators usually exert a higher relative usefulness, a lower adjusted noise-to-signal ratio, and are able to predict a larger share of the vulnerable states in our sample. This suggests that focusing on the development of domestic credit variables might not be sufficient. In an increasingly integrated economy, vulnerabilities that develop at a global level potentially transmit to countries around the world. Therefore, policy makers should also take these developments into account when deciding on countercyclical capital buffer rates.[23]

The evaluation of the predictive abilities of global variables is subject to a caveat: As these variables do not vary across countries, and as most countries had a crisis starting in 2008, the good performance of these variables can in part be explained by a clustering of crisis episodes within the same year, i.e., indicators based on global credit variables correctly predicted the current crisis in several of our sample countries. To a certain extent this puts the higher usefulness of global as compared to domestic variables in a perspective. However, the current crisis is certainly one of the best examples for a non-domestic vulnerability that spread to banking systems around the world. Thus, if the aim of the CCB is to increase the resilience of the banking system, it appears to be beneficial to take into account both domestic and global developments.

Multivariate models

While the signaling approach is a simple and useful way to assess the predictive abilities of individual indicators, a multivariate framework has the advantage of being able to assess the joint performance of several indicators. We therefore estimate simple logit models including several of the individual credit variables and assess their performance and usefulness. In order to account for unobserved heterogeneity across countries that might otherwise bias our results, we include a set of country

[23]According to the CRD IV, the CCB rate for a specific bank should be calculated as a weighted average of the bank's country exposures.

dummies.

Results for these models are presented in Table 2.4. Again, we start by considering only the domestic variables and focus on credit growth and the credit-to-GDP gap, as these variables performed well in the signaling approach and play a prominent role in the CRD IV regulations. Credit growth seems to dominate the credit-to-GDP gap, which is statistically not significant, in this simple model. Next, we gradually include the global credit variables, interactions between growth and leverage on the domestic and the global level as well as interactions between the domestic and the global variables.[24] The predictive power of the model improves with each step.[25]

In order to compare the models' predictive abilities with those of the individual indicators we once more apply the signaling approach by translating the predicted probabilities into country specific percentiles and determining the optimal threshold for the issuance of warnings as the one that maximizes the relative usefulness of the model (see Section 2.3.2). Table 2.5 shows that the relative usefulness of the domestic model is 0.236, which is lower than the one of the best individual indicators. However, the stepwise inclusion of the remaining variables improves the usefulness, so that Model 3 surpasses the best domestic as well as the best global indicators in terms of relative usefulness. This indicates the benefits of a multivariate framework as compared to single indicators. We will elaborate more on these benefits by taking into account not only credit variables, but also other variables that might affect the stability of the banking sector.

Models 4-7 provide the estimation results for the extended models. The sample size is somewhat smaller than in the Models 1-3, as the data is not available for

[24]We orthogonalize interaction terms with first-order predictors in order to avoid problems of multicollinearity (see e.g. Little, Bovaird, and Widaman 2006). In particular, when interacting two variables X and Y, we first form the simple product $X \times Y$ and then regress it on the original variables: $X \times Y = \alpha + \beta_1 \times X + \beta_2 \times Y + \epsilon$. We then take the residual from this regression—ϵ, which is orthogonal to X and Y—to represent the interaction between the two original variables. Variance inflation factors (VIF) smaller than ten for all variables indicate that we are able to get rid of multicollinearity problems in this way.

[25]Note that the interpretation of interaction effects in logit models is cumbersome. As pointed out by Ai and Norton (2003), the interaction effect is conditional on the independent variables (unlike interaction effects in linear models) and may have different signs for different values of the covariates. Moreover, the statistical significance of these effects cannot be evaluated with a simple t-test, but should be evaluated for each observation separately. Doing so allows us to conclude that for most observations only the Interaction(GC1×GC2) is significantly positive, while the other interactions are insignificant (although e.g. the Interaction(DC2×GC2) has a significantly negative sign in the regression itself).

Table 2.4: Multivariate models

	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5	(6) Model 6	(7) Model 7
Dom. Credit Growth (DC1)	6.38**	2.66	1.61	3.93	1.54	-0.16	0.70
	(2.59)	(2.95)	(2.75)	(3.27)	(3.73)	(4.78)	(3.60)
Dom. Credit to GDP Gap (DC2)	3.01	2.71	3.70	8.55***	12.98***	13.24***	20.04***
	(1.93)	(2.80)	(2.66)	(2.27)	(2.27)	(3.19)	(2.68)
Interaction(DC1 × DC2)			26.42	55.68**	55.12**	83.05**	53.94
			(21.83)	(22.77)	(22.35)	(38.97)	(35.03)
Glo. Credit Growth (GC1)		16.71***	16.07***	29.01***	25.99***	19.72*	6.72
		(4.26)	(4.80)	(5.61)	(8.88)	(11.12)	(12.82)
Glo. Credit to GDP Gap (GC2)		1.96	-2.74	6.74	12.19	26.84**	41.15***
		(7.67)	(6.57)	(6.67)	(8.89)	(11.72)	(15.31)
Interaction(GC1 × GC2)			391.54**	-486.53*	-324.72	-472.64	-973.05***
			(188.05)	(258.07)	(305.59)	(312.51)	(317.56)
Interaction(DC1 × GC1)			45.98	-56.67	-28.48	-129.64	34.59
			(75.98)	(56.65)	(68.99)	(124.41)	(128.36)
Interaction(DC2 × GC2)			-239.65***	-417.35***	-472.20***	-410.73***	-582.20***
			(49.73)	(67.99)	(91.07)	(100.92)	(109.21)
GDP Growth					19.64	41.84	30.80
					(18.97)	(26.08)	(27.05)
Inflation					-29.04**	-10.04	14.19
					(11.73)	(12.23)	(12.18)
Equity Price Growth					-1.01	-0.38	-0.15
					(1.10)	(1.14)	(1.35)
House Price Growth					16.73***	19.80***	18.05***
					(5.40)	(5.56)	(5.35)
Global GDP Growth					-10.24	-10.58	-9.88
					(12.62)	(13.68)	(13.79)
Global Equity Price Growth					7.39	7.61	7.78
					(4.80)	(5.42)	(6.12)
Global House Price Growth					16.29	14.97	30.09
					(18.34)	(20.67)	(22.55)
Banking Sector Capitalization							-136.85***
							(39.63)
Banking Sector Profitability							324.89***
							(76.45)
Country dummies	YES	YES	YES	YES	YES	YES	YES
Observations	1,220	1,220	1,220	919	919	756	756
Pseudo R-Squared	0.0894	0.108	0.133	0.210	0.278	0.272	0.336
AUROC	0.710	0.733	0.780	0.824	0.865	0.846	0.892
Standard error	0.0266	0.0232	0.0185	0.0195	0.0160	0.0165	0.0157

The table shows estimation results for multivariate logit models, where the dependent variable is set to 1 twelve to seven quarters preceding a banking crisis in a respective country. Observations for banking crises and six quarters following banking crises are omitted, while other dependent variable observations are set to 0. All regressions include country-specific dummy variables to account for unobserved heterogeneity across countries. Robust standard errors adjusted for clustering at the quarterly level are reported in parentheses. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

Table 2.5: Model evaluation

	μ	Threshold	TP	FP	TN	FN	T_1	T_2	Absolute Usefulness	Relative Usefulness	aNtS Ratio	% Predicted	Cond Prob	Diff Prob
Model 1	0.9	48	97	489	427	28	0.224	0.534	0.021	0.236	0.688	0.776	0.166	0.045
Model 2	0.9	43	114	525	391	11	0.088	0.573	0.030	0.336	0.628	0.912	0.178	0.058
Model 3	0.9	56	108	333	583	17	0.136	0.364	0.045	0.497	0.421	0.864	0.245	0.125
Model 4	0.9	67	71	174	501	26	0.268	0.258	0.041	0.456	0.352	0.732	0.290	0.164
Model 5	0.9	63	92	231	444	5	0.052	0.342	0.054	0.603	0.361	0.948	0.285	0.159
Model 6	0.9	63	66	179	408	6	0.083	0.305	0.051	0.595	0.333	0.917	0.269	0.160
Model 7	0.9	67	64	163	424	8	0.111	0.278	0.051	0.596	0.312	0.889	0.282	0.173
Model R1	0.9	42	91	417	366	6	0.062	0.533	0.036	0.401	0.568	0.938	0.179	0.069
Model R2	0.9	65	81	251	532	16	0.165	0.321	0.045	0.503	0.384	0.835	0.244	0.134
Model R3	0.9	69	34	224	451	14	0.292	0.332	0.032	0.357	0.468	0.708	0.132	0.065

The table shows results for the evaluation of the multivariate models presented in Tables 2.4 and 2.7. As for the individual indicators, we apply the signaling approach by transforming predicted probabilities into country-specific percentiles. The preference parameter of $\mu = 0.9$ indicates that a policy maker has a strong preference for the detection of crises (i.e., avoiding type I errors) as compared to the avoidance of false alarms (i.e., type II errors). The optimal threshold is calculated as the one that maximizes the relative usefulness and gives the percentile of the country-specific distribution at which the respective indicator issues a warning. The columns of the table report the number of observations: Where the indicator issues a warning that is indeed followed by a banking crises seven to twelve quarters ahead (TP); where the indicator issues a warning that is not followed by a crisis (FP); where the indicator issues no warning and there is no crises seven to twelve quarters ahead (TN); and where the indicator issues no warning although there is a crisis coming (FN). Furthermore, the table reports the fraction of type I errors $T_1 = FN/(TP + FN)$, the fraction of type II errors $T_2 = FP/(FP + TN)$, the absolute and the relative usefulness (see Section 2.3.2 for details), the adjusted noise-to-signal ratio (i.e., the ratio of false signals measured as a proportion of months where false signals could have been issued to good signals as a proportion of months where good signals could have been issued, or $(FP/(FP + TN))/(TP/(TP + FN)))$, the probability of a crisis conditional on a signal being issued (Cond Prob) and the difference between the conditional and the unconditional probability of a crisis (Diff Prob).

all variables across the whole period (see Table 2.1). In order to make results comparable, Model 4 re-estimates Model 3 on the reduced sample. The most striking difference between the two regressions is the coefficient for the domestic credit-to-GDP gap, which turns significant in the reduced sample. This indicates that any evaluation depends on the respective sample and should make policy makers cautious when generalizing findings from a particular sample of countries. However, the predictive abilities of the models are quite impressive. For example, Model 5, to which we refer as our *benchmark model*, achieves 60.3 % of the usefulness of a perfectly performing model and thus outperforms any individual indicator. The model issues a warning whenever the predicted probability is above its 63^{rd} percentile within the respective country. In this way, a warning is issued in 94.8 % of the quarters in our sample where a banking crisis occurs seven to twelve quarters ahead. The probability of a crisis conditional on a signal being issued is 28.5 %, which is 15.9 % higher than the unconditional probability of a crisis. Finally, the area under the ROC curve for this model is equal to 0.865, indicating a good predictive ability of the model for a wide range of policy makers' preference parameters (see Figure 2.2 for an illustration of the ROC curve for our benchmark model).[26]

We find that the credit variables are indeed among the most important predictors of vulnerable states of the economy. However, both model fit and model performance increase significantly when we include the other variables. For example, the consistently positive coefficient for house price growth indicates that asset price booms promote the build-up of vulnerabilities in the financial sector. This suggests that regulators should keep an eye on these developments instead of focusing exclusively on the development of credit variables. Moreover, Model 7 shows that banking sector variables exert a significant influence on the build-up of financial vulnerabilities.[27] We make the following observations: First, a country is more likely to be in a vulnerable state, when aggregate bank capitalization within the country is relatively low. This is a particularly important finding in the context of countercyclical capi-

[26]In contrast to the individual indicators and most of the credit models, the extended models perform well also for lower values of the preference parameter μ, which we see as another advantage of these models. These results are available upon request.

[27]Again, the sample for Models 6 and 7 is reduced as banking sector variables are not available for all our sample countries. As before, we first re-estimate Model 5 on the reduced sample (see Model 6) in order to make results comparable.

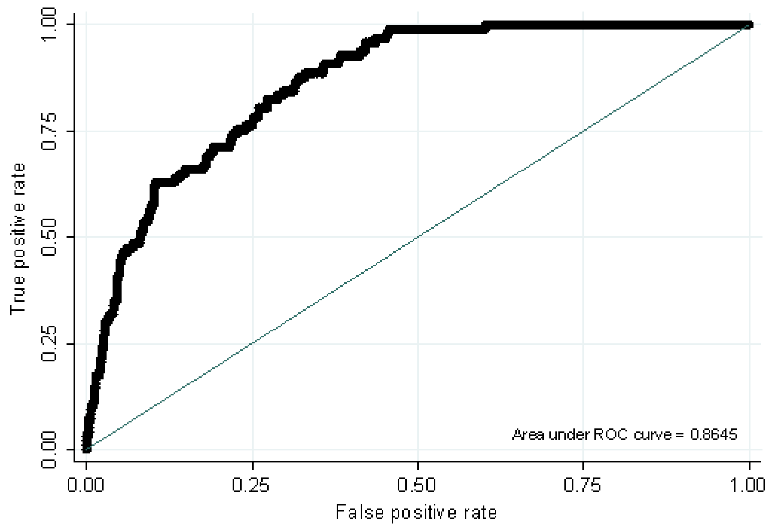


Figure 2.2: ROC curve for benchmark model (Model 5)

The figure shows the Receiver Operating Characteristic (ROC) curve for our benchmark model. The area under the ROC curve (AUROC) is equal to 0.8645.

tal buffers as it indicates that indeed regulators could improve the resilience of the banking system by requiring banks to hold more capital when vulnerabilities build up. Second, we find that future banking crises are more likely when profits in the banking sector are relatively high. As Borio et al. (2010) point out, periods of high bank profitability are typically associated with rapid credit growth, increased risk-taking and building up of vulnerabilities, which could explain the positive coefficient for the profitability variable preceding banking crises.

Figure 2.3 illustrates the relationship between predicted crisis probabilities from our benchmark model (Model 5) and actual banking sector capitalization in the countries that had a banking crisis in 2007/2008. Most countries exerted declining or constantly low levels of bank capitalization prior to the crisis, which is consistent with the evidence from Model 7.[28] At the same time, the benchmark model issues a warning already in late-2004/early-2005 in most cases.[29] Hence, if they had relied on this signal, regulators would have had enough time for the activation of the CCB prior to the crisis—even if we account for an announcement period of twelve months

[28] A notable exception is Austria (and to some extent Denmark), where aggregate banking sector capitalization actually increased prior to the crisis.

[29] Again, the model issues a warning whenever the predicted probability is higher than the optimal threshold within the country (indicated by the dashed horizontal line in the figure).

for the CCB.

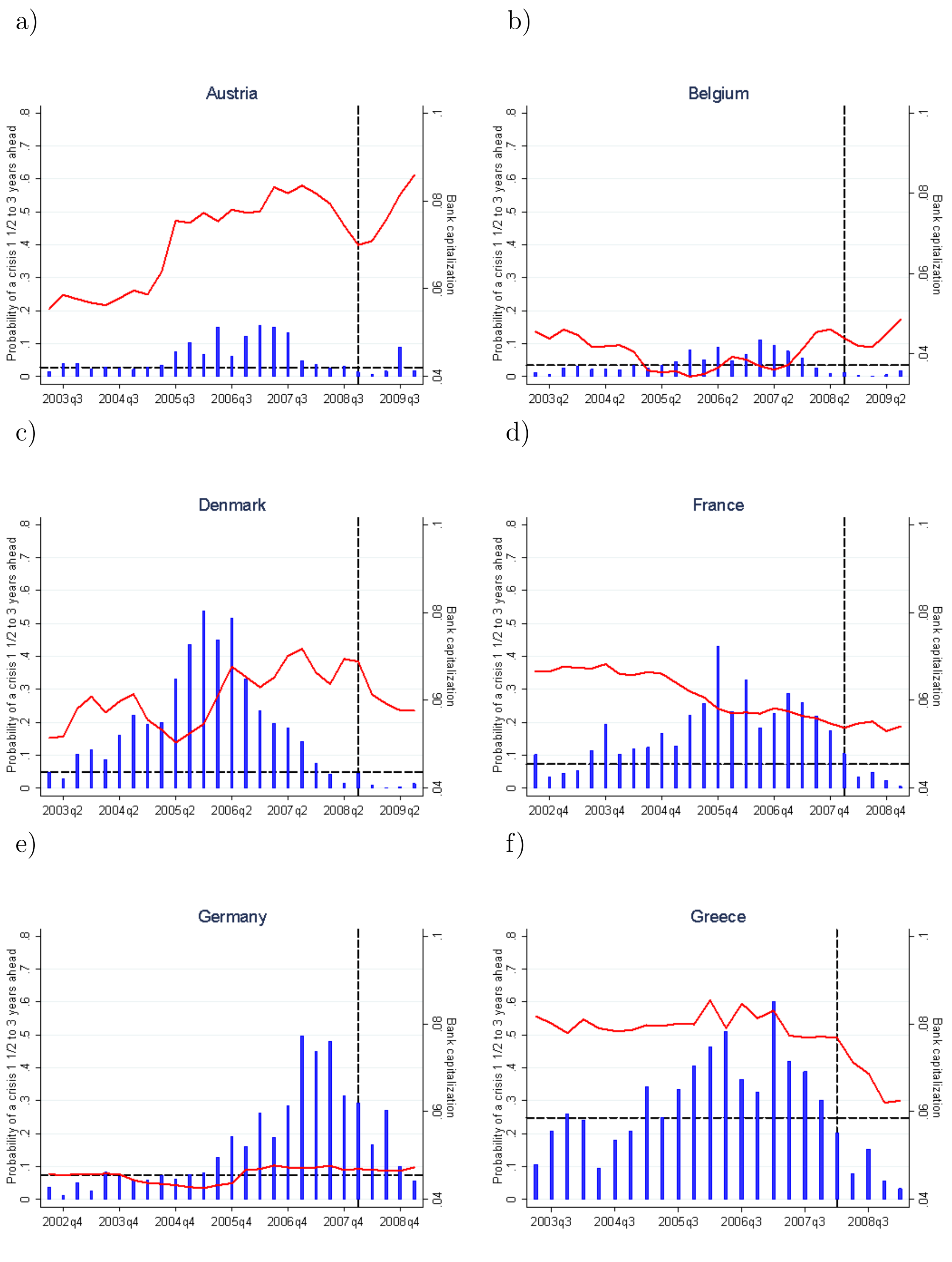


Figure 2.3: Predicted crisis probabilities and banking sector capitalization

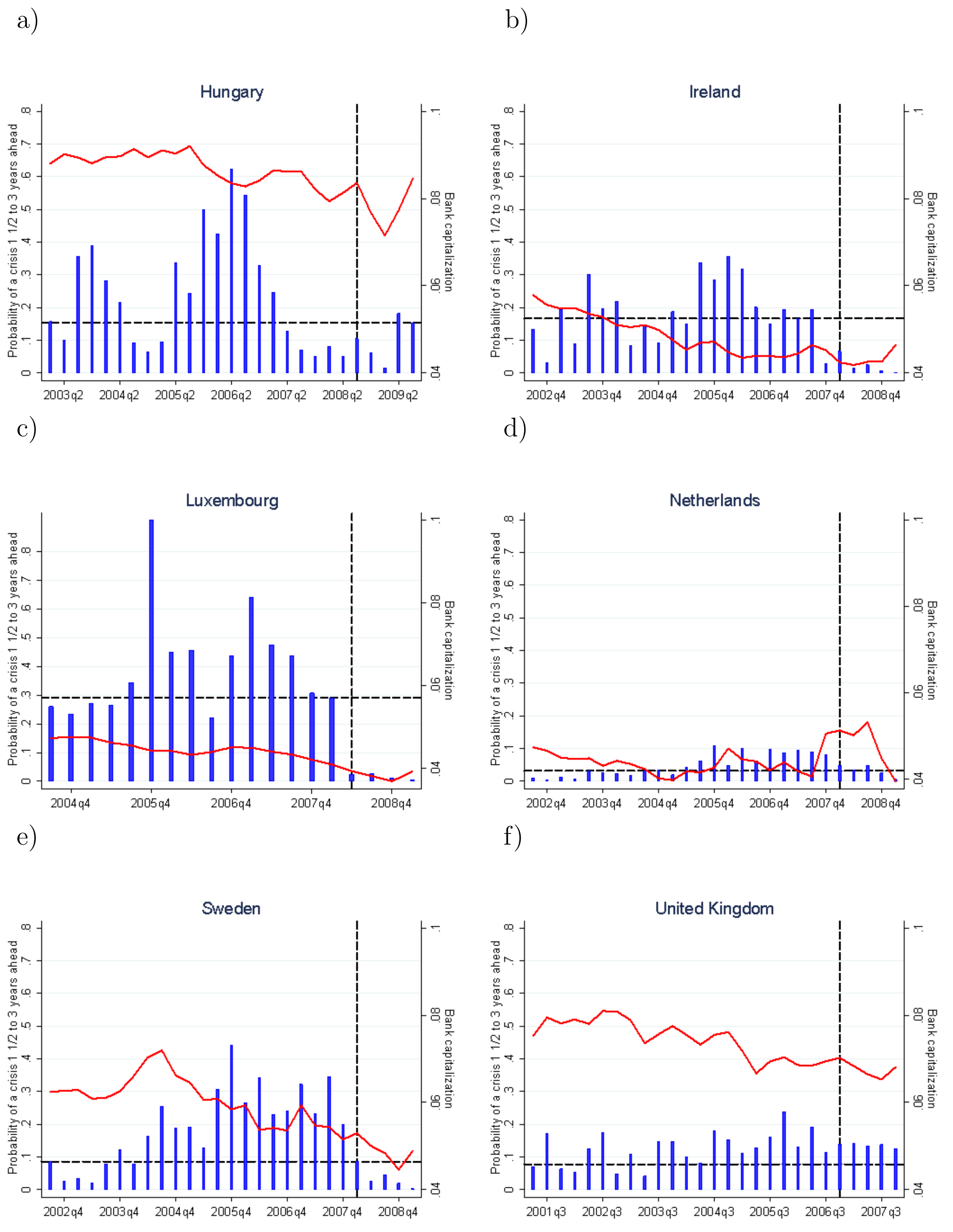


Figure 2.3 continued...

The figure plots the predicted probabilities (blue bars) from our benchmark model (Model 5 in Table 2.4) around the crises of 2008 in our sample countries (depicted by the dashed vertical lines). The optimal threshold for each country is depicted by the dashed horizontal line. The model issues a warning whenever the predicted probability is above this threshold. The red line shows the development of aggregate capitalization in the banking sector defined as total banking sector equity over total banking sector assets.

2.4.2 Out-of-sample performance of the models

Given the objective of the early warning systems, any assessment should focus on the out-of-sample performance. Moreover, as shown by e.g. Berg, Borensztein, and Patillo (2005), successful in-sample predictions are much easier to achieve than successful out-of-sample predictions. In order to assess the out-of-sample usefulness of the models we proceed as follows: First, we consecutively exclude countries that had a banking crisis prior to 2007 from the estimation of the benchmark model. Then, we test whether the model based on the remaining countries is able to predict the crises in the excluded ones.[30]

The results of this exercise are presented in Figure 2.4. The benchmark model signals the banking crises in the Nordic countries well before their onset in the early 1990s.[31] In both Finland and Sweden, the indicator is consistently above the threshold from 1988Q2 onwards, which is 11 quarters ahead of the crisis for Finland and 9 quarters ahead for Sweden. In both cases, banks would have had enough time to build up capital before the crisis if the countercyclical capital buffer had been activated. Similarly, the model issues a warning signal for Italy from 1991Q2 onwards, 11 quarters ahead of the crisis in 1994. In the United Kingdom, the crisis is relatively close to the beginning of the sample period. Yet, in those quarters preceding the crisis of 1991, the benchmark model consistently issues a warning signal. Overall, the benchmark model exhibits strong out-of-sample properties. Information from the current crisis seems to be useful for the prediction of other systemic banking crises in the European Union.

2.4.3 Robustness checks

In this section we modify the benchmark model (Model 5 of Table 2.4) in several ways in order to further assess the robustness of our results. The results from the robustness analysis are presented in Tables 2.6 and 2.7.

[30] In principle we could have tried to fit a model to the observations prior to 2007 in order to see whether this model would be able to predict the current crisis. However, as most of the crisis episodes in our sample occur after 2007, and as we particularly want to learn something from these episodes, we prefer the approach described above, i.e., we use the information from the current crisis and check whether it would have been useful for the prediction of past crises.

[31] The model issues a warning whenever the predicted probability is higher than the optimal threshold within the country (indicated by the dashed horizontal line in the figure).

a) b)

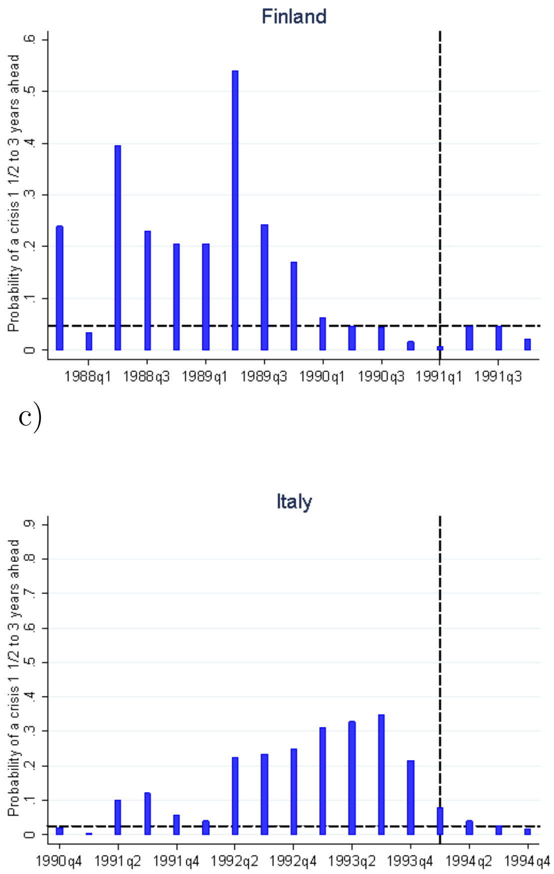


Figure 2.4: Out-of-sample performance of the model

The figure shows results for an out-of-sample evaluation of our benchmark model (Model 5 in Table 2.4). We exclude the respective country from the estimation and depict the predicted probabilities from a model based on the remaining countries around the crisis in the excluded country (dashed vertical line). The blue bars denote the predicted probabilities, while the horizontal dashed line presents the threshold.

First, we check whether our results depend on the definition of the dependent variable. Apart from the ESCB Heads of Research database used in our analysis, the most common definitions of systemic banking crises are provided by Reinhart and Rogoff (2011) and Laeven and Valencia (2012). Although the various databases are broadly consistent with each other, there are some deviations in the timing of crises as the definition of a systemic event in the banking sector requires a considerable amount of judgment. Columns 2 and 3 show that overall results are relatively similar for all three crisis definitions. Moreover, the area under the ROC curve is also greater than 0.8 for the other two models with the alternative crisis definitions, indicating good predictive abilities of the models.

Second, we include a dummy variable that is equal to one for each quarter in which the respective country is a member of the European Monetary Union (EMU). As expected, the coefficient for this dummy variable is positive and significant as most crises in our sample occur after the establishment of the EMU in 1999 (see column 4). However, the coefficients of the other variables remain largely unaffected by the inclusion of this dummy variable. Furthermore, the results are robust if we restrict the sample to include only countries from the EU-15 (column 5) or only countries that are part of the EMU (column 6).

Third, we augment the model with a money market rate (column 7). The estimated negative coefficient is potentially related to the 'great moderation', i.e., the general decline of inflation and money market rates over the sample period. The high R-squared and AUROC indicate that the fit of the model is superior compared to the other models. Despite this, we do not select this model as our benchmark model as its out-of-sample forecast abilities are inferior to the benchmark model, potentially due to an overfitting problem.

Fourth, following Lo Duca and Peltonen (2013), we transform all variables into country-specific percentiles before using them in the regression. This method can be seen as an alternative way to account for heterogeneity across countries as differences in levels of indicators between countries vanish for the transformed variables. Columns 8 and 9 show that most of the estimated coefficients have the same sign as in the benchmark model if we use this alternative method.

Finally, we analyze model performance across different forecast horizons (see also Schudel 2013). Specifically, we check how the performance of the benchmark model and the indicator properties of variables change if the time window of the vulnerable state preceding a systemic banking crisis is altered from the twelve to seven quarters used in the standard specifications. Results in Table 2.7 show that although the benchmark model is broadly robust to an alteration of the forecast horizon, the relative importance and the estimated signs of the coefficients tend to vary a bit. Particularly important are the reversed signs for domestic and global credit growth in the model with the twenty to thirteen quarters ahead definition of a vulnerable state and the strong influence of global asset prices in this model (Model R3). As

Table 2.6: Robustness checks

	(1) Benchmark	(2) RR	(3) LV	(4) EMU	(5) EU-15	(6) Euro	(7) Interest Rates	(8) Percentiles	(9) Percentiles
Dom. Credit Growth (DC1)	1.54	-10.01**	-0.58	-1.08	1.01	-4.23	2.83	-0.009	-0.000
	(3.73)	(4.71)	(3.28)	(4.29)	(4.57)	(6.77)	(5.86)	(0.008)	(0.010)
Dom. Credit to GDP Gap (DC2)	12.98***	23.91***	4.09	15.68***	12.46***	14.70***	37.86***	0.039***	0.050***
	(2.27)	(4.32)	(2.60)	(2.51)	(2.64)	(4.73)	(4.67)	(0.007)	(0.009)
Interaction(DC1 × DC2)	55.12**	35.24	26.75	57.40**	55.10*	138.12*	83.42*	0.018***	0.026***
	(22.35)	(36.30)	(26.96)	(26.67)	(31.69)	(71.79)	(46.34)	(0.005)	(0.005)
Glo. Credit Growth (GC1)	25.99***	32.12***	39.40***	49.67***	18.57**	13.00	108.66***	0.024**	0.018
	(8.88)	(10.77)	(15.26)	(10.14)	(9.42)	(10.23)	(16.80)	(0.010)	(0.011)
Glo. Credit to GDP Gap (GC2)	12.19	-8.68	42.49***	-4.67	20.22*	18.52*	-37.62**	-0.033***	0.004
	(8.89)	(10.83)	(15.62)	(12.87)	(11.24)	(10.52)	(17.82)	(0.008)	(0.013)
Interaction(GC1 × GC2)	-324.72	-255.97	1,718.19***	-806.86**	-390.66	-121.35	-1,941.20***	0.011	-0.005
	(305.59)	(320.49)	(587.11)	(345.60)	(296.43)	(301.21)	(527.98)	(0.009)	(0.010)
Interaction(DC1 × GC1)	-28.48	77.07	12.25	-17.42	-240.77**	-134.60	208.01	-0.001	-0.004
	(68.99)	(88.37)	(104.67)	(89.52)	(102.03)	(101.30)	(139.13)	(0.005)	(0.006)
Interaction(DC2 × GC2)	-472.20***	-754.19***	-339.87***	-617.47***	-436.37***	-276.74**	-1,466.07***	-0.042***	-0.062***
	(91.07)	(102.92)	(66.07)	(133.84)	(96.37)	(109.19)	(182.09)	(0.006)	(0.010)
GDP Growth	19.64	2.67	11.35	12.95	18.29	14.00	35.04	0.003	0.001
	(18.97)	(20.61)	(25.41)	(20.57)	(20.65)	(21.40)	(24.89)	(0.006)	(0.006)
Inflation	-29.04**	-3.98	-25.31*	-29.70**	-3.65	15.29	68.72***	-0.001	-0.004
	(11.73)	(11.60)	(15.21)	(12.35)	(10.79)	(10.41)	(20.10)	(0.005)	(0.006)
Equity Price Growth	-1.01	-0.20	-1.46	-1.57	-0.43	-0.14	-1.77	-0.008	-0.009
	(1.10)	(1.49)	(2.11)	(1.16)	(1.26)	(1.51)	(1.91)	(0.006)	(0.006)
House Price Growth	16.73***	14.03***	6.73	21.02***	23.45***	20.71***	20.73**	0.019***	0.015**
	(5.40)	(4.81)	(8.52)	(6.20)	(5.33)	(5.50)	(9.37)	(0.005)	(0.006)
Gov. Bond Yield							-28.69		
							(45.08)		
Money Market Rate							-125.08***		
							(38.55)		
Global GDP Growth	-10.24	-4.87	-40.28*	-8.78	-7.63	-10.67	0.08	-0.004	-0.004
	(12.62)	(12.25)	(20.80)	(12.60)	(12.95)	(15.21)	(11.66)	(0.009)	(0.009)
Global Equity Price Growth	7.39	7.11	21.31***	6.86	8.58*	10.72*	6.47	0.016	0.018*
	(4.80)	(4.56)	(7.62)	(4.99)	(5.07)	(6.14)	(4.61)	(0.010)	(0.010)
Global House Price Growth	16.29	8.29	51.73**	11.56	20.92	27.87	27.49	0.046***	0.054***
	(18.34)	(17.54)	(23.01)	(20.00)	(18.53)	(21.34)	(19.84)	(0.014)	(0.013)
D(EMU)				2.83***					
				(0.62)					
Country dummies	YES	YES	YES	YES	YES	YES	YES	NO	YES
Observations	919	835	893	919	869	664	862	919	919
Pseudo R-Squared	0.278	0.286	0.442	0.314	0.269	0.270	0.477	0.324	0.372
AUROC	0.865	0.827	0.807	0.887	0.847	0.836	0.942	0.892	0.909
Standard error	0.0160	0.0207	0.0223	0.0136	0.0172	0.0184	0.0091	0.0126	0.0117

The table shows several robustness checks for our benchmark model (Model 5 in Table 2.4). All specifications (except column 8) include country-specific dummy variables to account for unobserved heterogeneity across countries. Robust standard errors adjusted for clustering at the quarterly level are reported in parentheses. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

Table 2.7: Robustness—forecast horizon

	(1) Benchmark 7-12 quarters	(2) Model R1 1-6 quarters	(3) Model R2 1-12 quarters	(4) Model R3 13-20 quarters
Dom. Credit Growth (DC1)	1.54	-11.63	-3.06	-11.77***
	(3.73)	(8.06)	(4.28)	(4.07)
Dom. Credit to GDP Gap (DC2)	12.98***	61.69***	33.92***	16.03***
	(2.27)	(20.72)	(3.91)	(4.08)
Interaction(DC1 × DC2)	55.12**	-18.61	139.78***	-1.36
	(22.35)	(82.99)	(36.95)	(32.76)
Glo. Credit Growth (GC1)	25.99***	113.85***	93.09***	-50.73***
	(8.88)	(18.75)	(12.10)	(14.10)
Glo. Credit to GDP Gap (GC2)	12.19	17.90	2.08	28.57**
	(8.89)	(21.61)	(10.95)	(13.06)
Interaction(GC1 × GC2)	-324.72	6,895.95***	2,763.43***	-52.28
	(305.59)	(1,067.10)	(502.18)	(449.06)
Interaction(DC1 × GC1)	-28.48	453.38	223.97**	-606.47***
	(68.99)	(322.98)	(99.32)	(111.78)
Interaction(DC2 × GC2)	-472.20***	-1,947.93***	-1,193.54***	-458.57***
	(91.07)	(565.00)	(151.01)	(79.88)
GDP Growth	19.64	-28.92	-14.58	35.03
	(18.97)	(48.51)	(20.64)	(40.03)
Inflation	-29.04**	77.15***	22.54*	24.42**
	(11.73)	(24.02)	(13.25)	(10.59)
Equity Price Growth	-1.01	2.14	-0.45	-1.14
	(1.10)	(2.34)	(1.40)	(1.98)
House Price Growth	16.73***	-22.60**	7.29	8.62
	(5.40)	(9.98)	(6.19)	(5.91)
Global GDP Growth	-10.24	-0.39	-2.07	12.86
	(12.62)	(12.63)	(13.10)	(9.72)
Global Equity Price Growth	7.39	14.15***	15.06***	12.53***
	(4.80)	(5.08)	(4.91)	(4.76)
Global House Price Growth	16.29	-60.67**	-32.74	116.13***
	(18.34)	(30.62)	(23.07)	(21.94)
Observations	919	919	919	919
Pseudo R-Squared	0.278	0.781	0.617	0.340
AUROC	0.865	0.726	0.960	0.895
Standard error	0.0160	0.0179	0.0063	0.0134

The table shows the results of robustness analysis with respect to the forecast horizon. Column 1 re-estimates Model 5 from Table 2.4, which is referred as the benchmark model. The dependent variable in this regression is set to one twelve to seven quarters preceding a banking crisis in a respective country. In column 2, we replace the dependent variable with a dummy that is equal to one, six to one quarter before a banking crisis. Similarly, the dependent variable in column 3 equals one twelve to one quarter before a banking crisis in the respective country. Finally, the dependent variable in column 4 is equal to one twenty to thirteen quarters before the onset of a banking crisis in a respective country. Robust standard errors adjusted for clustering at the quarterly level are reported in parentheses. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

shown in Table 2.5, the benchmark model with a forecast horizon of twelve to seven quarters (Model 5) provides the highest absolute and relative usefulness measures, followed by the model with a forecast horizon of twelve to one quarter (Model R2). The performances of the models with the early (six to one quarter) and late (twenty to thirteen quarters) pre-crisis time horizons in terms of absolute and relative usefulness are broadly similar, but markedly lower than that of the benchmark model.

2.5 Conclusion

As a response to recent financial crises, the Basel III / CRD IV regulatory framework includes a countercyclical capital buffer (CCB) to increase the resilience of the banking sector and its ability to absorb shocks arising from financial and economic stress. In this context, this paper seeks to provide an early warning model, which can be used to guide the build-up and release of capital in the banking sector. Given the prominence of private credit variables in the upcoming regulations, the paper first examines the evolution of credit variables preceding banking crises in the EU Member States, and assesses their usefulness in guiding the setting of the CCB. Furthermore, the paper examines the potential benefits of complementing private credit variables with other macro-financial and banking sector indicators in a multivariate logit framework. The evaluation of the policy usefulness of the credit indicators and models follows the methodology applied in Alessi and Detken (2011), Lo Duca and Peltonen (2013) and Sarlin (2013).

The paper finds that, in addition to credit variables, other domestic and global financial factors such as equity and house prices and banking sector variables help to predict macro-financial vulnerabilities in EU Member States. We therefore suggest that policy makers take a broad approach in their analytical models supporting CCB policy measures. The models demonstrate good out-of-sample predictive power, signaling the Swedish and Finnish banking crises of the early 1990s at least 6 quarters in advance.

Chapter 3

Limits of Model-Based Regulation

3.1 Introduction

Following the financial crisis of 2008, policy makers around the world have concentrated their efforts on designing a regulatory framework that increases the safety of individual institutions as well as the stability of the financial system as a whole. Most prominently, the Basel III framework aims to enhance both the level and the quality of banks' regulatory capital (Basel Committee on Banking Supervision 2011). While there is relatively wide agreement on the necessity of such measures, a deeper debate has evolved on whether capital levels are appropriately measured in the current framework. Specifically, capital requirements under Basel III are defined in terms of risk-weighted assets (RWA), a measure that crucially depends on estimates from banks' internal risk models.

Proponents of risk-weighted regulation argue that it leads to a better allocation of resources, as banks are no longer penalized for having low-risk positions on their balance sheets. As model-based regulation sought to achieve a better alignment between regulatory capital and actual asset risk, the scope for regulatory arbitrage was meant to be reduced. However, although well-intended, critics argue that by now the regulatory system is much too complex, making it difficult for regulators to keep track of all the bank internal estimations required for the determination of regulatory capital ratios. Additionally, as it has become evident that RWA tend to vary across banks, regulators, investors, and even the banks themselves increasingly distrust this measure, preferring to rely on un-weighted capital ratios instead.[1] Overall, the calls for simpler capital rules seem to become louder.[2]

In this paper, we analyze how the introduction of risk-weighted capital charges affected banks' lending behavior towards the German corporate sector. The main thesis of the paper is that regulation based on models tends to reward "hard" information at the expense of "soft" information (or other information that is not included in the risk model). To the extent that the quality of a loan is a function of both soft and hard variables, this overweighting of hard information alters the relative mix of soft and hard information, thus changing the very quality of the loan pool.[3] Further, linking capital charges to model-based risk estimates creates a preferential treatment of loans to firms that score high on the dimensions used as inputs in the risk model. Consequently, we expect that loans to firms with low model-based risk estimates are expanded relatively more following the introduction of risk-weighted capital charges.

It is likely that shifts in the loan portfolio have a direct impact on the accuracy of banks' risk models. These models have been calibrated under a regime in which their outputs did not affect capital charges for loans. However, with risk-weighted regulation, the overreliance on hard information could induce a worsening of the borrower pool, so that model-based risk estimates would systematically underestimate the true riskiness of the borrowers. Since banks shift their lending towards borrowers with low model-based risk estimates, we expect average probabilities of default (PDs) for loan portfolios that apply the new regulatory approach to be lower. Further, as model-based regulation induces a change in the borrower pool, we expect risk estimates for these loans to be less accurate than risk estimates for loans under the old regime. While the first effect is likely to have a positive impact on financial stability, the second effect is likely to have a negative impact on financial stability.

To test these hypotheses, we exploit the institutional details of the German Basel II introduction in 2007. Following the reform, banks were allowed to choose between a new regulatory approach (referred to as the internal ratings-based ap-

[1]See, e.g., Basel Committee on Banking Supervision (2013), European Banking Federation (2012), and the studies by individual banks cited in these publications.

[2]See, e.g., Acharya, Engle, and Pierret (2013), Admati and Hellwig (2013), Haldane (2013), Hoenig (2013), Hellwig (2010), or Hoenig (2010).

