Three Essays on Financial Economics

D I S S E R T A T I O N

zur Erlangung des akademischen Grades
doctor rerum politicarum
(Doktor der Wirtschaftswissenschaft)

eingereicht an der

Wirtschaftswissenschaftlichen Fakultät
der Humboldt-Universität zu Berlin

von

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Tag des Kolloquiums: 4. Juli 2022

To Ziggy Stardust

Acknowledgements

I would like to thank my supervisor, Alex Stomper, who provided me with valuable feedback and support throughout my time as a PhD student. Moreover, I am grateful to Max Bruche, Jan Keil, and Farzad Saidi for their insightful feedback and stimulating discussions, which tremendously helped my progress as a researcher. I would also like to thank my collaborators. Philip Schaz contributed to the first chapter - thank you for the your support and guidance. I also extend my thanks to Simon Baumgartner, with whom I had many engaging discussions about chapter three. I am grateful to Philipp König and Christian Bittner for their invitation to a research visit at the Bundesbank, which inspired my third chapter. I would also like to thank the fellow PhD students that I met along the way for the stimulating discussions and invaluable memes, especially Hira Ghaffar, Benjamin Grosse-Rüschkamp, Friederike Heiny, Viola Kuzmova, and Constantin Reigber.

For Chapter One, I would like to thank Simon Baumgartner, Tobias Berg, Christian Bittner, Max Bruche, Marco Corsi, Matthias Kaldorf, Benjamin Grosse-Rüschkamp, Boryana Ilieva, Jan Keil, Thomas Krause, Beatriz Mariano, Karsten Müller, Farzad Saidi, Alex Stomper, Tobias Witter, the participants of the financial regulation workshop at the University of Zürich (October 2021), as well as the participants of the finance workshop at the University of Münster (November 2021) for valuable comments. For Chapter Two, I would like to thank Max Bruche, Beatriz Mariano, Alex Stomper, Farzad Saidi for valuable comments and my former working colleagues at Bruegel, and especially Guntram Wolff for setting the scene. For Chapter Three, I would like to thank Luise Peltzer for excellent research assistance, and the participants of the workshop on instrumentalizing economics for political goals (October 2021) organised by the Wissenschaftszentrum Berlin für Sozialforschung for valuable comments. Furthermore, I am grateful for all the feedback received from the participants of the finance brown bag seminars at Humboldt University.

Finally, I extend my sincerest thanks to Alice, my friends, and my Mom, who were always on my side with care, encouragement, and never-ending support along this journey. And to my dad, who was often in my thoughts – you are missed.

Berlin, February 2022

Pia Hüttl

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Abstract

Abstract - English

This thesis consists of three independent chapters linked by the European Debt Crisis as their common theme. Chapter One studies the effect of central bank collateral policy on bank credit supply and the real economy. In 2007, the European Central Bank replaces national collateral lists with a single list valid across the euro area. Banks holding newly eligible assets experience a reduction in their cost of funding. Consequently, these banks lend more compared to banks without such assets, especially to riskier and less productive borrowers located in other euro area countries. The borrowers in turn experience growth in employment and investment. The results highlight the unintended role of financial integration in fueling cross-border credit booms. Chapter Two investigates the political ties of too-big-to-fail bank boards in crisis times. We argue that after a bailout, governments are likely to influence bank board compositions to secure control rights. Combining two novel datasets on political ties of banks and state aid in the European Union, we find that the number of politically connected board members increases by 21.4% following government support. Bailed-out banks with such new political ties perform better in terms of market capitalisation and valuation than bailed-out banks without such ties. This evidence suggests a role of political board members in providing valuable information during crisis times. Chapter Three provides causal evidence on the effect of credit crunches on political radicalisation. We combine data on bank-firm connections and electoral outcomes at the city-level during the 2008-2014 Spanish Financial Crisis. First, we show that firms in a relationship with weak banks experience a reduction in their loan supply and employment growth. Next, we estimate the effects of unemployment on voting behaviour. We construct an instrument for unemployment based on the city-level exposure to foreign weak banks. We find that a one standard deviation increase in instrumented unemployment translates into a 7 percentage point increase in the radicalisation of voters.

Abstract - German

Diese Dissertation besteht aus drei unabhängigen Kapiteln, die durch die europäische Schuldenkrise als gemeinsames Thema verbunden sind. Kapitel eins untersucht die Auswirkungen der Sicherheitenpolitik der Zentralbank auf das Kreditangebot der Banken und die Realwirtschaft. Im Jahr 2007 ersetzt die Europäische Zentralbank die nationalen Sicherheitenliste durch eine einzige, für den gesamten Euroraum gültige Liste. Für Banken, die solche neu zugelassenen Sicherheiten halten, sinken die Finanzierungskosten. Folglich vergeben diese Banken mehr Kredite als Banken ohne solchen Sicherheiten, insbesondere an risikoreichere und weniger produktiven Kreditnehmern in anderen Ländern des Euroraums. Bei den Kreditnehmern wiederum nehmen Beschäftigung und Investitionen zu. Die Ergebnisse verdeutlichen die unbeabsichtigte Rolle der Finanzintegration beim Anheizen grenzüberschreitender Kreditblasen. Kapitel zwei untersucht die politischen Verbindungen von Bankvorständen in Krisenzeiten. Wir argumentieren, dass Regierungen nach einer staatlichen Bankenrettung die Zusammensetzung von Bankvorständen beeinflussen, um sich Kontrollrechte zu sichern. Die Kombination zweier neuartiger Datensätze zu politischen Verbindungen von Bankvorständen und staatlichen Beihilfen in der Europäischen Union erlaubt uns festzustellen, dass die Anzahl der politischen Vorstandsmitgliedern nach einer staatlichen Unterstützung um 21,4% steigt. Gerettete Banken mit solchen neuen politischen Vorständen schneiden in Bezug auf Marktkapitalisierung und Bewertung deutlich besser ab als gerettete Banken ohne solche Verbindungen. Dies deutet auf eine Rolle politischer Vorstandsmitglieder bei der Bereitstellung wertvoller Informationen in Krisenzeiten hin. Kapitel drei liefert kausale Belege für die Auswirkungen von Kreditkrisen auf politische Radikalisierung. Wir kombinieren Daten zu Bank-Firmen-Verbindungen und kommunaler Wahlergebnissen während der spanischen Finanzkrise 2008-2014. Zunächst zeigen wir, dass Unternehmen mit einer Beziehung zu schwachen Banken einen Rückgang ihres Kreditangebots und des Beschäftigungswachstums erleben. Anschließend schätzen wir die Auswirkungen der Arbeitslosigkeit auf das Wahlverhalten. Wir konstruieren ein Instrument für die Arbeitslosigkeit, das auf der Abhängigkeit gegenüber schwachen ausländischen Banken auf kommunaler Ebene basiert. Wir stellen fest, dass ein Anstieg der instrumentierten Arbeitslosigkeit zu einer Steigerung der Wählerradikalisierung um 7 Prozentpunkte führt.