[3]This resonates with the theory of multitasking proposed by Holmström and Milgrom (1991). In the context of securitization, this argument has been used in Rajan, Seru, and Vig (2012).

proach, short IRB) and a more traditional approach that did not rely on internal risk parameters (referred to as the standard approach, short SA). The introduction of IRB required an extensive risk management system that had to be certified by the regulator. Consequently, only very large banks introduced the new regulatory approach, while smaller regional banks opted for the standard approach to determine capital charges. In the first part of the paper, we analyze how banks that introduced the new regulatory approach adjusted their lending following the reform, as compared with banks that did not introduce the new approach. As we are trying to identify a supply side effect, we focus on firms that borrow from both types of banks. Identifying our coefficients from variation within the same firm allows us to control for credit demand (see Khwaja and Mian 2008).

Importantly, the introduction of IRB in German banks was staggered over time. As risk models need to be certified by the regulator on a portfolio basis, banks did not shift all their loan portfolios to the new approach at the same time.[4] While IRB banks report model-based risk estimates (i.e., PDs) for most of their loans, some of these loans are still subject to the standard approach, while others have already been shifted to IRB. Exploiting this setup, we are able to test for systematic differences in the prediction error (i.e., the difference between a dummy for actual default and the PD of the loan) between IRB loans and SA loans. As we have relationship-level data, we can systematically control for bank as well as firm heterogeneity.

The following findings emerge from our analysis. First, we show that indeed the reform changed both the quantity and the composition of bank lending. Risk weights are calibrated in a way that ensures that capital charges under IRB are on average lower than under SA.[5] Consequently, as the reform meant a reduction in capital charges for banks that introduced IRB, these banks increased their lending by about 9 percent as compared with banks that remained under the standard approach. Further, controlling for firm heterogeneity, we find that IRB banks increase lending to the same firm relatively more if model-based PDs for the firm are relatively low, but not if they are relatively high. For example, an increase of one standard deviation

[4]Banks had to shift whole portfolios of loans to the new approach. They were not allowed to pick individual loans for IRB. Furthermore, they were not allowed to move IRB portfolios back to SA.

[5]Regulators wanted banks to introduce the new approach and hence provided incentives for the costly implementation of IRB.

in firm PD induces a 1.2 percent smaller increase in loans from IRB banks. These estimation results are robust to the inclusion of bank fixed effects that control for heterogeneity across banks. Hence, credit supplied by banks that introduced IRB exhibits a higher sensitivity to model-based PDs than credit supplied by banks that remained under SA.

Second, we evaluate how these changes in the lending decision process affected banks' evaluation of credit risk. In 2008, IRB banks had transferred only a portion of their loan portfolios to the new approach. Exploiting this within bank variation, we analyze whether the predictive abilities of banks' PDs depend on the regulatory approach used for a specific loan. We observe that the average PD is always lower in IRB portfolios as compared with SA portfolios. This is consistent with the documented shift in lending towards firms with low model-based PDs. However, there seems to be no difference in the average default rate between the two types of loans. Risk estimates for IRB loans underpredict actual default rates, while there is no such effect in PDs for SA loans. Again, the result is robust to the inclusion of bank fixed effects that control for bank heterogeneity. While the effect is particularly strong directly after the reform, it is also present in later periods and persistent until the end of the sample period in 2011.

Our results could be biased if the order in which banks transfer their loan portfolios to IRB is driven by factors that explain differences in the predictive abilities of PDs for SA loans and IRB loans. Given the institutional details of the Basel II introduction in Germany, such a scenario is very unlikely. Nevertheless, to remove any remaining doubts, we focus on variation over time within the portfolio of IRB loans. For loans originated in 2005 or 2006, the average PD is similar to the actual default rate. In contrast, for loans originated after the Basel II reform, in 2007 or 2008, the actual default rate is higher than the average PD, indicating an underestimation of credit risk for this set of loans. The fact that the underestimation effect is much stronger for IRB loans that were originated after the reform as compared with IRB loans originated before the reform indicates that our findings are not driven by the selection of IRB loan portfolios. While these loans differ in the time of origination, they find themselves within the same portfolios within IRB banks, i.e., within those portfolios for which the new approach has already been implemented.

It is important to note that our findings do not imply that banks manipulate PDs for IRB loans. If this would be the case, we should observe an underprediction of actual default rates that is independent of the issuance date of IRB loans. Rather, we believe that the most likely explanation is a change in incentives induced by an overreliance on information included in banks' risk models (i.e., a change in the borrower pool after the reform). While the underestimation effect is particularly pronounced right after the introduction, it does not seem as if the model validation process within IRB banks is able to solve the problem. A validated risk model again provides incentives to bankers to expand lending to those borrowers that score particularly well in the modified risk model. Since risk models do not include the entire information set available to the banker (including the soft information that is costly to collect for the banker), the bias is likely to be persistent.

When assessing the impact of our results on financial stability, one potential caveat has to be taken into account: Apart from the PD, risk-weights in the model-based approach also depend on loan-specific factors such as the loss given default (LGD), exposure at default (EAD), and the maturity (M) of the loan. Risk-weights in the advanced IRB approach will be lower the better the estimate on any of these parameters. Hence, the reform provides additional incentives for banks to invest into the quality of these parameters, for example by increasing the level of collateralization for IRB loans. Consequently, overall loan quality might have improved, despite the fact that default rates are higher than PDs for IRB loans. An assessment of the reform on overall credit risk and bank stability needs to take all loan-specific factors into account. Nevertheless, we believe that the underestimation of actual default rates that we document is interesting in itself.

Our paper also has important policy implications regarding the design of the new regulatory framework, Basel III. Although the framework introduces a leverage ratio, its currently discussed level is rather low, so that risk-based requirements as in Basel II would remain the binding ones for most banks. Our findings highlight important deficiencies of such an approach. To be clear, this paper does not make the point that a leverage ratio is better able to regulate a bank's capital. But clearly, more research is required to evaluate the pros and cons of different approaches to capital regulation in a systematic manner.

Our paper connects several strands of the literature. A small but growing number of papers analyzes how ratings used for regulatory purposes affect financial stability. Most recently, the Basel Committee on Banking Supervision (2013) published an extensive study that showed a considerable impact of banks' modeling choices on risk-weights, documenting that estimated risk parameters vary widely across banks, even for the same exposures.[6] This generates a lot of uncertainty about risk-based capital ratios, and increasingly market participants seem to lose faith in the meaning of these measures, as documented by Demirgüç-Kunt, Detragiache, and Merrouche (2013).[7] Apart from its credibility problem, Hellwig (2010) argues that model-based capital regulation suffers from the fact that many of the risks involved are not exogenously given, but endogenously determined. As they depend on the behavior of the parties involved, they may change over time, and tracking them for regulatory purposes may be close to impossible.[8] An example is given by Acharya (2011), who argues that low risk weights for residential mortgage-backed securities made investment in this asset class attractive and endogenously turned it into a systemically important asset class. Moreover, Acharya, Engle, and Pierret (2013) question the predictive abilities of risk weights, as they are based on accounting data and can only be updated ex-post. According to their argumentation, banks game risk-weighted assets by shifting their portfolios towards assets with lower risk weights, and false and underestimated risk weights automatically lead to excessive leverage (see also Hoenig 2013). We document a shift in lending towards firms with low model-based PDs, followed by a systematic failure of internal risk models, hence providing direct empirical evidence in support of this view. What is more, our identification strategy in connection with the richness of our data set allows us to causally identify the effect of the shift towards model-based regulation on financial stability.

Further, our paper adds to the public choice literature on regulatory capture. A recent theoretical paper by Hakenes and Schnabel (2013) illustrates how the use of sophisticated risk models can induce inefficiently low levels of regulation in the banking sector. Empirical evidence that regulatory reform in the financial sector is

[6]See also Firestone and Rezende (2013) and Le Leslé and Avramova (2012).

[7]See also Hagendorff and Vallascas (2013), Das and Sy (2012), or Mariathasan and Merrouche (2012).

[8]Rajan, Seru, and Vig (2012) apply this reasoning in the context of securitization.

often driven by industry interests rather than public interest is offered by Becker and Opp (2013), who examine a change in capital charges on insurance holdings of mortgage-backed securities. We add to this literature by showing that the introduction of model-based capital regulation—arguably the most important innovation in bank regulation in recent decades—mainly served the interests of the industry itself and compromised financial stability.

3.2 The introduction of model-based regulation in Germany

One of the main objectives of bank regulation in recent decades has been to establish a closer link between capital charges and actual asset risk. Regulators around the world promoted the adoption of stronger risk management practices by the banking industry in order to achieve the ultimate goal of a sound and stable international banking system.[9] In 1988, the Basel I agreement introduced risk-based capital charges by assigning bank assets into different risk groups (or buckets) with pre-assigned risk-weights (Basel Committee on Banking Supervision 1988). Risk-weighted assets were calculated by multiplying these risk-weights (0, 20, 50, or 100 percent) with actual asset values, and capital requirements were defined in terms of risk-weighted assets.

The next revision of this regulatory framework, referred to as Basel II, tried to establish a more granular link between capital charges and individual asset risk. The new framework, introduced in Germany in 2007, allowed banks to use their own internal risk models to determine capital charges for credit risk (Basel Committee on Banking Supervision 2006). Under the internal ratings-based (IRB) approach, each exposure gets assigned an individual risk weight that crucially depends on the bank's estimated probability of default (PD) for a specific borrower.[10] Risk-

[9]The introduction of risk-weighted capital charges and potential problems related to them have been discussed in several papers, e.g. Brun, Fraisse, and Thesmar (2013), Hellwig (2010), Kashyap and Stein (2004), Daníelsson et al. (2001), Jones (2000), Brinkmann and Horvitz (1995). For an assessment from the side of the regulator see Basel Committee on Banking Supervision (1999).

[10]In the foundation IRB approach the bank estimates only the PD, while standard values are assumed for loss given default (LGD), exposure at default (EAD), and maturity of the loan. In the advanced IRB approach, the bank has to estimate all four parameters. As risk weights depend on

weighted assets are calculated by multiplying the—loan-specific—risk-weights with actual assets values, and capital requirements are defined in terms of risk-weighted assets as under Basel I.

In Germany, Basel II was implemented by revision of the Solvabilitätsverordnung (2006), which provides the foundation for national bank regulation. This code allows banks to choose between two broad methodologies for calculating their capital charges: The internal ratings-based approach described above and the so-called standard approach, that is basically equivalent to the old Basel I framework with fixed risk weights for corporate loans (100 percent of the loan amount net of collateral).[11]

The Solvabilitätsverordnung (2006) provides a comprehensive set of rules and guidelines for banks that want to use internal risk models for calculating their capital charges: PD models used for regulatory purposes should estimate creditors' one-year probability of default.[12] As the bank could have incentives to report low PDs in order to economize on regulatory capital, internal risk models are subject to a strong supervisory review—including on-site audit (see also Bundesbank 2004). In particular, the regulator requires a precise and consistent estimation of credit risk, and proof that the model has been used for internal risk management and credit decisions for at least three years before it may be used for regulatory purposes. Furthermore, the bank has to constantly validate its models and adjust them if their estimates are inconsistent with realized default rates. The supervisor certifies rating systems, continuously monitors compliance with minimum standards, and assesses banks' internal validation procedures (see also Bundesbank 2003).

PD models are estimated on a portfolio basis. For corporate loans, their most important determinant is accounting information from firms' financial statements (see, e.g., Initiative für den Finanzstandort Deutschland 2006; Krahnen and Weber 2001). For loans to small and medium enterprises (SMEs), where there is often a significant publication lag for accounting information, also target financial ratios or

the PD—our parameter of interest—in both approaches, we do not distinguish between the two in the empirical analysis.

[11]Exceptions are cases where borrowers have external credit ratings, as the SA allows banks to use these ratings to determine capital requirements. However, the German market for corporate bonds is very small; hence, very few companies have an external rating.

[12]According to § 125 of the Solvabilitätsverordnung (2006), a creditor is in default if (a) the bank has valid indications that the creditor will not be able to fulfill his obligations, or (b) the creditor is more than 90 days past due on his obligations.

industry characteristics may be used. Besides these quantitative factors, also qualitative information such as a firm's management quality or its competitive situation can be included in the models. However, since such information is by definition hard to quantify its impact on the rating is rather limited. A prominent PD model used for the estimation of corporate credit risk is Moody's RiskCalc^{TM} model (Moody's Analytics 2013). To obtain predicted probabilities of default for a given portfolio, historical information on corporate defaults is regressed on accounting information such as the equity ratio, capital structure, net debt ratio, sales growth, net profit ratio, personnel cost ratio, payables payment period, or cash flow per liabilities. In a second step, estimates from this model are used to attribute predicted PDs to current and new borrowers. In cases where loan officers consider model outputs to be unreasonable they have the option to overwrite the predicted PD. However, if such overwrites occur to frequently, the regulator may ask the bank to revise its model. Furthermore, a bank has to revise its model if the annual validation process reveals a considerable discrepancy between predicted PDs and actual default rates.

Besides loan-specific variables such as the loss given default, the exposure at default and the maturity of a loan, the firm-specific PD estimate is the key ingredient for the calculation of risk-weighted assets. Figure 3.1 shows the relationship between estimated PDs and corresponding risk-weights, assuming standard values for the remaining parameters. Risk-weight curves are relatively steep for the lowest PDs and become flatter for higher PDs. This is in line with the objectives of the new agreement: To provide banks with incentives to introduce IRB, risk-weight curves were calibrated in a way that ensured that capital requirements would be substantially lower under IRB than under SA (Basel Committee on Banking Supervision 2006, p. 12).

To be eligible for the model-based approach to capital regulation, banks need to fulfill certain conditions and minimum disclosure requirements. Since the organizational efforts as well as the administrative expenses for the introduction of the new approach are high, only large banks opted for its introduction (of our sample of 1,603 German banks, only 45 banks applied for an IRB license; nevertheless these banks account for about 50 percent of the loans in our sample). The introduction of new rating models is a complex process, so that banks did not apply the new approach

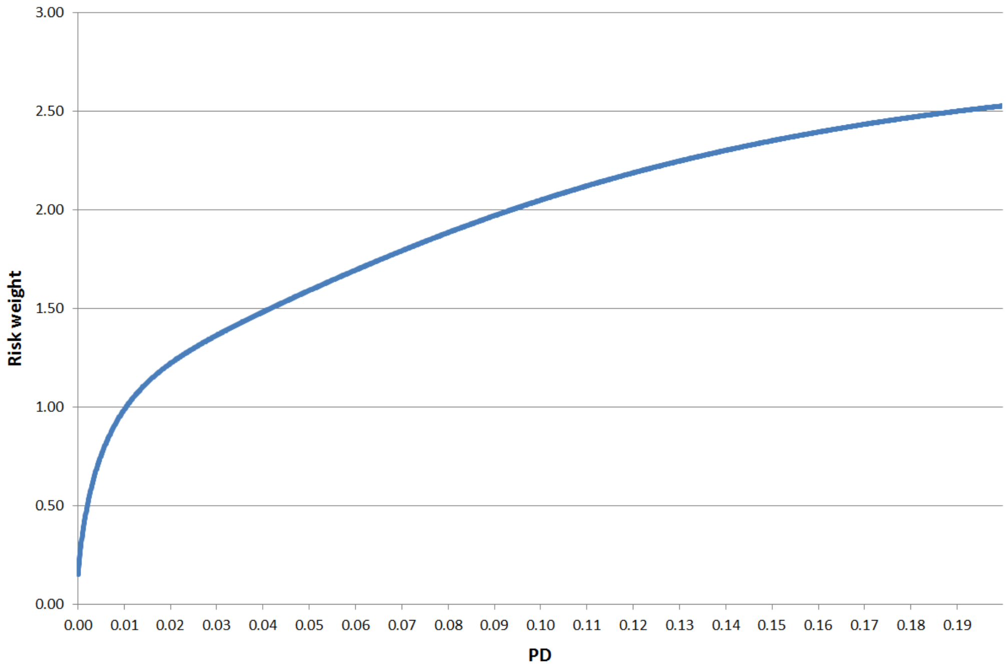


Figure 3.1: PDs and regulatory risk weights

The figure shows how estimated PDs map into regulatory risk-weights for loans in the corporate sector, assuming standard values for loss given default (45 percent) and loan maturity (2.5 years). The figure plots risk weights for loans to firms with a turnover larger than € 50 million. For loans to smaller firms, risk weights are multiplied with a correction factor depending on the exact amount of the turnover.

to all loans at once; rather, they agreed on a gradual implementation plan with the regulator.[13] The plan specified an order according to which different business units (loan portfolios) had to be shifted to IRB. As the calibration of a meaningful PD model requires a sufficient amount of data on past loan performance, banks typically started with loan portfolios in business units where they were relatively active. The phased roll-out of IRB means that during the transition, which typically lasts for several years, banks have both IRB and SA loans in their portfolios. We exploit this feature of the implementation process in our empirical section, where we compare PD estimations with actual default rates for loans that are subject to different regulatory approaches.

[13]See Solvabilitätsverordnung (2006), §§ 64-67 for details on the implementation plan.

3.3 Data

Our principal source of data is the German credit register compiled by Deutsche Bundesbank. As part of its supervisory role, the central bank collects data each quarter on all outstanding loans of at least € 1.5 million.[14] The data set starts in 1993 and includes information on the lender's and the borrower's identity, the amount of the loan outstanding and several other loan characteristics. As a response to the Basel II reform, reporting requirements for the credit register have been expanded considerably from 2008 onwards. In addition to the previous information, banks now also report exposure-level information on the regulatory approach (SA or IRB) and the estimated probability of default (PD). For loans under the IRB approach, the reported PD is the one that is used to determine regulatory capital charges. For loans under SA, banks also have to report PDs if they are estimated internally. As IRB banks aim to transfer all eligible loan portfolios to the new approach once the respective model is certified by the regulator, they report PDs for both IRB loans and SA loans. We use PDs for SA loans as a benchmark against which we evaluate the performance of PDs for IRB loans. Further, the database contains information on risk-weighted assets and loan loss provisions in case a loan defaults. The provisioning rules for loan losses are specified in the Solvabilitätsverordnung (2006). Banks have to make provisions that correspond to the expected loss as soon as there is information about repayment problems or default of a specific borrower (see § 125 of Solvabilitätsverordnung (2006)). We combine this exposure-level data with annual bank balance sheet information from Bundesbank's BAKIS database and annual firm balance sheet information from Bundesbank's USTAN database.

Our sample includes 1,603 German banks, 45 of which opted for IRB following the introduction of Basel II. Panel A of Table 3.1 shows that the average IRB bank is larger and less capitalized than the average SA bank, whereas average ROA is similar in the two groups of banks. Further, there are relatively more cooperative banks among the group of SA banks, whereas IRB banks are mostly large and internationally active commercial banks. Our empirical setup allows us to analyze the

[14]Since we focus on corporate lending, this cut-off does not constitute a big issue for our analysis. When matching firm balance sheet information from the Bundesbank USTAN database to the credit register, we find that—in the matched sample—lending recorded in the credit register makes up about 80-90 percent of firms' overall bank debt on average.

predictive abilities of PDs for loans subject to different regulatory approaches within the group of IRB banks only.

Descriptive statistics on the loan level are presented in Panel B of Table 3.1 and grouped by the regulatory approach used for the determination of capital charges. There are about twice as many SA loans (80,961) as compared with IRB loans (45,246) right after the introduction of the new regulatory approach. The first line of the table shows the average change in the amount of loans outstanding around the introduction of Basel II.[15] The average IRB loan in our sample was increased by about 6.5 percent over the Basel II introduction, while the average SA loan was increased by about 1.4 percent. Information on PDs becomes available in the credit register from 2008 onwards. The average PD in 2008Q1 is slightly higher for SA loans (2.6 percent) as compared with IRB loans (1.4 percent). While the PD estimates the firm-specific probability of default, the risk weight for a specific loan also incorporates loan specific information (e.g., the collateralization of the loan). For SA loans the corresponding risk weight does not depend on the PD and is equal to 100 percent of the unsecured fraction of the loan amount.[16] Overall, this translates into an average risk weight of 68.5 percent for SA loans, which is considerably higher than the average risk weight for IRB loans (47.8 percent). Furthermore, banks are required to report loan loss provisions for loans in default. Since certain loans are backed by collateral or guarantees, the consequences of a borrowers's default may vary. For both SA loans and IRB loans, loan loss provisions in case of a default are around 45 percent.

Finally, Panel C of Table 3.1 contains descriptives for several firm-level variables. First, we calculate a PD variable on the firm level by taking the average of all PDs assigned to the firm (by different banks) in the first quarter of 2008. The average for this firm-level variable is 2.2 percent and lies between the average PD for SA loans and the average PD for IRB loans. Second, several accounting variables are obtained

[15]The sample includes all loans in the credit register that have an observation both before and after the reform. We calculate the change in lending around the reform by collapsing all quarterly data for a given exposure into single pre-event and post-event periods by taking the average of the two years before and the two years after the Basel II introduction. The change in lending is defined as the difference in the logarithm of these averages, so that there is one observation per loan.

[16]The Basel regulations include a discount for loans to small and medium enterprises (SMEs) as the regulator wants to promote lending to these firms. Specifically, under Basel II, loans to firms with a turnover of € 50 million or less are subject to lower capital charges, as regular risk weights are multiplied with a correction factor depending on the exact amount of the turnover.

Table 3.1: Descriptives

Panel A: Bank descriptives

	SA banks (1,558 banks)		IRB banks (45 banks)	
	Mean	S.D.	Mean	S.D.
Bank assets (2006, in mn €)	1,330	3,750	133,000	259,000
Log bank assets (2006)	20.158	1.162	24.196	1.937
Bank equity ratio (2006)	6.366	4.202	4.246	2.471
Bank ROA (2006)	0.680	0.464	0.673	0.584
Bank type				
... commercial	14.0	–	54.3	–
... state	29.4	–	34.3	–
... cooperative	56.7	–	11.4	–

Panel B: Loan descriptives

	SA loans (81,961 loans)		IRB loans (45,246 loans)	
	Mean	S.D.	Mean	S.D.
Δ log(loans)	0.016	0.358	0.064	0.570
PD	0.026	0.060	0.018	0.061
RWA to loans	0.685	0.375	0.422	0.436
LLP to loans	0.438	0.286	0.478	0.305
Interest rate	0.068	0.040	0.075	0.042

Panel C: Firm descriptives

	(5,961 firms)	
	Mean	S.D.
Firm PD	0.022	0.031
Firm assets (2006, in mn €)	154	817
Firm debt / assets (2006)	0.343	0.202
Log firm assets (2006)	10.363	1.428
Firm ROA (2006)	7.909	6.982

Panel A shows descriptive statistics for the groups of SA and IRB banks. An IRB bank is defined as a bank that uses the internal ratings-based approach for some loans during our sample period, whereas an SA bank is defined as a bank that uses the Basel II standard approach in all its lending relationships. Panel B shows summary statistics for loans in the German credit register. Data are restricted to (a) loans that are larger than € 1.5 million (b) loans from commercial, state, or cooperative banks that are subject to the Basel II capital regulation (c) loans that have an observation both before and after the introduction of Basel II in 2007. Besides information on changes in lending around the reform the panel also includes information on loan interest rates, on the loan-specific ratio of risk-weighted assets to loans, of loan loss provisions to loans, and on the PD in 2008Q1, the first quarter for which this information is available. Panel C contains information on the firm level for a matched sample of 5,961 firms. Firm balance sheet information is obtained from Bundesbank's USTAN database.

by a hand-match of the Bundesbank USTAN database with the credit register.[17] The match was conducted based on company name, location, and industry segment that are available in both data sources. The matched dataset contains detailed information on lending relationships and balance sheet items for 5,961 distinct firms. We report summary statistics on total assets, debt to assets and return on assets (ROA) for this sample. The average size of our sample firms is 154 million euros, the average debt to asset ratio is 34.3 percent, and the average return on assets is 7.9 percent.

3.4 Banks' lending reaction to the introduction of IRB

In this section we document banks' lending reaction to the introduction of model-based capital regulation, i.e. the internal ratings-based approach. We expect two effects: First, as capital requirements are lower under IRB than under SA, we expect that banks that introduced the new approach expand their lending relative to banks that did not. Second, as the reduction in capital requirements is greatest for firms with relatively good hard information (i.e., firms with relatively low PDs), we expect that IRB banks' expansion in lending is greatest for these firms.

3.4.1 Bank-level lending

Acknowledging high organizational and administrative efforts for the introduction of IRB, the regulator provided banks with incentives to introduce the new approach by calibrating it in a way that ensured that requirements were lower under IRB than under SA (Basel Committee on Banking Supervision 2006, p. 12). Consequently, when banks introduced IRB in 2007Q1 they experienced a reduction in capital requirements for loans—both in absolute terms and relative to SA banks that did not introduce the new approach. Figure 3.2 shows that following this reform IRB banks expanded their lending to corporate borrowers in Germany. For each group

[17] Even though the credit register and the accounting information all come from Deutsche Bundesbank, the two data sets have no unique identifier. For a detailed description of the USTAN database see Bachmann and Bayer (2013).

of banks—SA banks and IRB banks—we sum all loans in a given quarter to obtain aggregate loans. The figure shows the logarithm of aggregate loans—scaled by its value in 2007Q1—for SA and IRB banks. Prior to the introduction the development of aggregate loans was relatively similar for the two groups of banks. Following the reform, however, we see a sharp increase in aggregate loans for IRB banks, while the loans of SA banks remain relatively constant or even decline.

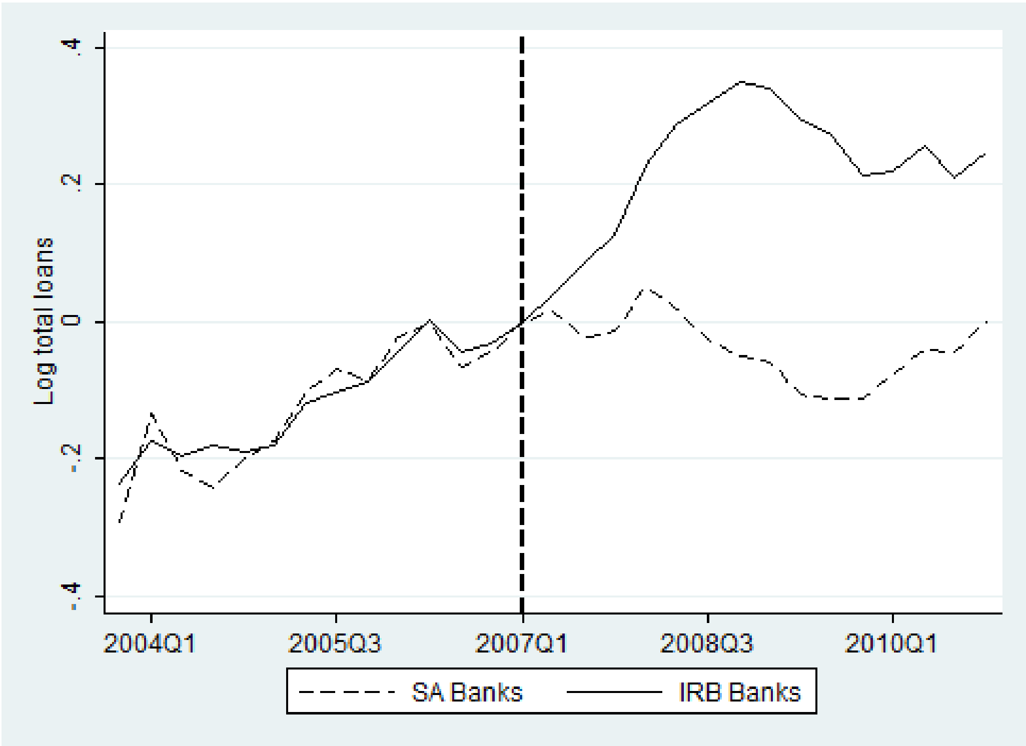


Figure 3.2: Aggregate lending around the Basel II introduction

The figure shows the development of aggregate lending in our sample for SA banks and IRB banks around the Basel II introduction in the first quarter of 2007. Aggregate numbers are obtained from the German credit register and calculated by summing all loans from the respective group of banks within a given quarter. Aggregate loans are standardized by their value in 2007Q1, and the figure shows the logarithm of this ratio (see Khwaja and Mian 2008 for a similar graphical illustration).

We formally test whether IRB banks expanded lending relative to SA banks following the reform by running simple, cross-sectional ordinary least squares (OLS) regressions. To avoid problems of autocorrelation we collapse quarterly bank-level loans into single pre-event and post-event time periods by taking the average of the two years before and the two years after the reform (Bertrand, Duflo, and Mullainathan 2004). The change in the logarithm of these averages serves as dependent variable in the following regression:

$$\Delta log(bank\ loans) = D(IRB\ bank) \times \beta_1 + B'\beta_2 + \epsilon, \tag{3.1}$$

where *D(IRB bank)* is a dummy that indicates whether the respective bank introduced the IRB approach during our sample period and B' is a vector of bank control variables that includes the pre-event values of the logarithm of assets, the ratio of equity to assets, the ROA, and a set of dummies that indicate the bank's type. To be clear, the regression includes one observation for each bank, measuring the change in aggregate loans over the Basel II introduction in 2007Q1.

Estimation results are presented in Table 3.2. The first column includes only the IRB bank dummy as an explanatory variable. Following the reform, IRB banks increased their lending by about 9 percent as compared with SA banks.[18] In column 2 we add several bank-level control variables, and find that smaller banks, better capitalized banks, and more profitable banks increased their lending relatively more following the reform. The coefficient for the IRB bank dummy doubles in magnitude as compared with column 1, and also becomes more significant. Finally, in column 3, we add bank type dummies and find that state banks reduced their lending relative to commercial banks and cooperative banks. The coefficient for the IRB bank dummy remains significantly positive. Overall, the findings in this section document that—as expected—those banks that opted for the introduction of the Basel II internal ratings-based approach, and hence experienced a reduction in capital charges for the average loan, increased their lending relative to banks that remained under the standard approach. In the next section, we check whether this increase in lending is particularly strong for loans to firms with relatively good hard information.

3.4.2 Loan-level lending and hard information

Under IRB, the capital charge for a specific loan depends on the estimated PD for that loan (see Section 3.2 for details). The PD is determined by the bank's internal risk model and depends on several hard information criteria. The better the hard

[18] According to Halvorsen and Palmquist (1980), the effect of dummy variables in semi-logarithmic equations is equal to $exp(\beta) - 1$.

Table 3.2: Bank-level lending

	Dependent variable: Δ log(bank loans)		
	(1)	(2)	(3)
D(IRB bank)	0.0867**	0.1754***	0.1115**
	(0.0346)	(0.0465)	(0.0505)
Log bank assets (2006)		-0.0147*	0.0073
		(0.0077)	(0.0086)
Bank equity ratio (2006)		0.0067*	0.0067*
		(0.0036)	(0.0039)
Bank ROA (2006)		0.0448*	0.0498**
		(0.0235)	(0.0239)
D(state bank)			-0.0772**
			(0.0355)
D(cooperative bank)			0.0461
			(0.0345)
Constant	0.1901***	0.4076**	-0.0411
	(0.0096)	(0.1673)	(0.1856)
Observations	1,603	1,547	1,547
R-squared	0.0015	0.0168	0.0336

The table shows results for simple OLS regressions, where the dependent variable is the change in the logarithm of aggregate bank lending over the Basel II introduction in 2007Q1. For each bank, we calculate aggregate lending by summing all loans in a respective period. We then collapse all quarterly data for a given bank into single pre-event and post-event periods by taking the average of the two years before and the two years after the Basel II introduction. The dependent variable in the regressions above is the difference in the logarithm of these averages, so that there is one observation per bank. The dummy variable *D(IRB bank)* indicates whether the respective bank adopted the Basel II internal ratings-based approach during our sample period. Robust standard errors are reported in parentheses. Note: * indicates statistical significance at the 10 % level, ** at the 5 % level and *** at the 1 % level.

information for a specific firm, the lower the PD for that firm, and the lower the capital charge for loans to that firm. Hence, we expect that IRB banks increase lending particularly to those firms where hard information is relatively good.

We test this assertion using loan-level data from the German credit register. In particular, we assess how the change in lending for a particular bank-firm relationship depends on the regulatory approach adopted by the bank as well as on the goodness of hard information provided by the firm. As on the bank level, we collapse the quarterly loan data into single pre-event and post-event time periods by taking the averages of the two years before and the two years after the reform. The change in the logarithm of loans from bank i to firm j serves as dependent variable in the following regression:

$$\Delta log(loans)_{ij} = \alpha_i + \alpha_j + D(IRB\ bank)_i \times Firm\ PD_j \times \gamma + \epsilon_{ij}, \tag{3.2}$$

where i denotes the individual bank, and j denotes the individual firm. As a proxy for the goodness of hard information for a specific firm, we use the average PD banks report for that firm in 2008Q1, the first quarter in which this information is available (see Section 3.3). The lower this PD, the better the hard information the firm is able to provide to its banks. The variable is interacted with the dummy that indicates whether the bank adopted IRB during our sample period. As we are trying to identify a supply side effect, it is important to control for a firm's demand for credit. We do this by including firm fixed effects, α_i, into our regression, hence ensuring that identification for the coefficient of interest comes only from variation within the same firm.[19] That is, we test whether—following the reform—the same firm obtains relatively more loans from IRB banks as compared with SA banks, and whether this effect depends on the hard information the firm is able to provide. In the most stringent specification we additionally include bank fixed effects, α_j, that allow us to systematically control for heterogeneity across banks. That is, we test whether the same bank increases its lending relatively more to firms with good hard information, and whether this effect depends on whether the bank is an IRB bank or not. Finally, to allow for potential correlation among changes in lending from the same bank we cluster standard errors at the bank level in all loan-level regressions.

Results for these regressions are presented in Panel A of Table 3.3. We start with a specification without any fixed effects that includes only the IRB bank dummy. As on the aggregate level, we find that following the reform loans by IRB banks are increased significantly more than loans by SA banks. In column 2 we add firm fixed effects in order to control for credit demand. The coefficient remains remarkably stable, indicating that changes in credit demand are not a big concern for our analysis. Economically, the two coefficients indicate that loans from IRB banks are increased

[19]Consequently, the sample is constrained to firms that have at least one loan from an IRB bank and at least one loan from an SA bank.

by about 4.5 percent relative to loans from SA banks.[20] We proceed by splitting the sample based on the goodness of hard information firms are able to provide. Column 3 includes only firms with a lower-than-median average PD in 2008Q1, while column 4 includes only firms with a higher than median PD.[21] In line with our assertion, we find that IRB banks increase lending to the same firm significantly more than SA banks when the firm's PD is relatively low (i.e., when hard information is relatively good and capital charges are relatively low), but not when the firm's PD is relatively high (i.e., when hard information is relatively bad and capital charges are relatively high). In column 5 we interact the IRB bank dummy with the firm PD variable and find the same effect: IRB banks increase lending to the same firm relatively more, but less so when the firm's PD is higher. This effect is robust to the inclusion of bank fixed effects in column 6 and the inclusion of firm and bank fixed effects in column 7. Economically, the coefficient indicates that an increase of one standard deviation in *Firm PD* (0.031, see Table3.1, Panel B) induces a 1.2 percent smaller increase in loans from IRB banks, which corresponds to roughly one quarter of the overall effect identified in columns 1-2.

Unfortunately the PD data in the credit register becomes available only in 2008Q1, one year after the Basel II introduction. Ideally, we would like to have a proxy for the goodness of a firm's hard information prior to the event. In the previous analysis we had to rely on the assumption that the PD data is relatively sticky, so that firm PDs in 2006Q4 are similar to those in 2008Q1. Alternatively, we can use different proxies for hard information from a matched sample that contains firm balance sheet information. While this sample is smaller than the original one, it has the advantage that balance sheet information is also available for 2006, the year before the reform. We now provide additional tests, using the matched sample, in order to validate the

[20]The magnitude is somewhat smaller than on the bank level, for which we have the following most likely explanations: (a) Our test shows the effect on the percentage change for the average loan. The effect will be relatively larger on the bank level if IRB banks increase larger loans relatively more compared with smaller loans; (b) our test focuses on changes in lending on the intensive margin, i.e. for loans that already existed prior to the reform. It could be that IRB banks also increase lending more on the extensive margin, i.e. they create more new loans following the reform as compared with SA banks. This would also magnify the effect on the bank level.

[21]Ideally, we would have used the average PD in 2006Q4 in this test, i.e. a pre-reform value. Unfortunately, information on PDs becomes available in the credit register only in 2008Q1, which is why we have to rely on the assumption that these PDs are relatively sticky in most cases. Additionally, we use alternative criteria for the goodness of hard information in a smaller matched sample for which we have firm balance sheet information (see Panel B of Table 3.3).

Table 3.3: Loan-level lending

Panel A: Firm PD

	Dependent variable: Δ log(loans)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	All	Low PD	High PD	All	All	All
D(IRB bank)	0.0486***	0.0445**	0.0759***	0.0150	0.0511***		
	(0.0173)	(0.0180)	(0.0245)	(0.0142)	(0.0193)		
D(IRB bank) × Firm PD					-0.5519***	-0.4529***	-0.3951**
					(0.1580)	(0.1434)	(0.1573)
Firm PD						-0.2990***	
						(0.0890)	
Constant	0.0286***						
	(0.0068)						
Firm FE	NO	YES	YES	YES	YES	NO	YES
Bank FE	NO	NO	NO	NO	NO	YES	YES
Observations	44,784	44,784	22,391	22,393	44,784	44,784	44,784
R-squared	0.0024	0.2268	0.1818	0.2890	0.2271	0.0402	0.2626

Panel B: Additional firm variables

	Dependent variable: Δ log(loans)					
	(1)	(2)	(3)	(4)	(5)	(6)
D(IRB bank) × Firm debt / assets (2006)	-0.4276***	-0.3020***				
	(0.0759)	(0.0683)				
D(IRB bank) × Log firm assets (2006)			0.0391***	0.0374***		
			(0.0101)	(0.0117)		
D(IRB bank) × Firm ROA (2006)					0.0060***	0.0034*
					(0.0021)	(0.0018)
D(IRB bank)	0.1609***		-0.4580***		-0.0239	
	(0.0299)		(0.1132)		(0.0304)	
FIRM FE	YES	YES	YES	YES	YES	YES
BANK FE	NO	YES	NO	YES	NO	YES
Observations	8,411	8,411	8,735	8,735	8,748	8,748
R-squared	0.3015	0.3659	0.3245	0.3880	0.3138	0.3784

The table shows how loan level lending changed over the Basel II introduction. For each bank-firm relationship, we collapse all quarterly data for into single pre-event and post-event periods by taking the average of the two years before and the two years after the Basel II introduction. The dependent variable in the regressions above is the difference in the logarithm of these averages, so that there is one observation per bank-firm relationship. Data are restricted to (a) loans that are larger than € 1.5 million (b) loans from commercial, state, or cooperative banks that are subject to the Basel II capital regulation (c) loans that have an observation in both the pre- and the post-event period (d) loans to firms that have at least one loan from an SA bank and one loan from an IRB bank. Panel A uses only data from the credit register, Panel B uses a matched sample that includes firm balance sheet information from Bundesbank's USTAN database. Robust standard errors adjusted for clustering at the bank level are reported in parentheses. Note: * indicates statistical significance at the 10 % level, ** at the 5 % level and *** at the 1 % level.

results from above.

Results for these tests are presented in Panel B of Table 3.3. As balance sheet variables that proxy for the goodness of a firm's hard information we use the firm's pre-event leverage, size, and profitability. The PD for a specific loan will typically be lower the lower the firm's leverage, the larger its size, and the higher its profitability.

The matched sample contains up to 8,748 loans to 1,712 distinct firms and hence corresponds to roughly one fifth of the original sample. In columns 1 and 2 we assess how a firm's leverage affects IRB banks' lending decisions. We find that an increase of one standard deviation in the firm debt to asset ratio (0.209, see Table 3.1, Panel B) induces a 6.1 to 8.5 percent smaller increase in lending from IRB banks. Similarly, an increase of one standard deviation in the logarithm of firm assets (1.775, see Table 3.1, Panel B) induces an increase in loans from IRB banks that is about 7 percent larger. Finally, also a firm's profitability affects IRB banks lending reaction: An increase in one standard deviation of pre-event ROA (5.876, see Table 3.1, Panel B) results in an increase in loans from IRB banks that is about 2 to 3.5 percent larger. All these estimation results are robust to the inclusion of both firm and bank fixed effects. Overall, the results presented in this section provide strong evidence that—following the reform—IRB banks expanded loans in particular to those firms that have relatively good hard information, i.e., those firms for which estimated PDs and hence capital charges are relatively low.

3.5 The impact of changed lending incentives on the quality of PD estimates in banks' internal models

In this section we evaluate how the changes in the lending decision process documented in the previous section affected banks' evaluation of credit risk. In particular, we investigate whether actual defaults deviate from the numbers implied by PD estimates, and whether this deviation depends on the regulatory approach used for a specific loan. Further, we examine whether the deviation depends on the timing of loan origination, that is, we check whether the pattern is different for loans that were originated under Basel II as compared with loans that were originated under Basel I. Finally, we provide several robustness checks.

3.5.1 Empirical strategy

We now explain the empirical strategy employed in order to validate the main argument of our paper: That the introduction of model-based capital regulation induced a change in lending behavior that affected the estimations derived from banks' internal risk models. For each quarter, we estimate the following equation in order to test the relationship between PDs and actual default rates:

$$y_{ijk} = \alpha + \delta \cdot 1_{(k \in T)} + \epsilon_{ijk}, \tag{3.3}$$

where j denotes the individual bank, i denotes the individual firm, and k indicates whether the loan belongs to an SA or to an IRB portfolio within the bank. The dependent variable y_{ijk} is defined as the difference between a dummy that indicates actual default and the PD that bank j attributes to loans to firm i. As PDs for loans vary between 0 and 1, y_{ijk} is positive for loans that actually default and negative for loans that do not default. The indicator variable $1_{(k \in T)}$ takes a value of 1 if loans to firm i belong to the IRB portfolio of bank j, and 0 if they belong to the SA portfolio. Further, the equation includes a constant α and a random error term ϵ_{ijk}. In order to allow for potential correlation among default events for loans to the same firm, standard errors are clustered at the firm level in all regressions.