Introduction

"Within our mandate, the ECB is ready to do whatever it takes to preserve the euro. And believe me, it will be enough."
Mario Draghi, President of the European Central Bank, 26 July 2012

Placing itself in the empirical banking and political economy fields of research, this thesis has the European debt crisis as its common theme. The first Chapter focuses on the period before the European debt crisis. While much is known about the crisis' negative fallout (Acharya et al. (2019), Popov and Van Horen (2015)), little attention has been paid to the boom period preceding the bust. Using a 2007 ECB collateral policy framework change, this Chapter sheds light on the role of central bank policy in those intra-euro area capital flows and its real effects. Chapter Two focuses on the European debt crisis once it unfolded. As billions of tax payer money was flowing into ailing banks, researchers theorised the most efficient state aid designs (Philippon and Schnabl (2013), Bruche and Llobet (2014)). This Chapter empirically verifies the role of governments in influencing bank board composition after state aid interventions and its impact on bank performance. Chapter Three looks at the debt crisis' aftermath, characterized by rising anti-establishment sentiment (Rodrik (2018), Algan et al. (2017)). As yet another level of real effects of banking crises, this Chapter investigates the channel through which credit crunches impact political radicalisation.

In Chapter One, I shed light on collateral policy, an important part of a central bank's policy tool kit. I show that when central banks change their collateral policy, this has an impact on both bank lending and the real economy. Banks with eligible assets on their balance sheet increase their lending in the syndicated loan market by

an economically significant 8.3% compared to unaffected banks. Consequently, these banks lend more to riskier and less productive borrowers located in other euro area countries. The borrowers in turn experience growth in employment and investment. These results have important lessons concerning the unintended consequences of financial integration. While increasing cross-border capital flows were a welcomed part of the euro area financial integration in the 2000s, I find that banks residing in the core of the euro area, instead of increasing loans towards productive sectors, funded riskier and less-productive borrowers in other euro area countries, and especially in the GIIPS. This result highlights the trade-off between financial stability and financial deepening, which is at the heart of macro-prudential policies (Giannetti and Jang (2020)). Similarly, these findings expand our understanding of the workings of collateral policy. My results show real effects of collateral policy. While this has been shown for both conventional and unconventional policies (Acharya et al. (2019)), I provide evidence that collateral policy, too, impacts firm level outcomes, such as employment and investment. This fills a gap in the literature and highlights the importance of considering collateral policy as an equally important monetary policy tool.

In Chapter Two, co-authored with Philipp Schaz, we investigate the relationship between political connections of bank boards and bank performance in times of banking crises. We find that the number of politically connected board members increases by 21.4% following government support. Bailed-out banks with these new political ties perform better in terms of market capitalisation and valuation than bailed-out banks without these ties. This evidence suggests a role of political board members in providing valuable information during crisis times. First, we confirm theoretical work done by Aghion and Bolton (1992) on the incompleteness in financial contracting and control allocation. Applying this pecking order theory of government structures to bank bailouts, we show that governments indeed influence bank board compositions as a way to secure control rights after a state aid intervention. Moreover, the political composition of bank boards depends on the different types of state aid measures applied to the respective banks. Second, we document

that the newly appointed political board members influence bank performance. Trying to give an answer to what these new politically appointed directors will do, we find that they improve bank performance in terms of market capitalisation and valuation. As opposed to the prevailing argument in the literature that politicians only cater to their private interest (Shleifer and Vishny (1994)), this finding highlights the role of political board members in providing valuable information during crisis times (Downs (1957)). These findings highlight the need further theoretical work on the optimal bailout schemes and political board members.

In Chapter Three, co-authored with Simon Baumgartner, we provide causal evidence on the effect of credit crunches on political radicalisation. We combine data on bank-firm connections and on electoral outcomes at the city level during the Spanish Financial Crisis. First, we show that firms in a relationship with weak banks experience a reduction in loan supply. Next, we estimate the effects of unemployment on voting behaviour. We construct an instrument for unemployment based on the city-level exposure to foreign weak banks. We find that a one standard deviation increase in instrumented unemployment translates into a 7 percentage point increase in the radicalisation of votes. This Chapter expands our understanding of the channels through which financial crises radicalize voters. Financial crises, as opposed to other types of crises, are often at the heart of this radicalisation (Mian, Sufi and Trebbi (2014)). However, the specific channels behind these dynamics have remained a black box this far. Doerr et al. (2021) suggest antisemitism in Germany's banking crisis of the 1930s as a key driver behind the rise in Nazi votes, Gyongyosi and Verner (2021) find a debtor-creditor conflict at the heart of the far-right support in Hungary. We provide causal evidence in support of the model of Guiso et al. (2017): Rising economic insecurity leads to higher support for populist parties. In detail, we find that credit supply shocks lead to political radicalisation through rising unemployment risk.