If we want to interpret the coefficient of interest, δ, as the causal impact of the regulatory approach on the prediction error y_{ijk} for a specific loan, the covariance between $1_{(k \in T)}$ and ϵ_{ijk} should be equal to zero ($Cov(\epsilon_{ijk}, 1_{(k \in T)}) = 0$). As banks that introduced the model-based approach tend to be larger, internationally more active and more sophisticated than banks that remained under the traditional approach, an estimation based on loans from both types of banks could have biased our coefficients. Fortunately, the institutional details of the German Basel II introduction described in Section 3.2 allow us to circumvent this concern by using within-bank variation in the regulatory approach. IRB institutions did not shift all their portfolios to the new approach at the same time, so that we can use variation between loans that have already been shifted to IRB and loans that are still under SA to identify δ in Equation (3.3).

Although the approach described above addresses many concerns, coefficients could be biased if there are omitted factors that determine whether firms are assigned to SA or IRB portfolios within IRB banks. To address this issue, we focus on firms that borrow from at least two banks, one bank where loans to the firm belong to a portfolio that has already been shifted to IRB and one bank where they are still under SA. Depending on the quarter, our sample contains up to 19,864 loans to 4,971 distinct firms that fulfill this criterion. For each quarter, we estimate:

$$y_{ijk} = \alpha_i + \alpha_j + \delta \cdot 1_{(k \in T)} + \epsilon_{ijk}, \tag{3.4}$$

where α_i and α_j denote firm and bank fixed effects, respectively, and the remaining variables are defined as in Equation (3.3). By adding α_i and α_j we are able to systematically control for heterogeneity across banks and across firms. That is, we can check whether the prediction error for loans to the same firm is greater when IRB instead of SA is used by the bank, and—similarly—whether the estimation error for loans from the same bank is greater when IRB instead of SA is used for loans to a specific firm.

The identification strategy described above provides an unbiased estimate of the impact of the regulatory approach on the prediction error as long as there is no systematic relationship between the point in time at which a specific portfolio is shifted to IRB and the bank's ability to estimate PDs for loans in that portfolio. As described in Section 3.2, banks typically shifted those portfolios first for which they had a sufficient amount of data to calibrate a meaningful PD model that could be certified by the regulator. Hence, any bias from selection of IRB portfolios should work against us: If anything, banks should be better able to predict actual default rates for those loan portfolios that have been certified by the regulator (i.e., those portfolios for which they have sufficient data and experience). Nevertheless, we further refine the identification strategy to remove any remaining doubts.

We argue that model-based regulation induced an overreliance on hard information, thus giving rise to underestimation of actual default rates. If this argumentation holds true, the effect should be particularly pronounced for loans that were originated after the introduction of model-based regulation. For those loans, capital charges depended on PD estimates at the time of loan origination, while they did not for loans that were originated before the reform and consequently shifted to IRB. We exploit this time series variation in the loan issuance date to circumvent the selection concern. Specifically, we restrict ourselves to loans that actually use the IRB approach and check whether the underestimation of actual default rates is greater for loans that were originated after the reform as compared with loans that were originated before the reform. We estimate the following equation:

$$y_{ij} = \alpha_j + \delta \cdot 1_{(c \in B)} + \epsilon_{ij}, \tag{3.5}$$

where $1_{(c \in B)}$ is an indicator variable that takes a value of 1 if the the IRB loan was issued after the implementation of Basel II (in the year 2007) and 0 otherwise. Note that this specification is not prone to selection concerns and therefore allows for an unbiased estimate of the effect of the regulatory approach on the functioning of PD models.[22]

3.5.2 Descriptive analysis

We start the analysis by assessing how PD estimates from banks' internal risk models compare with actual default rates for a respective set of loans. The information on PDs—and with it the information on actual defaults—becomes available in the credit register in 2008Q1. As described above, the analysis in this section focuses on IRB and SA loans from IRB banks only. Although the information is available on a quarterly basis, we evaluate loan portfolios once per year—at the end of each year—for reasons of presentability.[23] As stated in Section 3.2, PDs should estimate one-year default rates and a loan is considered to be in default if the borrower is 90 days past due on his obligations. Accordingly, the dummy variable *Actual Default* captures whether a loan is in default in at least one of the four quarters following the one in which the PD is evaluated. Importantly, all loans that are already in default in a respective quarter are excluded from the analysis.

[22] In contrast to previous estimations it is difficult to include also firm fixed effects in these regressions, as there are relatively few firms that obtained new loans both before and after the reform.

[23] Results for the remaining quarters are similar to the results we report, and available from the authors upon request. See also Figure 3.6 in the Appendix for an overview of average PDs and actual default rates for all quarters.

Table 3.4: Estimation error—descriptives

Panel A: IRB banks, IRB loans

	Observations	Actual default	PD	Actual default – PD
2008	50,163	0.0267	0.0151	0.0116
2009	47,167	0.0269	0.0198	0.0071
2010	47,019	0.0212	0.0213	-0.0001
2011	46,357	0.0222	0.0176	0.0046

Panel B: IRB banks, SA loans

	Observations	Actual default	PD	Actual default – PD
2008	22,751	0.0275	0.0270	0.0004
2009	23,426	0.0251	0.0284	-0.0033
2010	21,130	0.0192	0.0287	-0.0095
2011	18,894	0.0176	0.0235	-0.0059

Panel C: IRB vs. SA

	Difference in actual default	Difference in PD	Difference in difference
2008	-0.0008	-0.0120	0.0112
	[-0.6170]	[-31.7598]	[8.5746]
2009	0.0018	-0.0086	0.0103
	[1.3766]	[-18.2768]	[7.9694]
2010	0.0020	-0.0074	0.0094
	[1.7269]	[-14.2997]	[7.8497]
2011	0.0046	-0.0060	0.0106
	[3.7342]	[-13.7121]	[8.5387]

The table compares actual default rates with banks' estimated PDs in 2008Q4, 2009Q4, 2010Q4, and 2011Q4, respectively. Panel A includes all loans by IRB banks that were subject to the IRB approach in 2008Q1, the first period where this information is available. The column *Actual default* displays the mean of a dummy variable that is equal to 1 if the loan defaults in the year following the respective quarter. Loans that are already in default in the respective quarter are excluded. The column *PD* displays the average estimated one-year default rate for the same set of loans, and the column *Actual default - PD* displays the difference between the two. Panel B repeats the same analysis for all loans by IRB banks that were subject to the standard approach in 2008Q1. Panel C compares the two panels with each other by calculating—for each quarter—the difference between the values for IRB loans and for SA loans. The numbers in brackets are t-statistics.

Panel A of Table 3.4 shows descriptive statistics for lending relationships under IRB. There are 50,163 lending relationships in our sample that had already been shifted to IRB in 2008. During the sample period from 2008 to 2011, additional portfolios are shifted to IRB. Relationships that were under SA in 2008 but are moved to IRB before 2011 are constantly classified as SA loans, since this was the regulatory regime under which they were originated. New relationships are classified according to the regulatory approach under which they were issued. When comparing model based PDs with actual default rates, we observe that PDs for IRB loans underestimate actual defaults in 2008, 2009, and 2011. Only in 2010, the estimated PDs and actual defaults of IRB loans match.

In Panel B of Table 3.4 we repeat the analysis presented in Panel A for those loans that were still subject to the standard approach in 2008. These portfolios will be transferred to IRB once the respective model is certified by the regulator. While the underlying PD models should hence be similar for IRB loans and SA loans, capital charges under IRB depend on the estimated PDs while capital charges under SA do not. Interestingly, while PDs for IRB loans underestimated actual defaults on average, we do not find a similar pattern for IRB banks' SA loans. In 2008, the actual default rate almost matches the average PD, and in the remaining years it is even lower than the average PD (especially in 2010, a year with a very low actual default rate).

Figure 3.3 plots average PDs and actual default rates for IRB loans and SA loans over time.[24] In line with our expectation, average PDs for IRB loans are always lower than average PDs for SA loans. As shown in Section 3.4, IRB banks have shifted their lending more towards firms with low model-based PDs as capital charges under IRB are particularly low for loans to these firms. Kernel density plots for PDs further illustrate this point (see Figure 3.4). Clearly, the distribution for IRB loans is to the left of the distribution for SA loans in all years. This is confirmed in a Kolmogorov-Smirnov test for equality of distributions: The hypothesis that the distributions for SA loans and IRB loans are equal can be rejected at the 1 percent level in all cases.

Panel C of Table 3.4 and the lower part of Figure 3.3 further compare corresponding actual default rates for the two sets of loans. In stark contrast to PDs, actual default rates are similar for both portfolios in 2008 and 2009, and, are somewhat higher for IRB loans in 2010 and 2011 as compared with SA loans.

Finally, Panel C of Table 3.4 also reports differences in the difference between actual default rates and average PDs. In all years, this difference is larger for IRB loans: Compared to PDs for SA loans, PDs for IRB loans underestimate actual

[24]Again, for reasons of presentability, we evaluate loan portfolios only once a year. Results for the remaining quarters are very similar (see Figure 3.6 in the Appendix).

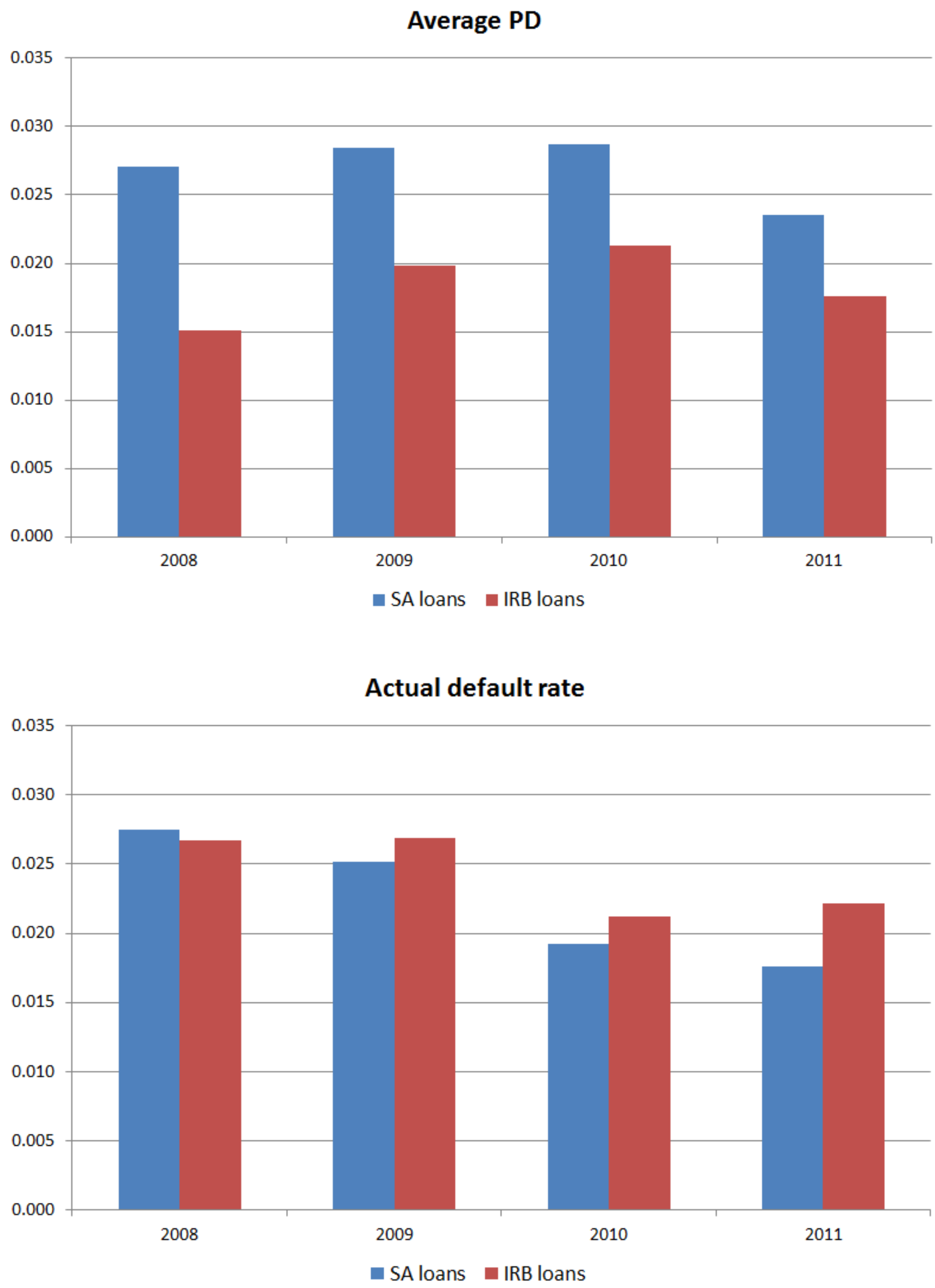


Figure 3.3: Average PDs and actual default rates

The figure shows average PDs and actual default rates for SA loans and IRB loans during the period from 2008Q1 to 2012Q2. For reasons of presentability we evaluate loan portfolios only once per year, at the end of each year (see Figure 3.6 in the Appendix for the remaining quarters). The sample includes all loans that are not in default at the respective point in time. For the top panel, we calculate the averages of reported PDs for the respective portfolios of loans. For the bottom panel, we create a dummy that equals 1 for loans that default in the year following the respective quarter, and calculate the average of this dummy variable for the respective portfolios of loans.

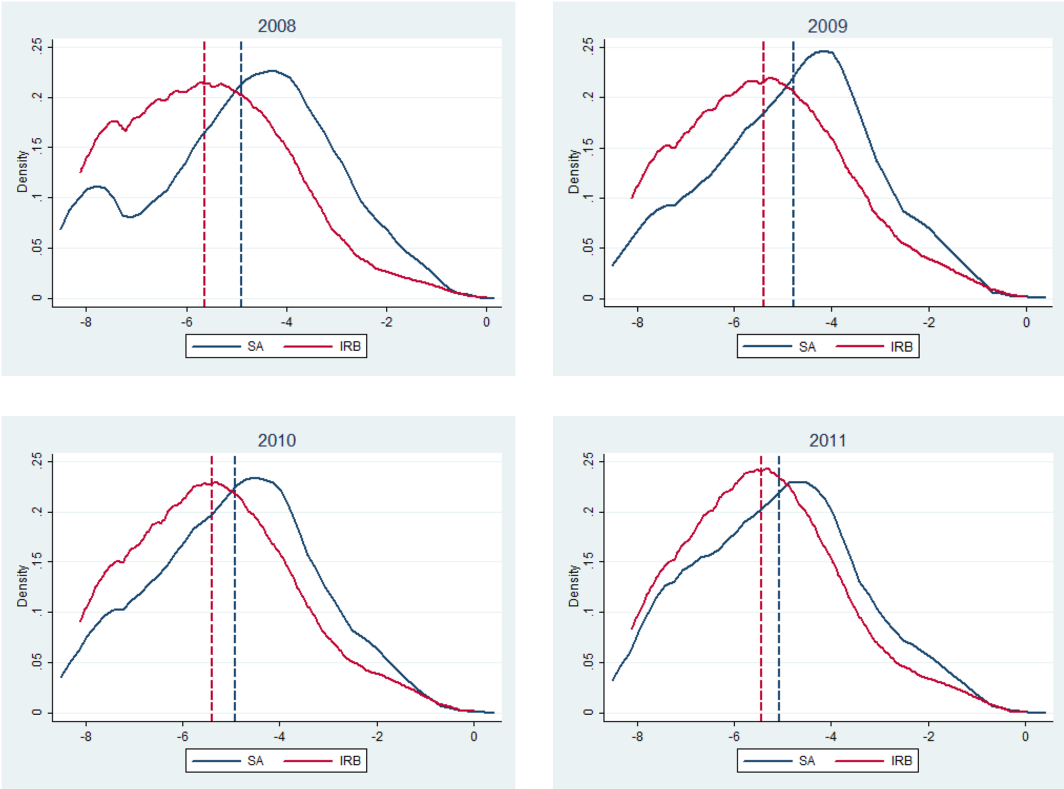


Figure 3.4: PD kernel densities

The figure shows Epanechnikov kernel densities for PDs at 2008Q4, 2009Q4, 2010Q4, and 2011Q4, respectively. PDs are reported in logarithms for reasons of presentability. The smoothing parameter in the density estimation is set to 0.4. The blue line corresponds to PDs for SA loans of IRB banks, the red line corresponds to IRB loans of IRB banks. Dashed vertical lines represent the respective mean of the distribution.

default. Albeit illustrative, the latter findings might be explained by borrower or bank specific factors. We therefore proceed by testing our assertions more formally in a regression framework.

3.5.3 Regression framework: IRB versus SA loans

Results for Equation (3.4) are presented in Table 3.5. We start with the specification without any fixed effects in the first four columns and estimate the equation separately for each quarter in order to ensure that each loan turns up only once in each regression.[25] In line with the findings in the previous section, the regressions show that the estimation error is significantly greater for IRB loans as compared with SA

[25] Again, we constrain ourselves to 2008Q4, 2009Q4, 2010Q4, and 2011Q4 for reasons of presentability. Results for the remaining quarters are very similar and available from the authors upon request.

Table 3.5: Estimation error—regressions

	Dependent variable: Estimation error (actual default – PD)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	2008	2009	2010	2011	2008	2009	2010	2011	2008	2009	2010	2011
D(IRB loan)	0.0112***	0.0103***	0.0094***	0.0106***	0.0113***	0.0079***	0.0073***	0.0096***	0.0076***	0.0074***	0.0071***	0.0090***
	(0.0014)	(0.0013)	(0.0013)	(0.0012)	(0.0021)	(0.0020)	(0.0019)	(0.0019)	(0.0028)	(0.0024)	(0.0025)	(0.0025)
Constant	0.0004	-0.0033***	-0.0095***	-0.0059***								
	(0.0011)	(0.0011)	(0.0010)	(0.0010)								
Firm FE	NO	NO	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES
Bank FE	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES	YES	YES
Observations	72,914	70,593	68,149	65,251	19,864	19,182	17,650	15,431	19,864	19,182	17,650	15,431
R-squared	0.0010	0.0009	0.0009	0.0011	0.6248	0.5910	0.5973	0.5861	0.6297	0.5989	0.6065	0.5914

The table shows loan-level regression results for 2008Q4, 2009Q4, 2010Q4, and 2011Q4, respectively. The dependent variable in all regressions is the difference between a dummy that indicates whether the respective loan defaults in the year following the respective period and the estimated PD of the loan. The sample includes all loans from IRB banks, where columns 5-12 are restricted to firms that have at least one IRB loan and at least one SA loan from an IRB bank. As we evaluate loans periodwise, there is one observation per bank-firm relationship in each regression. The dummy *D(IRB loan)* indicates the regulatory approach under which a specific loan was originated and is equal to 1 if the loan was issued under IRB. Columns 5-8 include firm fixed effects to control for heterogeneity across borrowers and columns 9-12 additionally includes bank fixed effects that control for heterogeneity across banks. Robust standard errors adjusted for clustering at the firm level are reported in parentheses. Note: * indicates statistical significance at the 10 % level, ** at the 5 % level and *** at the 1 % level.

loans, i.e.—compared to PDs for SA loans—PDs for IRB loans significantly under-predict actual default rates.[26] Next, we add firm fixed effects in columns 5 to 8. In these tests, the sample is constrained to firms that have at least one IRB loan and at least one SA loan from an IRB bank. The coefficient for the IRB loan dummy remains significantly positive in all cases. PDs are more likely to underpredict actual default if IRB instead of SA is used for a specific loan.

As a final test, we complete the specification by adding bank fixed effects in columns 9 to 12 of Table 3.5. The coefficient for the IRB loan dummy remains significantly positive, which means that—within the same bank—underprediction of actual default is more likely if IRB instead of SA is used for a particular loan. Overall, empirical results provide strong support for our assertion that PDs for loans under the IRB approach tend to underpredict actual default rates.

3.5.4 Regression framework: IRB loans issued before and after the event

In this section, we revisit potential selection concerns arising from the order in which IRB banks shift their loan portfolios from SA to IRB. As discussed in detail in Section 3.5.1, the selection of IRB portfolios was based on data quality and experience of the bank and should therefore result—if at all—in a downward bias of our coefficients. Nevertheless, we exploit time series variation in the date of loan issuance to remove any remaining doubts. To do so, we restrict ourselves to loans that actually use the IRB approach, and check whether the underestimation of actual default rates is greater for loans that were originated after the reform as compared with loans that were originated before the reform. In other words, we circumvent the selection concern by focusing on variation over time within the portfolio of IRB loans.

Specifically, we evaluate the performance of a sample of loans that were originated between 2005 and 2008, within two years before and after the reform. As our data is on the bank-firm level (and not on the contract level), we define the year of a loan issuance as follows: First, if a new bank-firm relationship is formed in a given year, it is clear that a new loan was originated in that year. Second, for existing bank-firm

[26] The coefficients for the IRB loan dummy in columns 1-4 correspond to the differences in differences in Panel C of Table 3.4.

relationships, we assume that a new loan was granted if we see an increase of at least € 1.5 million (the lower bound for being reported in the credit register) and of at least 30 percent of the amount already outstanding in a given quarter.[27] Panel A of Figure 3.5 shows actual default rates and PD averages for these loans in 2009Q4. Loans originated in 2005 or 2006 (pre-reform) exhibit average PDs that are relatively close to actual default rates. In contrast, the actual default rate is considerably higher than the average PD for loans originated after the Basel II reform in 2007 or 2008 (post-reform), indicating an underestimation of credit risk for this set of loans.

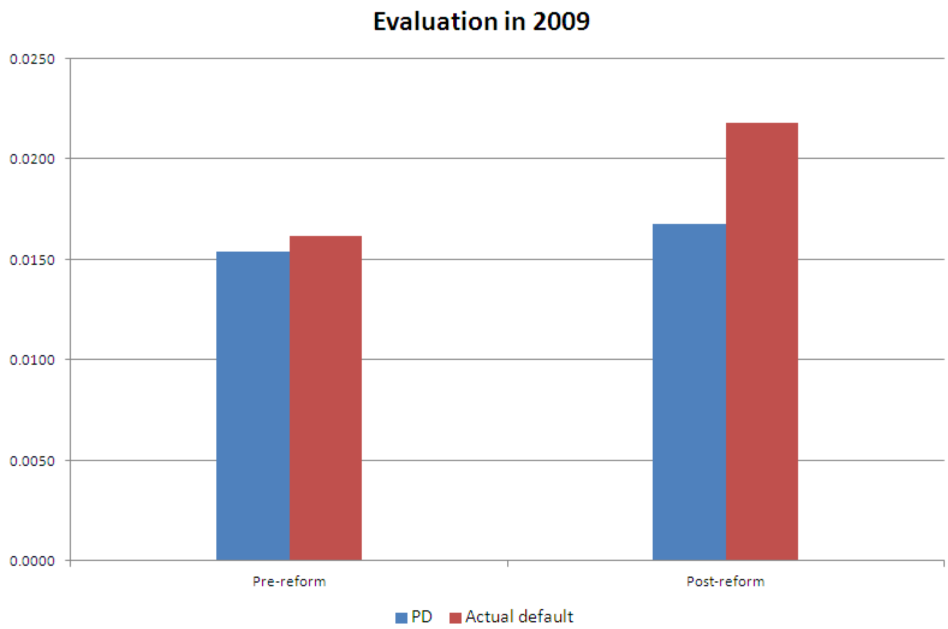
Panel A of Figure 3.5 evaluates loan performance in 2009, which means that loans originated in different years differ in the time elapsed since their origination.[28] To rule out that the length of a specific relationship explains part of our findings in Panel A, we repeat the analysis using a different evaluation horizon. In particular, we evaluate loan performance four years after origination. That is, loans originated in 2005 are evaluated in 2009Q4, loans originated in 2006 are evaluated in 2010Q4, and so on. Hence, Panel B of Figure 3.5 evaluates the performance of all loans that still exist four years after their origination.[29] Average PDs are slightly higher than actual default rates for loans originated before the reform, and considerably lower than actual default rates for loans originated after the reform.

Table 3.6 provides regression results for Equation (3.5). As before, we use the estimation error as a dependent variable and start with a specification without any fixed effects for the set of loans introduced in Figure 3.5, Panel A. We find a significant difference between the two regimes, i.e., PDs for loans originated under Basel II are significantly more likely to underestimate actual default rates than PDs for loans originated before the reform. Column 2 shows that this result is robust to the inclusion of bank fixed effects, which means that the same bank more often underestimates the actual default rate for loans that were originated under Basel II.

[27] We focus on large increases in the outstanding loan amount of a given bank-firm relationship since most firms keep a checking account with their banks whose balances keep varying around a certain level quarter by quarter. Importantly, our results do not depend on the exact definition of a new loan issuance, i.e., we have tried different cutoff values and obtained similar results.

[28] Evaluating loans in 2009 allows us to include loans that were originated within a two-year window around the reform, with the sample being relatively balanced between loans that were originated before and after the reform. The same test in 2010 yields similar results, but is less balanced since the share of loans originated before the reform is considerably lower.

[29] We also tried alternative evaluation horizons (three years, five years) and obtained similar results.



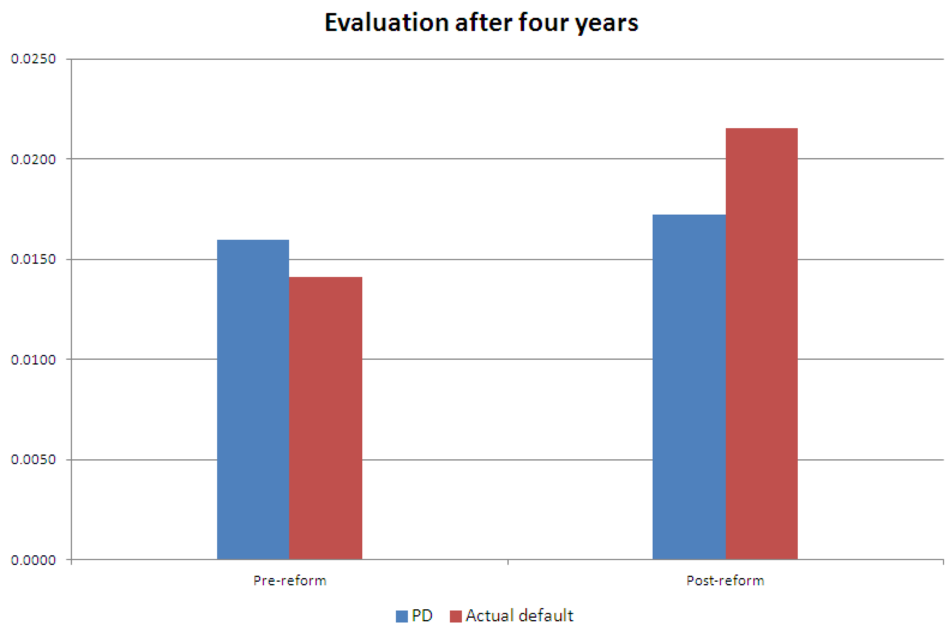


Figure 3.5: Average PDs and actual default rates by loan cohorts

The figure shows average PDs and actual default rates for loans under IRB that were originated in the years around the Basel II introduction, i.e., for bank-firm-relationships that did not exist before or that display a large increase (i.e., at least €1.5 million and at least 30 % of existing loan amount) in 2005 or 2006 (pre-reform), 2007 or 2008 (post-reform). In Panel A, all loans are evaluated in 2009Q4, whereas Panel B evaluates loans four years after their origination, i.e., loans originated in 2005 are evaluated in 2009Q4, loans originated in 2006 are evaluated in 2010Q4, and so on.

Finally, columns 3 and 4 repeat the estimations from the first two columns, using the set of loans with a four-year evaluation horizon from Figure 3.5, Panel B. Results are very similar.

Table 3.6: Estimation error by cohorts

	Dependent variable: Estimation error (actual default – PD)			
	Evaluation in 2009		Evaluation after four years	
	(1)	(2)	(3)	(4)
Basel II	0.0043***	0.0045***	0.0062***	0.0067***
	(0.0010)	(0.0010)	(0.0013)	(0.0013)
Constant	0.0008		-0.0018**	
	(0.0010)		(0.0009)	
Bank FE	NO	YES	NO	YES
Observations	67,015	67,015	49,382	49,382
R-squared	0.0002	0.0213	0.0005	0.0291

The table evaluates how the estimation error depends on the year of the loan origination. We include only loans that were originated in the years around the Basel II introduction, i.e., bank-firm-relationships that did not exist before or that display a large increase (i.e., at least €1.5 million and at least 30 % of existing loan amount) in the respective year. The dependent variable in all regressions is the difference between the dummy for actual default and the estimated PD for the loan. The dummy *Basel II* is equal to 1 if the loan was originated after the Basel II introduction (i.e., in 2007 or 2008) and equal to 0 if it was originated before (i.e., in 2005 or 2006). In columns 1 and 2, loans are evaluated in 2009Q4. In columns 3 and 4, loans are evaluated four years after their origination, i.e., loans originated in 2005 are evaluated in 2009Q4, loans originated in 2006 are evaluated in 2010Q4, and so on. Robust standard errors adjusted for clustering at the firm level are reported in parentheses. Note: * indicates statistical significance at the 10 % level, ** at the 5 % level and *** at the 1 % level.

Results in this section confirm that our findings in the previous section are not driven by the selection of IRB loan portfolios. We find a stronger underestimation effect for IRB loans that were originated after the reform as compared with IRB loans that were originated before the reform. While these loans differ in the time of origination, they find themselves within the same loan portfolios within IRB banks, i.e., those portfolios for which the new approach has already been implemented.

3.5.5 Further results

We further report some robustness tests to our main specification in Equation (3.4). Previously, we have given equal weight to all observations. However, one might argue that it is more important that IRB banks get PDs for larger loans right, as these loans are more important for the determination of overall required capital. If the underestimation effect is less severe for larger loans, it could be that on aggregate banks get required capital right. To test this, we report results for weighted regressions in columns 1 to 4 of Table 3.7, where we weight each observation by its loan size. Coefficients are somewhat smaller in these regressions as compared with the coefficients in the unweighted regressions (Table 3.5, columns 5 to 8). Nevertheless, they are still significant in most cases, indicating that the underestimation effect for IRB loans is also present if one considers the size of each loan.

Next, we use two alternative definitions for the dependent variable in the remaining columns of Table 3.7. First, in columns 5 to 8, we take the absolute value of the difference between the actual default dummy and the estimated PD for each loan as a left-hand-side variable. The coefficient for the IRB loan dummy is positive and significant in all cases. In previous regressions we investigated whether PDs for IRB loans are more likely to understate actual credit risk. By focusing on the absolute value of the estimation error, we treat an overestimation of actual default risk in the same way as an underestimation. Still, the regressions show that PD estimates for IRB loans are less precise than PD estimates for SA loans on average. Second, we focus only on loans that actually defaulted, i.e., on loans for which the difference between the actual default dummy and the PD is greater than 0, and set the difference for the remaining loans equal to 0. In this way, we check whether default risk for loans that actually defaulted was underestimated more by PDs for IRB loans. The positive and significant coefficients for the IRB loan dummy in columns 9 to 12 show that this is indeed the case. PDs for loans that defaulted were on average lower if the IRB instead of the standard approach was used for the loan.

Table 3.7: Estimation error—further results

	Value weighted				Absolute error				Positive error			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	2008	2009	2010	2011	2008	2009	2010	2011	2008	2009	2010	2011
D(IRB loan)	0.0087**	0.0053***	0.0045***	0.0068***	0.0040**	0.0030*	0.0043**	0.0098***	0.0077***	0.0054***	0.0058***	0.0097***
	(0.0036)	(0.0019)	(0.0017)	(0.0020)	(0.0021)	(0.0018)	(0.0017)	(0.0018)	(0.0019)	(0.0017)	(0.0015)	(0.0017)
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	19,864	19,182	17,650	15,431	19,864	19,182	17,650	15,431	19,864	19,182	17,650	15,431
R-squared	0.6332	0.6583	0.5594	0.6543	0.6540	0.6463	0.6586	0.6334	0.6598	0.6379	0.6406	0.6142

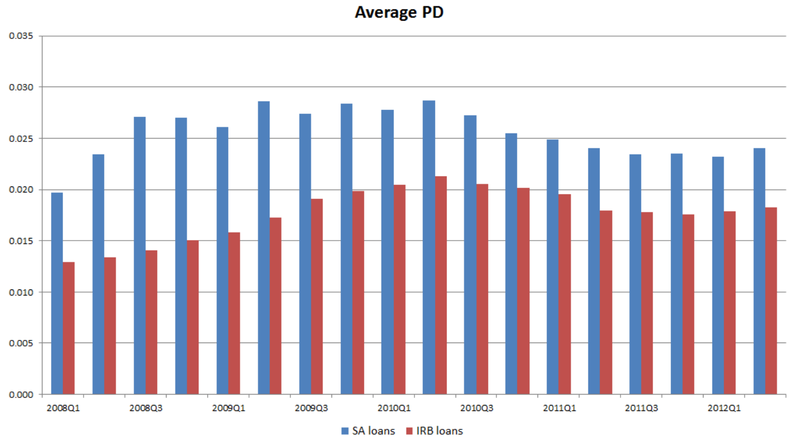
The table shows further loan-level regression results for 2008Q4, 2009Q4, 2010Q4, and 2011Q4, respectively. Columns 1-4 provide weighted regression results, where the dependent variable is the same as in Table 3.5 and observations are weighted by the size of the respective loan. In column 5-8 the dependent variable is equal to the absolute value of the difference between the dummy for actual default and the estimated PD for the loan. In columns 9-12 the dependent variable is equal to the difference between the dummy for actual default and the estimated PD for the loan for positive values of this difference and set to 0 for negative values. As before, the sample includes all loans from IRB banks to firms that have at least one IRB loan and at least one SA loan from an IRB bank. Loans are evaluated periodwise, so that there is one observation per bank-firm relationship in each regression. The dummy *D(IRB loan)* indicates the regulatory approach under which a specific loan was originated and is equal to 1 if the loan was issued under IRB. All columns include firm fixed effects to control for heterogeneity across borrowers. Robust standard errors adjusted for clustering at the firm level are reported in parentheses. Note: * indicates statistical significance at the 10 % level, ** at the 5 % level and *** at the 1 % level.

3.6 Conclusion

The regulation of bank capital requirements is one of the most controversial topics in today's world of banking. Most recently, Basel-type model-based regulation has come under pressure as there seems to be growing distrust among investors on the validity of regulatory risk weights. In this paper, we use data from the German credit register to show that the introduction of the Basel II internal ratings-based (IRB) approach affected both the quantity and the composition of bank lending. Specifically, banks that introduced the IRB approach increased their lending following the reform, in particular to firms with relatively low model-based PDs. In the second part of the paper we examine how this change in the composition of borrowers affected the validity of internal risk estimates. We find that risk estimates for IRB loans tend to underestimate actual default rates for IRB loans, while there is no such effect for SA loans. Moreover, the underestimation effect is worse for those IRB loans that were originated after the reform.

While we cannot—and also do not want to—rule out additional problems associated with model-based regulation, our empirical findings strongly suggest that overreliance on borrowers with favorable value parameters for the PD models plays a crucial role in explaining the underestimation of actual default rates. An alternative explanation for problems with model-based regulation would be a pure manipulation story: It could be that banks simply shift existing PDs downwards in order to economize on regulatory capital. Our time-series tests (i.e., the comparison of estimation errors for loans issued before and after the reform) can be seen as evidence against such an explanation: If banks simply manipulated PDs of existing borrowers after the reform, the estimation error should be high for all IRB loans, irrespective of the date of loan origination. We have shown, however, that the estimation error is considerably larger for loans that were issued after the reform, where banks had incentives to lend to firms that score well on the dimensions used in the risk models.

A3 Appendix to Chapter 3



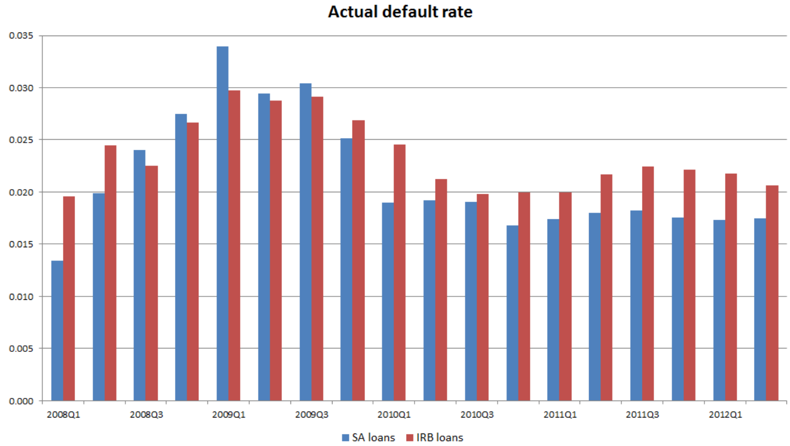


Figure 3.6: Average PDs and actual default rates—all quarters

The figure shows average PDs and actual default rates for SA loans and IRB loans during the period from 2008Q1 to 2012Q2. The sample includes all loans that are not in default at the respective point in time. For the top panel, we calculate the averages of reported PDs for the respective portfolios of loans. For the bottom panel, we create a dummy that equals 1 for loans that default in the year following the respective quarter, and calculate the average of this dummy variable for the respective portfolios of loans.

Chapter 4

The Political Economy of Bank Bailouts

4.1 Introduction

There is now a growing literature that examines the various economic trade-offs that accompany bank bailout decisions.[1] Proponents of bank bailouts argue that bank failures generate significant negative externalities that can have debilitating real effects. Thus, every effort should be made to avoid bank failures. Critics, on the other hand, voice concerns about the fiscal costs and moral hazard problems that accompany bank bailouts. Most of these discussions, however, omit an important factor that could affect bank bailout decisions, namely the personal interests of politicians involved in these decisions.[2] Politicians may follow their own interests (i.e., constituents and special interest pressure in order to increase their probability of re-election) or their own ideological preferences (e.g., the conservative principle of limited intervention in private markets; see Peltzman 1985, Poole and Rosenthal 1996). Several anecdotes suggest that the electoral cycle and the competitiveness of the electoral process affect public bailout policies, none clearer than the 10 billion Euro bailout of the state-owned BayernLB just three months after a state election—contrary to the pre-election claim that the bank would generate a profit in 2008.[3] In

[1]See Merton (1977), Keeley (1990), Demirgüç-Kunt and Detragiache (2002), Dam and Koetter (2012), Gropp, Hakenes, and Schnabel (2011). A detailed discussion of state-supported schemes for financial institutions is provided by Beck et al. (2010).

[2]A notable exception is Brown and Dinç (2005), who provide evidence that politicians in emerging countries delay bank failures until after the election.

[3]The bailout accounted for 2 % of the state gross domestic product and for approximately 30 % of annual state expenditures.

this paper, we examine political considerations that could affect bailout decisions.

We provide empirical evidence about the determinants of public bailout policies. More precisely, we analyze capital injections into distressed savings banks by German local politicians to examine their motives and incentives. German savings banks are owned by their municipalities, and politicians tend to be members of their supervisory board. They thus have a significant control over the banks they govern and plausibly derive both pecuniary and non-pecuniary benefits from this control. Individual savings banks are interconnected by a state-level association that operates a safety net for these banks.[4] In case of distress, these associations decide whether to inject funds or restructure the respective bank (e.g., by cutting down operations of the distressed bank or by organizing a distressed merger with another savings bank). Since the funds available to the association are provided by all individual savings banks in the respective state, the safety net basically constitutes an insurance scheme. Each association has a board of experts that employs pre-defined criteria to decide about the respective interventions and subsequent restructuring activities.[5] However, local politicians can circumvent this process by using taxpayers' money to support the bank in distress. In this case, the politician keeps control over the savings bank in his municipality. This set-up allows us to differentiate between alternative motives of politicians that could drive bailout decisions.

Given that savings banks have an extensive safety net in place, it is a priori unclear why politicians frequently engage in bailouts.[6] On the one hand, it could be that politicians—in comparison to the association that has to rely on broader perspective—have better information about the prospects of 'their' savings bank. Since local politicians are often members of the banks' supervisory board, they should have a profound knowledge about the bank's operations and potential causes for the distress event. By using taxpayers' money, politicians can prevent the association from taking inefficient restructuring measures or merger decisions. On the other

[4]This safety net does not provide deposit insurance, but a so-called institution guarantee. If the association believes that a specific bank has severe solvency problems it may organize a distressed merger (Sparkassen-Finanzgruppe 2004).

[5]See Section 4.2 for details on the composition of the associations' boards as well as the constitution of these associations.

[6]About one third of the distress events in our sample constitute capital injections from the owner.

hand, it could be that local politicians base their decisions on personal interests (e.g, their probability of re-election) or ideology. In addition, politicians may value to have a savings bank under their control, since they can influence important credit allocation decisions, organizational policies and the distribution of the banks' earnings (Sapienza 2004). If the association merges a distressed savings bank with another savings bank, politicians are likely to loose their influence within the new bank. While capital injections by the politician can prevent this outcome, voters may perceive the bailout as a waste of taxpayers' money and may punish the politician in the subsequent election. In a sense, voters exert discipline on the politician who decides on the bailout.