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Chapter 1

Flight Abroad within the Euro Area: Evidence From an ECB Collateral Framework Change

1 Introduction

Does a central bank's collateral policy impact bank lending? And if so, how did it contribute to the boom-bust cycle of the euro area in the 2000s? Much is known about the negative fallout from the European debt crisis in 2010-2012 (Acharya et al. (2018) and Popov and Van Horen (2015)). However, little attention has been paid to the boom period preceding the bust, and the role of central bank policy in those intra-euro area capital flows. Looking at that period helps us better understand the mechanisms behind the build up of such imbalances. Monetary policy impacts economies not only domestically (Bernanke and Blinder (1992)) but also internationally (Bremus and Fratzscher (2015), Temesvary, Ongena and Owen (2018)), in this case spreading permissive financial conditions across countries (Bruno and Shin (2015)). Using a ECB collateral policy framework change, this chapter sheds light on the role of central bank policy in the last phase of the euro area credit boom.

As a first step, this chapter verifies empirically the importance of central bank collateral policy for the real economy via the supply of bank credit to firms. Following the collateral framework change, banks which have eligible assets on their balance sheet increase their lending by 8.3% compared to unaffected banks, once loan demand is controlled for. In a second step, interpreting this collateral framework change as a positive shock to funding costs for affected banks allows me to investigate the differential capital flow dynamics within the euro area (Giannetti and Laeven (2012). I find that affected core banks exhibit a "flight abroad" effect, increasing lending towards borrowers located in other euro area countries, and especially in the GIIPS.[1] Also, the banks affected invest into riskier and less-productive borrowers, active in the non-tradable sector. On the firm level, the effects translate into real outcomes: In the two years following the framework change, firms with a relationship to those banks experience an increase in employment and in investment. This is especially true for riskier and less productive firms.

[1] GIIPS are Greece, Ireland, Italy, Portugal, Spain.

I focus on the changeover to a single collateral list implemented by the ECB in January 2007, a crisis-unrelated policy change. Before the changeover, each national central bank set different collateral rules. Banks, depending on their location, could pledge different assets in their refinancing operations with the respective national central bank. This was the case for bank loans; their eligibility varied significantly across the euro area. In most euro area countries, only loans held from domestic firms were eligible, and both the terms and the maximum amounts differed. After the introduction of the single collateral list, the eligibility was streamlined, and euro area banks could now pledge bank loans which had originated in the whole euro area as collateral. I classify banks into "affected" and "unaffected" to account for the different exposure to other, non-domestic, euro area borrowers in their syndicated loan portfolio prior to the framework change.

I use a difference-in-difference approach to tackle the impact of a collateral framework change on bank lending. In doing so, I compare changes in the lending of "affected" banks which actively issue loans to firms in other euro area countries on the syndicated loan market (treated) relative to a control group of "unaffected" banks which are less active in euro area cross-border lending (control). My identifying assumption is that unaffected banks provide a counter-factual for the lending of the banks affected in the absence of a framework change.

A possible argument against the causal interpretation of the estimated effects is that my treatment lacks random assignment. Instead, it is based on a variable that affects treated and control groups differentially and correlates with the framework change. To tackle such endogeneity concerns, I include bank level control variables for size, leverage profitability, and for a bank's liquidity position on both the asset and the liability side. Another concern is that the borrower pool is not orthogonal to a bank's loan portfolio. In other words, borrowers might differ for affected and unaffected banks, and hence banks would face different investment opportunities after the framework change. To address this concern, I add firm $\times$ quarter fixed effects in the most stringent specification, which absorb any time-varying difference

in firm-specific factors such as loan demand, along the lines of Khwaja and Mian (2008). Another possible argument is that the framework change in January 2007 coincides with other events that affect bank lending. As long as these events affect both the treatment and control group the same way, they are taken care of by the Difference-in-Difference setup. If this was not the case, then my results could be biased. The financial crisis in 2008/09 is such a possible confounding event. While I argue that this framework change is a crisis-unrelated policy change, research has shown that European banks exposed to U.S. subprime market cut their lending already well ahead of the start of the financial crisis (Puri, Rocholl and Steffen (2011), Huber (2018)). Since affected banks are the ones more active on the European syndicated loan market, they might also be more active internationally. I take care of this by keeping the event window very short, from Q1 2006 to Q2 2008. As a robustness check, I restrict the analysis to four quarters before and after the framework change; the results remain unchanged.[2] In addition, it might be that banks self-select into the treatment group by anticipating the framework change. While the ECB's changeover to a single collateral framework was announced two years in advance, the details of the assets to be included were not publicly known before the announcement date. Lastly, I confirm the parallel trend assumption using parametric tests: loan issuance between affected and unaffected banks did not differ systematically in the period prior to the framework policy change.

My analysis makes the following contributions to the literature. First, I add to the literature on the effect of monetary policy on cross-border capital flows.[3] Bremus and Fratzscher (2015) demonstrate that more expansionary monetary policy in the source country induces higher bank lending abroad. Temesvary, Ongena and Owen (2018) focus on Turkey and US monetary policy and find similar results. Narrowing it down to cross-border banking flows, Giannetti and Laeven (2012) find that the supply

[2] This confounding event would at most bias my results downward. Banks affected by the financial crisis usually decrease lending to the corporate sector (Chodorow-Reich (2014)), while I expect the framework change to stimulate loan issuance of affected banks.

[3] "Cross-border" in my case relates to the capital flows within the euro area, subject to a single monetary policy by the ECB.

of credit increases when cost of funding in the lender's home market decreases.[4] Interpreting the ECB framework change as such a reduction in the cost of funding, I find that the "flight abroad" effect is more pronounced for affected banks residing in the core of the euro area. Consequently, those banks lend to riskier and less productive borrowers in the non-tradable sector located in other euro area countries. The results highlight the unintended role of financial integration in fueling cross-border credit booms: investment into such industries has been identified in the literature as fostering boom-bust credit cycles (Gourinchas and Obstfeld (2012), Müller and Verner (2021)) as opposed to funding productive investment (Levine (2005)).