Our empirical setup allows us to differentiate between these two alternative explanations. If local politicians are better informed in comparison to associations, no statistical relationship between political variables such as the electoral cycle or the competitiveness of the political process and capital injections should exist. The same is true for ideology: If politicians' decisions are only driven by local knowledge, we should not observe differences in bailout probabilities between conservative and non-conservative politicians.

For a sample of 148 distress events of German savings banks between 1994 and 2010, we find that politicians' interests and ideology have a major impact on their bailout decisions. Politicians are about 30 % less likely to inject capital into a distressed bank in the twelve months before an election as compared with the twelve months following an election. If there is high competition in the electoral process, a political bailout is 15 % less likely. Also a politician's ideology explains bailout decisions: Capital injections are 18 % less likely if the politician is a member of the conservative party, reflecting the conservative ideology of limited state interventions. These findings clearly suggest that local knowledge obtained from close proximity to the bank is not the main driver of politicians' bailout decisions. Rather, decisions seem to be motivated by personal interests. The findings are robust to the inclusion of a wide set of macroeconomic as well as bank-specific control variables.

We further find that politicians in municipalities with a high fiscal deficit are less likely to bail out distressed banks. This can be interpreted as an example for the disciplining effects of fiscal federalism. Moreover, we do not find that personal connections between the board of the association and the board of the respective bank in distress affect the associations' decision to support the bank. This suggests that the decision process at the association is rather transparent and follows predetermined rules.

In the second part of the paper we evaluate consequences of political bailouts. In particular, we compare developments at banks that received capital injections from the owner to developments at banks that were supported by the association. Such a comparison could be subject to selection bias for two reasons: First, we do not have accounting information on banks that were involved in a distressed merger following the event. Since the association may decide to organize distressed mergers for those banks with the worst prospects, comparing the remaining association bailouts to the average owner bailout could suffer from a bias. Second, there might be unobserved variables that jointly affect the politician's bailout decision and the future performance of the bank.

In order to address the first concern we focus on a sample of banks that do not have a potential merger partner in their association. Further, we use the fact that political and ideological variables are important determinants for politicians' bailout decisions. Apart from their influence on the probability of a bailout by the politician, the dummies for the electoral cycle, for competitive counties and for conservative bank chairmen should not have an influence on a bank's future performance. Therefore, we can use these variables as instruments. The comparison of the long-run performance of banks bailed out by the owner and banks bailed out by the association yields a consistent pattern: Banks that obtained support from the association perform better and are also better capitalized in the years following the distress event.

It could be that politicians are not primarily concerned about the health of the bank itself, but rather care about the general economic development within their region. As a final piece of evidence, we compare the development of county-level macroeconomic variables around the distress events. We do not find differential effects on aggregate lending in counties with different types of events. However, following the distress event, the share of all loans within a given county that are extended by state banks increases in counties with owner bailouts and decreases in countries with support measures from the association. Both in counties with bailouts from the owner and in counties with support measures from the association, the GDP growth rate is relatively stable. Similarly, there are no significant changes in the share of employees within the population. Overall, we do not observe a better macroeconomic performance of counties in which the bank distress event was resolved by the owner as compared with the association.

The German savings bank sector provides an ideal set-up for our analysis for several reasons. First, savings banks in Germany represent a relatively homogeneous group. They operate in predefined geographic regions and are small in comparison to commercial banks. Consequently, bailout decisions concerning these banks are not distorted by too-big-to-fail arguments. Second, the savings bank organization has an extensive guarantee system that ensures the solvency and liquidity of its member institutions. Assuming that the organization's decisions on capital injections and distressed mergers are driven by economic considerations, they provide an ideal benchmark against which the decisions by local politicians can be evaluated. Third, institutional quality in Germany is rather high (e.g., corruption is extremely low). Therefore, the impact of political and ideological factors that we examine is not distorted by other institutional issues. Finally—and perhaps most importantly—Deutsche Bundesbank provides detailed information about distress events of savings banks that allows us to identify the capital injections of different parties as well as other restructuring measures around the event.

Our paper has important policy implications on the optimal proximity between banks and politicians or regulators that decide on bailouts. Although close proximity between politicians and banks might result in local knowledge for the decision maker, we document that outcomes are driven by personal incentives and ideology. A larger distance between policymakers and banks requires policymakers to rely on broad perspective. However, a larger distance is also likely to reduce personal stakes of politicians, and may therefore result in more efficient decisions on financial sector interventions. Our findings can be considered as relevant for the debate about the optimal level of banking supervision in the United States (Agarwal et al. 2012b), or the discussion about a unified banking supervision within the Euro zone. Since bailout decisions have dramatic consequences on the resulting market structure as well as on banks' risk taking[7], an understanding of politicians' incentives is of major importance.

This paper is, to the best of our knowledge, the first one that explicitly examines how political incentives affect bank bailout decisions in a developed country. The most related paper is Brown and Dinç (2005), who find for a sample of 21 emerging markets that failures of the largest banks in these countries are significantly more likely directly after an election as compared with the time before an election. While their paper is about the delay of bad news about bank failures prior to elections, we provide evidence that local politicians exploit their power to keep control of a bank if political circumstances allow it. Furthermore, we broaden the analysis by investigating not only the influence of the electoral cycle, but also the one of political competition and ideology. Another example of political influence on bank bailout decisions is provided by Imai (2009). He shows that bank regulators in Japan delay declarations of bank insolvency in counties that support senior politicians of the party in power.[8] Dinç (2005) and Sapienza (2004) show that government-owned banks increase their lending in election years relative to private banks.[9]

Our paper also relates to the current literature on public bailout policies and moral hazard. Dam and Koetter (2012) show that bailout expectations among German banks that are partly explained by political variables influence the risk-taking behavior of these banks. Banks that are more likely to be bailed out engage in additional risk-taking. Gropp, Hakenes, and Schnabel (2011) argue that an increase of the bailout probability of a bank increases risk taking incentives of the competing banks since government guarantees distort competition.

Finally, our paper is related to a broader literature on the political economy of finance. Especially in the aftermath of the recent crisis, several papers examine how legislation on the financial industry is affected by lobbying of special interest groups

[7] See Dam and Koetter (2012), Gropp, Hakenes, and Schnabel (2011).

[8] The influence of political incentives on bailout decisions is not constrained to the banking sector. Faccio, Masulis, and McConnell (2006) find that firms in 35 countries are more likely to be bailed out by the government if one of their top officers or a large shareholder is a member of the national government or parliament.

[9] For Germany, Vins (2008) and Englmaier and Stowasser (2012) examine how savings banks adjust their behavior around elections. They find that layoffs of employees, closures of branches or merger activities of these banks are significantly less likely prior to an election. At the same time, savings banks increase their lending around elections in order to induce favorable economic outcomes for the politicians.

and voter interests (Mian, Sufi, and Trebbi 2010, 2012, McCarty et al. 2010). Lobbying by financial institutions affects the regulatory environment and might have negative consequences for financial stability (see Romer and Weingast 1991 for the U.S. in the 1980s). Kroszner and Strahan (1999) provide evidence that special interests of the financial industry affected the timing of bank branch deregulation in the U.S. Similarly, Nunez and Rosenthal (2004) show that both ideology and interest group interventions are important for U.S. legislation on bankruptcy. In another recent paper, Agarwal et al. (2012a) examine whether the foreclosure decisions of banks during the recent crisis reflect these banks' political concerns and find that banks delayed foreclosures on mortgages located in districts whose representatives are members of the Financial Services Committee in the U.S. House of Representatives. Again, politicians and bankers seem to affect each others actions. Compared to the papers mentioned above our study takes a somewhat different approach. Rather than investigating how decisions of politicians are influenced by the financial industry, we concentrate on politicians' incentives to keep control of a bank that is currently in their sphere of influence.

The remainder of the paper is organized as follows. The next sections provides an overview of our institutional setup. In Section 4.3 we describe the construction of our dataset. Results on the influence of political variables on bailout decisions among German savings banks are presented in Section 4.4. In Section 4.5, we examine how the consequences of bailouts depend on the type of the bailout. Finally, we conclude in Section 4.6.

4.2 Institutional background: Local politicians and the German savings bank sector

The German financial sector can be classified as bank-based, with a universal banking system. One of the particularities of the German banking sector is its so-called three-pillar structure, referring to the three different legal ownership forms of German banks. The three forms are savings banks, private banks and credit cooperatives. The focus in this paper is on savings banks that granted 24.3 % of all corporate loans and 25.4 % of all consumer loans in Germany in 2010.[10] At this point in time, the savings bank association consisted of 429 individual banks with a combined balance sheet total of € 1,084 billion, 15,600 branches and about 250,000 employees.

The structure of the German savings banking sector is illustrated in Figure 4.1. Each savings bank operates in a predefined geographic area. By statutes, the savings banks do not compete with each other and only operate in the geographic region of the municipality that formally owns the bank. Since savings banks are owned by the local municipalities, the head of the municipal government, who is either a city mayor or a county administrator, is the chairman of each savings bank's supervisory board. We exploit this link between politicians and banks in the empirical analysis of our paper. The position as a chairman gives local politicians a strong influence on the appointment of the banks' management, the distribution of its earnings and—as they have a say on the allocation of large loans—the distribution of credit.[11] The supervisory board has about 15 members and the members besides the chairman consist of representatives from local authorities as well as savings bank employees. The representatives from local authorities make up about two thirds of the board members and are in most cases politicians from the local parliament.

The individual savings banks are connected through twelve savings bank associations at the state level.[12] These associations operate guarantee funds in order to ensure the liquidity and solvency of their member institutions in case of distress. The guarantee funds function like an insurance scheme: If one of its members gets into distress, the other banks in the association have to step in and provide support. Specifically, the resources for the guarantee funds are provided by the individual savings banks within the association.[13] The main support measures are capital in-

[10]All numbers are taken from Sparkassen-Finanzgruppe (2010). The German market for corporate loans had a volume of € 1,306 billion and the German market for consumer loans had a volume of € 229 billion in 2010. The shares given in the text are calculated as percentages of these volumes.

[11]Since savings banks are on average small institutions, large loans bear a particular risk for these banks. Therefore these banks generally have a credit committee in place which has to approve loans made by the bank that exceed a certain volume. Local politicians are often members of this credit committee.

[12]The associations do not exactly match the 16 German states. For example, four of the former GDR states form a single association. The twelve organizations also form the "Deutscher Sparkassen- und Giroverband" at the federal level.

[13]The savings bank sector operates a three-layer liability scheme where the regional funds constitute the first layer. If the funds of an individual association are not sufficient to support one of its member institutions the other associations have to step in due to a supraregional compensation

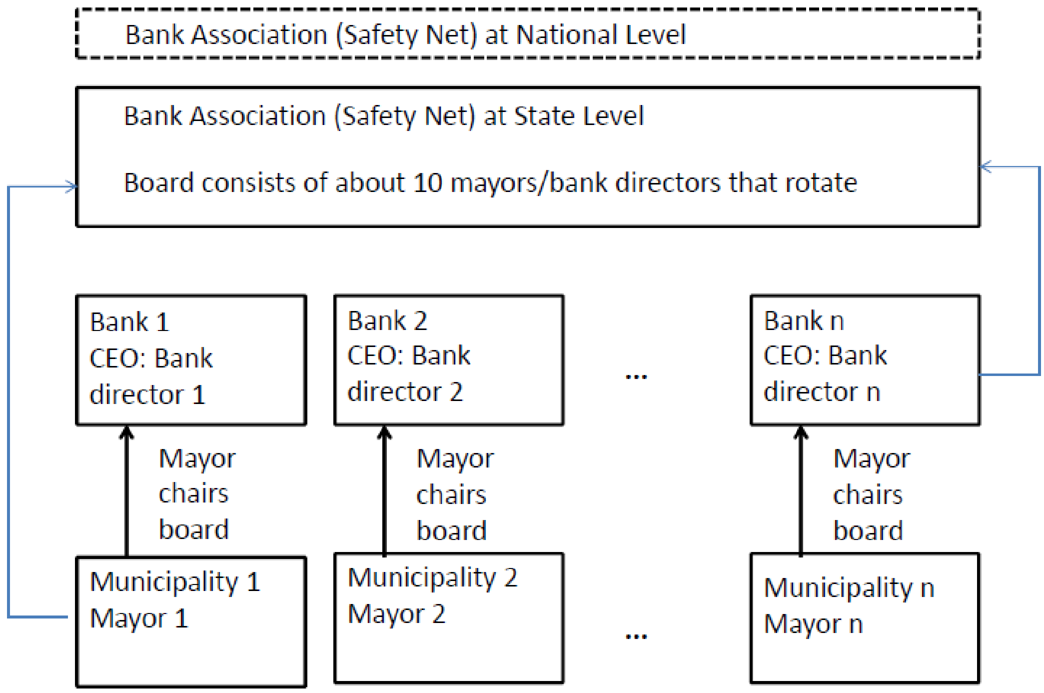


Figure 4.1: Institutional setup

Figure 4.1 illustrates the institutional setup for our analysis. The main institutions are the savings bank associations that operate the savings bank guarantee funds, the local counties or cities that own and back the individual banks, and of course the banks themselves. The figure shows that there are several personal and institutional connections within this system.

jections and debt guarantees. If a savings bank receives support from the association it has to agree on a restructuring plan that may include an organizational restructuring, a dismissal of the management and—in the worst case—a merger of the bank with another bank in the association.[14] In this case, the chairman of the bank will lose her/his position. Also a restructuring plan can pose severe restrictions on a bank's operations and, hence, constrain the power of the chairman. Alternatively, politicians can step in and use taxpayers' money to inject capital into the savings bank. In this case, the supervisory board can decide about potential restructuring measures without any intervention from the association. Hence, using taxpayers'

scheme. If these funds are still not sufficient, there is a joint liability scheme with central savings banks ("Landesbanken") and central building societies ("Landesbausparkassen").

[14]The decision on support measures is made by the board of the association, which is elected by the assembly of the association. Each member institution sends three people—usually the chairman of the board, the director of the bank and another member of the board—to the annual assembly of their association. At these meetings, members of the board of the association are elected among participants for terms that last for four to five years (see, e.g., Rheinischer Sparkassen- und Giroverband 2009 for more information).

money to save the banks allows politicians to prevent restructuring measures by the association. As we will document in the subsequent section (Section 4.3.3), there is considerably less restructuring in cases where the local politician instead of the association organizes the bailout of the savings bank. The main task of our empirical analysis is to understand the motives of politicians who inject money into a savings bank. In particular, we investigate whether their decision is based on superior information about the economic situation of the savings banks or political considerations.

Since supervisory boards of our sample banks are chaired by local politicians, we briefly summarize the German political system. Germany is organized as a parliamentary democracy with three layers of government: The federal republic, 16 states ("Bundesländer"), and 402 county districts consisting of 295 rural counties that are headed by local administrators, and 107 urban municipalities that are headed by city mayors. Separate elections on each layer take place in regular intervals. The focus of our paper is on the elections in rural countries and urban municipalities that take place every five years.[15] County/city elections take place at the same point in time within a state, but these points may differ across states. However, several German states have their county/city elections in the same year, so that we identify four main electoral cycles that correspond to the relevant elections for most of our sample banks.[16]

4.3 Data

Our analysis covers the German savings bank sector over the period from 1994 to 2010. We combine several confidential data sets from the Bundesbank's supervisory

[15] Laws on these elections are enacted at the state level. While the electoral cycle for county/city parliaments is five years in almost all German states(with the exception of Bavaria and Bremen, that have a six year and a four year cycle, respectively), there are some differences in the elections of local heads of government. In many German states mayors or district administrators are directly elected in separate elections that take place on the same day as the election of the local parliament. However, in some states the terms of mayors or district administrators are longer than the terms of local parliaments, whereas in other states the local head of government is appointed by the local parliament (and not directly elected). In order to be consistent across states, we focus on the timing of parliamentary elections on the county or city level in the empirical analysis. These elections are important for the bank's chairman as well as other members of the bank's supervisory board.

[16] These cycles are 1994-1998, 1999-2003, 2004-2008 and 2009-2010.

and statistics departments to compile a unique dataset that allows us to cleanly identify distress events of savings banks. In the first part of this section we explain the construction of this distress event variable. In the second part we describe bank-level and macroeconomic variables, while the third part illustrates restructuring activities around the distress events in our sample. The final part introduces the political variables and explains the motivation behind them.

4.3.1 Distress events

We define a particular savings bank to be in distress in a given year whenever it either receives external support (in form of capital injections and/or guarantees) from the owner and/or association or when it is taken over by another savings bank in a distressed merger. Identifying distress events in the savings bank sector is cumbersome, since not all kinds of potential support measures can be identified from banks' balance sheets (e.g., guarantees provided by third parties do not show up in the balance sheet). Furthermore, many savings banks have been involved in mergers without being in distress. We therefore combine four sources from Deutsche Bundesbank's supervisory data to cleanly identify distress events; that is, the Bundesbank's prudential data base for banking supervision (BAKIS), the monthly balance sheet statistics (BISTA), the borrowers' statistics, and the Bundesbank's data base on distress events (see Appendix for a detailed description of the four underlying data sets). Additionally, we consult local media coverage on distress events obtained from the GENIOS data base in order to verify our event dates.

First, we identify capital support measures by the owner (i.e., local politicians) by exploiting a peculiarity in savings banks' balance sheets. For historical reasons, the equity of these banks usually consists solely of contingency funds (so called "Sicherheitsrücklage"). These funds were originally provided by the owner of the bank in the year of foundation and then accumulated over the years out of the bank's retained earnings. However, if the savings bank—besides its equity in the contingency funds—also has subscribed capital unequal to zero, then this usually indicates an undisclosed participation of the bank owner (so-called "stille Einlage"). We therefore define an increase in subscribed capital that cannot be explained by takeovers or restructuring of equity positions as capital injections from the bank owner.[17] By using historical data of subscribed capital from the monthly balance sheet data (BISTA) we are able to identify the size of the capital injection as well as the particular month in which the event occurred.

Second, we code capital support measures by the savings bank association. Whenever one of the associations provides support to a savings bank—most often in the form of guarantees—this event is recorded in the so called "Sonderdatenkatalog 1" of the BAKIS database.[18] The data source is, however, only available at annual frequency. To determine the month of these events within a given year, we consult two further databases: First, we obtain data on capital adequacy ratios from the monthly balance sheet database BISTA;[19] and second, we identify large write-offs from the borrowers' loan statistics that is available on a quarterly basis.[20] We are therefore able to verify our identified events from two distinct Bundesbank data sources. In those cases in which we can only identify the respective quarter, we always assign the mid month of the respective quarter as the event month. We cross-check our event dates with media coverage on local distress events obtained from the GENIOS data base and find that the dates are broadly consistent with the coverage in the local press. There are some cases where savings banks received support from the association and the owner within the same year (four cases); we assign these events to the source that provided the larger amount of funds.[21]

Third, we obtain information on distressed mergers from the Bundesbank database on distress events.[22] A takeover of a distressed savings bank is organized by the

[17]In some German states the savings bank law allows undisclosed participation not only from the owner of the bank, but also from the savings bank association. However, this is the rare exception and we rule out these cases using the BAKIS database as described in the subsequent paragraph.

[18]Banks are legally bound to report this information to Bundesbank and BaFin. In contrast to pure balance sheet information this dataset contains confidential supervisory information.

[19]Large increases in the capital adequacy ratio in a certain month indicate that the savings bank received capital support at this time. Capital adequacy ratios in the BISTA are available on a monthly basis until the end of 2007, and on a quarterly basis from 2008 on.

[20]Large write-offs on loans in a given month indicate that the savings bank experienced a distress event at this time. Loan portfolio write-off data is available from 2002 on in the borrowers' statistics; therefore, it can be used to double-check the information on the timing of bailout events, in particular by the banking association, for roughly half of the time-period of our dataset. For the period before 2002 we have to rely on the evolution of the capital adequacy ratio in order to identify the timing of the distress event within a year.

[21]All results also hold if we exclude these cases.

[22]As the distress database is only available until 2006, we define distressed mergers in the years 2007-2010 as passive mergers where the bank that was taken over experienced a severe distress

savings bank association which identifies another savings bank in close geographic proximity to acquire the bank in distress. While capital injections as well as provisions of guarantees occur right after the bank falls short of regulatory capital (the distress event), there is generally a time gap between the actual distress event and the merger. In order to identify the actual date of the distress event we once more rely on large write-offs from the borrowers' loan statistics (as described above). For the savings bank that had a distressed merger before 2002 (the year when the borrowers' statistics database was initiated) we consult local media coverage from the GENIOS data base where it is available. For the remaining cases we have to make an assumption about the date of the distress event: We assume that the distress event occurred in December of the year before the actual merger took place.[23] As we are mainly interested in identifying whether a distress event took place before or after an election, this assumption is critical only for those cases where the distress event occurred within an election year. These are very few cases and excluding them does not affect our main findings.[24]

Overall, we identify 148 distress events of German savings banks during our sample period from 1994 to 2010. Among these 148 distress event, more than one third was resolved by capital injections from the owner (55 cases). The remaining 93 events were dealt with by the association. Out of these 93 cases, 44 banks experienced a distressed merger in the year following the distress event (see Table 4.1, Panel A). A definition of all variables is provided in Table 4.9 in the Appendix.

4.3.2 Bank and macroeconomic variables

Annual bank balance sheet data for all German savings banks is based on the unconsolidated balance sheet and income statement reports provided by the BAKIS

event in the three years before the merger (i.e., a moratorium, a capital support measure, or a very low capital ratio).

[23]We have also experimented with setting the month at March, June or September of the year before the distressed merger. Our results are unaffected by this choice.

[24]Out of the distress events resolved by the saving banks association, we have to make an assumption for seven events that occur within an election year. Assuming that these events took place in December actually biases our results against finding a significant effect of the electoral cycle, as some of them might have happened before the election and our main argument is that directly before an election support measures by the association are relatively more likely than support measures by the owner. Hence, assuming that these events took place in December is the most conservative assumption we can make.

Table 4.1: Descriptive statistics

Panel A: Events	Obs.
Support from owner	55
Support from association	93
... capital support	49
... distressed merger	44
Total	148

Panel B: Bank variables	All banks			Support from owner			Support from association		
	Obs.	Mean	S.D.	Obs.	Mean	S.D.	Obs.	Mean	S.D.
Total assets (€ mn)	8,246	1,780	2,530	636	2,770	4,150	706	1,660	1,810
Log(Total assets)	8,246	20.81	0.95	636	21.15	1.02	706	20.74	1.01
Total assets / GDP (in %)	8,228	37.24	31.90	636	53.50	51.88	706	39.47	41.57
Market share (in %)	8,219	22.50	16.39	636	23.83	15.55	706	16.88	16.33
Capital ratio (in %)	8,246	4.55	1.04	636	4.30	0.88	706	3.99	0.94
ROA (in %)	8,239	0.75	0.50	635	0.57	0.52	706	0.54	0.69
NPL ratio (in %)	8,195	3.79	2.61	634	4.06	2.79	703	5.26	3.42
Deposit ratio (in %)	8,245	67.47	9.49	635	61.14	10.60	706	65.47	11.19
Loans to owner / GDP (in %)	8,229	1.03	1.41	636	1.08	1.85	706	0.90	1.16
Conditional on distress									
Bank Chairman in Ass. Board	148	0.20	0.40						

Table 4.1 continued...

Panel C: Macro & Other variables	All banks			Support from owner			Support from association		
	Obs.	Mean	S.D.	Obs.	Mean	S.D.	Obs.	Mean	S.D.
GDPPC growth (in %)	8,246	1.288	3.816	636	1.040	3.925	706	1.874	4.034
GDPPC (in €)	8,228	23,771	8,528	636	27,280	7,931	706	22,648	6,542
Log(GDPPC)	8,228	10.024	0.313	636	10.173	0.285	706	9.988	0.281
Government debt / GDP (in %)	8,246	4.623	1.983	636	3.931	2.028	706	4.862	2.241

Panel D: Political variables	Obs.	Support from owner	Support from association
All	148	0.372	0.628
12-24 months before election	31	0.355	0.645
0-12 months before election	26	0.154	0.846
0-12 months after election	30	0.500	0.500
12-24 months after election	34	0.441	0.559
24-36 months after election	27	0.370	0.630
No competitive county	73	0.438	0.562
Competitive county	75	0.307	0.693
No conservative chairman	88	0.455	0.545
Conservative chairman	60	0.250	0.750

The table shows descriptive statistics for the banks in our sample. In Panel A we report the number of distress events, where we distinguish between support measures from the owner and support measures from the association. Panel B shows descriptive statistics for key bank variables. The unit of observation is a bank-year. The first three columns show statistics for all banks in our sample, whereas the other columns include only bank-year observation of banks that experienced support measures from the owner or the association during our sample period. Panel C provides descriptive statistics for macro control variables and a dummy variable that we use in the empirical analysis. Finally, Panel D shows the distribution of capital injections from the owner and support measures by the association, and how this distribution depends on political variables. For example, of the 148 distress events in our sample, 37.2 % were capital injections from the owner, while 62.8 % were support measures from the association. Depending on the values of the political variables this distribution differs.

database.[25] Table 4.1, Panel B, provides sample statistics for balance sheet items used in the empirical analysis. We compare the values of banks that had a distress event during our sample period with those of the average savings bank (633 in total). Banks that received capital injections from the owner are larger than average, both in terms of total assets as well as in terms of total assets divided by county-level GDP, while banks that were supported by the association are of similar size as the average bank.[26] Further, the bank's regional market share (proxied by the share of branches within the county) is slightly higher than the sample mean for banks that received support from the owner and significantly lower than average for banks that received support from the association. Overall, these descriptive statistics suggest that banks that are relatively important (as measured by size) tend to be bailed out by the owner.

Not surprisingly, the ratio of total equity to total assets is lower for banks that experienced either type of support measure. Moreover, these banks also have a lower ROA and a higher ratio of non-performing loans to customer loans on average. In contrast, the deposit ratio (savings deposits, term deposits, and time deposits to total assets) is significantly lower for banks that received support from the owner. The table further reports statistics on the amount of loans granted by the bank to its owner divided by county-level GDP, which is slightly higher for banks that obtain support measures from the owner as compared to those banks that are supported by the association.

We define an additional variable that we use in the empirical analysis for the 148 distress cases. The dummy variable *Bank Chairman in Ass. Board* indicates whether the distressed bank's chairman is also a member of the board of the association.[27] As the board of the association makes the decision on potential support measures by the association, the bank's chairman might be able to influence this decision if he

[25]We apply a very thorough merger treatment to the dataset: After the merger of two banks we artificially create a third bank (for the time after the merger) in the dataset. Note that the merger treatment causes the total number of banks in the dataset to exceed the maximum number of banks in a given time period.

[26]A definition of all variables is provided in Table 4.9 in the Appendix.

[27]Information on the composition of the boards of the association at each point in time is hand-collected from the respective annual reports of the associations. We carefully match association board members with chairmen of the individual banks by comparing both the name of the chairman as well as the county/city he is from.

is a member of this board. Overall, the politician is also member of the association board in 20% of the savings banks considered.

Our regional variables are gathered from various data sources. We obtain information on county level GDP per capita, its growth rate as well as the ratio of government debt to GDP on the county/city level from the 16 German State Statistical Offices. Descriptive statistics for these variables are provided in Panel C of Table 4.1. On average, banks experiencing a bailout by the politician are located in a municipality with lower GDP growth in comparison to the municipalities of banks that are bailed out by the association. Furthermore, municipalities where politicians conduct bailouts have a higher GDP per capita and are less indebted than the average municipality.

4.3.3 Restructuring efforts following bailouts

Having introduced bank-level variables, we can illustrate differences in restructuring between bailouts by politicians and bailouts by the association. Table 4.2 presents the growth rates in customer loans, employees, personal expenditures and the number of branches of the bank around the bailout events. As we have no accounting information on the operations of savings banks that were merged with other banks, we have to exclude these banks for this table. If politicians try to avoid painful restructuring measures of savings banks in distress, consequences for stakeholders should be more severe for banks that receive capital support from the association.

The first line of the table shows the average annual growth rate prior to the event of those banks that experienced the respective type of distress event during our sample period. For example, banks that received support from the association during our sample period had an average customer loan growth rate of 6.3 % in the years between the beginning of our sample period in 1994 and the year of the distress event. Similarly, column 2 shows that the average growth rate was 5.8 % for those banks that received capital injections from the owner and column 3 shows that the difference between the two groups of banks is not significant. In the bailout year, the average growth rate is significantly lower than the pre-event average for both types of events. However, the decline in the average growth rate is more than twice as large if the funds are provided by the association, and column 3 shows that customer loan growth in the bailout year is significantly higher if the bank is saved by the owner. The effect is similar in the year following the bailout, in the second and even in the third year after the bailout. This indicates that the restructuring plan imposed by the association has severe consequences for the bank's customers. This effect is dampened if the support measures come from the owner of the bank. Politicians try to avoid consequences for the customers of the bank, a behavior that is consistent with the personal interest explanation if one keeps in mind that the customers of the bank are in many cases identical to the politician's constituents.

A similar effect can be observed if we look at employee growth rates: Except for the second year after the bailout, there is no significant decline in the employee growth rate for banks that receive capital injections from the owner, which is rather surprising given that distress events usually lead to an organizational restructuring. In contrast, employee growth rates are significantly lower around capital support measures from the association. As expected, restructuring a bank in distress involves layoffs. Unfortunately we have information on the number of branches of the banks in our sample only until 2004, which reduces the number of observations. However, evidence points into the same direction as with the employee growth rate: The decline in the number of branches seems to be more severe for support measures from the association. The growth rate of personnel expenditures is somewhat lower around both types of events, and the difference between the two is not significant. To a certain extent, also employees at banks that are supported by the owner suffer from the distress event. Overall, however, the evidence suggests that politicians try to limit these negative consequences for stakeholders in the bank by conducting almost no restructuring activities.

4.3.4 Political variables

As explained in Section 4.2, local politicians often chair the supervisory board of the savings bank in their municipality. We hand-collect information on the identity and the position of distressed savings banks' chairmen from the banks' annual reports

Table 4.2: Change in key variables

Percentage Change in...		Customer Loans			Employees			Personnel Expenditures			Number of Branches		
		(1) Association	(2) Owner	(3) Difference	(4) Association	(5) Owner	(6) Difference	(7) Association	(8) Owner	(9) Difference	(10) Association	(11) Owner	(12) Difference
Pre Bailout	Mean	0,063	0,058	0,004	-0,007	-0,001	-0,006	0,038	0,033	0,006	-0,013	-0,027	0,014
	Median	0,057	0,059		-0,006	-0,003		0,037	0,029		0,000	0,000	
	S.D.	0,078	0,069		0,055	0,044		0,105	0,071		0,074	0,091	
	Obs.	169	266		169	266		169	266		151	244	
Bailout Year	Mean	0,000***	0,028***	-0,028**	-0,009	0,004	-0,013	0,020	0,028	-0,008	-0,081***	-0,102***	0,021
	Median	-0,010	0,020		-0,014	-0,005		0,018	0,036		0,000	-0,010	
	S.D.	0,062	0,057		0,071	0,062		0,084	0,065		0,152	0,187	
	Obs.	41	54		41	54		39	54		32	32	
Bailout Year + 1	Mean	-0,016***	0,016***	-0,032***	-0,028**	-0,004	-0,023*	0,004*	0,004**	-0,001	-0,087***	-0,039	-0,048
	Median	-0,030	0,016		-0,017	-0,014		0,010	0,016		0,000	0,000	
	S.D.	0,066	0,041		0,050	0,063		0,087	0,073		0,188	0,074	
	Obs.	41	45		40	45		40	45		31	26	
Bailout Year + 2	Mean	-0,018***	0,024***	-0,042***	-0,030**	-0,014*	-0,016*	0,008	0,019	-0,011	-0,141***	-0,128***	-0,013
	Median	-0,016	0,028		-0,027	-0,011		-0,003	0,019		-0,004	-0,033	
	S.D.	0,052	0,039		0,033	0,040		0,085	0,066		0,281	0,204	
	Obs.	33	38		33	38		33	38		24	23	
Bailout Year + 3	Mean	-0,014***	0,025***	-0,039***	-0,038***	-0,011	-0,027*	0,013	0,006**	0,007	-0,110***	-0,029	-0,082
	Median	-0,007	0,022		-0,029	-0,021		0,008	0,015		-0,018	0,000	
	S.D.	0,044	0,050		0,042	0,064		0,056	0,068		0,228	0,116	
	Obs.	31	36		30	36		31	36		18	19	

The table shows changes in key variables of savings banks around the years of capital injections. The first row shows pre-event statistics of banks that experienced a distress event during our sample period. All bank-year observations prior to the event denoted on top of the column are included. The other rows show the statistics for the event year as well as the years following the event. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level, in a two-sided test of the mean of bank-year observations prior to the event and bank-year observations in the respective year around the event (columns 1-2, 4-5, 7-8, and 10-11). In columns 3, 6, 9, and 12 * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level, in a two-sided test of the mean of bank-year observations of banks that received capital injections from the association and bank-year observations of banks that received capital injections from the owner in the respective year around the event.

as published in the Bundesanzeiger.[28] We use various internet sources in order to determine the party membership of these chairmen. Results and dates of elections on the county/city level are obtained from the 16 German State Statistical Offices. We carefully match counties and cities with municipal owners of our sample banks.[29] In this way, we are able to obtain information on the elections in all municipalities that own one of our sample banks.

In the following analysis we test whether there is a statical relationship between a politician's decision to provide support to a bank and the electoral cycle. To do so, we define *Electoral Cycle Dummies* as follows: The dummy variable *D(0-12 months)* takes a value of one during the 12 months after the local election and zero otherwise. The dummy variables *D(12-24 months)* takes a value of one for the time from the 12^{th} to the 24^{th} month following the local election and zero otherwise. The dummy variables *D(24-36 months)* and *D(36-48 months)* are defined accordingly. The 12 months preceding an election serve as the benchmark category against which the other time periods are evaluated.

Additionally, local politicians who care about their probability of being re-elected may base their bailout decision on the political competitiveness of their city/county. We thus define the variable *Competitive County* as follows: We calculate the vote share margin between the first and the second party within the county/city from the respective state election.[30] We then define a dummy that is equal to one if the vote share margin is smaller than the median and zero otherwise. We take this as a proxy for political competition within the county/city: The smaller the vote share margin between the first and the second party, the more intense the political competition and the more effective the disciplining role voters can exert on politicians.

As laid out in the introduction, a politician's bailout decisions might be influenced by his/her ideology. To proxy for a politician's ideology we define the dummy

[28]This information is available online from 2006 onwards (www.bundesanzeiger.de). For earlier observations, we consulted microfiche versions of the Bundesanzeiger provided by the university and regional library in Bonn.

[29]In cases where several municipalities jointly own a savings bank there is generally one dominant county or city that owns the largest share of the bank. We account for this by matching the respective bank to the county or city in which its headquarters are located.

[30] We use county/city level state election results as a proxy for political competitiveness as these elections are relatively similar across states so that results from different states can easily be compared to one another.

variable *Cons. Bank Chairman:* The variable is equal to one if the chairman of the bank is a member of the German conservative party ("CDU/CSU"). A fundamental conservative principle is the one of limited government intervention in markets. If politicians act according to this principle, we would expect less capital injections from the owner if the chairman of the bank is a CDU/CSU member.

In Table 4.1, Panel D, we display the relationship between the political/ideological variables introduced in this section and our identified distress events. The relative frequencies of capital injections by politicians display a clear pattern over the electoral cycle: In the 12 months before the election, the share of owner-bailouts in all distress events is considerably lower (15.4 %) than in the 12 months following the election (50.0 %). Further, the likelihood of a bailout by the politician in a competitive county/city is around 31 % conditional on bank distress, compared with 44 % in non-competitive counties/cities. Finally, out of our 148 distress events, 88 cases occurred at banks where the chairman is not a member of the conservative party ("CDU/CSU"), while the other 60 cases occurred at banks with a conservative party chairman. Capital injections from the owner are much less frequent when the chairman of the bank is a politician from a conservative party. This seems to be in line with the conservative ideology of limited state intervention.

To sum up, the descriptive analysis suggests a strong relationship between political and ideological variables and politicians willingness to use taxpayers' money to support banks in distress. This relationship should not be present if politicians base their intervention decisions on superior information obtained in their roles as bank chairmen.

4.4 Political determinants of bank bailouts

In this section, we present the results of our empirical analysis. We start by investigating the timing of distress events by applying a hazard model. We proceed by modeling the owner's decision to bail out a bank conditional on distress. Finally, we end the section by examining the impact of the fiscal situation of the municipality as well as other political factors on the owner's bailout decision.

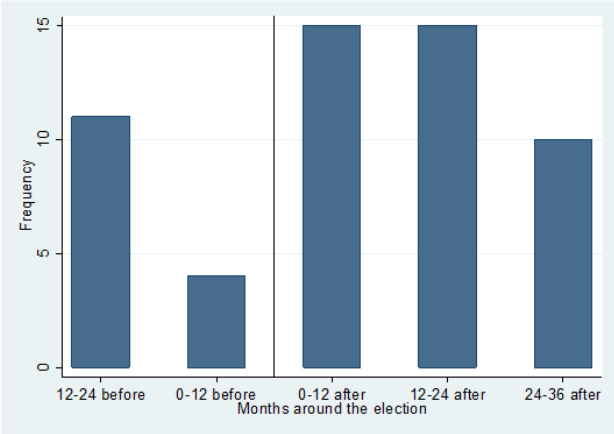
4.4.1 The timing of distress events

Figure 4.2 displays the distribution of distress events over the electoral cycle. Panel A focuses on capital injections from the owner and displays a clear pattern over the electoral cycle: Capital injections from the owner are less likely in the 12 months before an election, while support measures by the association are relatively evenly distributed over the cycle (Panel B). Panel C shows the distribution of all 148 distress events over the electoral cycle. Although the bar for the 12 months before the election is a bit lower than the other ones, we do not observe a clear relationship between bank distress events per se and the electoral cycle in Germany. This is in contrast to findings for emerging economies (Brown and Dinç 2005), which might be explained by a strong supervision of the banking sector, requiring the disclosure of monthly capital adequacy ratios. In such a supervisory environment bankers do not have the opportunity to delay distress events.

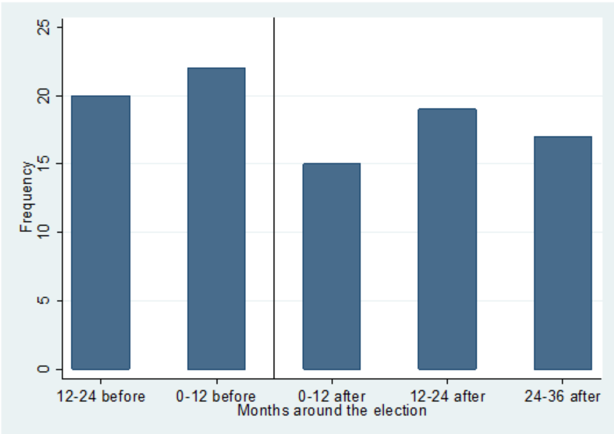
We formally test whether the electoral cycle influences the timing of bank distress events by using a hazard model. Potentially, if banks know about differences in politicians' willingness to bail them out, they might have an incentive to delay distress events. We define the period from the beginning of our sample in 1994 until a distress event as the time until distress for each bank. Thus, the hazard rate, $h(t)$, is the probability that a bank distress occurs at time t, given that no distress occurred until then. Following Brown and Dinç (2005), we test whether distress events depend on the electoral cycle, using an exponential hazard model:

$$h_i(t) = exp(\beta_0' \cdot x_{it-1} + \beta_1' \cdot Electoral\ Cycle_{it} + \beta_2' \cdot time_t + \beta_3 \cdot association_i) \quad (4.1)$$

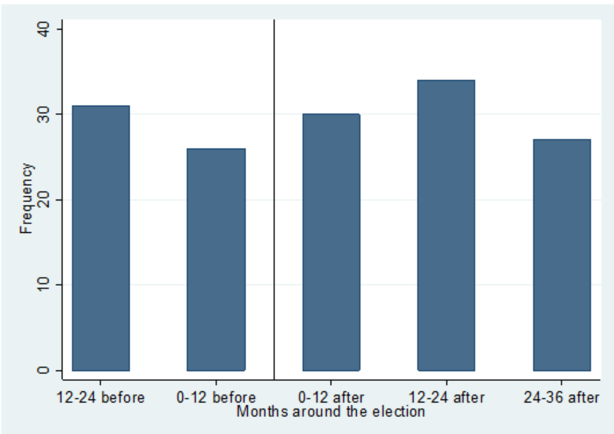
where x_{it-1} denotes a vector of covariates for bank i at time or duration $t-1$; β is a vector of unknown parameters to be estimated. The vector $Electoral\ Cycle_{it}$ includes our dummies for the electoral cycle. In the case of no failure, the electoral cycle dummies take a value of one if the bank's accounting year t falls into the respective period. The regression also includes time as well as association fixed effects. Since the cycles of the local elections are to a large extent synchronized (see Section 4.2), year fixed effects would absorb the $Electoral\ Cycle_{it}$. Therefore, we define time fixed effects which take the value of one during one particular election



Panel A: Capital injections by owner



Panel B: Support measures by association



Panel C: Total distress events (Panel A + Panel B)

Figure 4.2: Support measures and the electoral cycle

Figure 4.2 illustrates how the number of banks that receive support measures varies over the electoral cycle, where the vertical black line indicates the election date. The top panel shows the number of capital injections from the owner, the second panel shows the number of support measures by the association, i.e. the number of capital injections from the association plus the number of distressed mergers, and the third panel shows the sum of the first two panels across the electoral cycle.