Second, by looking explicitly at collateral policy as part of a central bank's toolkit, I expand the literature on the bank lending channel. This literature highlights the role of bank balance sheet liquidity, bank equity, and negative rates in impacting the transmission of monetary policy (Kashyap and Stein (2000), Jiménez, Ongena et al. (2012), Heider, Saidi and Schepens (2019)). On bank collateral, Koulischer and Struyven (2014) provide a micro-foundation on how the transmission of monetary policy depends on it: Changes in collateral reduce funding costs, leading to an increase in bank lending. Heider and Hoerova (2007) look at secured vs. unsecured interbank markets and find that a wider range of available collateral alleviates banks' funding constraints. Empirically, several papers related to crisis-induced changes in the collateral framework and its impact on bank lending exist. Van Bekkum, Gabarro and Irani (2018) document that changes in collateral eligibility concerning residential mortgage backed securities (RMBS) affects bank lending behaviour in the mortgage market. Delatte, Garg and Imbs (2019) find that a collateral framework change linked to the 2012 Additional Credit Claims (ACC) programme has an positive impact on credit volumes supplied to firms in France. Corradin, Heider and Hoerova (2017) find similar results in terms of asset prices. This chapter is the first to examine empirically how collateral policy in crisis-unrelated times impacts bank

[4] In a more recent chapter, Giannetti and Jang (2020) focus on the question who lends before crises, and show that it is especially new banks entering a foreign market which take on risk during periods preceding banking crises.

lending behaviour to firms participating in the syndicated loan market and its real effects.

This chapter also contributes to the ever growing literature on real effects. Chodorow-Reich (2014)'s important work on firm-bank relationships during crises reveals that firms with a relationship to banks affected by the Lehman crisis suffered more in terms of employment than firms without such a connection. Acharya et al. (2018) tackles with a similar set-up the real effects of the European debt crisis and the unconventional monetary policy in the euro area (Acharya et al. (2019)). In the latter case, the authors document zombie lending by banks that remained weakly capitalized post OMT-announcement; firms receiving loans from such banks do not undertake real economic activity, but build cash reserves. Focusing on central bank collateral policy, Pelizzon et al. (2019) find that eligibility in the Eurosystem collateral framework has quantifiable effects on debt financing decisions of firms which issued newly eligible bonds. Mesonnier, O'Donnell and Toutain (2022) document that such eligibility translates also into a relative reduction in rates for new loans issued to eligible firms. My findings highlight real effects of collateral policy: Firms in a relationship to banks affected by collateral policy increase both employment and investment.

The rest of the chapter is organized as follows. Section 2 gives the background for the choice of the framework change, Section 3 describes data, variables and the empirical strategy. Section 3 presents the results, and Section 5 runs robustness checks. Section 5 concludes.

2 Institutional Setting

2.1 ECB framework change

On 1 January 2007, the ECB introduced a single collateral framework list valid for the whole euro area.[5] Prior to this, national central banks set the eligibility of collateral following a two-tier system. Tier-one assets consisted of marketable debt instruments fulfilling euro area-wide eligibility criteria. Tier-two assets fulfilled only national eligibility criteria, allowing central banks to incorporate peculiarities of the respective domestic banking sector in the collateral framework[6]. A major drawback of such an arrangement was that banks, depending on their respective locations, had a different collateral pool at their disposal. Therefore, substantial differences resulted when accessing the ECB refinancing operations. As a response to this shortcoming, the ECB introduced a single list of eligible collateral to replace the two-tier-system. Consequently, certain asset classes stopped being eligible, such as equities in Spain, Netherlands and Portugal. Others became eligible throughout the euro area.

In particular, the introduction of the single list of collateral established the eligibility of bank loans as the biggest asset class. Before the framework change, the respective national central banks of Germany, Austria, Spain, and France accepted bank loans to domestic companies as collateral. Cross-border loans were not accepted. Outside of these four countries, the respective national central banks did not accept any bank loans as collateral - neither domestic nor cross-border. The change introduced an aspect which was a novelty for all banks: not only domestic but also bank loans issued to other euro area borrowers could now be pledged as a collateral for ECB refinancing operations.

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[5] Central banks provide credit to banks conditional on collateralisation, applying eligibility criteria for the collateral to be provided.

[6] For a detailed account of the functioning of the Eurosystem Collateral Framework, see Bindseil (2017).

The ECB published the official announcement containing details on the inclusion of bank loans on 22 July 2005.[7] The eligible debtors are defined as non-financial corporations and general governments, located in the euro area. Restrictions apply, among others, on the quality of the loans and the minimum loan amount. Importantly, these criteria apply also to a syndicated member's share in a syndicated loan, which is the market this analysis is focusing on. The implementation started only one and a half year later, on 1 January 2007.[8]

With the inclusion of bank loans issued in the euro area in the list of eligible collateral, banks holding those assets experience a reduction in their cost of funding, as more assets can be pledged in ECB refinancing operations. Therefore, I expect banks with eligible loans on their balance sheet to increase their lending more than banks which do not have eligible loans on their balance sheet. The null hypothesis is that collateral policy is irrelevant for bank lending behaviour.

2.2 Euro area financial integration

Next, I try to disentangle the workings of the framework change amidst increasing financial integration in a heterogeneous currency union. After the introduction of the Euro as a common currency in 1999, there was a major increase in cross-border banking activity within the euro area. Similar to what Giannetti and Laeven (2012) find on a global scale, one can observe a "flight abroad" effect in the euro area during the 2000s, as capital was flowing from the core to the GIIPS countries. Figure 1.1 shows the highly divergent current account balances of both the GIIPS and the core countries in the 2000s. In an influential paper, Blanchard and Giavazzi (2002) suggested that

> "this development is exactly what theory suggests when countries become more closely linked in goods and financial markets. To the extent

[7] See ECB press release "Eurosystem collateral framework: inclusion of non-marketable assets in the Single list", 22 July 2005.

[8] It is reasonable to argue that the framework change is a crisis-unrelated policy change.

> that they are the countries with higher rates of return, poor countries should see an increase in investment. And to the extent that they are the countries with higher growth prospects, they should also see a decrease in saving. Thus, poor countries should run larger current account deficits. Symmetrically, richer countries should run larger current account surpluses". *(page 2, Blanchard and Giavazzi (2002))*

Yet, the financial crisis, and especially the European debt crisis revealed major imbalances within the currency union, which put into question the "catch-up growth" hypothesis. Angeloni, Merler and Wolff (2012) find that as capital was flowing from the core to the GIIPS countries, investment was mainly into the less productive, non-tradable sector.

The ECB framework change was introduced at the height of the euro area cross-border credit boom. It was intended to foster financial integration even further.[9] Hence, when looking at the framework change as an exogenous shock to bank's funding costs, one can expect affected banks to react differently, depending on whether they reside in the core or the GIIPS countries. I expect affected banks residing in the core of the euro area to increase the euro area bias in their loan portfolio. I also expect affected banks to invest in the non-tradable sector (less-productive sector) and in riskier borrowers, typical of boom-bust patterns.[10]

In sum, I want to test the following: First, whether the ECB framework change led to an increase in bank lending. Second, whether this decrease in cost of funding led to a "flight abroad" effect, and if so, whether these funds were flowing into riskier and less-productive borrowers.

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[9] See ECB's monthly bulletin: *"by increasing the liquidity of an entire asset class, such as bank loans, the single list of collateral promotes the smooth functioning of the euro area financial system."*

[10] Müller and Verner (2021) test the theory prediction that the sectoral allocation of credit matters for distinguishing between "good" and "bad" credit booms. They find that credit to non-tradable sectors, including construction and real estate, is associated with a boom-bust pattern in output.

3 Empirical Strategy and Data

3.1 Empirical Strategy

Bank-Firm-Level I use a difference-in-differences set-up to test these hypotheses. First, I compare the lending behaviour of banks with different euro area loan portfolios before and after the ECB collateral framework change in January 2007.

I classify banks into "affected" and "unaffected" to account for different exposure to other euro area borrowers in the banks' syndicated loan issuance. Assuming that the framework change is more important for banks which were already actively issuing loans to other euro area borrowers, I identify such banks according to their issuance history: First, I cumulate bank i's issuance by euro area borrower origin (excluding domestic) over the period prior to the collateral framework change, from Q1 2003 to Q2 2005.[11] I then cumulate the total loan issuance of bank i over the same period. The resulting ratio is the exposure measure $\textit{Affected(\%)}_{05,i}$, the ratio of bank i's loan issuance to other euro area borrowers over its total loan issuance in the period Q1 2003 to Q2 2005. I exclude domestic loan issuance from this measure.

I set-up the following baseline specification on the bank-firm-quarter-level:

$$y_{ijt} = \beta_1 \text{ Affected}(\%)_{05,i} \times \text{Post07}_t + \beta \mathbf{X'}_{i,t-4} + \mu_{ij} + \mu_{jt} + \epsilon_{ijt}, \qquad (1.1)$$

where y_{ijt} is the logarithm of (one plus) the loan issuances in million USD to firm j provided by bank i (as lead or participating bank) at time t. In subsequent specifications y_{ijt} can also refer to bank i's ex-ante risk taking, $ln(\sigma(ROA_f)^{3y})$, the three-year standard-deviation of loan-financed firm j's ROA from year t-3 to t-1. [12]. Affected$(\%)_{05,i}$ is equal to the share of euro area loans in bank i's total syndicated loan portfolio. Post07$_t$ is equal to one after the framework change was

[11] I use the issuance history up to Q2 2005, which covers the period before the official announcement of the inclusion of bank claims into the single collateral list on July 2005.
[12] This definition of risk taking behaviour follows Heider, Saidi and Schepens (2019).

implemented in January 2007.[13] $\mathbf{X'}_{i,t-4}$ is a vector of bank-level controls for size, leverage, profitability, and a bank's liquidity position, both on the asset (cash ratio, liquidity ratio) and on the liability side (deposits ratio), all lagged by 4 quarter. μ_{ij} denotes bank × firm fixed effects and μ_{jt} country or firm × quarter fixed effects.

In the baseline specification the event window is relatively short, from Q1 2006 to Q2 2008, allowing for 4 quarters before and 6 quarters after the framework change, to take care of potential confounding events. The financial crisis in 2008/09 is such a possible confounding event. While I argue that the collateral framework change is a crisis-unrelated policy change, research has shown that European banks exposed to U.S. subprime market cut their lending already well ahead of the start of the financial crisis (Puri, Rocholl and Steffen (2011), Huber (2018)). As a robustness check, I restrict the event window to Q1 2006 to Q4 2007, the results remain unchanged.[14] The coefficient of interest, β_1, measures how affected banks respond to the framework change relative to the control group. I expect $\beta_1 > 0$, as lower funding costs for affected banks stimulates loan issuances after the framework change. This is in line with Koulischer and Struyven (2014), who argue that banks holding eligible assets experience a reduction in their cost of funding, as more assets can be pledged in central bank refinancing operations. I cluster standard errors on the bank-level, the level at which the treatment occurs, to adjust for serial correlation within treated units.

My identifying assumption is that banks less active in cross-border euro area loan syndication (unaffected) provide a counter-factual for banks more actively lending to euro area firms (affected) in the absence of a framework change.