Table 4.3: Hazard model

	All		
	(1)	(2)	(3)
D(0-12 months after)	0.319	0.445	-0.069
	(0.432)	(0.478)	(0.585)
D(12-24 months after)	0.181	0.183	-0.330
	(0.329)	(0.387)	(0.574)
D(24-36 months after)	0.072	-0.135	-0.311
	(0.333)	(0.362)	(0.442)
D(12-24 months before)	0.484	0.582	0.370
	(0.382)	(0.462)	(0.548)
Total Assets / GDP (t-1)		0.043	0.069
		(0.177)	(0.178)
Capital Ratio (t-1)		-0.107	-0.317*
		(0.117)	(0.168)
ROA (t-1)		-0.416***	-0.470***
		(0.125)	(0.135)
NPL Ratio (t-1)		-0.001	-0.001***
		(0.001)	(0.000)
Market Share (t-1)		-0.013**	-0.019**
		(0.006)	(0.008)
Deposit Ratio (t-1)		-0.018**	-0.035**
		(0.008)	(0.015)
GDPPC Growth (t-1)		0.020	-0.002
		(0.030)	(0.036)
Log(GDPPC) (t-1)		-0.416	-0.646***
		(0.345)	(0.121)
Time Dummies	YES	YES	YES
Association Dummies	NO	NO	YES
Observations	8,232	8,135	8,135

The table shows results for the following exponential hazard model:

$$h_i(t) = exp(\beta_0' \cdot x_{it-1} + \beta_1' \cdot Electoral\ Cycle_{it} + \beta_2' \cdot time_t + \beta_3 \cdot association_i),$$

where x_{it-1} denotes the a vector of covariates for bank i at time or duration $t-1$; β is a vector of unknown parameters to be estimated. The vector $Election\ Cycle_{it}$ indicates our dummies for the electoral cycle. Regressions include both savings banks that experienced a distress event during our sample period and savings banks that did not. Time dummies indicate the four election cycles in our sample (1994-1998, 1999-2003, 2004-2008, 2009-end of sample), while association dummies indicate the regional savings bank association of the bank. Standard errors are clustered by year. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

cycle (5 year interval) and zero otherwise (see Section 4.2). Standard errors are clustered by year.[31] We also employed a simple probit model instead of the hazard model, which yields very similar results.

The regressions include all bank-year observations for savings banks (those that

[31] Alternatively we cluster standard errors by association. This results in lower standard errors.

experienced a distress event as well as those that did not), starting in 1994. Table 4.3 presents our findings for the relationship between all distress events and the electoral cycle. In column 1 we only include time fixed effects as well as the $Electoral\ Cycle_{it}$ dummies. None of the dummies are significant. Thus, there is no relationship between the timing of distress events of state owned banks and the electoral cycle in Germany. This observation is unchanged if we add control variables in column 2. The control variables indicate that distress is less likely for large (measured by market share), profitable banks and those banks that take a higher fraction of customer deposits. Association dummies are included in column 3 to control for the fact that economic conditions differ among states. Results remain unchanged: There is no statistical relationship between the electoral cycle and distress events.

Having shown that the occurrence of distress events does not depend on the electoral cycle, we now turn to politicians' decisions to inject money into a distressed bank. We therefore focus on the 148 distress cases and examine how political and ideological variables affect a politician's decision to bail out one of these banks.

4.4.2 The impact of political factors on the bailout decision by politicians

It is a priori unclear why politicians should conduct capital injections into savings banks in distress, as the savings bank organization—as described in Section 4.2—has an extensive guarantee system. By modeling a politician's bailout decision we aim at differentiating between two possible explanations for this decision: Either the politician has more information about the economic situation of the bank and—therefore—aims to avoid restructuring measures by the association; or the politician cares about his/her probability of re-election and/or his/her ideology and therefore bases his decision on these factors.

Figure 4.3 displays the frequency distribution of owner bailouts over the electoral cycle on a biannual basis. Only one out of 55 cases of capital support by the owner occurs in the six months directly preceding an election. This suggests that politicians are reluctant to use taxpayers' money in order to support a savings bank in distress

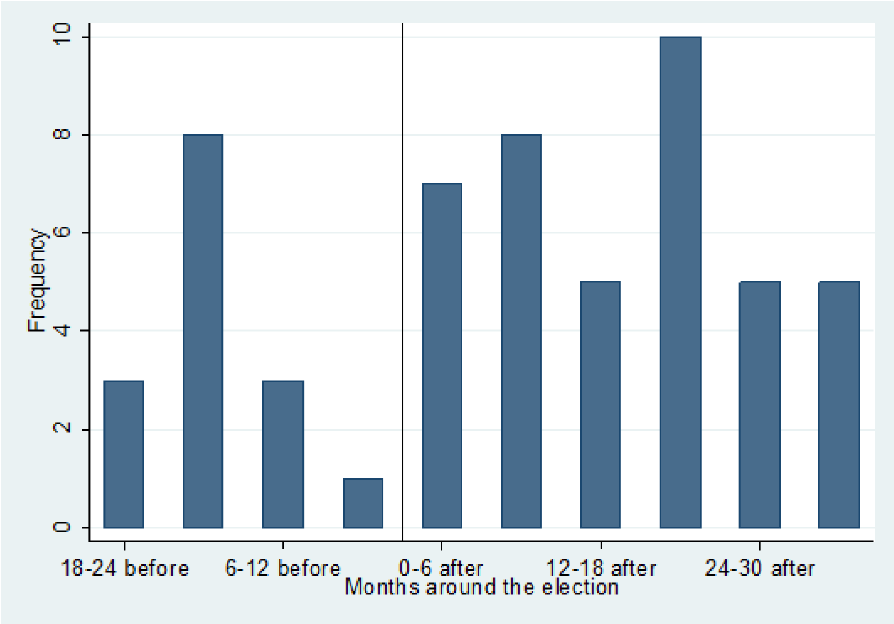


Figure 4.3: Capital injections from the owner and electoral cycle

Figure 4.3 illustrates how the number of banks that receive capital injections from the owner varies over the electoral cycle, where the vertical black line indicates the election date.

right before an election.[32] The relative percentage of owner's injections to total distress events is shown in Figure 4.5. Again, there is a clear indication that the probability of injecting money into a distressed bank is considerably lower in the year before the election.

To test these patterns in a formal way, we use a linear probability model in order to assess the relative likelihood of the two possible outcomes: Bailout by the politician and support measures by the association. We use the 148 distress cases in our sample to estimate the following equation:[33]

$$Event\ Type_{ijkt} = association_j + time_t + POL'_{kt}\beta + B'_{it-1}\gamma + C'_{kt-1}\delta + \epsilon_{ijkt}, \quad (4.2)$$

where i denotes the individual bank, j the association to which the bank belongs, k the county or city of the bank, and t the year in which the distress event occurred.

[32]Note that Figure 4.3 is identical to Panel A of Figure 4.2, using a 6 months interval instead of a 12 months interval. We used a 12 months interval in Figure 4.2 as we cannot identify the exact timing within the year for some distressed merger events. When we add these events to the first half of the year we create an artificial pattern of more events in the first six months compared to the second six months (and the opposite if we add these events to the second part of the year).

[33]Using a nonlinear logit model gives results that are very similar to the results from our linear specification (see Table 4.10 in the Appendix).

The dependent variable is a dummy called $Event\ Type_{ijkt}$ and takes the value of one if the bank distress is resolved by the politician and the value of zero if the distress is resolved by the association.[34] The political variables include dummy variables for the electoral cycle, the political competition within the county, and the ideology of the politician. They are summarized in the vector POL_{kt}. Bank level control variables are denoted by the vector B_{it-1} and include the bank's relative size to county/city GDP, the capital ratio, the return on assets, the non-performing loans ratio, the market share, and the deposit ratio. They are lagged by one year in order to obtain pre-event values. Regional control variables are also lagged by one year and include the level and the growth rate of county-level GDP per capita. They are summarized in the vector C_{kt-1}. In our most stringent specification, we include two sets of dummy variables, one of them indicating the association to which the bank belongs and the other one indicating the time of the event. The specification further includes a random error term ϵ_{ijkt}. The primary variables of interest are the political variables in the vector POL_{kt}. Coefficients for these variables should be insignificant if politicians' decisions are driven by informational advantages, while they should be significant if decisions are driven by politicians' personal interests.

Table 4.4 presents estimation results for Equation (4.2). We start with a benchmark specification without any political variables in column 1. The regression shows that larger banks or banks with a higher deposit ratio are less likely to receive capital injections from the owner. The opposite is true for banks with a higher local market share. One could argue that these banks are more important for regional development within the county and therefore the owner has a greater interest in keeping control of the bank and wants to avoid a painful restructuring plan or even a distressed merger. Finally, the regression shows that counties or cities with higher GDP per capita growth are less likely to use taxpayers' money in order to bail out a savings bank in distress.

We proceed by stepwise including the political variables into the regression model. Findings confirm our descriptive analysis presented in Panel D of Table 4.1. Political variables seem to have a strong influence on the type of the bailout for a savings bank

[34] Cases in which both the association and the owner inject money into the bank are classified as the category that contributed the larger amount of capital. See Section 4.3.1 for details.

Table 4.4: Event type

	Dependent Variable: Event Type				
	(1)	(2)	(3)	(4)	(5)
Total Assets / GDP (t-1)	-0.138**	-0.177***	-0.116*	-0.160**	-0.157**
	(0.056)	(0.048)	(0.060)	(0.055)	(0.059)
Capital Ratio (t-1)	-0.034	-0.042	-0.019	-0.034	-0.065
	(0.037)	(0.045)	(0.037)	(0.044)	(0.052)
ROA (t-1)	0.067	0.071	0.039	0.046	-0.017
	(0.071)	(0.058)	(0.079)	(0.063)	(0.055)
NPL Ratio (t-1)	-0.022*	-0.021	-0.023*	-0.022*	-0.019*
	(0.012)	(0.012)	(0.011)	(0.011)	(0.010)
Market Share (t-1)	0.009***	0.010***	0.009**	0.009***	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Deposit Ratio (t-1)	-0.007	-0.007	-0.005	-0.005	-0.004
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
GDPPC Growth (t-1)	-0.020*	-0.025**	-0.019*	-0.023**	-0.021**
	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)
Log(GDPPC) (t-1)	0.030	0.040	-0.049	-0.051	0.016
	(0.095)	(0.113)	(0.092)	(0.114)	(0.110)
D(0-12 months after)		0.286***		0.301***	0.265**
		(0.082)		(0.080)	(0.102)
D(12-24 months after)		0.390***		0.384***	0.413***
		(0.092)		(0.088)	(0.098)
D(24-36 months after)		0.230**		0.222**	0.233**
		(0.090)		(0.100)	(0.088)
D(12-24 months before)		0.296**		0.310**	0.275*
		(0.137)		(0.129)	(0.139)
Competitive County			-0.150**	-0.118	-0.166**
			(0.068)	(0.070)	(0.077)
Cons. Bank Chairman			-0.181**	-0.200**	-0.141
			(0.080)	(0.086)	(0.081)
Time Dummies	YES	YES	YES	YES	YES
Association Dummies	NO	NO	NO	NO	YES
Observations	148	148	148	148	148
R-squared	0.240	0.305	0.277	0.341	0.490

The table shows results for an OLS estimation of the following equation:

$$Event\ Type_{ijkt} = association_j + time_t + POL'_{kt}\beta + B'_{it-1}\gamma + C'_{kt-1}\delta + \epsilon_{ijkt},$$

where i denotes the individual bank, j the association, k the county or city where the bank is located, and t the year of the event. The dummy $Event\ Type_{ijkt}$ equals one if the bank received capital injections from the owner and zero if the bank received support measures from the association. The vector of political variables is denoted by POL_{kt}, B_{it-1} includes bank-level control variables, and C_{kt-1} is the vector of regional control variables. All columns include time dummies for the four election cycles in our sample (1994-1998, 1999-2003, 2004-2008, 2009-end of sample), and column 5 additionally includes a set of dummy variables that indicate the association of the bank. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

in distress. In the twelve months before an election, the probability that a politician resolves a distressed bank is 23 to 36 percent lower as compared to the other years in the electoral cycle (column 2). Politicians are about 15 percent less likely to support a distressed bank if political competition within the county or city of the bank is relatively high (column 3). This is in line with the personal interest explanation: Voters exert more discipline if the political competition is more intense. Although a politician might want to prevent restructuring of a distressed bank in order to keep it under her control, she cannot do so if this will be perceived as a waste of taxpayers' money and hence be punished in the next election. The more intense the political competition, the more severe the threat of punishment. Further, column 3 shows that capital injections from the owner are about 18 percent less likely if the bank chairman is a member of the conservative party, which is in line with the conservative ideology of limited state interventions. The results hold when we run a horse-race of all political variables in column 4. The explanatory power of the model significantly improves when the political variables are included: The R^2 increases from 0.240 in the benchmark case to 0.341. The results are further robust to the inclusion of association dummies (column 6).

4.4.3 Fiscal and other factors affecting the bailout decision of politicians

How does the fiscal situation of the local municipality affect the decisions of politicians to resolve bank distress? On the one hand, politicians of municipalities with a high level of fiscal debt are less capable to further increase spending. On the other hand, a high level of fiscal debt could indicate a politician's attitude for fiscal discipline.

As indicated in the previous section, politicians are less likely to support banks whose assets are relatively large as a fraction of the municipalities' GDP (see also Table 4.5, columns 1 and 2). Since bailouts of large banks tend to be expensive, this result is likely to reflect fiscal boundaries of local politicians. Once we include a measure for the fiscal deficit of the community we obtain a significantly negative relationship: Politicians of highly indebted communities are less likely to resolve bank distress (columns 3 and 4). This is an example of the disciplining effect of fiscal federalism.

We examine further variables that might affect politicians' willingness to bail out banks. In columns 5 and 6, we include a proxy for personal connections between the association board and the board of the respective bank in distress (*Bank Chairman in Ass. Board*). This variable is equal to one if the chairman of the bank is also a member in the board of the association. This board decides on support measures provided by the association and it is possible that the politician tries to use her/his influence to obtain support without further restructuring. If this would be the case, we would expect that politicians are less likely to use taxpayers' money to resolve distressed banks. In a way, this variable tests whether the decision process at the association is rather transparent and follows pre-determined rules, or whether it is prone to favoritism. The dummy is insignificant, which illustrates once again the rather transparent decision process of the savings bank associations. If the association was prone to favoritism we would have expected a significantly negative coefficient for this dummy.

Next, we test for a link between the bailout decision and funding that the respective municipality obtains from the distressed bank. Politicians might have incentives to keep control over a savings bank if this bank provides a large fraction of loans to the politicians' municipalities. We include the amount of loans that the municipality is borrowing from the distressed bank divided by local GDP. We detect no significant relationship between this measure and the probability of the owner to resolve a bank in distress (columns 7 and 8).

Finally, the horse race in columns 9 and 10 shows that the political variables exert a strong and persistent influence on politicians' decisions to inject money into distressed banks.

4.5 Consequences of political bailouts

Having established that the decision by politicians to inject funds into distressed banks depends on political as well as ideological factors, we now examine whether there are differences in the long-run performance of distressed banks that were ei-

Table 4.5: Fiscal variables and alternative stories

	Dependent Variable: Event Type									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Capital Ratio (t-1)	-0.034	-0.088	-0.048	-0.087	-0.034	-0.088	-0.034	-0.089	-0.045	-0.068
	(0.037)	(0.056)	(0.039)	(0.055)	(0.036)	(0.057)	(0.039)	(0.058)	(0.048)	(0.055)
ROA (t-1)	0.067	0.030	0.073	0.036	0.065	0.034	0.069	0.033	0.054	0.002
	(0.071)	(0.059)	(0.075)	(0.063)	(0.071)	(0.058)	(0.071)	(0.059)	(0.068)	(0.060)
NPL Ratio (t-1)	-0.022*	-0.016	-0.018	-0.013	-0.022*	-0.016	-0.023*	-0.017	-0.019	-0.016*
	(0.012)	(0.010)	(0.011)	(0.010)	(0.012)	(0.010)	(0.012)	(0.010)	(0.011)	(0.009)
Market Share (t-1)	0.009***	0.009**	0.010***	0.009***	0.010***	0.009**	0.009***	0.009**	0.010***	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Deposit Ratio (t-1)	-0.007	-0.006	-0.005	-0.007	-0.006	-0.006	-0.006	-0.006	-0.004	-0.005
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)
GDPPC Growth (t-1)	-0.020*	-0.014	-0.019*	-0.015	-0.020*	-0.015	-0.020*	-0.015	-0.022*	-0.022**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)
Log(GDPPC) (t-1)	0.030	-0.052	-0.090	-0.076	0.068	-0.079	0.027	-0.046	-0.110	-0.064
	(0.095)	(0.119)	(0.114)	(0.127)	(0.119)	(0.155)	(0.096)	(0.128)	(0.159)	(0.156)
Total Assets / GDP (t-1)	-0.138**	-0.139***	-0.132**	-0.142***	-0.144**	-0.132**	-0.109	-0.107	-0.164	-0.121
	(0.056)	(0.042)	(0.055)	(0.045)	(0.057)	(0.048)	(0.102)	(0.093)	(0.107)	(0.111)
Government Debt / GDP (t-1)			-0.044**	-0.037**					-0.025	-0.023
			(0.015)	(0.016)					(0.019)	(0.020)
Bank Chairman in Ass. Board					-0.082	0.047			0.012	0.124
					(0.120)	(0.124)			(0.119)	(0.108)
Loans to Owner / GDP (t-1)							-0.015	-0.018	0.003	-0.011
							(0.042)	(0.041)	(0.037)	(0.035)
D(0-12 months after)									0.302***	0.269**
									(0.082)	(0.109)
D(12-24 months after)									0.363***	0.429***
									(0.110)	(0.103)
D(24-36 months after)									0.224**	0.247**
									(0.098)	(0.088)
D(12-24 months before)									0.313**	0.298**
									(0.124)	(0.133)
Competitive County									-0.099	-0.157
									(0.070)	(0.092)
Cons. Bank Chairman									-0.172*	-0.138
									(0.087)	(0.090)
Time Dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Association Dummies	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Observations	148	148	148	148	148	148	148	148	148	148
R-squared	0.240	0.407	0.268	0.420	0.244	0.408	0.241	0.408	0.349	0.503

The table shows how fiscal and other variables affect the likelihood of a bailout from the owner. As before the dependent variable is a dummy that equals one if the bank received capital injections from the owner and zero if the bank received support measures from the association. Bank control variables are the same as in Table 4.4. Additionally, we include the county-level ratio of government indebtedness to GDP (*Government Debt / GDP*), a dummy variable *Bank Chairman in Ass. Board* that takes the value of one if the chairman of the bank in distress is a member of the board of the local savings bank association, and the variable *Loans to Owner/GDP*) that gives the amount of credit extended by the savings bank to the local government divided by local GDP. As before, all variables are lagged by one period. Columns 1, 3, 5, 7, and 9 include time dummies for the four election cycles in our sample (1994-1998, 1999-2003, 2004-2008, 2009-end of sample), and columns 2, 4, 6, 8, and 10 include additional dummies that indicate the association of the bank. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

ther resolved by politicians or by the savings bank association. Furthermore, since politicians may care about the development of their municipality as a whole rather than the performance of their savings banks, we also compare the macroeconomic development of municipalities whose savings banks were bailed out by politicians to the development of municipalities whose banks were supported by the association.

4.5.1 Bank performance following bailouts

Descriptives

We start with descriptive statistics for changes in key variables for banks that experienced a distress event.[35] As documented in Section 4.3.3, bailouts by politicians are associated with less restructuring activities, which could affect banks' long-run performance. On the one hand, performance could be negatively affected if the politician tries to prevent necessary restructuring measures that might negatively affect his probability of re-election. On the other hand, less restructuring might be optimal if politicians have better information about the situation of their bank. Comparing the long-run performance of banks that received support from either politicians or the association helps us to further distinguish between these two explanations.

Descriptive statistics are shown in Table 4.6. For each bank, we calculate the four-year change as compared with the bailout year for several key variables, the average between the four-year change and the five-year change, and so on (up to seven years). We then average these changes across banks that received support from either the association or the owner and compare the values for these two groups of banks. The comparison yields a clear picture: Irrespective of the chosen horizon, banks that obtained support from the association improved their performance considerably more in the long run as compared to banks that received support from the owner. For example, the capital ratio rises significantly more for banks whose distress case was resolved by the association. Interestingly, only banks that received support from the association are able to considerably reduce their non-performing loans ratio. Similarly, there is a higher reduction in the ratio of loan loss provisions to customer loans for banks that obtained support from the association. Finally, the return on

[35] As in Section 4.3.3, we cannot include banks that were merged by the association since we do not have data on their future performance.

assets for this group of banks increased by about 0.2 percent more on average as compared to banks that obtained support from the owner.

Table 4.6: Long-run performance—descriptives

	Association			Owner			Difference
	(1) Obs.	(2) Mean	(3) S.D.	(4) Obs.	(5) Mean	(6) S.D.	(2)-(5)
Capital Ratio							
t=4	35	0.590	0.615	39	0.254	0.413	0.336***
t=5	29	0.578	0.647	34	0.229	0.452	0.349**
t=6	24	0.499	0.647	27	0.277	0.500	0.222
t=7	22	0.618	0.563	22	0.303	0.478	0.315*
NPL Ratio							
t=4	34	-3.238	4.209	38	0.106	3.077	-3.344***
t=5	29	-4.011	4.136	34	-0.001	3.569	-4.010***
t=6	24	-4.907	4.285	27	-0.795	3.826	-4.111***
t=7	22	-5.118	4.515	22	-1.140	3.577	-3.977***
LLP to CL							
t=4	34	-0.698	0.759	39	-0.287	0.837	-0.411**
t=5	29	-0.759	0.767	34	-0.343	0.824	-0.415**
t=6	24	-0.750	0.793	27	-0.384	0.908	-0.365
t=7	22	-0.813	0.823	22	-0.493	0.860	-0.320
ROA							
t=4	34	0.271	0.649	39	0.050	0.508	0.221
t=5	29	0.290	0.594	34	0.062	0.464	0.228*
t=6	24	0.213	0.537	27	0.015	0.566	0.198
t=7	22	0.309	0.526	22	0.069	0.482	0.240

The table shows changes in key variables for banks that experienced a distress event. With t denoting the number of years since the bailout event, we calculate for each bank and for $t \in \{4, 5, 6, 7\}$

$$\frac{1}{t+1-4}\sum_{i=4}^{t} var_i - var_0,$$

where var_i denotes the value of the variable in the ith year after the bailout and var_0 denotes the value in the bailout year. We then average these changes across banks. Column 7 shows the difference in the mean between the two groups of banks, where *, **, and *** indicate statistical differences in the mean at the 10 %-level, 5 %-level, and 1 %-level, respectively.

Addressing selection

There are two potential sources of selection bias that might explain why banks that receive support from the association perform better in the long run as compared to banks that receive support from the owner. First, following the distress event, we do not have accounting information for banks that experienced a distressed merger.

The association is likely to organize distressed mergers for the 'worst' distress cases. Hence, comparing the remaining association bailouts to the average owner bailout might suffer from a bias. Second, there might be unobserved variables that jointly affect the politician's bailout decision and the future performance of the bank.

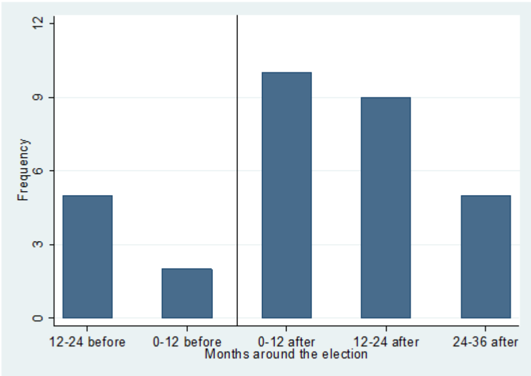
To circumvent the first issue, we restrict the sample to those savings banks that do not have a potential merger partner. In particular, we require that the bank in distress does not have another savings bank in close geographic proximity that has at least 1.5 times its size (in terms of total assets) as well as a capital ratio and an ROA higher than the median in our sample.[36]. In this way, we obtain a subsample of 56 distress cases for which we are able to obtain five-year changes in the key variables from the previous section.[37] By only focusing on this subsample, we ensure that the comparison between association and owner bailouts is a fair comparison.

To address the second issue, we use the fact that political and ideological variables are important determinants for politicians' bailout decisions. Apart from their influence on the probability of a bailout by the politician, the dummies for the electoral cycle, for competitive counties and for conservative bank chairmen should not have an influence on a bank's future performance. Therefore, we can use these variables as instruments.

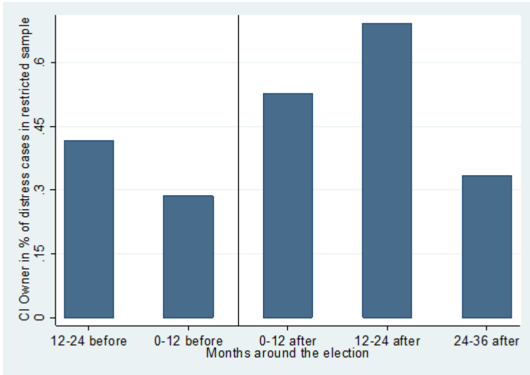
We start by illustrating our identification strategy graphically in Figure 4.4. In Panel A and B we display the absolute and the relative frequency distribution of capital injections from the owner across the electoral cycle within the subsample of banks that do not have a potential merger partner. The pattern in the subsample is similar to the one in the full sample (see Figures 4.3 and 4.5): The probability for a capital injection from the owner is considerably higher after the election as compared to the period before the election. More specifically, there are only 6 cases of capital injections from the owner in the two years before the election, while there are 19 cases in the two years after the election.

[36]We define a savings bank to be in 'close geographic proximity' of a bank in distress if it is located in a county neighboring the one of the distressed bank. Further, we altered the criteria for a potential merger partner and found that our results do not depend on the exact definition (in particular, we tried different size cutoffs (same size, two times the size) and omitted the capital ratio and ROA criteria in alternative specifications).

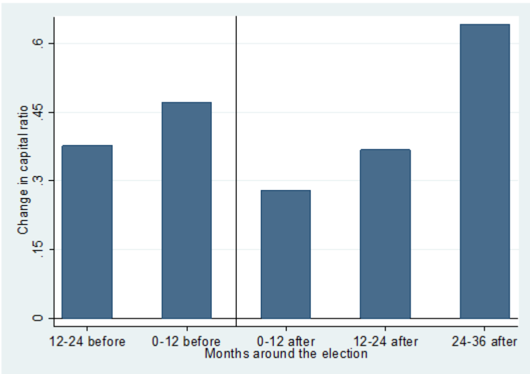
[37]We cannot include distress cases from 2005 or later years as we need at least five years of accounting information for the bank following the distress event.



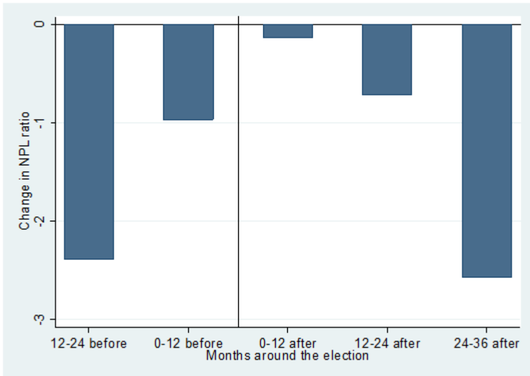
Panel A: CI Owner



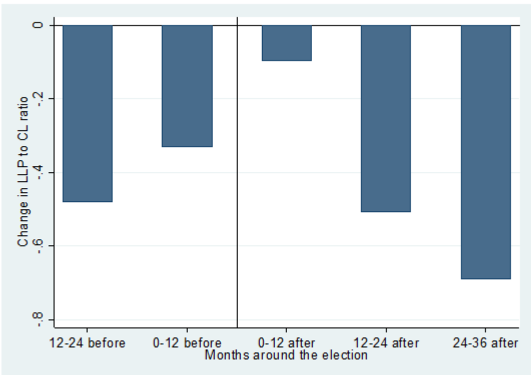
Panel B: CI Owner (relative frequency)



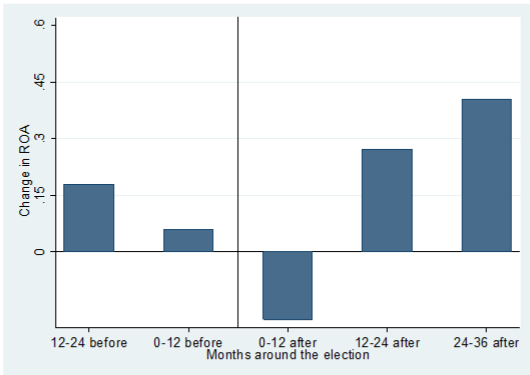
Panel C: Capital ratio



Panel D: NPL ratio



Panel E: LLP to CL ratio



Panel F: ROA

Figure 4.4: Long-run performance and electoral cycle

Figure 4.4 illustrates how the long run performance of banks in distress depends on the timing of the distress event over the electoral cycle, where the vertical black line indicates the election date. We restrict the sample to banks without a potential partner for a distressed merger to account for selection bias. Panel A shows the number of capital injections from the owner across the electoral cycle in the restricted sample, whereas Panel B shows the relative frequency. Further, we calculate the five-year change in the capital ratio (Panel C), the non-performing loans ratio (Panel D), the ratio of loan loss provisions to customer loans (Panel E), and the ROA (Panel F), and then show the average of this change across banks that experienced a distress event at the same time during the electoral cycle.

Table 4.7: Long-run performance—regressions

	Capital Ratio				NPL Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	IV	IV	OLS	IV	IV	IV
Owner	-0.389**	-0.833**	-1.122***	-1.145***	5.002***	3.425*	8.942***	8.540***
	(0.151)	(0.335)	(0.383)	(0.377)	(0.927)	(1.960)	(2.279)	(2.161)
Constant	0.578***	0.792***			-4.011***	-3.250***		
	(0.105)	(0.180)			(0.644)	(1.054)		
Association Dummies	NO	NO	YES	YES	NO	NO	YES	YES
Time Dummies	NO	NO	NO	YES	NO	NO	NO	YES
Observations	56	56	56	56	56	56	56	56
R-squared	0.110	0.114	0.132	0.144	0.350	0.316	0.406	0.455

	LLP to CL				ROA			
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	OLS	IV	IV	IV	OLS	IV	IV	IV
Owner	0.629***	0.910**	0.485	0.459	-0.289*	-0.522*	-0.292	-0.283
	(0.184)	(0.388)	(0.424)	(0.401)	(0.145)	(0.306)	(0.352)	(0.343)
Constant	-0.759***	-0.894***			0.290***	0.402**		
	(0.128)	(0.208)			(0.101)	(0.164)		
Association Dummies	NO	NO	YES	YES	NO	NO	YES	YES
Time Dummies	NO	NO	NO	YES	NO	NO	NO	YES
Observations	56	56	56	56	56	56	56	56
R-squared	0.178	0.142	0.341	0.399	0.069	0.024	0.168	0.194

The table examines how banks' long-run performance following a distress event depends on the type of the distress event. We restrict the sample to banks without a potential partner for a distressed merger to account for selection bias. The dependent variable is the the five-year change in the capital ratio as compared to the bailout year in columns 1-4, the five-year change in the non-performing loans ratio in column 5-8, the five-year change in the ratio of loan loss provisions to customer loans in columns 9-12, and the five-year change in ROA in columns 13-16. Columns 1, 5, 9, and 13 report results for simple OLS regressions, where *Owner* is a dummy equal to one if the bank received capital injections from the owner and equal to zero if it received support from the association. The remaining columns show results for two-stage least squares regressions. In the first stage, we regress the dummy variable *Owner* on the political variables from above (dummies for the electoral cycle, competitive counties, and conservative bank chairmen), and the additional dummy variables specified at the bottom of the table. In the second change, predicted probabilities from the first stage are used to predict the five-year change in the respective variable. Again, we include the additional dummy variables denoted at the bottom of the table. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

In Panels C to F, we display average values for five-year changes in the bank performance measures from above (i.e., capital ratio, non-performing loans ratio, ratio of loan loss provisions to customer loans, and ROA), grouped by the electoral cycle.[38] In general there should be no relationship between banks' future performance and the timing of the distress event within the electoral cycle. We know, however, that the probability for capital injections from the owner is considerably higher after the election as compared to the time before the election. Therefore, differences in future bank performance across the electoral cycle can be attributed to the actions of politicians. Performance measures in Panels C to F display a clear pattern across the electoral cycle. In particular, improvements in the capital ratio and reductions in the non-performing loans ratio as well as the ratio of loan loss provisions to customer loans are considerably smaller for distress events that occurred in the 12 months following an election, when bailouts from the owner are relatively more likely. Similarly, improvements in profitability are smaller for banks that were bailed out in the 12 months following an election. It is important to note that these documented differences in future performance do not depend on the time horizon. We have tried alternative horizons (i.e., four-year changes and six-year changes) and find similar patterns.

Finally, we investigate how future bank performance depends on the type of the bailout in a regression framework. Again, we start with the five-year change in the capital ratio as a dependent variable. Column 1 of Table 4.7 shows estimates from a simple OLS regression, which confirm that banks receiving capital injections from the owner exhibit lower increases in the capital ratio. As described above, we proceed by using the dummies for the electoral cycle, for competitive counties and for conservative bank chairmen as instruments in a two-stage least squares regression. The first stage regression is similar to the regressions in Table 4.4, while restricting the sample to the distress cases without a potential merger partner. Results for the second stage regressions are presented in columns 2-4 of Table 4.7. Five years after the bailout, the capital ratio increased significantly more for banks that were resolved by the association. Remarkably, the magnitude of the coefficient is considerably larger

[38] Specifically, we average the five-year change in the respective variable across banks in the restricted sample for which the distress event occurred at the same time in the electoral cycle.

in the IV regression as compared to the OLS regression: Capital ratios increase by about 1 percent more if the distress case is resolved by the association as compared to the owner. The results are robust to the inclusion of association and time dummies. Again, we observe similar patterns for the other performance measures: Banks receiving capital injections from the owner experienced smaller improvements in the non-performing loans ratio, the ratio of loan loss provisions to customer loans and the profitability measured by ROA. As the number of observations is very small in these regressions, the findings are particularly impressive. As before, they do not depend on the exact definition of the time horizon (e.g., see Table 4.11 in the Appendix, where we use four-year changes in the variables instead of five-year changes).

4.5.2 Macroeconomic performance following distress events

In the previous section we showed that savings banks that experience a bailout from the association perform considerably better in the long-run as compared to savings banks that experience a bailout from the owner. By saving the bank from severe restructuring measures that would be imposed by the association, politicians seem to hurt the long run performance of the bank. However, it could be that politicians are not primarily concerned about the health of the bank itself, but rather care about the general economic development within their region. In order to assess this concern we examine the macroeconomic development of the county in which the respective savings bank is located.

In particular, we replicate the estimations from Section 4.5.1, using six county-level indicators (i.e., the share of aggregate financing provided by state banks, the ratio of aggregate loans to GDP, the ratio of aggregate loans to private companies to GDP, the ratio of capital expenditures by firms in the manufacturing sector to GDP, real GDP growth, and the share of employees in the population) as dependent variables. Since we can also track the macroeconomic development of counties whose savings banks got involved in a distressed merger, we only have to worry about omitted variables that affect the owners' bailout decision and the macroeconomic development at the same time (i.e., the second concern in the previous section). To address this concern we use—as before—our political variables as instruments. The

Table 4.8: Macroeconomic developments—regressions

	State Bank Loan Share				Loans to GDP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	IV	IV	OLS	IV	IV	IV
Owner	0.0630***	0.0902**	0.2436***	0.2156***	0.2462**	0.2660	0.6606	0.5703
	(0.0208)	(0.0456)	(0.0754)	(0.0753)	(0.1224)	(0.2845)	(0.4262)	(0.4388)
Constant	-0.0337**	-0.0444**			-0.0791	-0.0865		
	(0.0131)	(0.0207)			(0.0750)	(0.1217)		
Association Dummies	NO	NO	YES	YES	NO	NO	YES	YES
Time Dummies	NO	NO	NO	YES	NO	NO	NO	YES
Observations	104	104	104	104	88	88	88	88
R-squared	0.0824	0.0672	0.2345	0.3734	0.0449	0.0446	0.0362	0.1921

	Loans to Private Corporate Sector to GDP				Private Capital Expenditures to GDP			
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	OLS	IV	IV	IV	OLS	IV	IV	IV
Owner	0.0241	0.0310	0.0247	0.0464	0.0003	0.0129	0.0161	0.0150
	(0.0165)	(0.0404)	(0.0376)	(0.0466)	(0.0054)	(0.0129)	(0.0180)	(0.0200)
Constant	-0.0068	-0.0093			0.0009	-0.0039		
	(0.0101)	(0.0170)			(0.0033)	(0.0055)		
Association Dummies	NO	NO	YES	YES	NO	NO	YES	YES
Time Dummies	NO	NO	NO	YES	NO	NO	NO	YES
Observations	83	83	83	83	88	88	88	88
R-squared	0.0256	0.0236	0.1975	0.4191	0.0000	0.0112	0.0636	0.0910

	Real GDP Growth				Share of Employees in Population			
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
	OLS	IV	IV	IV	OLS	IV	IV	IV
Owner	0.0036	-0.0215	-0.0205	-0.0445	0.0041	-0.0037	-0.0173	-0.0360*
	(0.0162)	(0.0383)	(0.0528)	(0.0605)	(0.0045)	(0.0108)	(0.0154)	(0.0194)
Constant	0.0770***	0.0864***			0.0098***	0.0127***		
	(0.0100)	(0.0164)			(0.0028)	(0.0046)		
Association Dummies	NO	NO	YES	YES	NO	NO	YES	YES
Time Dummies	NO	NO	NO	YES	NO	NO	NO	YES
Observations	88	88	88	88	91	91	91	91
R-squared	0.0006	0.0037	0.1692	0.2103	0.0093	0.0013	0.1504	0.2797

The table examines how macroeconomic developments on the county level following a distress event depend on the type of the distress event. The sample includes all observations for which we are able to obtain the dependent variable, which is the five-year change in share of loans in the county that is extended by state banks in columns 1-4, the the five-year change in the ratio of aggregate loans to GDP as compared to the bailout year in column 5-8, the five-year change in the ratio of aggregate loans to the private corporate sector to GDP as compared to the bailout year in column 9-12, the five-year change in the ratio of capital expenditures in the manufacturing sector to GDP as compared to the bailout year in column 13-16, the five-year real GDP growth rate in columns 17-20, and the five-year change in the share of employees in the population in columns 21-24. Columns 1, 5, 9, 13, 17, and 21 report results for simple OLS regressions, where *Owner* is a dummy equal to one if the bank received capital injections from the owner and equal to zero if it received support from the association. The remaining columns show results for two-stage least squares regressions. In the first stage, we regress the dummy variable *Owner* on the political variables from above (dummies for the electoral cycle, competitive counties, and conservative bank chairmen), and the additional dummy variables specified at the bottom of the table. In the second change, predicted probabilities from the first stage are used to predict the five-year change in the respective variable. Again, we include the additional dummy variables denoted at the bottom of the table. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

second stage results for five-year changes in the macroeconomic variables are summarized in Table 4.8. The first four columns indicate that the type of the support measure affects the county-level structure of financing: The share of loans in the county extended by state banks relatively increases in counties where the savings bank was bailed out by the owner. Moreover, the OLS regression in column 5 indicates that counties with bailouts from the owner see a relative increase in financial depth (column 5). However, the difference between the two types of events vanishes in the two-stage least squares regressions (columns 6 to 8). Next, we restrict ourselves to loans to private, non-financial companies and exclude loans to the public sector from the loans to GDP ratio. Columns 9-12 suggest no difference between the different types of support measures: All coefficients are close to zero, and also the OLS coefficient is now insignificant. Overall, it does not seem as if the type of support measures affects financing conditions for the private sector.

In the remainder of the table, we evaluate the ratio of capital expenditures by firms in the manufacturing sector to GDP, real GDP growth, and the share of employees in the population. There are no significant differences between counties where banks received support from the owner and counties where the distress case was resolved by the association. These findings suggest that politicians' decision to use taxpayers' money to bail out a savings bank is not driven by concerns about the general economic development within their region.

4.6 Conclusion

In this paper we document that public bailout policies in Germany are driven by political interests and ideology. The probability of politicians injecting taxpayers' money into a distressed bank is about 30 % lower in the year before an election. High competition in the electoral process reduces the probability of a public bailout by 15 %. We also show that ideology matters for bailout decisions. Capital injections are 17 % less likely if the politician is a member of the conservative party. Furthermore, the long-run performance of banks that were bailed out by politicians is considerably lower as compared with banks that were supported by the association. To rule out the possibility that politicians support their savings bank in order to promote the general economic development within their municipality, we compare different measures of macroeconomic performance between banks obtaining support from the association and politicians. We cannot detect any positive long-run effects in municipalities whose savings banks obtained support from politicians.

These findings are surprising since politicians tend to be members of the banks' supervisory boards and—therefore—have local knowledge about the distressed banks. If politicians would take advantage of their local knowledge, we should observe no statistical relationship between political/ideological factors and public capital injections. Our paper contributes to the debate about the proximity of banks and politicians/regulators that decide on recapitalization in case of distress. While local politicians have the advantage of local knowledge, decision makers with a larger distance to the bank have to rely on broader perspective. Nevertheless, we show that local politicians' decisions are influenced by political factors and ideology. Thus, our papers illustrates the advantages of larger distance and broader perspective in designing an effective regulatory banking supervision. This is particularly important given the current discussion on a unified European banking supervision. Our results suggest that such a regulatory design could have considerable advantages.