[13] Given that the average maturity of syndicated loans is 5 years in my sample, I work with the implementation date (Q1 2007) rather than the announcement date (Q2 2005). As a robustness check, I use the announcement date and find no significant results.

[14] This confounding event would at most bias my results downward. Since affected banks are the ones more active on the European syndicated loan market, they might also be more active internationally. Banks hit by the financial crisis usually decrease lending to the corporate sector Chodorow-Reich (2014). By contrast, I expect the framework change to stimulate loan issuance of affected banks.

For a causal interpretation of the estimated effects, the following concerns need to be dismissed. A first concern is that my treatment is lacking random assignment, and is based instead on a variable that affects treated and control group differentially and correlates with the framework change. To tackle this, I include bank-level control variables for size, leverage profitability, and a bank's liquidity position, both on the asset and on the liability side. Another concern is that the borrower pool is not orthogonal to a bank's loan portfolio. In other words, borrowers might differ for affected and unaffected banks, and hence banks would face different investment opportunities after the framework change. To address this concern, I include bank-firm fixed effects and firm-quarter fixed effects. The former captures lending from the same bank to the same firm. The latter allows identifying loan supply, as I compare the lending of affected and not affected banks to the same borrower, absorbing loan demand, similar to Khwaja and Mian (2008).

Also, I exploit the fact that the framework change is relevant only for banks residing in the euro area. I build a placebo group of affected banks residing outside the euro area, but inside the EU, which are active in the euro area. I then run this "falsification test", confirming that the framework change does not have an impact on the placebo group of affected non-euro-area lenders.

Lastly, I confirm the parallel trend assumption through a parametric test. Loan issuance between the affected and the unaffected banks did not differ systematically in the period prior to the framework policy change.

Firm-Level As another set of analyses, I test whether the positive credit supply shock is reconfirmed on the firm-level, and trace potential real effects. Given data limitation, the analysis now is on a yearly, and not on a quarterly basis. Pelizzon et al. (2019) point out that the inclusion of bond issuing firms into the collateral framework of the ECB has an effect on firms' financing decisions. Also, Grosse-Rueschkamp, Steffen and Streitz (2019) find a capital structure effect of monetary policy on the firm-level. First, I test whether firms with a prior relationship to

affected banks experience a credit expansion. Here, I only consider banks with an above median share of euro area loans in their syndicated loan portfolio to be "affected" banks.

I estimate the following regression on the firm-level:

$$\Delta y_{jt} = \delta_1 \text{ Affected}_j \times \text{Post}_{07,t} + \delta \mathbf{X'}_{j,t-1} + \mu_{dt} + \mu_{ct} + \mu_{ct} + \epsilon_{jt} \qquad (1.2)$$

To test for a credit expansion on the firm-level due to the collateral framework change, the dependent variable is $pr(Loan)_{jt}$, an indicator variable equal to one if firm j obtains a bank loan in the respective year t, and zero otherwise. The treatment group indicator $Affected_j$ equals one if a firm has a lending relationship with an affected bank in 2006, prior to the implementation of the framework change, and zero otherwise. *Post* equals one after the implementation of the framework change in 2007, and zero otherwise. $\mathbf{X'}_{j,t-1}$ is a vector of time-varying firm-level controls to capture firm demand: log of total assets, leverage and liquidity, all lagged by one year. μ_j denote firm fixed effects, μ_{dt} denote industry $\times$ year and μ_{ct} country $\times$ year fixed effects. The sample period is from 2006 to 2008, allowing for 1 year before and 2 years after the framework change. The coefficient of interest, δ_1, measures the probability of obtaining a loan if the firm j has a lending relationship with an affected bank. I expect $\delta_1 > 0$, reflecting the positive coefficient estimate $\beta_1 > 0$ found on the firm-bank-level. In order to interpret the estimated coefficients as a loan supply effect also on the firm-level, I use firm fixed effects and country $\times$ year and industry $\times$ year fixed effects to absorb time-varying loan demand per country and per industry.

Second, I study whether this additional credit translates into real outcomes in terms of employment and investment on the firm-level. The dependent variables are *ln(Employment)*, the log of the number of employees at firm j in year t and *ln(Investment)* the log of investment, respectively. In this case, the coefficient of interest, δ_1, measures the impact on some real outcome if firm j has a lending relationship with an affected bank. I expect $\delta_1 > 0$, as firms, faced with an increased

credit supply from affected banks, increase both their number of employees as well as their investment.

3.2 Data

Lending Data I analyse banks' lending behaviour using Dealscan data on the syndicated loan market.[15] In this market, different banks form a syndicate to then jointly lend to a single borrower. The lending syndicate includes one lead bank and a number of participating banks. Lead arrangers are those members of a syndicate typically responsible for traditional bank duties, including negotiating conditions of deals, due diligence, and monitoring (Ivashina and Scharfstein (2010)). Participants are usually not in direct contact with the borrower, but merely supply credit. I consider both lending by lead arrangers and participants to capture total loan supply on the syndicated loan market, based also on recent work by Blickle et al. (2020).[16] Also, I restrict the sample to loans by banks to non-financial firms and consider lending only by commercial, savings, cooperative and investment banks.

I decompose syndicated loan deals into loan portions provided by each lender to obtain granular loan-level data. Whenever Dealscan provides information on lending shares of each bank, I use this information to split loan volume accordingly. In other cases, I follow Schwert (2018) to estimate lending shares via a tobit estimation using information on the facility amount, the number of participants, borrower and lender sales. Transactions with deal status 'canceled', 'suspended', or 'rumor' are removed and all loan nominations transformed into million U.S. Dollars (USD) using the spot exchange rate at origination, provided by Dealscan. If after this allocation procedure the loan portion is smaller than 10,000 USD, I drop the observation to

[15] Tabakis and Tamura (2013) provide an excellent overview of the pros and cons of using different types of credit claims, and specifically syndicated loans, as collateral in the Eurosystem.