A4 Appendix to Chapter 4

Description of Bundesbank data sources

The Bundesbank's prudential data base (BAKIS): This database (for which the German Banking Act forms the legal basis) contains micro data on German banks which is available from the 1990s on and used for both supervisory monitoring of financial institutions and research purposes. These data contain sensitive and confidential supervisory information and, therefore, can only be used at the Bundesbank premises and the results may be published only after a thorough anonymization of the data.[39] From the BAKIS data base we obtain bank balance sheet data to construct control variables for our regression analysis. More importantly, we also get access to the "Sonderdatenkatalog 1" which is a special dataset containing confidential information which banks are legally bound to report to Bundesbank and BaFin and, amongst others, allow us to identify capital support measures savings banks received from the association.

The monthly balance sheet statistics (BISTA): This data base gives a comprehensive overview on German financial institutions' business activities. Hereby, banks are legally bound to report their balance sheet data on a monthly and highly disaggregated basis. For our project a major challenge was to access historical BISTA data which allows us to identify the size of the capital injection as well as the particular month this event occurred. Moreover, the BISTA database also provides us with information on each bank's lending to municipalities (which is used to identify further motives behind bank bailouts).

The quarterly borrowers' statistics: This database contains domestic loan portfolio exposures and write-off data on the bank-portfolio level (i.e., lending to the German real sector can be identified for 24 corporate and 3 retail portfolios per bank). Loan exposure data is available from the early 1990s on while data on write-offs can be accessed from 2002-2010. In our empirical study data from the borrowers' statistics is used to double-check the information on the timing of bailout

[39] For a detailed description of the BAKIS data base see, for example, Memmel, C. and I. Stein (2008), "The Deutsche Bundesbank's Prudential Database (BAKIS)", in: Schmollers Jahrbuch 128, Duncker & Humblot, Berlin, pages 321-328.

events, in particular by the banking association, for roughly half of the time-period of our dataset. For the period before 2002 we have to rely on the evolution of the capital adequacy ratio in order to identify the timing of the distress event within a year.

The Bundesbank's distress data base: This database contains information on distress events which occurred at German financial institutions from the early 1990s on. For our analysis we rely on information on so-called "distressed mergers"; that is, we need to distinguish distressed (or restructuring) mergers from pure "economy of scale mergers". As the distress database is only available until 2006, we define a distressed merger in the years 2007-2010 as a passive merger where the bank that was taken over experienced a severe distress event (i.e., a moratorium, a capital support measure, or a very low capital ratio) in the three year before the merger.

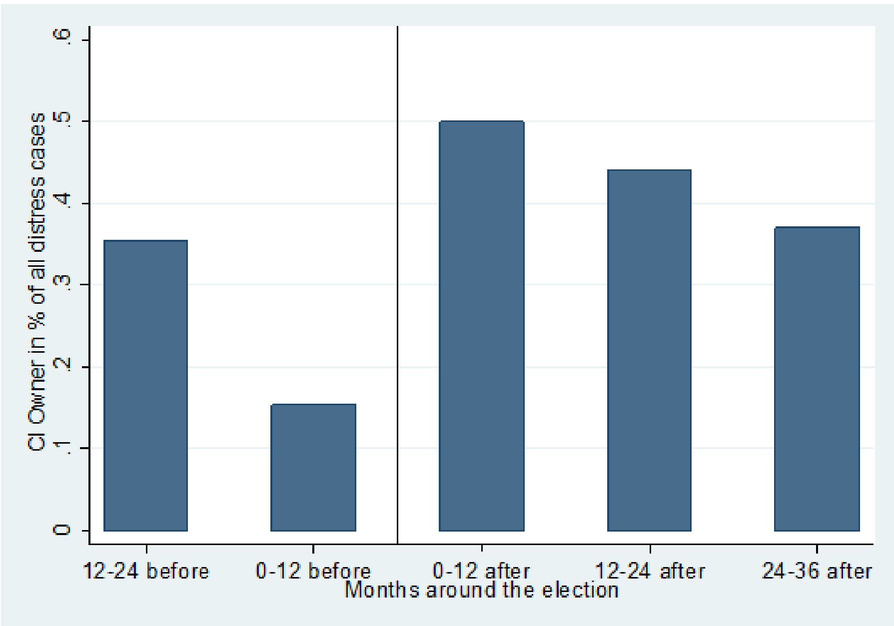


Figure 4.5: CI from owner and electoral cycle (in % of all distress events)

Figure 4.5 illustrates how the number of banks that receive capital injections from the owner varies over the electoral cycle, where the vertical black line indicates the election date.

Table 4.9: Variable definitions

Panel A: Events	
Support from owner	Capital injections from the bank owner are identified by an increase in a bank's subscribed capital that cannot be explained by takeovers or restructuring of equity positions (so called "stille Einlage"). Note that for historical reasons, the equity capital of savings banks usually consists solely of contingency funds (so called "Sicherheitsrücklage"). These funds were originally provided by the owner of the bank in the year of foundation and then cumulated over the years out of the bank's retained earnings. However, if the savings bank—besides its equity in the contingency funds—also has subscribed capital unequal to zero, then this usually indicates an undisclosed participation of the bank owner.
Support from association	
... capital support	Capital injections or guarantees from the association, obtained from "Sonderdatenkatalog 1" of the Bundesbank BAKIS database
... distressed merger	Information on distressed mergers is taken from the Bundesbank distress data base. As this database is only available until 2006, we define a distressed merger in the years 2007-2010 as a passive merger where the bank that was taken over experienced a severe distress event in the three years before the merger (i.e., a moratorium, a capital support measure, or a very low capital ratio).
Panel B: Bank Variables	
Control Variables	
Total Bank Assets	Total assets (in Mio. EUR)
Log Bank Assets	Logarithm (ln) of total assets
Total Assets / GDP	Total assets to GDP ratio (county level, in %)
Capital Ratio	Equity capital to total assets ratio (in %)
ROA	Return (operative result) on total assets (in %)
NPL Ratio	Non-performing loans to customer loans ratio (in %)
Market Share (in %)	Share of bank branches in the respective county where very small branches (e.g., branches from the Deutsche Postbank) are excluded. Note that until 2004 banks are legally bound to report the exact location of each of their branches to the Deutsche Bundesbank; from 2005 on the share of branches can be proxied from banks' voluntary reporting and from cross-sectional information.
Deposit Ratio	Savings deposits, term deposits, and time deposits to total assets ratio (in %)
Loans to Owner / GDP	Claims against municipal governments to GDP ratio (county level, in %)
Conditional on Distress	
Bank Chairman in Ass. Board	Dummy = 1 if the chairman of the bank in distress is also a member of the board of the association.
Restructuring Variables	
Growth Rate (Employees)	Year-on-year change of number of bank employees (growth rate)
Growth Rate (Number of Branches)	Year-on-year change of number of bank branches (growth rate, available until 2004)
Growth Rate (Customer Loans)	Year-on-year change of customer loans to total assets ratio (growth rate)
Growth Rate (Pers. Expenditures)	Year-on-year change of personnel expenditures (growth rate)
Loan Loss Provisions / Customer Loans	Loan loss provisions to customer loans (in %)

Table 4.9 continued...

Panel C: Macro & Other Variables	
GDPPC Growth	Year-on-year change of real GDP per capita (county level, in %)
Log(GDPPC)	Logarithm (ln) of real GDP per capita (county level)
Government Debt / GDP	Municipal government debt to GDP (county level, in %)
Restructuring Variables	
State Bank Loan Share	Share of loans in the German credit register that is granted by state banks in a given year
Loans to GDP	Loans in the German credit register aggregated at the county level and divided by county-level GDP
Loans to Private Corporate Sector to GDP	Loans in the German credit register to private companies aggregated at the county level and divided by county-level GDP
Private Capital Expenditures to GDP	Capital expenditures by companies in the manufacturing sector aggregated at the county level and divided by county-level GDP
Real GDP Growth	Year-on-year change in real GDP (county level, in %)
Share of Employees in Population	Ratio of employees to total inhabitants (county level)
Panel D: Political Variables	
D(12-24 months before)	Dummy = 1 if the last county/city elections took place 12-24 months before the distress event.
D(0-12 months before)	Dummy = 1 if the last county/city elections will take place 0 to 12 months before the distress event.
D(0-12 months after)	Dummy = 1 if the last county/city elections took place 0 to 12 months after the distress event.
D(12-24 months after)	Dummy = 1 if the last county/city elections took place 12-24 months after the distress event.
D(24-36 months after)	Dummy = 1 if the last county/city elections took place 24-36 months after the distress event.
No Competitive County	Dummy = 0 for a non-competitive county.
Competitive County	Dummy = 1 for competitive counties. Hereby, the vote share margin between the first and the second party within the county from the respective state election is calculated. Then the dummy is defined as equal to one if the vote share margin is smaller than the median and zero otherwise. This taken as a proxy for political competition within the county/city: The smaller the vote share margin between the first and the second party, the more intense the political competition and the more effective the disciplining role voters can exert on politicians.
No Conservative Bank Chairman	Dummy = 0 for a non-conservative chairman.
Conservative Bank Chairman	Dummy = 1 if the chairman of the savings bank's supervisory board is a member of a conservative party (i.e., "CDU" or "CSU").

The table shows a description of the variables we use in the empirical analysis.

Table 4.10: Event type—logit models

	Dependent Variable: Event Type				
	(1)	(2)	(3)	(4)	(5)
Total Assets / GDP (t-1)	-0.755**	-1.093***	-0.707**	-1.058***	-1.243**
	(0.299)	(0.262)	(0.337)	(0.309)	(0.595)
Capital Ratio (t-1)	-0.248	-0.334	-0.190	-0.326	-0.705
	(0.182)	(0.251)	(0.184)	(0.278)	(0.524)
ROA (t-1)	0.353	0.458	0.237	0.411	-0.215
	(0.420)	(0.357)	(0.458)	(0.407)	(0.669)
NPL Ratio (t-1)	-0.149*	-0.154*	-0.154*	-0.154*	-0.237**
	(0.078)	(0.093)	(0.080)	(0.089)	(0.116)
Market Share (t-1)	0.051***	0.062***	0.051***	0.060***	0.067*
	(0.016)	(0.018)	(0.018)	(0.018)	(0.035)
Deposit Ratio (t-1)	-0.038*	-0.044*	-0.028	-0.032	0.001
	(0.023)	(0.025)	(0.026)	(0.027)	(0.038)
GDPPC Growth (t-1)	-0.109*	-0.130*	-0.111*	-0.135*	-0.139*
	(0.060)	(0.068)	(0.060)	(0.069)	(0.079)
Log(GDPPC) (t-1)	0.179	0.186	-0.217	-0.290	0.272
	(0.552)	(0.676)	(0.584)	(0.749)	(0.865)
D(0-12 months after)		2.191***		2.381***	2.614*
		(0.701)		(0.768)	(1.381)
D(12-24 months after)		2.753***		2.818***	3.571**
		(0.696)		(0.743)	(1.461)
D(24-36 months after)		1.976**		2.015**	2.804*
		(0.781)		(0.978)	(1.526)
D(12-24 months before)		2.361**		2.583**	3.551
		(1.105)		(1.245)	(2.273)
Competitive County			-0.846**	-0.752*	-1.887**
			(0.401)	(0.430)	(0.752)
Cons. Bank Chairman			-0.950***	-1.140***	-1.132**
			(0.360)	(0.440)	(0.465)
Time Dummies	YES	YES	YES	YES	YES
Association Dummies	NO	NO	NO	NO	YES
Observations	148	148	148	148	148
Pseudo R-Squared	0.209	0.283	0.244	0.318	0.492

The table re-estimates the results from Table 4.4, using a nonlinear logit specification instead of the OLS specification. As before, the dependent variable $Event\ Type_{ijkt}$ equals one if the bank received capital injections from the owner and zero if the bank received support measures from the association. All columns include time dummies for the four election cycles in our sample (1994-1998, 1999-2003, 2004-2008, 2009-end of sample), and column 5 additionally includes a set of dummy variables that indicate the association of the bank. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

Table 4.11: Long-run performance—alternative horizon

	Capital Ratio				NPL Ratio			
	(1) OLS	(2) IV	(3) IV	(4) IV	(5) OLS	(6) IV	(7) IV	(8) IV
Owner	-0.367***	-0.621**	-0.886***	-0.965***	4.155***	2.935*	8.368***	7.530***
	(0.133)	(0.266)	(0.299)	(0.301)	(0.857)	(1.767)	(2.193)	(2.141)
Constant	0.590***	0.709***			-3.238***	-2.667***		
	(0.091)	(0.142)			(0.586)	(0.932)		
Association Dummies	NO	NO	YES	YES	NO	NO	YES	YES
Time Dummies	NO	NO	NO	YES	NO	NO	NO	YES
Observations	66	66	66	66	64	64	64	64
R-squared	0.107	0.055	0.208	0.267	0.275	0.251	0.300	0.357

	LLP to CL				ROA			
	(9) OLS	(10) IV	(11) IV	(12) IV	(13) OLS	(14) IV	(15) IV	(16) IV
Owner	0.621***	0.903***	0.593	0.448	-0.299**	-0.489*	-0.540	-0.428
	(0.170)	(0.343)	(0.404)	(0.403)	(0.146)	(0.292)	(0.364)	(0.368)
Constant	-0.698***	-0.832***			0.271***	0.362**		
	(0.117)	(0.184)			(0.101)	(0.157)		
Association Dummies	NO	NO	YES	YES	NO	NO	YES	YES
Time Dummies	NO	NO	NO	YES	NO	NO	NO	YES
Observations	65	65	65	65	65	65	65	65
R-squared	0.175	0.139	0.265	0.321	0.063	0.037	0.084	0.127

The table shows robustness checks for the estimations presented in Table 4.7. In particular, we use four-year changes in the respective variables instead of five-year changes. As before, we restrict the sample to banks without a potential partner for a distressed merger to account for selection bias. Columns 1, 5, 9, and 13 report results for simple OLS regressions, where *Owner* is a dummy equal to one if the bank received capital injections from the owner and equal to zero if it received support from the association. The remaining columns show results for two-stage least squares regressions. In the first stage, we regress the dummy variable *Owner* on the political variables from above (dummies for the electoral cycle, competitive counties, and conservative bank chairmen), and the additional dummy variables specified at the bottom of the table. In the second change, predicted probabilities from the first stage are used to predict the five-year change in the respective variable. Again, we include the additional dummy variables denoted at the bottom of the table. * indicates statistical significance at the 10 %-level, ** at the 5 %-level, and *** at the 1 %-level.

Chapter 5

Does Financial Structure Shape Industry Structure? Evidence from Timing of Bank Liberalization

5.1 Introduction

It has been well accepted that financial systems can influence the allocation of credit and shape an economy's growth path.[1] What is not as well understood is how the nature of credit provision affects this allocation process.[2] One reason for our lack of understanding is the inherent difficulty in identifying significant changes in the nature of credit provision within an economy. This paper exploits variation in the efficiency of the domestic banking sector at the time of liberalization to identify large changes in the nature of credit provision, which have a significant impact on the allocation of credit in the liberalizing countries. These changes in credit allocation shape countries' industry structure by influencing the financial structure of firms—ultimately affecting growth paths of industries in the economy.

[1]One potential channel is that lenders and intermediaries screen out bad projects (Bagehot 1873, Schumpeter 1912, Diamond 1984, Boyd and Prescott 1986). Another theory argues that pressures from external financiers encourage managers to pursue value-maximizing investment policies (Jensen 1986). See also Wurgler (2000).

[2]This issue is best illustrated by the recent vociferous debate surrounding the bailout of banks. Proponents argue that the allocation of credit would be hampered if old banks were replaced by new ones since old banks have relationships with firms—especially smaller entrepreneurial firms—which could not simply be substituted by new credit providers. Critics argue that new banks would be able to allocate credit as well as their older counterparts.

We draw insights from the industrial organization and trade literature and argue that foreign bank entry will interact differentially with the domestic banking sector, depending on the efficiency of the domestic banking sector at the time of liberalization.[3] The rationale is simple: Foreign bank entry induces competitive pressure, which is likely to be better absorbed by domestic banks that operate close to the efficiency frontier. These banks are likely to invest and innovate in response to the competitive threat—for example by improving their technology and processes—while such a change might be harder for banks further away from the frontier. Consequently, we would expect considerably larger changes in the nature of credit provision in countries that liberalize when most domestic banks are far away from the efficiency frontier as compared with countries that liberalize when most domestic banks are able to compete with foreign entrants. Specifically, the market share of foreign banks should dramatically increase in countries with a weak domestic banking sector, while the effect should be more moderate in countries with a better developed domestic sector.

There are several reasons why one would expect that changes in the nature of credit provision have an impact on the allocation of credit within the economy. First, if foreign banks are more efficient in providing capital, they would intermediate capital to sectors in the economy that could have been rationed out by inefficient domestic banks. Moreover, it is also possible that the arm's length nature of foreign bank financing plays a role. In particular, domestic banks might be better at screening soft information borrowers (e.g., local entrepreneurial firms; see Berger, Klapper, and Udell 2001 and Mian 2006), while foreign banks may have comparative advantages

[3]A recent and growing trade literature documents effects of a reduction in trade barriers on product and labor markets. Aghion et al. (2003, 2008) document a heterogeneous response of firms' efficiency and productivity following the elimination of entrance barriers to foreign firms. They argue that technologically advanced firms often gain in efficiency following foreign entrance since they are more likely to respond to the threat of entry by investing in new technologies. However, firms that are far from the frontier are adversely affected by the entrance threat. Consequently, opening of product markets amplifies initial differences in productivity. This theoretical argument is supported by empirical findings. Aghion et al. (2003) (for India), Aghion and Bessonova (2006) (for Russia) and Aghion et al. (2009) (for the UK) find that a removal of restrictions on foreign firm entry has a more positive effect on the economic performance for domestic firms and industries that are initially closer to the technology frontier. In a related paper, Sabirianova, Terrell, and Svejnar (2005) show that the more foreign firms enter a market, the higher the productivity gap between foreign and domestic firms. In a theoretical paper, Lehner and Schnitzer (2008) study how spillover effects from foreign bank entry depend on competition in the domestic banking sector. They find that an increase in the number of banks is more likely to have positive welfare effects the more competitive the domestic banking sector.

in screening hard information borrowers (e.g., larger firms). This could potentially shape the allocation of credit to firms within an industry and across industries.

Using information from Abiad and Mody (2005) and Bekaert and Harvey (2004) on liberalization events in 26 emerging markets, this paper examines how the removal of entry barriers for foreign banks affects the nature of credit provision, and how this effect depends on the efficiency of the domestic banking sector at the time of liberalization. We obtain a comprehensive data set from Bureau van Dijk's Bankscope data base, covering banking sectors for our sample countries for the period from 1995 to 2007. Using bank-level data, we find that, following liberalization, banks operating close to the efficiency frontier increase their lending relative to banks further away from the frontier, where bank efficiency is based on the bank's profitability, the cost-to-income ratio, or the non-performing loans ratio prior to the event.

We aggregate the bank efficiency variable on the country level to obtain our key measure, the share of efficient domestic banks at liberalization. We use this measure to split our sample countries into those with a lower than median and those with a higher than median share of efficient domestic banks at liberalization. Aggregating the Bankscope data on the country level, we find an increase in the aggregate supply of credit and a moderate increase in the market share of foreign banks in countries with relatively efficient domestic banks. In contrast, in markets with relatively inefficient banks, foreign lending crowds out domestic lending, resulting in an aggregate loan supply that is lower than before.

We proceed by showing that this differential change in the nature of credit provision across economies shapes the financial structure of firms and countries' overall industry structure. In particular, we find that smaller firms are negatively affected in countries with less efficient domestic banks at the time of liberalization. This result can be rationalized easily: Since smaller firms are likely to be the ones where soft information is relatively important, they are particularly harmed by the decline in domestic lending in these economies after banking markets are opened up. In contrast, small firms are better off in economies with an efficient domestic banking sector at the time of liberalization.[4] In addition, at the industry level, we find that

[4] Beck et al. (2008) argue that small firms are relatively opaque and hence benefit more from the reduction of informational problems that comes along with more efficient financial intermediation.

those industries that are more reliant on external finance benefit from liberalization, irrespective of domestic banks' inherent efficient. Thus, changes in the nature of credit provision shape the structure of financing within an industry and across industries.

Finally, we also evaluate how changes in the nature of credit provision affect economic growth. We find that, following liberalization, there is a higher growth rate and lower growth volatility for industry sectors in markets with relatively more efficient domestic banks. The growth effect is driven by industries with a large share of small and medium enterprises. In markets with relatively efficient domestic banks such industries accelerate in growth following liberalization, while we find the opposite for markets with relatively inefficient banks.

It is important for our analysis that the events in our sample countries are exogenous in the sense that they are not systematically related to countries' future growth prospects or the occurrence of banking sector crises. We are confident that endogeneity is not an issue for three reasons: First, we use within-country, cross-sectional variation at the bank level, at the industry level, and at the firm level, and document differential effects of liberalization on banks, industries, and firms within the same country. Hence, our identification strategy mitigates endogeneity concerns. Second, we study the dynamic effects of liberalization in our sample countries and find that key variables change after, not before the event. Finally, political processes and external pressures applied by the IMF or the World Bank in many of our sample countries also help to mitigate concerns regarding endogeneity.

As mentioned before, we assess the robustness of our results by using various definitions for initial banking sector efficiency. Further, as the entry mode chosen by foreign banks could be another important determinant of the outcome of banking sector liberalization, we show that the efficiency effect is present both in countries where foreign banks entered mostly via greenfield investments and in countries where they entered by taking over domestic banks. As we show that the reaction of remaining domestic banks depends on their initial efficiency, the selection of banks that were taken over could be an issue. We show that this selection does not depend on banks' initial efficiency, hence mitigating this concern. Finally, we control for other events that might affect lending in our sample countries, like changes in creditor rights or current account liberalization.

Our results underscore the importance of domestic institutions in fostering growth. The main message that comes out from our results is that domestic institutions need to be developed to a reasonable degree for financial liberalization to have a positive impact. In other words, we highlight the importance of the timing of liberalization by showing that it has a direct effect on the structure of lending within an economy, and that this has real effects on growth and industry structure.

The remainder of the paper is organized as follows: In Section 5.2 we describe the bank liberalization reforms in emerging markets that constitute our event as well as our underlying data sources. Section 5.3 illustrates the consequences of our event for loan supply and financial structure (foreign vs. domestic banks) in our sample countries. We investigate how the documented changes in financial structure affect economic outcomes and industry structure in Section 5.4 and confirm our results on the firm-level in Section 5.5. In Section 5.6 we provide some additional robustness checks, before we conclude in Section 5.7.

5.2 Liberalization reforms and data

5.2.1 The event: Bank liberalization reforms across the world

During the last two decades many emerging markets throughout the world have opened their banking sectors to foreigners as part of a broader process of financial liberalization.[5] The Washington Consensus—actively promoted by the IMF and the World Bank—pushed for the elimination of all entry barriers and state involvement (World Bank 2002). This view is based on the classical Shaw (1973) – McKinnon (1973) framework in which the opening of financial markets increases the efficiency of financial intermediation. Moreover, foreign banks are expected to import capital, to stimulate competition, to introduce new technologies, and to import better supervision and regulation from their home countries (Levine 1996). In line with this view, many countries removed entry barriers and consequently saw a sharp increase

[5]See, e.g., Williamson and Mahar (1998), Kaminsky and Schmukler (2008) or Abiad, Detragiache, and Tressel (2010) for a documentation of liberalization reforms across the world in recent decades.

in foreign bank ownership.[6]

Nevertheless, there is disagreement about the appropriate market opening strategy. Some policy makers fear financial fragility following liberalization since several countries experienced banking sector crises shortly after the financial sector was deregulated (Demirgüç-Kunt and Detragiache 1999, Kaminsky and Reinhart 1999 or Weller 2001). The debate about the advantages and risks of liberalization resulted in different market opening strategies between different emerging markets. Some countries opted for the removal of all entry barriers for foreign banks as suggested by, e.g., Sachs and Warner (1996).[7] In Hungary, for example, the majority of the banking sector was basically sold overnight to foreign-owned banks (Kiraly et al. 2000). Other countries were more reluctant to open their markets. In these countries, foreign entrance and operations remain restricted by government regulations.[8]

We collect information on banking market opening policies for a large sample of emerging market economies that had not yet fully liberalized their banking markets at the onset of our sample period in 1995. Abiad and Mody (2005) provide an indicator described below that codes changes in entry restrictions for foreign banks. The Abiad and Mody (2005) database provides information on countries from several regions, but does not include Eastern European economies. These countries are, however, of special interest, since they opened their banking markets during the 1990s in the quest to join the European Union. Therefore, we construct the Abiad and Mody (2005) indicator on foreign entrance restrictions for these countries based on data from the Bekaert and Harvey (2004) database on important financial, economic and political events in emerging markets. Specifically, Abiad and Mody (2005) construct an index that is concerned with restrictions on foreign bank entry that,[9]

[6] Claessens et al. (2008) document for a sample of 103 developing countries an increase in foreign ownership from 23% in 1995 to 38% in 2006.

[7] Sachs and Warner (1996) argue that high product market competition through liberalization fosters allocative efficiency which in turn promotes economic growth.

[8] For reviews about experiences with the removal of entry barriers see Barros et al. (2005) and Coricelli (2001).

[9] Abiad and Mody (2005) use information on seven different dimensions of financial sector policy to calculate an index of financial reform. The subindex on entry barriers incorporates information on four dimensions: Restrictions on foreign bank entry, restrictions on domestic bank entry, restrictions on branching, and restrictions on banking activities. The first of these dimensions exactly matches our variable of interest, hence we take over the coding of our event variable from this sub-subindex.

- is coded as 0 when no entry of foreign banks is allowed or tight restrictions on the opening of new foreign banks are in place;
- is coded as 1 when foreign bank entry is allowed, but nonresidents must hold less than 50 percent equity share;
- is coded as 2 when the majority of share of equity ownership of domestic banks by nonresidents is allowed or equal treatment is ensured for both foreign banks and domestic banks or an unlimited number of branching is allowed for foreign banks.

Overall, we obtain information on the market opening strategies of 26 emerging economies.[10] We use central bank sources in order to double check information on all liberalization reforms.[11] Of these 26 countries, 22 reduced restrictions on foreign bank entry during our sample period. The remaining four countries did not reduce restrictions and remain only partially liberalized during our whole sample period. Countries are located in Central and Eastern Europe, Asia, Latin America and Africa.

An overview of our sample countries and their respective reform years can be found in Panel A of Table 5.1. The table also contains a short description of the reforms themselves. Many countries eliminated limitations on foreign ownership in the banking sector, while others like Indonesia or Taiwan also made it easier for foreign banks to open branches. Some countries like Guatemala or Costa Rica took a more gradual approach and liberalized only partly. All in all, we have a diverse set of reforms that captures the different facets of bank liberalization. In Figure 5.1 the development of the foreign market share aligned around the respective reform year of our sample countries is plotted.[12] Many countries were already partially liberalized

[10]We include all countries not yet fully liberalized at the onset of our sample period in 1995 for which we are able to obtain the coding of the event variable. For 17 of our 26 sample countries liberalization information is taken from the Abiad and Mody (2005) database and for the remaining 9 countries from the Bekaert and Harvey (2004) database.

[11]Overall, the quality of the two databases is very good. We make only two minor corrections: In Mexico, restrictions on foreign bank entry were removed in 1997, two years earlier than reported in the Abiad/Mody database (Hernández-Murillo 2007). For Bulgaria, the event definition is not unambiguously clear from the two databases. We consult a paper by Miller and Petranov (2001) to obtain the correct date of liberalization.

[12]The foreign market share is defined as the share of total bank assets owned by foreign banks within the respective country.

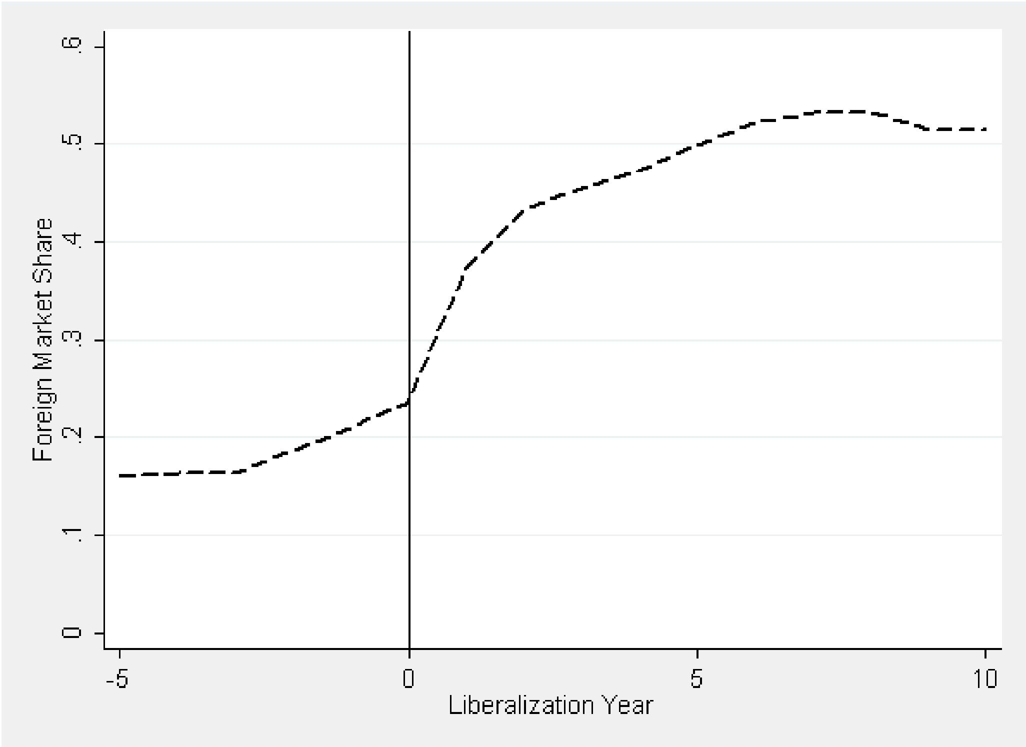


Figure 5.1: Impact of liberalization on financial structure

The figure align countries around their respective liberalization event (inidcated by the vertical line) and shows the development of foreign banks' market share in the average sample country.

at the onset of our sample period, so that the average market share of foreign banks five years before liberalization was about 18 %.[13] However, foreign bank operations were still restricted and highly regulated in these countries. The figure shows that the foreign market share rose to about 50 % five years after liberalization, which illustrates that liberalization had a significant impact in our sample countries.

5.2.2 Bank data

We obtain bank balance sheet data and time series information on bank ownership from Bureau van Dijk's Bankscope database. This database contains detailed information on up to 30,000 banks and goes back until the early 1990's. A problem with the database is that each version covers only the most recent years. To gather data on the earlier years, we merge information from the 2011 internet version of

[13]Foreign bank presence prior to liberalization can also be explained by historical reasons in some of our sample countries. For example, in Mexico, foreign banks were allowed to enter before restrictions were put in place in 1982. These restrictions were removed again in the 1990s (see Hernández-Murillo 2007).

Table 5.1: Descriptive statistics

Panel A: Sample countries and liberalization events

	Event Year	Index Value	Description	Source
Bolivia	1998	1→2	limitation of voting power of majority shareholders of foreign banks eliminated	Abiad/Mody
Brazil	—	1	—	Abiad/Mody
Bulgaria	1998	1→2	privatization process of former state-owned banks with particular focus on foreign investors	Miller/Petranov
Costa Rica	1999	0→1	establishment of fully-owned foreign subsidiaries allowed; establishment of branches of foreign banks still forbidden	Abiad/Mody
Czech Republic	1999	1→2	privatization process of government banks; large stakes sold to foreign banks	Bekaert/Harvey
Egypt	1996	1→2	49% limit on foreign investors' share in joint-venture banks removed	Abiad/Mody
Estonia	1999	1→2	consolidation of the banking market, large stakes of former state-owned banks sold to foreign investors	Bekaert/Harvey
Ghana	1999	1→2	majority foreign ownership of banks allowed	Abiad/Mody
Guatemala	2004	0→1	foreign banks allowed to establish local subsidiaries subject to the conditions of the Monetary Board	Abiad/Mody
Hungary	1996	1→2	foreign investors in the banking sector granted treatment equivalent to that given domestic financial institutions	Bekaert/Harvey
India	—	1	—	Abiad/Mody
Indonesia	1998	1→2	controls on foreign bank branching lifted, up to 85% equity participation allowed	Abiad/Mody
South Korea	1998	1→2	banks being restructured exempted from foreign ownership restrictions	Abiad/Mody
Lithuania	1999	1→2	removal of restrictions on foreign investment	Bekaert/Harvey
Malaysia	—	1	—	Abiad/Mody
Mexico	1997	1→2	removal of all restrictions on foreign bank entry	Hernández-Murillo
Nepal	2001	1→2	limit on foreign ownership of banks increased from 49% to 66%	Abiad/Mody
Pakistan	—	1	—	Abiad/Mody
Poland	1999	1→2	national treatment for financial institutions from OECD member countries; removal of restrictions on purchase of bigger stock blocks by foreign investors	Bekaert/Harvey
Romania	1999	1→2	all credit operations extended by nonresidents to residents with a maturity exceeding one year were liberalized; privatization of former state-owned banks with focus on foreign investors	Bekaert/Harvey
Singapore	1999	1→2	40% limit on foreign investors total shareholding in local banks lifted	Abiad/Mody
Slovakia	2000	1→2	branches of foreign financial institutions allowed to acquire real estate to operate their business	Bekaert/Harvey
Slovenia	2000	1→2	relaxation of rules on foreign investment; foreign banks allowed to open branches	Bekaert/Harvey
Taiwan	2003	1→2	banking restrictions relaxed to make it easier for foreign banks to set up branches; foreign banks are accorded national treatment to enable them to compete with domestic banks on an equal footing	Abiad/Mody
Thailand	1997	1→2	limits of 25 % of equity participation of banks by nonresidents raised to 100% based on case-by-case approach of the MOF's approval	Abiad/Mody
Zimbabwe	2000,2002	0→1→2	entry criteria objectified by the Banking Act amendment	Abiad/Mody

Table 5.1 continued...

Panel B: Bankscope data

Country	Banks	Observations	Total assets	Total loans	Market share	ROA	$\frac{\text{Equity}}{\text{Total assets}}$	$\frac{\text{Liquid assets}}{\text{Total assets}}$
Bolivia	10	90	348	221	0.098	0.22	0.11	0.16
Brazil	78	581	4,086	1,381	0.014	2.13	0.34	0.22
Bulgaria	9	76	504	261	0.017	1.86	0.44	0.17
Costa Rica	25	182	311	174	0.054	2.34	0.16	0.21
Czech Republic	15	85	3,755	1,604	0.050	-0.28	0.36	0.10
Egypt	19	201	2,945	1,292	0.053	1.11	0.32	0.09
Estonia	7	27	1,964	1,431	0.072	1.58	0.26	0.13
Ghana	6	56	169	68	0.079	3.36	0.44	0.14
Guatemala	22	251	283	140	0.026	-0.64	0.18	0.10
Hungary	7	53	2,106	1,257	0.080	0.87	0.3	0.13
India	63	740	5,448	2,620	0.017	0.83	0.19	0.07
Indonesia	61	336	1,820	841	0.021	-0.25	0.31	0.11
Korea Rep. of	29	226	30,695	19,260	0.047	0.01	0.11	0.05
Lithuania	6	40	949	621	0.067	-0.30	0.3	0.14
Malaysia	25	200	6,369	3,850	0.050	1.09	0.27	0.10
Mexico	14	87	4,871	2,888	0.027	0.23	0.28	0.22
Nepal	7	78	168	79	0.092	1.39	0.27	0.07
Pakistan	17	192	1,723	848	0.048	0.60	0.26	0.09
Poland	17	124	2,800	1,376	0.024	1.25	0.22	0.14
Romania	8	56	1,227	605	0.063	0.63	0.43	0.17
Singapore	16	119	14,851	7,368	0.103	1.40	0.24	0.20
Slovakia	6	27	1,906	829	0.019	0.37	0.37	0.10
Slovenia	15	148	1,321	738	0.066	1.01	0.25	0.13
Taiwan	44	487	17,080	9,672	0.023	0.38	0.15	0.10
Thailand	9	90	9,763	6,531	0.093	-0.92	0.13	0.11
Zimbabwe	7	52	663	225	0.080	7.12	0.25	0.11

Table 5.1 continued...

Panel C: Macro data & efficiency estimates

Country	Initial ROA	d(initial ROA)	Initial cost to income ratio	Initial NPL ratio	Herfindahl index	Log(inflation)	GDP growth
Bolivia	0.00	0	0.66	0.05	0.13	1.75	3.62
Brazil	0.22	1	0.76	0.10	0.08	2.23	2.97
Bulgaria	0.36	1	0.69	0.27	0.14	2.78	3.47
Costa Rica	0.06	1	0.77	0.03	0.17	2.59	5.07
Czech Republic	0.01	0	0.67	0.22	0.16	1.57	3.47
Egypt	0.08	1	0.54	0.14	0.14	1.86	4.79
Estonia	0.95	1	0.81	0.02	0.46	1.97	7.32
Ghana	1.00	1	0.38	0.13	0.20	3.07	4.91
Guatemala	0.04	0	0.73	0.07	0.09	2.13	3.97
Hungary	0.03	0	0.61	0.05	0.14	2.34	3.76
India	0.02	0	0.60	0.14	0.08	1.92	6.99
Indonesia	0.01	0	0.88	0.49	0.10	2.43	3.76
Korea Rep. of	0.00	0	1.13	0.07	0.10	1.48	4.98
Lithuania	0.06	1	0.76	0.13	0.25	0.91	6.34
Malaysia	0.00	0	0.37	0.19	0.10	1.26	5.46
Mexico	0.08	1	0.73	0.11	0.16	2.34	2.97
Nepal	0.50	1	0.37	—	0.17	1.87	3.94
Pakistan	0.02	0	1.40	0.20	0.14	2.01	4.48
Poland	0.07	1	0.66	0.13	0.10	1.90	4.81
Romania	0.51	1	0.46	0.08	0.19	3.25	3.36
Singapore	0.00	0	0.35	0.05	0.24	0.64	6.57
Slovakia	0.00	0	0.70	0.14	0.17	1.99	5.31
Slovenia	0.05	1	0.53	0.07	0.19	1.98	4.35
Taiwan	0.01	0	—	—	0.06	0.74	4.79
Thailand	0.02	0	0.49	0.43	0.13	1.39	3.70
Zimbabwe	0.17	1	0.32	—	0.26	4.98	-2.65

Table 5.1 continued...

Panel D: List of ISIC Rev. 3 industries and corresponding characteristics

ISIC	Sector	External Dependence	SME Share	ISIC	Sector	External Dependence	SME Share
151	Meat, fish, fruits, vegetables	0.14	0.0382	261	Glass and glass products	0.53	0.0505
152	Dairy products	0.14	0.0382	269	Non-metallic mineral products	0.06	0.1417
153	Grain mill products, starches, animal feeds	0.14	0.0382	271	Basic iron and steel	0.09	0.0162
154	Other food products	0.14	0.0382	272	Basic non-ferrous metals	0.01	0.0476
155	Beverages	0.08	0.0404	281	Structured metal products	0.24	0.0998
160	Tobacco products	-0.45	0.0030	289	Other metal products	0.24	0.0998
171	Spinning, weaving, finishing of textiles	0.4	0.0281	291	General purpose machinery	0.45	0.1368
172	Other textiles	0.4	0.0281	292	Special purpose machinery	0.45	0.1368
173	Knitted and crocheted fabrics	0.4	0.0281	300	Office, accounting, computing machinery	1.06	0.0285
181	Wearing apparel except fur	0.03	0.0818	311	Electric motors, generators, transformers	0.77	0.0344
182	Dressing and dyeing of fur	0.03	0.0818	312	Electricity distribution and control apparatus	0.77	0.0344
191	Tanning, dressing and processing of leather	-0.14	0.1045	313	Insulated wire and cable	0.77	0.0344
192	Footwear	-0.08	0.0161	314	Accumulators, primary cells, batteries	0.77	0.0344
201	Saw milling and planing of wood	0.28	0.2137	315	Lighting equipment, electric lamps	0.77	0.0344
202	Wood products	0.28	0.2137	319	Other electrical equipment	0.77	0.0344
210	Paper and paper products	0.18	0.0303	321	Electronic valves, tubes	1.04	0.0309
221	Publishing	0.2	0.1632	322	TV/radio transmitters	1.04	0.0309
222	Printing	0.2	0.1632	323	TV/radio receivers	1.04	0.0309
231	Coke oven products	0.33	0.0926	341	Motor vehicles	0.39	0.0228
232	Refined petroleum products	0.33	0.0926	351	Shipbuilding	0.46	0.0656
241	Basic chemicals	0.205	0.0121	359	Transport equipment	0.31	0.0121
242	Other chemicals	0.22	0.0580	361	Furniture	0.24	0.0909
251	Rubber products	0.23	0.0315	369	Other manufacturing	0.47	0.1695
252	Plastic products	1.14	0.0609		Average	0.37	0.0707

Table 5.1 continued...