[16] In their paper "the myth of the lead arranger", they find that lead arrangers sell their entire loan share for 27 percent of term loans and 48 percent of Term B loans, typically shortly after syndication.

remove erroneously small loans. I then aggregate all loan issuances between a bank-firm combination to obtain bank i's loan issuance to firm j in quarter t, which I define as a bank-firm-quarter observation. Total loan volume in a given quarter is the sum of all new loans issued by bank i to firm j. In doing so, I only account for transactions happening when a syndicated loan is issued, disregarding its maturity profile. I hence only account for flows on a bank-firm-quarter-level.

Table 1.1 presents summary statistics on the bank-firm-quarter-level over the sample period Q1 2006 to Q2 2008. The average loan issuance to firm j amounts to 171.13 million, the average spread over LIBOR to 120 basis points, and the average maturity of the loans to 5.4 years. Half of the loan issuances have at least one designated lead arranger, and are given out to firms located in the euro area. The share of issued loans to firms located in the euro area as well as to domestic firms amounts to approximately 24 % over total loan issuance, respectively. Domestic firms are defined as firms which have their headquarters in the same country as the corresponding bank.

Firm Variables To control for firm-characteristics, I obtain annual firm accounting data for European firms from Compustat. I aggregate the Dealscan bank-firm-quarter to the firm-year-level, to match borrowers in Dealscan with firms in Compustat, based on Chava and Roberts (2008), updated in April 2018. Combining those two databases reduces observations, since not all firms have balance sheet data available on Compustat, especially the smaller ones. Eventually, I obtain a sample of 1795 firms, 1192 of which have a pre-framework relationship with an affected euro area bank, and 603 which have not. Variables are winsorized at the 1st and 99th percentile. Financial firms (SIC codes 6000-6999) are dropped. Table 1.2 shows summary statistics. The average firm with an relationship to affected banks obtains loans with a larger volume, has more total assets, a higher market to book ratio and a higher dividend payout. Also, they employ more people and invest more.

Financial Variables To control for bank characteristics, I match the banks included in the DealScan database with bank balance sheet data from CapitalIQ (SNL financial). Table 1.3 presents summary statistics for all euro-area banks in the period prior to the framework change (Q1 2006 - Q4 2006) included in my sample. On the asset side, banks hold on average 66% loans and 32% securities over total assets. On the funding side, deposits make up 42% and equity 5.2% of total assets on average. The Return on Equity (ROE) amounts to 11.3% on average across the sample period.

Table 1.4 presents univariate evidence on the difference in bank characteristics between the treatment (affected) and control (unaffected) groups. Affected banks are banks which have an above median share of euro area loan issuances in their syndicated loan issuances in the period before the framework change was announced (Q1 2003 - Q2 2005). Affected banks are similar in terms of size and cash, but have a slightly higher loans-to-asset ratio (66.6 % vs 64.4%). On the funding side, affected banks rely more on deposits (44.7 % versus 38.0 %). Similarly, banks affected hold more securities (35.3 % vs 26.9 %), significant at the 5% level. It is important to highlight that such cross-sectional differences between treatment and control banks are no threat to the identification strategy, as they are differenced out in the difference-in-differences setup.

4 Main Results

I present the results in four steps. The first set of analyses are on the bank-firm-quarter-level. This allows me to include firm-quarter fixed effects in all specifications, which is a rigorous way of addressing the concern that affected and unaffected banks face different loan demand after the collateral framework change. In section 4.1, I find that affected banks increase their bank lending following the collateral policy framework change. Section 4.2 presents evidence that the framework change impacts the geographical distribution of banks' loan portfolios: Especially affected banks residing in the core increase lending towards the rest of the euro area. Next, I shed light on the borrower characteristics and risk-taking of affected banks. Section 4.3 provides evidence on the validity of the parallel trend assumption using a parametric test. Another set of analyses are on the firm-year-level. Section 4.4 evaluates the impact of collateral policy on the firm-level: I find that firms with a relationship to affected banks experience an increase in loan issuance, employment, and investment.

4.1 Effect of a Collateral Framework Change on Bank Lending

Table 1.5 presents the results from estimating Equation (1.1). Conditional on the firm receiving a loan, the dependent variable is log loan issuance plus one. Each column includes more stringent levels of fixed effects. Column (1) includes bank $\times$ firm fixed effects, which compares lending of affected banks versus unaffected banks to the same firm before and after the collateral framework change in January 2007. The coefficient of interest is positive but insignificant, while the Post07 indicator variable is positive and significant at the 10% level. Both affected and unaffected banks increase lending after the framework change. In Column (2), time-varying differences across banks driven by firms operating in different countries are taken care of by including country $\times$ time fixed effects. Also, I add firm $\times$ time fixed

effects to control for time-varying differences across banks driven by firms, i.e. loan demand. I find a positive treatment effect, significant at the 5% level: banks with more exposure towards other euro area firms increase their lending after the framework change. This result is also economically significant. A one-standard-deviation increase in a bank's affected ratio (13.80 percentage points) translates to a increase in bank lending of 8.3% ($0.006 \times 13.80 = 0.0828$). In Column (3), I add bank-level control variables to refine the comparison between treatment and control group. I include the log of assets, the leverage (equity) ratio, return on equity (ROE), cash, liquidity, and deposits over total assets as a proxy of a banks' liquidity situation, all lagged by 4 quarters. These are control variables which matter for the transmission of monetary policy as identified by the literature (Kashyap and Stein (2000)). The magnitude of the difference-in-differences estimate increases slightly to 0.007.