Panel E: Amadeus data

Country	Period	Observations	Firms	Assets	Debt	$\frac{\text{Debt}}{\text{Assets}}$	Sales	Tangibility	$\frac{\text{Ebit}}{\text{Assets}}$	d(small)	d(young)	d(exdep)
Bulgaria	1995-2004	2,823	627	11,300	2,546	0.22	11,400	0.53	0.06	0.71	0.24	0.50
Czech Republic	1995-2005	9,788	1,926	20,900	4,042	0.20	24,900	0.47	0.07	0.40	0.72	0.66
Estonia	1995-2006	1,044	175	7,816	2,325	0.28	10,300	0.52	0.09	0.63	0.58	0.58
Hungary	1995-2007	1,516	576	30,800	3,980	0.17	198,000	0.48	0.08	—	—	0.51
Lithuania	1995-2008	706	199	7,178	2,089	0.26	8,066	0.51	0.07	0.73	0.91	0.56
Poland	1995-2009	7,554	1,919	15,200	3,788	0.24	22,200	0.50	0.08	0.43	0.25	0.51
Romania	1995-2010	5,402	1,414	9,615	1,883	0.18	8,396	0.47	0.15	0.67	0.31	0.52
Slovakia	1995-2011	1,656	392	27,500	3,994	0.18	32,500	0.49	0.05	0.28	0.57	0.52

Panel A provides information on our sample countries and the coding of our event variable. Our sample period is from 1995 to 2007. Information on liberalization events is obtained from an extended version of the financial liberalization database used by Abiad and Mody (2005), the Bekaert and Harvey (2004) database on important financial, economic and political events in emerging markets and papers by Hernandez-Murillo (2007) and Miller and Petranov (2001). The liberalization variable takes values between 0 and 2, with 0 indicating tight restrictions on foreign entry, 1 indicating partial liberalization and 2 indicating full liberalization. Four of our sample countries (Brazil, India, Malaysia and Pakistan) were partially liberalized in 1995 and had no reform during our sample period. Panel B reports mean values of balance sheet and income statement items obtained from the Bankscope database. Values are in millions of U.S. dollars. Panel C shows efficiency estimates on the country level that are calculated as follows: We first define a dummy that takes the value of 1 if the bank has—in the year prior to liberalization—an ROA in the top quartile of all banks in our sample (*efficient bank*). We then calculate the share of efficient domestic banks at liberalization as the market share (in terms of total assets) of domestic banks that are efficient according to the definition above (column 1). We divide our sample into countries with lower and countries with a higher than median value of this variable (column 2). Instead of ROA, we alternatively use the initial cost to income ratio (column 3) or the initial non-performing loans ratio (column 4) as obtained from the World Bank. The panel further provides mean values for macro control variables by country. Panel D reports two industry characteristics used in the regression analysis. Column 1 reports the aggregated value of capital expenditures minus cash flow from operations over capital expenditures for U.S. industries in the 1980s as calculated in Rajan and Zingales (1998). Column 2 shows the share of total employment of companies with less than 20 employees within U.S. industries in the early 1990s as reported by Beck et al. (2006). Panel E reports mean values for for the firm balance sheet and income statement items as obtained from the Amadeus database. Values are in thousands of U.S. dollars. We report mean values for total assets, total debt, the ratio between the two, total sales, a measure of tangibility as defined by fixed assets to total assets, the ratio of EBIT to total assets and three dummy variables: *d(small)* takes a value of 1 if the average firm size (as measured by total assets) prior to liberalization is lower than median, *d(young)* takes a value of 1 if the average firm age prior to liberalization is lower than median, and *d(exdep)* takes a value of 1 if the firm is an industry with a higher than median value of external financial dependence according to Rajan and Zingales (1998).

the database with data from older CD-ROM versions. In this way, we obtain a consistent dataset for the entire sample period from 1995 to 2007. The years 1993 and 1994 are excluded due to very thin data availability. To avoid a possible distortion of results due to other forces being at work during the global financial crisis, we also exclude the years 2008 and 2009. Careful revision of the data is necessary to avoid double counting and the inclusion of irrelevant data. We eliminate unconsolidated statements whenever both unconsolidated and consolidated statements are available for a certain bank. Moreover, we eliminate all statements of non-bank financial institutions, such as clearing institutions, central banks or securities firms.

In large parts of the paper we distinguish between domestic and foreign lending. Hence, we need to identify ownership of our sample banks. Bankscope includes detailed information on ownership, giving both name and nationality of a bank's shareholders as well as their respective shares in the bank. Banks are coded as foreign if at least 50 % of their assets are foreign owned. Unfortunately, even with different versions of the Bankscope database, ownership information is only available for the years 2000 to 2007. Hence, for the years 1995 to 1999, we consult the banks' or central banks' websites in order to check whether there was a takeover.

Our sample provides information on 842 distinct foreign and domestic banks. We aggregate loans from all banks within a given market in order to calculate country level loan supply in a given year. In the bank level regressions our focus is on domestic banks. Among the 842 banks, our sample contains 542 distinct domestic banks with 4,604 bank-year observations.[14] Panel B of Table 5.1 contains descriptive statistics of the domestic bank balance sheet information by country. It reports the number of banks per country as well as mean values for bank assets, loans, ROA, market share, a solvency measure (equity by total assets) and a liquidity measure (liquid assets by total assets). The number of domestic banks per country ranges from six in Ghana, Lithuania and Slovakia up 78 in Brazil. Importantly, the sample is not dominated by a single country: India, the country with the most bank-year observations, makes up approximately 15 % of the overall sample. Within the sample, domestic banks from Korea, Taiwan or Singapore are the largest on average, while somewhat surprisingly,

[14]Besides foreign banks, we exclude also banks that were taken over by foreign banks from the bank level analysis. If foreign entrants selected these banks based on their efficiency this could create a potential bias to our analysis. However, we show in Section 5.6 that this is not the case.

African banks are the most profitable ones.

5.2.3 Efficiency classification of banking markets and macroeconomic data

As outlined in the introduction, we want to analyze how differences in the efficiency of domestic banking markets affect outcomes of banking sector liberalization. In order to determine the efficiency of a banking sector we would ideally want to know how efficiently banks screen and monitor investment projects. Since this information is empirically not observable, we present several accounting based measures.

As a starting point, we focus on banks' profitability. We calculate the fraction of domestic banks (weighted by total assets) in a given market whose ROA is in the top quartile of all sample banks in the year before the respective market is liberalized.[15] For the four countries that did not experience a liberalization event during our sample period the measure displays the share of efficient domestic banks at the onset of our sample period in 1995.[16] The first column of Table 5.1, Panel C displays for each sample country the resulting share of efficient banks at liberalization (*Initial ROA*). There is considerable variation in this measure across our sample countries. In Eastern Europe, the Czech Republic opened its banking sector while there were basically no profitable domestic banks. In Poland a fraction of 7 percent of total banking assets was managed by relatively efficient banks. Most Asian countries opened their banking markets while having a rather inefficient domestic banking sector. Among the countries that did not open their markets, only Brazil has a considerable fraction of profitable domestic banks. Since we conduct sample splits based on the *Initial ROA* measure, we define a dummy that takes the value of one if *Initial ROA* is above the median and zero otherwise. The respective classification is displayed in column 2.

Alternatively, we use the cost to income ratio *(Initial cost to income ratio)* and the non-performing loans ratio *(Initial NPL ratio)* to obtain a measure for banking efficiency. We obtain data for the aggregate banking sectors of every sample

[15] We have used alternative cut-offs for the definition of the initial efficiency variable. Our results are robust to these changes.

[16] Zimbabwe is the only sample country that had two events. We consistently code the first event as the liberalization event. Results are unaffected by this decision.

country in the year before liberalization from the World Bank World Development Indicators.[17] The coding of these two indicators is displayed in columns 3 and 4. Throughout the main part of the paper we consistently apply the measure *Initial ROA*. In Section 5.6, we verify that our most important findings are robust to the two alternative definitions of banking sector efficiency (*Initial cost to income ratio* and *Initial NPL ratio*).

The last three columns of Table 5.1, Panel C, report macro controls obtained from the World Bank World Development Indicators. We control for banking market concentration by including the Herfindahl index of each banks' market share in a given banking market (Herfindahl index), the logarithm of the inflation rate (log(inflation)) and the annual GDP growth rate (GDP growth).

5.2.4 Industry data

To investigate whether the effects of bank liberalization are transmitted to the real economy, data on industry output is collected from UNIDO's INDSTAT4 (2011) database. This database contains time series information on 127 countries for the period 1990-2008. The measure of industry output reported in the UNIDO database is based on the census concept and covers only activities of an industrial nature.[18] The data is originally stored in national currency valued at current prices. In order to make data from different countries comparable, it is converted into current U.S. dollars using the average period exchange rates as given in the International Financial Statistics (IFS). Following Rajan and Zingales (1998), the analysis is confined to manufacturing sectors (U.S. SIC 2000-3999) in order to reduce the dependence on country-specific factors like natural resources. The UNIDO dataset is classified by ISIC Rev. 3 codes. Using three-digit industry codes as the level of analysis, a panel of up to 47 industries per country-year is obtained. The basic industry specification includes 10,520 country-industry-year observations for 1,132 distinct country-industries.

In Section 5.4, we examine whether industries that differ in certain characteris-

[17]Series GFDD.EI.07, defined as operating expenses as a share of the sum of net interest revenue and other operating income, and series GFDD.SI.02, defined as the ratio of defaulting loans (payments of interest and principal past due by 90 days or more) to total gross loans.

[18]For details on the INDSTAT data see UNIDO Statistics Unit (2011).

tics are heterogeneously affected by liberalization. Industry measures for external dependence and the share of small and medium enterprises (SMEs) are obtained from papers by Rajan and Zingales (1998) and Beck et al. (2008), respectively.[19]

Rajan and Zingales (1998) measure a firm's dependence on external finance as capital expenditures minus cash flow from operations divided by capital expenditures and aggregate data for U.S.-based publicly listed companies of the 1980's into an industry index. They argue that industries differ in their use of external finance for technological reasons that are persistent across countries. The U.S. is taken as a benchmark economy as its capital markets are among the most advanced in the world and hence there are relatively few frictions, market imperfections or policy distortions. Moreover, data for publicly traded companies is used as these are relatively large companies that are financially not constrained. Therefore the amount of external finance they use is a relatively pure measure of the demand for external finance. As Rajan and Zingales (1998) point out, the identifying assumption that technological differences persist across countries does not require that industries have the same value for external financial dependence in every country, but that the ranking among them remains relatively stable across countries. Panel D of Table 5.1 shows that there is considerable variation in the amount of external finance that industries need in order to fund their operations. Sectors with the highest dependence on external finance are Plastic Products and Office/Accounting/Computing Machinery. On the contrary, Tobacco and Leather sectors seem to rely mostly on internal funding.

The share of small and medium enterprises within an industry is obtained from Beck et al. (2008) and calculated as the industry's share of total employment by firms with less than 20 employees. The paper rests on the same identifying assumptions as Rajan and Zingales (1998) and also uses the U.S. as a benchmark economy to measure an industry's technological share of small firms. Beck et al. (2008) emphasize that even if there are policy distortions and market imperfections in the U.S., the

[19]For this the three-digit ISIC Rev. 3 codes used in the UNIDO (2011) dataset are matched to three-digit ISIC Rev. 2 codes used in these earlier papers. ISIC Rev. 3 codes are generally finer than ISIC Rev. 2 codes so that in some cases several of the sectors in this paper have the same value for the respective measure. The United Nations Statistics Division provides tables with correspondences between different sector classifications on its website. As a consistent matching of ISIC Rev. 3 sectors 331 Medical instruments, 332 Optical instruments and 333 Watches and clocks is not possible, these sectors are excluded from the analysis.

approximation remains valid as long as these distortions do not systematically distort the ranking of industries. Again, Panel D of Table 5.1 shows considerable variation between industries. While Wood products and Printing and Publishing are industries with relatively high shares of SMEs, there are few small firms in the Tobacco or the Basic Chemicals industry.

5.2.5 Firm data

Unfortunately, no database contains detailed information on firms from all our sample countries for the period from 1995 to 2007. Nevertheless, we would like to use firm level data in order to support our argumentation and present evidence on the mechanism behind our industry results. As a compromise, we use Bureau van Dijk's Amadeus database to obtain data on a subsample of countries, the Eastern European countries. The database contains balance sheet and other financial information on public and private firms from 43 European countries. Similar to the industry level, we focus on the manufacturing sector and include only firms from U.S. SIC sectors 2000-3999. Our basic firm-level regressions contains 30,489 observations for 7,228 distinct firms from eight Eastern European countries.

Panel E of Table 5.1 provides a description for the Amadeus data. It covers the period from 1995 to 2004 and is hence well-balanced around the years 1999 and 2000, in which most Eastern European countries liberalized their banking sectors. Dependent variables in the regressions are a firm's total debt and the ratio of total debt to total assets. Firms from all countries are comparable in size (as measured by total assets or sales), tangibility (defined as fixed assets over total assets) and profitability (defined as EBIT over total assets). The last three columns show the average values of three dummy variables: The first one takes a value of one if the average firm size (as measured by total assets) prior to liberalization is lower than median, the second one takes a value of one if the average firm age (in years) prior to liberalization is lower than median, and the third one takes a value of one if the firm operates in an industry with a higher than median value of external dependence according to Rajan and Zingales (1998). Importantly, each dummy varies significantly within each country, so that differences between firms of different external dependence, different size, or different age cannot be attributed to differences between countries.[20]

5.3 Loan supply and financial structure

One important motive to open banking sectors is that policy makers expect an inflow of new capital through foreign banks. Foreign banks that enter emerging markets are generally multinational institutions that do not rely on domestic deposits to fund their loans. From this perspective, liberalization should result in an improved access to international capital markets and, therefore, an increase of aggregate loan supply (Beim and Calomiris 2001). However, this argument neglects potential reactions by domestic banks' to the increase in competition. We argue that only banks that operate relatively efficient are able to adapt to the new situation, for example by improving their technology and processes to counter the competitive threat. As these banks are able to compete with foreign entrants, efficiency gains and improved access to international capital markets might induce them to increase their loan supply. Banks that operate relatively inefficient, on the other hand, may fail to adapt to the new competitive environment, are likely to lose market shares and could even be driven out of the market. In this case, foreign lending would simply be a substitute for domestic lending and one would not expect a positive effect on aggregate lending within the economy. Similarly, one might expect no positive effect on aggregate lending if foreign banks enter the market by taking over domestic banks.

The effect of opening the banking sector on lending in the economy might even be negative. For instance, foreign banks may be unable or unwilling to take over all of the domestic banks' customers. Literature suggests that foreign banks—who are mostly large and multinational enterprises—prefer lending to large and transparent companies that are able to provide hard information (see, e.g., Stein 2002, Berger et al. 2005, or Mian 2006). Financing to smaller companies that rely on soft information might suffer from liberalization, if domestic banks previously acting as relationship lenders are replaced by foreign entrants.[21]

[20]In Hungary, the liberalization event was relatively early in 1996. Amadeus does not contain information on Hungarian firms in 1995, so that we are unable to define the dummy variables for initial size and initial age.

[21]In a theoretical model, Detragiache, Tressel, and Gupta (2008) show that foreign bank entry may worsen welfare if domestic banks are better at monitoring soft information customers. In the

Thus, the effect of liberalization on aggregate loan volume is a priori not obvious. The overall effect should depend on two factors: The efficiency of the domestic banking sector at the time of liberalization and the relative importance of the SME sector that depends on external financing.

5.3.1 Bank-level evidence on loan supply

We start by documenting the reaction of individual domestic banks to liberalization. In particular, we investigate the effect of liberalization on the loan supply of these banks. We estimate the following equation:

$$\log(loans)_{ijt} = \alpha_i + \alpha_{jt} + \phi' B_{it} + \delta \cdot (initial\ ROA_i \cdot event_{jt}) + \epsilon_{ijt} \tag{5.1}$$

where i indexes the individual bank, j country and t time; α_i are bank fixed effects and α_{jt} are year-country interactions that control for year-specific shocks to certain banking markets; bank control variables are denoted by B_{it} and include the bank's market share, the ratio of equity to assets and the ratio of liquid to total assets; each bank's efficiency in the year before liberalization is measured by the bank's ROA in that year (*initial ROA*). Alternatively, we classify banks as efficient if their ROA is among the top quartile of all our sample banks (foreign and domestic) in the year before the respective liberalization event and interact the resulting dummy variable (*d(efficient bank)*) with the event variable. Finally, ϵ_{ijt} is a random error term. To allow for correlation between observations from the same country, standard errors in all our regressions are clustered at the country level.

Results are reported in Table 5.2. We start with including only the event variable in Equation (5.1). The coefficient δ_1 is negative, but not significant (column 1). In column 2 we include the interaction between initial ROA and liberalization, which enters with a positive sign and is significant at the 10%-level. The more efficiently a domestic bank operates prior to liberalization, the more it increases its loan supply following the event. In column 3 we use the dummy *d(efficient bank)* instead of the

empirical part of the paper they find support for this prediction and show that there is a negative correlation between foreign bank penetration and the depth of the private credit market in poor countries. Their findings are consistent with the above argument: Foreign bank entry may increase total lending and welfare within a country, but this is not guaranteed and crucially depends on the efficiency of the domestic banking sector at liberalization.

Table 5.2: Bank-level loans

	(1) Log(loans)	(2) Log(loans)	(3) Log(loans)	(4) Log(loans)	(5) Log(loans)	(6) Log(loans)
Event	-0.200	-0.292	-0.350*	-0.226		
	(0.185)	(0.177)	(0.181)	(0.161)		
Interaction (Event × initial ROA)		0.100*				
		(0.053)				
Interaction (Event × d(efficient bank))			0.846***	0.496**	0.470**	0.316*
			(0.199)	(0.185)	(0.217)	(0.182)
Observations	4604	4604	4604	4604	4604	4604
Distinct banks	542	542	542	542	542	524
R-squared	0.245	0.253	0.261	0.340	0.430	0.602
Year effects	YES	YES	YES	YES	—	—
Bank effects	YES	YES	YES	YES	YES	YES
Country trends	NO	NO	NO	YES	—	—
Year-country interactions	NO	NO	NO	NO	YES	YES
Bank controls	NO	NO	NO	NO	NO	YES

The table reports coefficients for different specifications of the following equation: $\log(loans)_{ijt} = \alpha_i + \alpha_t + \phi' B_{it} + \delta_1 \cdot event_{jt} + \delta_2 \cdot (initial_i \cdot event_{jt}) + \epsilon_{ijt}$, where i denotes the individual bank, j country and t time. Dependent variable in all regressions is the logarithm of the loan supply of private domestic banks. Variables of interest are the event variable, an interaction between liberalization and initial ROA (column 2) and and interaction between liberalization and a dummy variable taking a value of 1 if the bank is among the 25% most efficient banks in the year of liberalization (columns 3-6). We use bank and year fixed effects, country-specific trends or a full set of year-country interactions in order to control for unobserved heterogeneity. Time varying bank control variables include the bank's market share within the country, a solvency measure defined as equity over total assets and a liquidity measure defined as liquid assets over total assets. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

bank's initial ROA. The negative coefficient for the event variable means that the average inefficient domestic bank decreased its loan supply following liberalization, while the positive sign for the larger interaction term indicates that the average efficient domestic bank increased its loan supply. These statistically significant effects are also economically meaningful: Inefficient domestic banks decreased their lending by 29.5 % on average, while efficient banks increased their loan supply by 64.2 % following the event. The result is robust to the inclusion of country-specific trends in column 4, year-country interactions in column 5, and time-varying bank control variables in column 6. Further robustness checks are provided in Section 5.6.

These bank level results suggest that the timing of liberalization has an important effect on the loan supply of domestic banks. Initially efficient domestic banks increase their lending relative to domestic banks that were inefficient at liberalization. This result is important as it suggests that the initial efficiency of domestic banks is a crucial determinant of post-liberalization financial structure.[22]

5.3.2 Country-level evidence on loan supply

We now test how liberalization affects aggregate, domestic and foreign loan supply and how this outcome depends on the average initial efficiency of domestic banks prior to the event. To do so, we use the country-level measure of banking sector efficiency defined in Section 5.2.3 (see also Table 5.1, Panel C). Specifically, we calculate the fraction of banks (weighted by total assets) in a given market with an ROA in the top quartile of all sample banks (domestic as well as foreign owned) in the year before liberalization and divide our sample into countries were this fraction is lower than the median and countries where it is higher than the median (see variable *d(initial ROA)* in Panel C of Table 5.1). For each subsample, we estimate:

$$\log(loansupply)_{jt} = \alpha_j + \alpha_t + \psi_k' C_{jt} + \delta \cdot event_{jt} + \epsilon_{jt} \tag{5.2}$$

where j indexes country and t time. The dependent variable is the logarithm of either aggregate, domestic or foreign loan supply; C_{jt} is a vector of macro controls and includes GDP growth, the logarithm of inflation, and the Hirschman-Herfindahl Index to control for competition within the banking sector; $event_{jt}$ is the liberalization variable. Country and time fixed effects account for unobserved heterogeneity and are denoted by α_j and α_t, respectively, and ϵ_{jt} is a random error term. As in the bank level regressions, standard errors are clustered at the country level.

The results from the regressions are reported in Table 5.3. We start by investigating the effect of liberalization on aggregate loan supply and include all our sample countries in column 1. Panel A shows that on average there was no significant increase in aggregate loan supply after the market opening. This finding is

[22]The focus in this section is on the reaction of domestic banks, i.e., we do not account for the reaction of foreign banks and banks that were taken over by foreign banks. If banks that were taken over by foreign entrants were selected based on their efficiency this could create a potential bias to our analysis. We address this issue in Section 5.6. Furthermore, we look at aggregate outcomes in the next two sections, hence also incorporating lending by foreign banks and banks that were taken over by foreign banks.

Table 5.3: Aggregate, domestic and foreign lending

Panel A: Aggregate loan supply

	(1) All countries	(2) Low efficiency	(3) High efficiency	(4) All countries
Event	0.054	-0.318**	0.419	-0.318**
	(0.190)	(0.131)	(0.286)	(0.128)
Interaction (event × d(initial ROA))				0.738**
				(0.306)
Observations	338	169	169	338
Distinct countries	26	13	13	26
R-squared	0.690	0.683	0.721	0.709
Year effects	YES	YES	YES	YES
Country effects	YES	YES	YES	YES
Macro controls	YES	YES	YES	YES

Panel B: Foreign loan supply

	(1) All countries	(2) Low efficiency	(3) High efficiency	(4) All countries
Event	0.654***	0.600*	0.705*	0.600**
	(0.224)	(0.289)	(0.350)	(0.283)
Interaction (event × d(initial ROA))				0.106
				(0.444)
Observations	328	162	166	328
Distinct countries	26	13	13	26
R-squared	0.698	0.684	0.705	0.698
Year effects	YES	YES	YES	YES
Country effects	YES	YES	YES	YES
Macro controls	YES	YES	YES	YES

Panel C: Domestic loan supply

	(1) All countries	(2) Low efficiency	(3) High efficiency	(4) All countries
Event	-0.428*	-0.978**	0.111	-0.978***
	(0.241)	(0.331)	(0.209)	(0.325)
Interaction (event × d(initial ROA))				1.089***
				(0.384)
Observations	337	168	169	337
Distinct countries	26	13	13	26
R-squared	0.425	0.412	0.502	0.471
Year effects	YES	YES	YES	YES
Country effects	YES	YES	YES	YES
Macro controls	YES	YES	YES	YES

The table shows the impact of liberalization on aggregate (Panel A), foreign (Panel B) and domestic (Panel C) loan supply at the country level. Columns 1 and 4 include the whole sample, column 2 includes only the countries with a lower than median share and column 3 includes only the countries with a higher than median share of efficient domestic banks at liberalization. In column 4 we include an interaction between the event and a dummy that is equal to one for countries with a higher than median share of efficient domestic banks at liberalization. We use country and year fixed effects in order to control for unobserved heterogeneity. Furthermore, all regressions include GDP growth, inflation and the Hirschman-Herfindahl Index as time-varying macro control variables. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

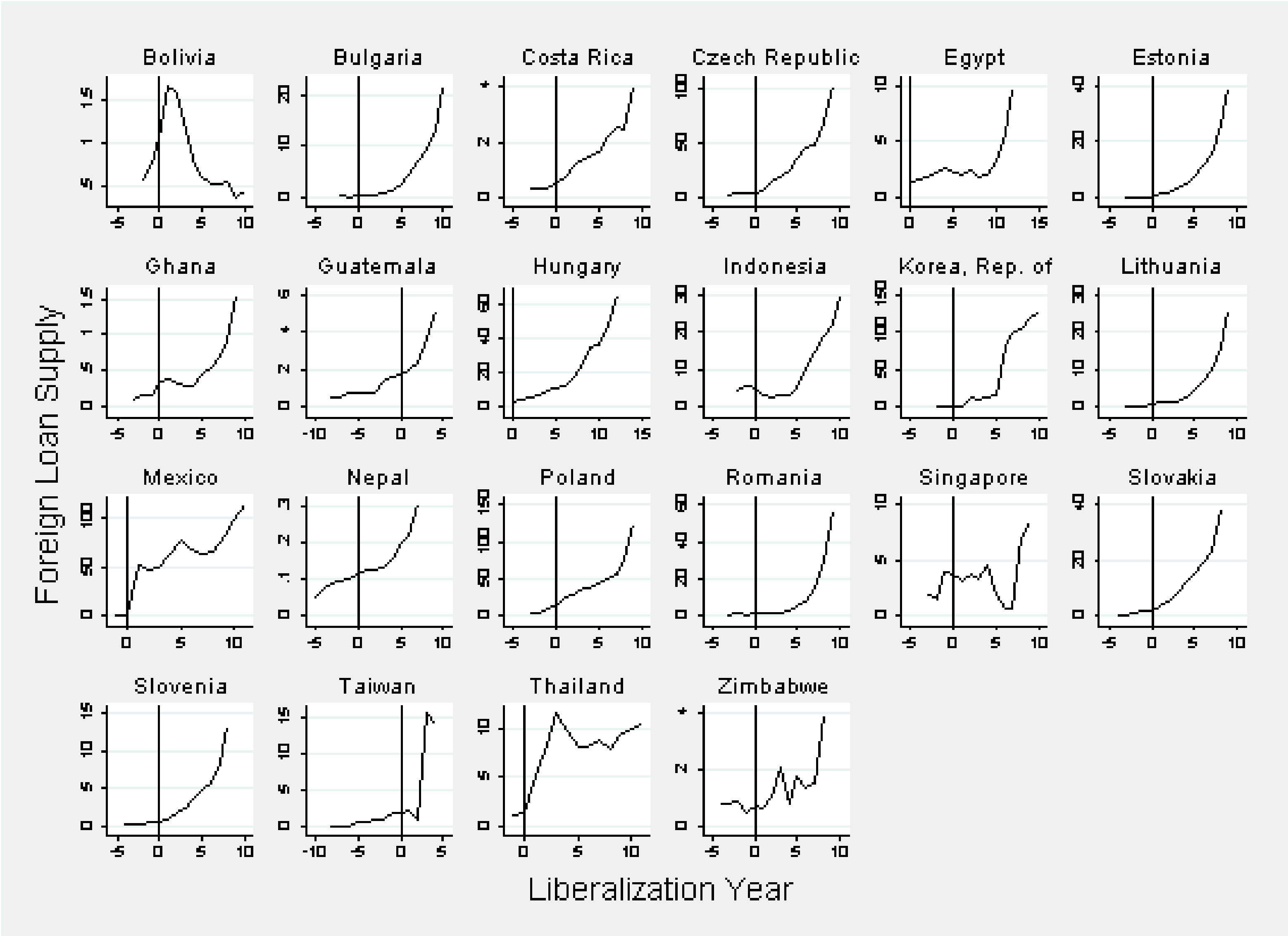


Figure 5.2: Impact of liberalization on foreign loan supply

The figure shows the development of foreign banks' loan supply within our sample countries around the liberalization events (indicated by the vertical lines). All values are in billions of U.S. dollars.

remarkable: Foreign banks are expected to bring fresh capital into emerging markets, which should translate into an increase in aggregate loan supply. The insignificant coefficient for liberalization suggests that in many cases foreign lending just substitutes domestic lending. Interesting heterogeneities emerge when we divide our sample countries into those with a lower and those with a higher than median share of efficient domestic banks at liberalization. For countries with relatively inefficient domestic banks, the coefficient for liberalization is significantly negative (Panel A, column 2), while countries with relatively efficient domestic banks see the expected increase in the aggregate supply of credit (Panel A, column 3, the p-value is 0.16).

Panels B and C of Table 5.3 illustrate the underlying cause of this finding. As expected, all countries experience significant increases in foreign lending following the event (Table 5.3, Panel B). This is illustrated in Figure 5.2, which shows the development of foreign banks' loan supply around liberalization reforms in our sample countries. However, the increase in foreign lending translates into an even larger decrease in domestic lending in countries with relatively inefficient domestic banks at liberalization (Panel C, column 2). Domestic lending in countries with relatively efficient domestic banks, on the other hand, is not significantly affected by the event (Panel C, column 3). This heterogeneous reaction to liberalization by domestic banks in countries with differing levels of financial development is directly reflected in the development of aggregate loan supply. While foreign banks replace domestic lenders in countries with relatively inefficient domestic banks, it seems as if they do not take over all of their customers, so that aggregate lending decreases. In countries with relatively efficient domestic banks, on the other hand, foreign lending seems to be more of a complement to domestic lending. The substitution effect, if present at all, is much weaker in these countries, so that we see a modest increase in aggregate lending. In column 4 we reproduce the results of columns 2 and 3 in one regression by including an interaction between the event and a dummy variable that takes the value of one in markets with a higher than median share of efficient domestic banks.

Our findings are illustrated in Figure 5.3, which shows Epanechnikov kernel densities of residuals from a regression of aggregate loan supply on country and year dummies as well as the macro control variables from above. Densities are plotted for countries with an above/below median share of efficient domestic banks at liberalization before and after the event. There is a clear rightward shift for countries with relatively efficient domestic banks, while the density for countries with rather inefficient domestic banks is shifted to the left. Both shifts are statistically significant, as the the Kolmogorov-Smirnov test for the equality of distribution functions is rejected at the 1 % level.

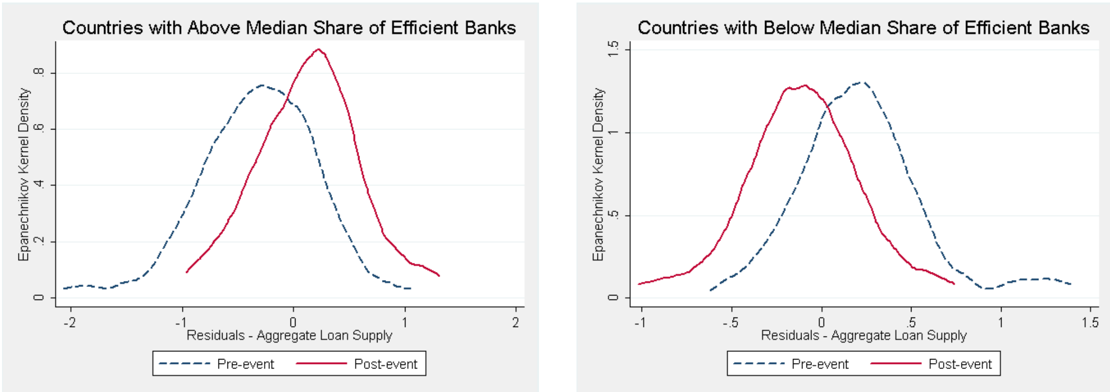


Figure 5.3: Aggregate loan supply

The figure plots Epanechnikov kernel densities of residuals from an estimation of the following equation: $\log(loansupply)_{jt} = \alpha_j + \alpha_t + \psi_k' C_{jt} + \epsilon_{jt}$, where $log(loansupply)_{jt}$ is the logarithm of aggregate lending within a country-year, α_j and α_t are country and time fixed effects, respectively, and C_{jt} is a vector of macro control variables that includes GDP growth, inflation and the Hirschman-Herfindahl Index. The left panel shows countries with a higher than median share of efficient domestic banks at liberalization, and the right panel shows countries with a lower than median share.

An interpretation of our findings is that many domestic banks are able to compete with foreign entrants in initially efficient markets. Hence, they remain active in the market and continue to serve their customers. In inefficient markets foreign lending simply replaces domestic lending and aggregate lending declines. Two reasons could explain this finding. On the one hand, it could be that foreign banks make better lending decisions than inefficient domestic banks and simply refuse to take over some of their unhealthy customers. In this case, the decline in the total volume of credit might still be accompanied by an increase in the efficiency of financial intermediation, as more efficient foreign banks are better able to channel capital into its most productive use. On the other hand, it might be that foreign banks over-engage in 'cherry-picking' their customers and also refuse to take over some of the healthy customers of the domestic banks they replace. For example, it could be that small firms who rely on relationship lending become credit-constrained after liberalization. We investigate this issue in Sections 5.4 and 5.5, but first we provide some direct evidence for changes in countries' financial structures (domestic vs. foreign lending).

5.3.3 Financial structure

We documented in Section 5.3.2 that countries with relatively inefficient domestic banking sectors experience large declines in domestic lending following the event. Thus, it is likely that the average efficiency of domestic banks at the time of liberalization affects the post-event market structure (i.e., relative market share of domestic and foreign banks). Changes in financial structure should be more pronounced in countries with less developed domestic banking sectors. To test this prediction, we regress the market share of foreign banks—which we measure as the share of foreign bank assets in all bank assets for each country—on our event variable, including country and year fixed effects as well as the vector of macro control variables from above. Additionally, we use the ratio of foreign banks to all banks within the country as a dependent variable.

Results are reported in Table 5.4. Columns 1 and 4 use the entire sample. As already noted in Section 5.2.1 and visualized in Figure 5.1, we see a significant increase in the market share of foreign banks following liberalization. Similarly, the number of foreign banks in all banks goes up when entry barriers are removed (column 4). In columns 2 and 3 we apply the same sample split as in Section 5.3.2 and run separate regressions for countries with a lower than median and those with a higher than median share of efficient domestic banks at liberalization. We see a significant increase in the market share of foreign banks in both subsamples. However, the effect is much stronger in countries with relatively inefficient domestic banks. An increase of our index by one unit implies a 23.7% increase in the foreign market share in markets with inefficient domestic banks, compared to a 9.4% increase in markets where domestic banks are relatively efficient.

These results are expected, given our findings in Section 5.3.2. In particular, the documented decrease in domestic lending in inefficient markets implies an additional increase in the market share of foreign banks. The differential effect is even more pronounced if we look at the ratio of foreign banks to all banks in columns 5 and 6. The coefficient for the liberalization variable in the less efficient markets has about

Table 5.4: Financial structure

	(1) Foreign market share All countries	(2) Foreign market share Low efficiency	(3) Foreign market share High efficiency	(4) Foreign bank share All countries	(5) Foreign bank share Low efficiency	(6) Foreign bank share High efficiency
Event	0.164***	0.237**	0.094*	0.102***	0.145**	0.060**
	(0.057)	(0.102)	(0.048)	(0.034)	(0.062)	(0.027)
Observations	338	169	169	338	169	169
Distinct countries	26	13	13	26	13	13
R-squared	0.621	0.507	0.698	0.532	0.594	0.502
Year effects	YES	YES	YES	YES	YES	YES
Country effects	YES	YES	YES	YES	YES	YES
Macro Controls	YES	YES	YES	YES	YES	YES

The table shows the impact of liberalization on financial structure. The dependent variable is the market share of foreign banks (measured as the share of bank assets owned by foreign banks divided by all bank assets in the country) in columns 1-3 and the percentage number of foreign banks in all banks in columns 4-6. Columns 1 and 4 include the whole sample, columns 2 and 5 only the countries with a lower than median and columns 3 and 6 only the countries with a higher than median share of efficient domestic banks at the time of liberalization. We use country and year fixed effects in order to control for unobserved heterogeneity. Furthermore, all regressions include GDP growth, inflation and the Hirschman-Herfindahl Index as time-varying macro control variables. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

2.5 times the size of the coefficient for the more efficient markets. Foreign banks seem to replace domestic banks in these markets. For both the foreign market share and the foreign bank ratio, the difference between the coefficients for the inefficient and the efficient markets is significant at the 1%-level.

Taken together, our findings are in line with our previous results. In both inefficient and efficient markets liberalization increases the market share of foreign banks. In markets with more efficient banks at the time of liberalization, however, the market as a whole also grew (Table 5.3). Many domestic banks are able to compete with foreign entrants and remain active in the market. Hence, the ratio of foreign banks to all banks increases only slightly. On the other hand, we observe a large increase in this ratio in markets with largely inefficient domestic banks at the time of liberalization. Thus, the timing of liberalization affects financial structure.

5.4 Industry evidence

Given the vital role banks play in the allocation of capital within the economy, we expect that the choice of the liberalization policy transmits to the real sector. We start by examining how liberalization affects industry output and hence economic growth. There are three reasons why we expect a positive impact in countries with relatively efficient domestic banks. First, we find a positive effect of liberalization on aggregate loan supply in these countries. A large literature following King and Levine (1993a) and King and Levine (1993b) documents that financial deepening translates into economic growth. Secondly, foreign banks in transition countries are found to be more cost efficient and provide better services than their domestic counterparts (Bonin, Hasan, and Wachtel 2005a). As foreign banks took over market shares from domestic banks, this should translate into an increase in the efficiency of financial intermediation. Thirdly, stimulating competition from foreign entrants should induce relatively efficient domestic banks to improve their technology and services. Hence, the efficiency of domestic lending should also improve in financially developed countries.

In contrast, the impact in countries with relatively inefficient domestic banks is less clear cut. Although we observe a decrease in aggregate loan supply in these countries (Section 5.3.2), the market share that is taken over by foreign banks is particularly large in countries with relatively inefficient domestic banks (Section 5.3.3). To the extent that these foreign banks indeed improve the allocation of capital, a positive effect on industry output could be observed also in financially less developed countries.

5.4.1 Economic growth

To investigate the effect of liberalization on industry output, we use the UNIDO data described in Section 5.2.4. As in the loan supply regressions in Section 5.3.2 we use the dummy *d(initial ROA)* to split our sample into countries with a lower than median and those with a higher than median share of efficient domestic banks at liberalization and estimate the following specification for each subsample:[23]

$$\log(Y)_{ijt} = \alpha_{ij} + \alpha_t + \delta \cdot event_{jt} + \epsilon_{ijt} \quad (5.3)$$

where i indexes industry, j country and t time; the dependent variable is the logarithm of industry output. We include country-industry interactions in all regressions in order to account for any unobserved time-invariant determinants of industry performance (e.g., natural endowments, location). Time fixed effects α_t control for changes in economic performance over time. As before, standard errors are clustered at the country level.

Table 5.5: Industry output

	(1) Log(output)	(2) Log(output) Low efficiency	(3) Log(output) High efficiency	(4) Log(output)
Event	0.096	-0.250*	0.463**	-0.250*
	(0.164)	(0.138)	(0.196)	(0.136)
Interaction (event × d(initial ROA))				0.713***
				(0.234)
Observations	10,520	5,690	4,830	10,520
Distinct country-industries	1,132	637	495	1,132
R-squared	0.334	0.365	0.336	0.348
Year effects	YES	YES	YES	YES
Country-industry-interactions	YES	YES	YES	YES

The table shows estimation results for different specifications of the following equation: $log(Y)_{ijt} = \alpha_{ij}+\alpha_t+\delta_1 \cdot (event_{jt})+\epsilon_{ijt}$, where i denotes industry, j country and t time. The dependent variable is the logarithm of country-industry output. All regressions include country-industry interactions and time fixed effects to account for unobserved heterogeneity and a random error term ϵ_{ijt}. Columns 1 and 4 use the whole sample of countries, column 2 uses only countries with a lower than median share, and column 3 uses only countries with a higher than median share of efficient domestic banks at liberalization. In column 4 we include an interaction between the event and *d(initial ROA)*, a dummy taking the value of 1 in countries with a higher than median share of efficient domestic banks at liberalization. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

On average, industry output is not affected by the event. The coefficient for liberalization is positive but insignificant (Table 5.5, column 1). Thus, in line with our finding that liberalization did not have an effect on aggregate credit supply,

[23] Alternatively, we use the whole sample and include an interaction between the event and *d(initial ROA)*.

we do not observe an effect on growth in the average industry. Next, we apply the sample split discussed above, as pooling countries with efficient and inefficient domestic banking sectors at liberalization masks cross-sectional heterogeneity.

Column 2 includes only the less efficient markets. The coefficient for the event variable is now negative and significant at the 10%-level. The negative impact on aggregate loan supply in these countries corresponds to a negative impact on output in the average industry. In column 3, we include only the more efficient countries. The coefficient for liberalization is positive and significant at the 5%-level, indicating that liberalization promotes economic growth in these countries. This finding underlines our conjecture that domestic banks need to be sufficiently developed before entry barriers are removed in order for liberalization to have a positive impact.[24] Column 4 shows that the difference between the two groups of countries is significant at the 1%-level.

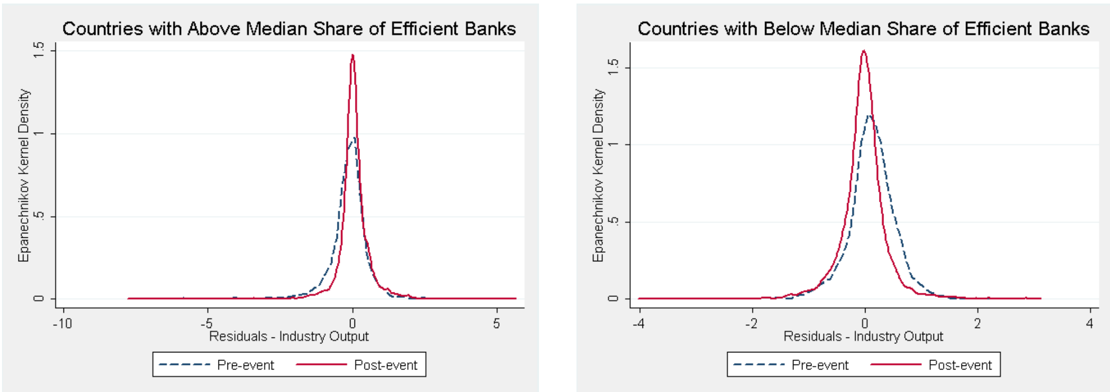


Figure 5.4: Industry output

The figure plots Epanechnikov kernel densities of residuals from an estimation of the following equation: $\log(Y)_{ijt} = \alpha_{ij} + \alpha_t + \epsilon_{ijt}$, where $log(Y)_{ijt}$ is the logarithm of country-industry output in a certain year, and α_{ij} and α_t are country-industry and time fixed effects, respectively. The left panel shows countries with a higher than median share of efficient domestic banks at liberalization, and the right panel shows countries with a lower than median share.

Again, we illustrate our findings by plotting Epanechnikov kernel densities of residuals from a regression of industry output on country-industry interactions and year fixed effects. Figure 5.4 shows a rightward shift in the density following liberalization in countries with relatively efficient domestic banks, while residuals in

[24]Bekaert, Harvey, and Lundblad (2005) document that equity market liberalization increases annual real economic growth by about 1 %. In line with our findings they show that the largest growth responses occur in countries with high quality institutions.

countries with rather inefficient banks are shifted to the left. The Kolmogorov-Smirnov test for the equality of distribution functions indicates that both shifts are significant at the 1 % level.

5.4.2 Differential effects on output

A potential problem with the analysis in the previous section is that countries that differ in the development of the domestic banking sector differ in several other dimensions that might have an influence on industry performance. While the country-industry interactions absorb any fixed differences between countries, time-varying omitted variables pose a threat to identification and might bias our results. To address this problem, we replace the year effects by year-country interactions and include an interaction between the liberalization variable and industry characteristics (i.e., an industry's external financial dependence and an industry's SME share) into Equation (5.3):

$$\log(Y)_{ijt} = \alpha_{ij} + \alpha_{jt} + \lambda \cdot industry\ characteristic_i \cdot event_{jt} + \epsilon_{ijt} \qquad (5.4)$$

We start by examining whether there is a differential impact for industries that differ regarding their external dependence. The idea is that if liberalization is indeed the driving force behind our results, any effect it might have on industries should be especially relevant for those industries that are more dependent on external finance. Accordingly, any increase in the efficiency of the capital allocation process that might be induced by liberalization should be reflected in a positive sign for the interaction coefficient.

Column 1 of Table 5.6 shows results for our whole sample of countries. The coefficient for the interaction is positive and significant at the 1%-level. Indeed, foreign entry seems to induce an improvement in the efficiency of financial intermediation that is reflected in a more positive development of industries that are more dependent on external finance. The differential effect is economically large: According to the model, moving from the 25th percentile (0.14) to the 75th percentile (0.47) of external financial dependence corresponds to a 16.1 % larger increase in industry output following liberalization in our average sample country (see last line of Table 5.6).

Given our results until now, it is somewhat surprising that the interaction is also positive for countries with a lower than median share of efficient domestic banks at liberalization (column 2). Even though output in the average industry is negatively affected in these countries, those industries that rely on external finance the most have relatively higher output following the event. Although aggregate lending declines in these economies, foreign banks seem to improve the efficiency of capital allocation and lend to more productive firms on average. In other words: Not the quantity, but the average quality of lending increases in these countries.

Table 5.6: Industry output by external dependence and SME share

	(1) Log(output)	(2) Log(output) Low efficiency	(3) Log(output) High efficiency	(4) Log(output)	(5) Log(output) Low efficiency	(6) Log(output) High efficiency
Interaction (event × external dependence)	0.454***	0.473**	0.434**			
	(0.120)	(0.198)	(0.145)			
Interaction (event × SME share)				-0.014	-1.235***	1.364**
				(0.492)	(0.301)	(0.574)
Observations	8,799	4,742	4,057	8,606	4,634	3,972
Distinct country-industries	942	527	415	921	515	406
R-squared	0.485	0.511	0.464	0.466	0.493	0.449
Year-country interactions	YES	YES	YES	YES	YES	YES
Country-industry-interactions	YES	YES	YES	YES	YES	YES
Differential (75th vs. 25th perc.)	16.1%	19.7%	15.4%	-0.1%	-7.3%	8.8%

This table shows how liberalization affects industries with different levels of external financial dependence and different shares of small and medium enterprises. Columns 1-3 provide results for the following equation: $log(Y)_{ijt} = \alpha_{ij} + \alpha_{jt} + \delta \cdot (external\, dependence_i \cdot event_{jt}) + \epsilon_{ijt}$, whereas columns 4-6 estimate the following equation: $log(Y)_{ijt} = \alpha_{ij} + \alpha_{jt} + \delta \cdot (SME\, share_i \cdot event_{jt}) + \epsilon_{ijt}$. The dependent variable is the logarithm of industry output. Columns 1 and 4 report results for the whole sample. Columns 2 and 5 include only countries with a lower than median share of efficient domestic banks at liberalization, and columns 3 and 6 include only the countries with a higher than median share. All regressions include country-industry interactions to account for unobserved heterogeneity and year-country interactions that control for country specific developments within a certain year (and absorb the event coefficient itself). We also include a random error term ϵ_{ijt}. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

As mentioned above, the literature suggests that large and multinational foreign banks prefer lending to large and transparent companies, while small firms typically have problems in obtaining loans from them (Stein 2002, Berger et al. 2005, Mian 2006). Consequently, small firms might become credit constrained if foreign banks replace domestic banks previously acting as relationship lenders. This effect should be particularly relevant in markets that are characterized by a low share of efficient domestic banks. On the other hand, Beck et al. (2008) show that small-firm industries grow disproportionately faster in countries with well-developed financial systems. The basic argument is that more efficient financial intermediation reduces informational problems and the need for collateral. Consequently, small firms that are informationally opaque and rely more on intangible assets than large firms disproportionately benefit from financial development. We argue that liberalization increases the efficiency of financial intermediation, particularly in countries with sufficiently developed domestic banking sectors. Hence, liberalization could affect small-firm industries in a positive way. Overall, we expect a negative impact on small firm industries in countries with less developed domestic banks, while the effect should be rather positive in countries with more efficient banking sectors.

Columns 4-6 of Table 5.6 show results for the regressions using the *SME share* as an industry characteristic. If we include all sample countries in column 4, the coefficient for the interaction between an industry's SME share and liberalization is insignificant and very close to zero. Thus, in the average country, the effect of liberalization for industries with high shares of SMEs is not different from the effect for the average industry. However, we uncover interesting heterogeneities as we split the sample into countries with a lower than median and countries with a higher than median share of efficient domestic banks at liberalization.

The coefficient for the interaction term is negative and significant at the 1%-level in countries with rather inefficient domestic banks. This result is consistent with our previous findings. These markets are characterized by large drops in domestic lending following the event. Since the foreign banks who replace domestic lenders mostly rely on hard information, SMEs are unable to obtain the same level of funding in the post liberalization period. The reduction in funding opportunities translates into lower output for these firms. Interestingly, one cannot observe a similar effect in countries with rather efficient domestic banks. On the contrary, small-firm industries seem to disproportionately benefit from the improvement in financial intermediation that liberalization fosters in these countries (column 6). Better screening and monitoring devices on the part of banks help these firms to overcome financial constraints, a finding that is in line with the results of Beck et al. (2008).

Our results suggest that liberalization indeed increases the efficiency of financial intermediation and hence has a positive impact on industry growth rates within the liberalizing country. This is unambiguously true if domestic banks are sufficiently developed at the time of liberalization, so that they are able to compete with foreign entrants. If this is not the case, particularly small and opaque firms might be harmed by liberalization, as they lose the domestic relationship lenders they need in order to obtain funding. Section 5.5 provides further evidence on the firm level regarding this issue.

5.4.3 Differential impact on industry volatility

We conclude the industry section with some evidence on the effect of liberalization on output volatility. Evidence on whether liberalization increases or decreases volatility of industrial production is mixed. While Morgan, Rime, and Strahan (2004) show that the allowance of interstate banking reduced economic growth volatility within U.S. states, Morgan and Strahan (2004) cannot confirm their finding in a study using international data for nearly 100 countries in the 1990s.[25] If anything, their results suggest that a larger foreign bank presence in non-industrial countries is associated with more, not less, volatility. Regarding equity market liberalization, Bekaert, Harvey, and Lundblad (2006) show that liberalization did not—as often claimed—increase consumption growth volatility. Instead, they find a significant decrease in volatility for many countries. However, they conclude that volatility may not decrease or even increase in countries that have a poorly developed financial sector.

To investigate the impact of bank liberalization on the volatility of industrial production, we use an econometric framework similar to the one developed by Morgan, Rime, and Strahan (2004). Specifically, we estimate regressions of the following structure:

$$Fluctuation_{ijt} = \alpha_{ij} + \alpha_t + \delta_1 \cdot event_{jt} + \epsilon_{ijt} \tag{5.5}$$

where i indexes industry, j country and t time. Country-industry interactions and time fixed effects are represented by α_{ij} and α_t, ϵ_{ijt} is a random error term. $Fluctuation_{ijt}$ equals the absolute deviation from conditional mean growth in indus-

[25] Morgan, Rime, and Strahan (2004) examine fluctuations in state gross product, employment or personal income within U.S. states, while Morgan and Strahan (2004) focus on volatility in real GDP and real investment growth.

try output. Specifically, it is equal to the absolute values of the residuals from a regression of country-industry growth rates on a full set of country-industry interactions and year fixed effects:

$$Growth_{ijt} = \alpha_{ij} + \alpha_t + u_{ijt}, \tag{5.6}$$

and,

$$Fluctuation_{ijt} = \left| u_{ijt} \right|. \tag{5.7}$$

Hence, the fluctuation in economic growth for a given country-industry-year can be interpreted as the size of the deviation from average growth for that country-industry over our sample period and from average growth for all country-industries in that year.

Table 5.7 provides results for the estimation of Equation (5.5). In column 1 we include all sample countries. The coefficient for liberalization is positive but insignificant, indicating that the event did not affect economic volatility in our average sample country. Again, we uncover interesting heterogeneities if we divide our sample into countries with a lower than median and countries with a higher than median share of efficient domestic banks at liberalization. Volatility in economic production increases in countries that have mostly inefficient domestic banks (column 2). As foreign banks took over large shares of the market in these countries, this result is in line with arguments that point at possible withdrawals of funds by foreign banks at economic downturns.

In contrast, we document significantly lower volatility in countries with rather efficient domestic banking sectors at liberalization (column 3). Bank liberalization seems to stabilize the economy if the domestic banking sector is sufficiently developed. Column 4 shows that the difference between the two groups of countries is significant at the 1%-level. The more efficient domestic banks are at liberalization, the more stable the economy after the event. Overall, results in this section complement the equity market liberalization results of Bekaert, Harvey, and Lundblad (2006) and confirm our previous finding that only countries that have a well functioning domestic financial sector are able to leverage the maximum benefits from liberalization.

Table 5.7: Industry volatility

	(1) Output fluctuation	(2) Output fluctuation Low efficiency	(3) Output fluctuation High efficiency	(4) Output fluctuation
Event	0.019	0.094**	-0.071*	0.094**
	(0.036)	(0.036)	(0.034)	(0.036)
Interaction (event × d(initial ROA))				-0.165***
				(0.049)
Observations	8,936	4,784	4,152	8,936
Distinct country-industries	957	520	437	957
R-squared	0.022	0.013	0.034	0.024
Year effects	YES	YES	YES	YES
Country-industry-interactions	YES	YES	YES	YES

The table provides evidence on how liberalization affects industry volatility by estimating different specifications of the following equation: $Fluctuation_{ijt} = \alpha_{ij} + \alpha_t + \delta_1 \cdot (event_{jt}) + \epsilon_{ijt}$. We regress fluctuations in the growth rates of industry output on a full set of country-industry interactions, time fixed effects and our liberalization variable. Columns 1 and 4 use the whole sample of countries, column 2 uses only countries with a lower than median share, and column 3 uses only countries with a higher than median share of efficient domestic banks at liberalization. In column 4 we include an interaction between the event and *d(initial ROA)*, a dummy taking the value of 1 in countries with a higher than median share of efficient domestic banks at liberalization. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

5.5 Firm evidence

In this section, we extend our analysis by presenting firm level evidence for a subsample of eight Eastern European countries.

5.5.1 Debt taking

We start by estimating the general effect of liberalization on firm debt taking in the Eastern European countries. Specifically, we estimate the following equation:

$$\log(debt)_{ijt} = \alpha_i + \alpha_t + \theta' F_{it} + \delta \cdot event_{jt} + \epsilon_{ijt} \tag{5.8}$$

where i denotes the individual firm, j denotes country and t time. The dependent variable is the logarithm of total debt or, alternatively, the debt to asset ratio. Firm and time fixed effects (α_i and α_t) account for unobserved heterogeneity. Additionally we include the logarithm of total sales, the firm's ROA (EBIT/Assets) and a measure of tangibility (tangible assets over total assets) as time-varying firm control variables. Finally, the model includes our liberalization variable from above, as well as a random error term ϵ_{ijt}. As before, standard errors are clustered at the country level.

Column 1 of Table 5.8 provides results for this specification. It can be seen that on average firms had lower debt following liberalization. Similarly, the debt to asset ratio decreased on average following the reform (column 6). We expect this result to be driven by markets with rather inefficient domestic banks, hence we include an interaction between the event variable and the dummy *d(initial ROA)* defined in Section 5.2.3. Indeed, the interaction is significantly positive in both cases: The reduction in average lending and the average debt to asset ratio is much stronger in markets with relatively inefficient domestic banks, which is the expected result given our findings in previous sections (columns 2 and 7). As shown in Table 5.9 in the Appendix these results are robust to the inclusion of industry-year interactions and the use of the ratio of debt to pre-liberalization assets as a dependent variable.

5.5.2 Differential impact on firms

We proceed by investigating whether liberalization had a differential impact on firms in our sample countries. Specifically, we extend Equation (5.8) in the following way:

$$\begin{aligned}\log(debt)_{ijt} \quad &= \alpha_i + \alpha_t + \theta' F_{it} + \delta \cdot event_{jt} \\ &+\eta \cdot event_{jt} \cdot d(initial\ ROA)_j \\ &+\kappa \cdot event_{jt} \cdot firm\ characteristic_i \\ &+\nu \cdot event_{jt} \cdot d(initial\ ROA)_j \cdot firm\ characteristic_i + \epsilon_{ijt}\end{aligned} \tag{5.9}$$

In addition to the event variable and its interaction with the dummy *d(initial ROA)*, we include an interaction between liberalization and one of three dummy variables: The first dummy takes a value of one for firms in industries that have a higher than median value of external dependence according to Rajan and Zingales (1998); the second equals one if the average firm size (as measured by total assets) prior to the event is above median; and the third is equal to one if the firm is younger than median prior to the liberalization event. To make the equation complete, we also

Table 5.8: Firm-level evidence

	(1) Log(debt)	(2) Log(debt)	(3) Log(debt)	(4) Log(debt)	(5) Log(debt)	(6) $\frac{\text{Debt}}{\text{Assets}}$	(7) $\frac{\text{Debt}}{\text{Assets}}$	(8) $\frac{\text{Debt}}{\text{Assets}}$	(9) $\frac{\text{Debt}}{\text{Assets}}$	(10) $\frac{\text{Debt}}{\text{Assets}}$
Event	-0.388***	-0.560***	-0.490**	-0.534**	-0.437*	-0.046**	-0.056**	-0.051**	-0.050	-0.041
	(0.109)	(0.159)	(0.157)	(0.198)	(0.185)	(0.018)	(0.022)	(0.021)	(0.031)	(0.030)
Interaction (event × d(initial ROA))		0.313***	0.225***	0.151*	0.166*		0.019**	0.010**	0.009	0.007
		(0.063)	(0.060)	(0.071)	(0.072)		(0.006)	(0.004)	(0.005)	(0.004)
Interaction (event × d(exdep))			-0.115***					-0.009***		
			(0.014)					(0.001)		
Interaction (event × d(exdep) × d(initial ROA))			0.142**					0.017**		
			(0.048)					(0.006)		
Interaction (event × d(small))				0.031					-0.012*	
				(0.024)					(0.006)	
Interaction (event × d(small) × d(initial ROA))				0.261***					0.022**	
				(0.041)					(0.007)	
Interaction (event × d(young))					-0.123***					-0.021***
					(0.009)					(0.004)
Interaction (event × d(young) × d(initial ROA))					0.293**					0.011
					(0.110)					(0.017)
Log(sales)	0.476***	0.469***	0.470***	0.574***	0.580***	-0.007	-0.008	-0.008	-0.009	-0.008
	(0.050)	(0.050)	(0.051)	(0.061)	(0.058)	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)
Tangibility	0.819***	0.822***	0.822***	0.724**	0.722**	0.027	0.027	0.028	0.014	0.016
	(0.213)	(0.213)	(0.213)	(0.237)	(0.244)	(0.032)	(0.032)	(0.032)	(0.034)	(0.036)
EBIT/Assets	-1.751***	-1.698***	-1.697***	-1.689***	-1.698***	-0.193***	-0.190***	-0.190***	-0.175***	-0.174***
	(0.066)	(0.069)	(0.069)	(0.121)	(0.113)	(0.013)	(0.015)	(0.015)	(0.016)	(0.016)
Observations	30,489	30,489	30,489	17,020	17,020	30,489	30,489	30,489	17,020	17,020
Distinct firms	7,228	7,228	7,228	2,755	2,755	7,228	7,228	7,228	2,755	2,755
R-squared	0.088	0.092	0.092	0.108	0.106	0.042	0.043	0.043	0.042	0.042
Firm effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

In this table we examine how firm debt taking and corporate capital structure are affected by liberalization. The dependent variable is the logarithm of a firm's total debt in columns 1-5 and the ratio of total debt to total assets in columns 6-10. Regressions include an interaction between liberalization and a dummy variable taking the value of 1 for countries with a higher than median share of efficient domestic banks at liberalization. We define three dummy variables that take the value of 1 for firms a) in industries with above median external financial dependence according to the measure defined by Rajan and Zingales (1998); b) with above median average size (measured by total assets) prior to liberalization; c) with below median age prior to liberalization. These dummies are interacted with the event and the dummy indicating efficient markets. Furthermore, we include a measure of size (the logarithm of total assets), a measure of profitability (ROA), and a measure of tangibility (tangible assets over total assets) as time-varying firm controls. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

include a triple interaction between the event, the efficiency dummy and the firm characteristic. It should be noted that the coefficients for efficiency and the firm characteristics as well as the interaction between the two are absorbed by the firm fixed effects.

We start with the results for external dependence. The positive coefficient for the triple interaction in column 3 of Table 5.8 indicates that financially dependent firms are able to obtain relatively more debt in markets with efficient domestic banks as compared to markets with inefficient domestic banks. Firms in these countries benefit from a better capital allocation process and do not suffer from the reduction in aggregate lending that we observe in inefficient markets.[26] This result is robust to the use of the debt to assets ratio instead of the logarithm of total debt as a dependent variable (column 8).

Next, we investigate the role of firm size in columns 4 and 9 of Table 5.8.[27] The triple interaction is positive and significant in both cases, indicating that smaller firms are relatively better off in markets with efficient domestic banks. While both large and small firms are better off in efficient markets, the effect is particularly pronounced for the latter, indicating that large firms are able to obtain funding in both types of markets, while small firms depend on their domestic relationship lenders and become credit constrained if these lenders are driven out of the market in countries with initially inefficient domestic banks.

As an alternative to firm size, we use firm age as a measure of opaqueness in columns 5 and 10 of Table 5.8. Results are qualitatively very similar; the positive coefficient for the triple interaction shows that younger firms are relatively better off in countries with relatively efficient domestic banks at liberalization.[28] Overall, the results on the firm level confirm our previous results on the bank and the industry level. Only countries with a sufficiently developed banking sector are able to leverage

[26] In the output growth regressions in Section 5.4.2 the interaction between liberalization was positive also for countries with inefficient domestic banks, which indicates that—in line with an increase in the efficiency of financial intermediation—those firms that are able to obtain funding following the event are more productive on average.

[27] The sample for these regressions is smaller as we can only include firms for which we have estimates for initial size (or age), i.e., firms for which we have balance sheet information in the year before liberalization. However, sample selection does not affect our results: Results for the first three columns are very similar on the restricted sample and are available from the authors upon request.

[28] Again, results for the ratio of debt to pre-event assets are provided in Table 5.9 in the Appendix.

the maximum benefits from liberalization. If domestic banks are unable to compete with foreign entrants, they might be driven out of the market and especially small and opaque firms might become credit constrained.

5.6 Robustness checks

5.6.1 Selection concerns

In this section we undertake several checks to assess the robustness of our findings. First, we consider the issue that is introduced if we exclude banks from the analysis that were taken over by foreign banks during our sample period. If banks that were taken over by foreign entrants are selected based on their efficiency this could create a potential bias to our analysis. On the one hand, foreign banks might 'cherry pick' especially efficient banks for takeovers. On the other hand, mostly state owned banks were sold by governments to foreign banks. These banks are likely to operate inefficiently (see e.g. Bonin, Hasan, and Wachtel 2005b).

In order to test whether banks that were taken over were selected based on their efficiency, we estimate a probit model. The dependent variable $Takeover$ is a dummy that takes the value of one if a bank was taken over by a foreign bank during the sample period and the value of zero otherwise. Explanatory variables are the ROA, the cost to income ratio, and the cost to asset ratio together with further bank characteristics. Table 5.10 in the Appendix shows that banks that were taken over were not selected based on their efficiency or performance. Columns 1-3 include all bank-year observations, in columns 4-6 we collapse the data on the bank level. Neither the bank's ROA, nor the cost to income or the cost to asset ratio seem to have an impact on the probability with which a domestic bank is taken over by foreign entrants. Nevertheless, we find that foreign banks took over larger banks and more liquid banks on average.

Next, we investigate whether our results are driven by the mode of entry chosen by foreign banks. We already showed that takeover banks were not selected based on their efficiency or performance. Nevertheless it could be that domestic banks in markets where foreign banks entered mostly by taking over domestic banks are affected differently than domestic banks in markets where foreign banks entered via greenfield investments. To test this, we calculate the share of domestic banks that were taken over by foreign banks in each market and divide our sample into the countries where this share is lower than median and countries where it is higher than median. Columns 1 and 2 of Table 5.11 in the Appendix show that the differential effect for banks of differing levels of efficiency is present in both subsamples. Hence, the efficiency of domestic banks at liberalization seems to be more important than the mode of entry chosen by foreign banks.

5.6.2 Endogeneity concerns regarding the event

Another issue concerns the potential endogeneity of liberalization reforms. It could be that countries opened their banking sectors when growth prospects were good and the need for capital was high. Alternatively, it could be that countries were forced to open their banking sectors when they had crises in their domestic banking markets. In both cases, liberalization would not actually be causal for the documented effects on loan supply and industry output. Our identification strategy in all sections took these issues into account, as we documented differential effects for banks of varying degrees of initial efficiency or industries and firms with varying degrees of external financial dependence. Political processes and external pressures applied by the IMF or the World Bank should also help to mitigate these concerns.

In column 3 of Table 5.11 we re-estimate Equation 5.1 and include the vector of macro control variables from the country-level regressions. While the sample size is significantly reduced, the coefficient for the interaction between liberalization and the initial efficiency remains significantly positive. To further address reverse causality issues, we study the dynamic effects of liberalization reforms on the loan supply of domestic banks. In columns 4 and 5 of Table 5.11 we replace the liberalization index with four variables: $Before_0$ takes the value of the *event* variable in the reform year and the pre-reform value of the *event* variable in all other years. $Before_1$ is equal to $Before_0$ forwarded by one year, and $After_1$ is equal to $Before_0$ lagged by one year. Finally, $After_2$ is equal to the *event* variable lagged by two years. If the liberalization reforms were endogenous to the development within the domestic banking sector, we should see significant changes in the lending behavior of domestic banks prior to the reform. Table 5.11 shows that this is not the case: The coefficient for $Before_1$ is insignificant for both inefficient (column 4) and efficient (column 5) domestic banks.[29] Hence, the decision to liberalize was not driven by current developments in the domestic banking sector. This assuages any remaining concerns of biases driven by endogeneity.

5.6.3 Concerns regarding alternative events

We also control for other reforms that took place in our sample countries that might have an influence on our results. We include indices for creditor rights and capital account liberalization that are obtained from papers by Djankov, McLiesh, and Shleifer (2007) and Abiad, Detragiache, and Tressel (2010), respectively. Column 6 of Table 5.11 provides estimation results for the bank-year observations where both indices are available. As expected, the coefficient for creditor rights is positive and significant at the 1%-level. Improvements in the protection of creditors induce an increase in the individual bank's supply of credit. In contrast, capital account liberalization does not have an influence on the loan supply of the average domestic bank, as indicated by the negative but insignificant coefficient. Importantly, the inclusion of the two indices does not affect our results on the efficiency of domestic banks; the interaction remains significant at the 1%-level.

5.6.4 Concerns regarding the efficiency classification of domestic banking markets

A potential concern using ROA as a measure for bank efficiency could be that this measure is influenced by market competition. I.e., banks operating in markets that are highly regulated and therefore not competitive earn a higher margin and, therefore, a higher ROA. As a final robustness check we use alternative criteria to classify banks prior to liberalization. As shown in Table 5.1, Panel C, we obtain two variables from the World Bank's Global Financial Development Database: The aggregate cost-

[29] As before we define a bank as initially efficient if it has an ROA in the top quartile of all banks in our sample in the year prior to liberalization.

to-income ratio and the non-performing loan (NPL) ratio of each banking sector of our sample countries. We use the values of these variables in the year before liberalization in the respective country and split our sample into countries with a higher than median cost-to-income ratio or NPL ratio and those with a lower than median ratio.

Results for the country-level regressions with these alternative classification criteria are presented in Table 5.12. Panel A shows that changes in financial structure are more pronounced in countries with a relatively high cost-to-income ratio, i.e., countries where the banking is rather inefficient at the time of liberalization. While aggregate lending evolves similarly in both groups of countries (columns 1 and 2), those with a higher cost-to-income ratio see a greater decline in domestic lending (columns 3 and 4), and—correspondingly—a greater increase in the foreign market share (columns 5 and 6). In Panel B we use the aggregate non-performing loans ratio as a classification criterion. Again, we find that changes in financial structure are more pronounced in countries with rather inefficient banks at liberalization, i.e., countries where the non-performing loans ratio is relatively high. These countries see a much greater decline in domestic lending following the event, which translates into a higher increase in the foreign market share.

5.7 Related literature and discussion

5.7.1 Related literature

Our paper connects to a large literature that debates the effect of financial liberalization on economic outcomes and presents a mixed set of results (see for a summary Beim and Calomiris 2001). Overall, there seems to be a trade-off between positive economic outcomes and instability associated with financial liberalization. The benign view of bank liberalization is that foreign bank entry improves the functioning of credit markets and this in turn promotes economic growth (e.g., Levine 1996, Levine 2001). This could be for several reasons. First, if domestic firms are financially constrained, which is likely to be the case in emerging and transition economies, entry of foreign banks relaxes financial constraints—a pure supply effect—and this promotes growth. Second, to the extent that foreign banks are more efficient in disbursing capital—due to superior screening and monitoring technology—their entry may lower the cost of providing funds, and improve the allocative efficiency of capital. This in turn promotes growth. Finally, entry of foreign banks may generate positive spill-over effects on existing domestic banks and this may again increase the efficiency of the banking system.[30]

There is, however, a malign view of financial liberalization as well. Some scholars (e.g., Vives 2001) have expressed strong concerns that allowing foreign bank entrance may increase the fragility of the banking sector and this may in turn hurt growth. Stiglitz (1994) discusses potential costs for domestic banks and local entrepreneurs as a consequence of foreign bank entrance (see also Aghion, Bacchetta, and Banerjee 2004), whereas Stiglitz (2000) argues that hastily financial liberalization was one of the major causes of financial and economic instabilities in East Asia and Latin America at the end of the last century.[31] Another channel through which foreign bank entry may reduce welfare is pointed out by Detragiache, Tressel, and Gupta (2008), who argue that foreign bank entry may hurt certain soft information borrowers and thus reduce welfare. Essentially, cream skimming of hard information borrowers leads to credit rationing of soft information borrowers (see also Sengupta 2007). We rationalize the differences between the two streams by arguing and showing that liberalization of banking markets has differential effects that depend on the efficiency of the domestic banks.

[30]These arguments go back to the original work of McKinnon (1973), Shaw (1973). Several empirical studies have analyzed this bright side view of opening banking markets. On a positive note, there is evidence that tighter regulations on bank entry increases the cost of financial intermediation (Demirgüç-Kunt, Laeven, and Levine 2004) and are associated with lower efficiency of the banks (Barth, Caprio, and Levine 2004). Similarly, bank branch deregulation in the U.S. improved the efficiency of financial intermediation (Jayaratne and Strahan 1998), promoted economic growth (Jayaratne and Strahan 1996) and had a positive impact on the creation of new incorporations (Black and Strahan 2002). Further, equity market liberalization seems to foster economic growth (Bekaert and Harvey 2004) and to reduce consumption growth volatility (Bekaert, Harvey, and Lundblad 2006).

[31]Empirically, Morgan and Strahan (2004) use international data from almost 100 countries and conclude that at least for non-industrial countries greater bank integration is associated with higher volatility. Moreover, several countries experienced banking sector crises shortly after the financial sector was deregulated, suggesting that indeed there is some connection between liberalization and financial fragility (Demirgüç-Kunt and Detragiache 1999, Kaminsky and Reinhart 1999). There is also a trade liberalization literature that cautions against the benefits of liberalization and stresses the importance of domestic institutions in fostering growth, e.g. Rodriguez and Rodrik (2000) express reservations against the bright side view of liberalization and assert that contrary to expectations, liberalization may be detrimental to growth.

Several papers empirically examine the impact of foreign bank entry on the efficiency of financial intermediation (Berger et al. 2000, Claessens, Demirgüç-Kunt, and Huizinga 2001, Dages, Goldberg, and Kinney 2005, Degryse et al. 2012, Giannetti and Ongena 2012). Bonin, Hasan, and Wachtel (2005a) provide evidence that banks with a strategic foreign owner in 11 Eastern European transition countries provide better services and are more cost efficient than private domestic or government banks. Potential spillover effects on domestic banks are documented by Claessens, Demirgüç-Kunt, and Huizinga (2001), who find that foreign entry improves the efficiency of domestic financial institutions (see also Unite and Sullivan 2003). The effect of foreign entry on firms' access to finance is subject to debate. For example, while Clarke, Cull, and Martinez-Peria (2006) find that all companies—including small and medium ones—report lower financing constraints with greater foreign bank participation in a sample of 35 emerging markets, Gormley (2010) shows for India that only a small set of very profitable firms benefited from foreign entry while the average firm is less likely to have a loan.[32] Other papers investigate whether increased foreign ownership in the banking sector has real effects for the economy. While Bruno and Hauswald (2009) find that foreign banks seem to relax financial constraints and hence promote economic growth, evidence from other studies (Berger, Klapper, and Udell 2001, Mian 2006, Detragiache, Tressel, and Gupta 2008, Giannetti and Ongena 2009) suggests that these benefits materialize only for large and transparent companies. Our paper adds to this literature as it sheds light on the exact relationship between foreign bank entry, the development of the domestic banking sector, economic growth and industry structure, thus helping to explain the controversial findings mentioned above.

Our paper is also related to literature following Lucas (1990) famous article on why we do not observe capital flows from developed countries to emerging markets, although emerging markets have lower levels of capital per worker and hence a higher marginal product of capital. Gourinchas and Jeanne (2013) document an interesting feature of this puzzle: They show that foreign capital does not even flow to those emerging markets that have grown fastest in the past and hence revealed their high

[32]Several other papers examine the impact of foreign ownership on bank lending and lending conditions, see e.g. De Haas and Van Lelyveld (2011), Jeon, Luca, and Wu (2011), Claeys and Hainz (2013).

marginal productivity. But a poor country's inability to obtain foreign financing does not seem to hurt its growth prospects: Bosworth and Collins (1999) find that nonindustrial countries that relied less on foreign capital grew faster over the period 1970–2004. Similarly, Prasad, Rajan, and Subramanian (2007) show that, among nonindustrial countries with high rates of investment, those that relied less on foreign capital grew faster than those that relied more on foreign capital. As an explanation for this finding they suggest that nonindustrial countries do not have corporations or financial systems to channel the arm's-length foreign capital into its most productive uses. We provide evidence from bank liberalization that is supportive of their claim that countries with underdeveloped financial sectors are unlikely to be able to use foreign capital to finance growth.

5.7.2 Conclusion

This paper argues that the nature of financial structure (supply of financing) impacts the industry structure through its influence on the allocation of credit to firms within industries and across industries. We exploit the variation in the efficiency of the domestic banking sector at the time of liberalization across 26 emerging economies to identify large changes in the nature of credit provision within an economy. Following liberalization there is an increase in the aggregate supply of credit in countries with relatively efficient domestic banks. In markets with relatively inefficient banks, foreign lending largely crowds out domestic lending. There is a higher growth rate and lower growth volatility for industry sectors in markets with relatively more efficient domestic banks following liberalization. These results are driven by industries that are more reliant on external financing and by smaller firms in economies with more efficient domestic banks. To the contrary, in particular smaller firms are negatively affected in countries with relatively inefficient domestic banks, resulting in a negative impact on growth and volatility for the average industry in these countries. Thus, the timing of liberalization of credit markets interacts with the efficiency of the incumbent domestic banking sector which has implications on the allocation of credit and overall economic growth. Our findings illustrate that domestic institutions need to be sufficiently developed for liberalization to have a positive impact.

Table 5.9: Firm-level evidence—robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\frac{\text{Debt}}{\text{Initial Assets}}$	$\frac{\text{Debt}}{\text{Initial Assets}}$	$\frac{\text{Debt}}{\text{Initial Assets}}$	$\frac{\text{Debt}}{\text{Initial Assets}}$	$\frac{\text{Debt}}{\text{Initial Assets}}$	Log(debt)	$\frac{\text{Debt}}{\text{Assets}}$	$\frac{\text{Debt}}{\text{Initial Assets}}$
Event	-0.157**	-0.223**	-0.221**	-0.220**	-0.213**	-0.557***	-0.057**	-0.223**
	(0.050)	(0.082)	(0.080)	(0.081)	(0.081)	(0.158)	(0.021)	(0.083)
Interaction (event × d(initial ROA))		0.137***	0.157***	0.068***	0.103***	0.309***	0.022***	0.136***
		(0.018)	(0.027)	(0.011)	(0.014)	(0.071)	(0.006)	(0.014)
Interaction (event × d(exdep))			-0.003					
			(0.008)					
Interaction (event × d(exdep) × d(initial ROA))			-0.039					
			(0.025)					
Interaction (event × d(small))				-0.012				
				(0.017)				
Interaction (event × d(small) × d(initial ROA))				0.127**				
				(0.038)				
Interaction (event × d(young))					-0.014*			
					(0.006)			
Interaction (event × d(young) × d(initial ROA))					0.095**			
					(0.039)			
Log(sales)	0.268***	0.264***	0.263***	0.257***	0.257***	0.467***	-0.007	0.262***
	(0.059)	(0.059)	(0.058)	(0.057)	(0.058)	(0.047)	(0.005)	(0.056)
Tangibility	0.382**	0.385**	0.382**	0.379**	0.375**	0.821***	0.032	0.408***
	(0.119)	(0.117)	(0.116)	(0.116)	(0.118)	(0.214)	(0.031)	(0.113)
EBIT/Assets	-0.591***	-0.554***	-0.554***	-0.549***	-0.552***	-1.662***	-0.188***	-0.550***
	(0.038)	(0.040)	(0.041)	(0.040)	(0.039)	(0.102)	(0.016)	(0.050)
Observations	17,020	17,020	17,020	17,020	17,020	30,489	30,489	17,020
Distinct firms	2,755	2,755	2,755	2,755	2,755	7,228	7,228	2,755
R-squared	0.215	0.224	0.225	0.228	0.226	0.143	0.093	0.283
Firm effects	YES	YES	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES	YES	YES
Industry-year interactions	NO	NO	NO	NO	NO	YES	YES	YES

This table provide robustness checks for the firm level results. Columns 1-5 repeat the the estimations shown in Table 5.8 and use the ratio of total debt to total *pre-event* assets as a dependent variable instead. Columns 6-8 include industry-year interactions in addition to firm and year indicators. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

Table 5.10: Selection of takeover banks

	(1) Takeover	(2) Takeover	(3) Takeover	(4) Takeover	(5) Takeover	(6) Takeover
ROA	-0.006			-0.000		
	(0.006)			(0.021)		
$\frac{\text{Cost}}{\text{Income}}$		-0.005			-0.031	
		(0.011)			(0.047)	
$\frac{\text{Cost}}{\text{Assets}}$			0.156			-0.594
			(0.206)			(0.575)
Log(assets)	0.135**	0.129**	0.133**	0.167**	0.167**	0.156**
	(0.062)	(0.060)	(0.061)	(0.069)	(0.066)	(0.068)
Market share	-0.013	0.030	-0.002	0.002	-0.027	0.088
	(1.152)	(1.139)	(1.150)	(1.420)	(1.417)	(1.416)
Solvency	0.004	0.003	0.003	0.005	0.005	0.004
	(0.004)	(0.003)	(0.003)	(0.007)	(0.006)	(0.006)
Liquidity	0.056	0.059	0.059	0.820*	0.828*	0.798*
	(0.245)	(0.246)	(0.245)	(0.464)	(0.467)	(0.464)
Observations	6,452	6,440	6,452	760	760	760
Pseudo R-Squared	0.154	0.154	0.154	0.129	0.129	0.130
Year effects	YES	YES	YES	YES	YES	YES
Country effects	YES	YES	YES	YES	YES	YES

The table reports estimated coefficients and standard errors from probit models estimated with maximum likelihood. The dependent variable *Takeover* is a dummy that takes the value of one if a bank is taken over by a foreign bank during the sample period and zero otherwise. Columns 1-3 include all bank-year observation and the value of the explanatory variables in the respective year. In columns 4-6 we collapse the data on the bank level and use mean values of the explanatory variables. Standard errors adjusted for clustering at the bank level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

Table 5.11: Bank-level loans—robustness

	(1) Log(loans) Few takeovers	(2) Log(loans) Many takeovers	(3) Log(loans)	(4) Log(loans) Inefficient banks	(5) Log(loans) Efficient banks	(6) Log(loans)
Event	-0.322	-0.405	0.045			-0.177
	(0.255)	(0.245)	(0.155)			(0.157)
Interaction (event	0.988***	0.654**	0.534**			0.722***
× d(efficient bank))	(0.292)	(0.233)	(0.221)			(0.189)
Before_1				-0.104	-0.253	
				(0.138)	(0.221)	
Before_0				-0.04	-0.28	
				(0.182)	(0.193)	
After_1				0.181	0.05	
				(0.179)	(0.131)	
After_2				0.227	0.323*	
				(0.271)	(0.186)	
Capital account liberalization						-0.015
						(0.074)
Creditor rights						0.850***
						(0.172)
Observations	2,474	2,130	2,742	2,065	677	3,477
Distinct banks	280	262	409	320	89	497
R-squared	0.257	0.278	0.486	0.528	0.556	0.129
Year effects	YES	YES	YES	YES	YES	YES
Bank effects	YES	YES	YES	YES	YES	YES
Macro controls	NO	NO	YES	YES	YES	NO
Bank controls	NO	NO	YES	YES	YES	NO

The table reports additional robustness checks for the bank level results reported in Table 5.2. Dependent variable in all regressions is the logarithm of the loan supply of private domestic banks. Variables of interest are the event variable and an interaction between liberalization and a dummy variable taking a value of 1 if the bank is among the 25% most efficient banks in the year of liberalization. In columns 1 and 2 we distinguish between markets were foreign banks entered mostly via greenfield investments and markets where foreign banks entered mostly via takeover. The specification in column 3 includes macro and time varying bank control variables, and in columns 4 and 5 we use lags and leads of the event variable in order to investigate the dynamics around liberalization, where column 4 includes only inefficient and column 5 includes only efficient domestic banks. Finally, column 6 controls for capital account liberalization and creditor rights. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

Table 5.12: Measures of bank efficiency—robustness

Panel A: Cost-to-Income Ratio

	(1) Aggregate loan supply High CIR	(2) Aggregate loan supply Low CIR	(3) Domestic loan supply High CIR	(4) Domestic loan supply Low CIR	(5) Foreign market share High CIR	(6) Foreign market share Low CIR
Event	0.226	0.243	-0.673	-0.090	0.289**	0.107**
	(0.341)	(0.168)	(0.409)	(0.234)	(0.094)	(0.044)
Observations	156	169	155	169	156	169
Distinct countries	12	13	12	13	12	13
R-squared	0.654	0.704	0.407	0.487	0.711	0.444
Year effects	YES	YES	YES	YES	YES	YES
Country effects	YES	YES	YES	YES	YES	YES
Macro controls	YES	YES	YES	YES	YES	YES

Panel B: NPL Ratio

	(1) Aggregate loan supply High NPL	(2) Aggregate loan supply Low NPL	(3) Domestic loan supply High NPL	(4) Domestic loan supply Low NPL	(5) Foreign market share High NPL	(6) Foreign market share Low NPL
Event	-0.027	0.279	-1.110**	-0.150	0.331***	0.154***
	(0.301)	(0.301)	(0.366)	(0.335)	(0.096)	(0.049)
Observations	156	143	155	143	156	143
Distinct countries	12	11	12	11	12	11
R-squared	0.669	0.656	0.427	0.511	0.669	0.650
Year dummies	YES	YES	YES	YES	YES	YES
Country dummies	YES	YES	YES	YES	YES	YES
Macro controls	YES	YES	YES	YES	YES	YES

This table provides results for different definitions of the bank efficiency variables. Panel A uses the aggregate cost-to-income ratio while Panel B uses the non-performing loans ratio, where both variables are obtained from the World Bank and measured in the year before liberalization in the respective country. The dependent variable is the logarithm of aggregate loans in columns 1 and 2, the logarithm of total loans from domestic banks in columns 3 and 4, and the foreign market share in columns 5 and 6. Standard errors adjusted for clustering at the country level are reported in parentheses. * indicates statistical significance at the 10%-level, ** at the 5%-level and *** at the 1%-level.

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