4.2 Flight Abroad Effect in the Euro Area and Borrower Characteristics

The results so far indicate that banks holding newly eligible euro area loans significantly increase their lending after the framework change, compared to banks without such an exposure. Interpreting this framework change as a positive shock to banks' funding costs, I now investigate the differential capital flow dynamics within the euro area. I first investigate the existence of a "flight abroad" effect by looking at the geographical distribution of affected core versus affected GIIPS banks' loan portfolios. Second, I look at borrower characteristics.

4.2.1 The Geographical Distribution of Loans

Following Giannetti and Laeven (2012), banks experiencing a decrease in their cost of funding increase their share of foreign loans during good times (so-called "flight abroad" effect). Table 1.6 investigates if similar patterns can be observed within

the euro area. When looking at the geographical distribution of loans, I differentiate between loan issuance of core- and GIIPS-lenders vis-a-vis a certain group of borrowers. In this case, I also add banks' country-time fixed effects to control for time-varying differences across banks driven by factors at the level of their home countries. Figure 1.3 provides a graphical representation of the coefficient estimates provided in Table 1.6.

First, I run the specification for banks located in the core (Column 1 to 4) and find a positive and significant treatment effect across different geographies. Affected core banks more active in the euro area syndicated loan market decrease lending to domestic borrowers (Column 1), a result which is statistically significant at the 10% level. At the same time, they increase lending to other euro area borrowers (Column 2) and especially to borrowers located in the GIIPS countries (Column 3). These results are statistically significant at the 1% level. I do not find evidence for an effect on lending to foreign borrowers (Column 4). In Column (5) to (7), I look at banks located in the GIIPS. I do not find a difference in lending of affected GIIPS banks to domestic (Column 5) or other euro area borrowers (Column 6), relative to unaffected GIIPS banks. Affected GIIPS banks increase lending towards borrowers located in foreign markets. This result is weakly significant, at the 10% level (Column 7).

These results confirm Giannetti and Laeven (2012)'s "flight abroad" effect in the euro area: Affected core euro area banks, which experienced a reduction in their funding costs due to the framework change, decrease their domestic loan issuance, and increase their loan issuance towards borrowers located in the rest of the euro area and especially in the GIIPS. At the same time, I can exclude the following alternative explanation: by including loans originated in the euro area into the list of eligible assets, euro area borrowers could have increased in attractiveness for affected banks. In this case, one should expect an increase in lending to other euro area borrowers from both core and GIIPS banks. However, the evidence does not support this hypothesis. Instead, the framework change seems to exacerbate the flight abroad effect, as capital was flowing from core to GIIPS countries.

4.2.2 Borrower Characteristics

The framework change happened in the run up to the financial crisis as well as the European debt crisis. It can hence shed light on the borrower characteristics in the boom period preceding the crisis years. Amidst such periods, credit quality deteriorates as lenders search for yield (Aliber and Kindleberger (2015) and Greenwood and Hanson (2013)) At the same time, recent work by Müller and Verner (2021) highlights that credit to non-tradable sectors, including construction, is associated with a boom-bust pattern in output. Here, I provide evidence that banks affected by the framework change extended their loans i) to riskier borrowers and ii) to the non-tradable sector, thus contributing to the euro area imbalances.[17]

Table 1.7 presents the results on a bank's ex-ante risk taking. I estimate again Equation (1.1), with the dependent variable being $ln(\sigma(ROA_f)^{5y})$ this time. $Ln(\sigma(ROA_f)^{5y})$ is defined as the five-year standard-deviation of loan-financed firm j's ROA from year *t-5* to *t-1*.[18]. In Column (1), I only include bank fixed effects. The estimate is positive but insignificant. The Post07 indicator variable is negative and significant at the 5% level. Both affected and unaffected banks decrease their risk-taking after the framework change. Once I add quarter $\times$ and country $\times$ year fixed effects to remove unobserved time-varying country factors of firms in Column (2), the estimate turns positive and significant at the 5% level: more affected banks finance riskier firms when the framework change happens. This effect is also economically relevant. A one-standard-deviation increase in the bank's affected ratio (12.903 percentage points) translates to an 19.1% increase in ROA volatility (0.015 $\times$ 12.903 = 0.1906). In Column (3), I add bank-level controls, which leave the difference-in-differences estimate almost unchanged. The evidence suggest that post-framework change, affected banks extend their loan to riskier borrowers.

[17] At the core of the euro area problems was also a large build-up of debt in the private sector, fueled by low borrowing costs and increasing financial integration of the euro area, as opposed to the narrative focusing only on public debt levels (see i.e. Sandbu (2015) for an excellent account of the euro crisis).

[18] This definition is standard in the literature, see Heider, Saidi and Schepens (2019).

I now turn to borrower characteristics in Table 1.8. In the first two Columns, I split the sample according to firms active in the tradable or the non-tradable sector. I classify the firms being active in one or the other sector based on firm j's primary SIC code reported in Dealscan following Müller and Verner (2021) and Giannetti and Jang (2020).[19] In Column (1) the coefficient of interest is positive but insignificant for firms active in the tradable sector. Column (2) reports that affected banks increase their loan issuance especially with respect to firms active in the non-tradeable sector. This result is significant at the 5% level. In Column (3) and (4) I split the borrower sample into bottom and top half according to their ROA volatility, my preferred measure of bank risk taking. I do not find significant results vis-a-vis safer borrowers (Column 3). Vis-a-vis riskier borrowers (Column 4), affected banks increase their loan issuance. This evidence reconfirms the bank risk-taking hypothesis shown in Table 1.7.

4.3 Parametric Test: Parallel Trends

The identifying assumption relies on the fact that unaffected banks provide a valid counter factual for lending of affected banks in the absence of a framework change. In this section, I perform a parametric test of the parallel trends assumption for the main results.

Figure 1.2 plots coefficient estimates of the effect of an increase in collateral availability on the lending of euro area banks. I consider a 10-quarter event window, spanning 4 quarters before the implementation in January 2007 to 6 quarters thereafter, based on the following equation:

$$y_{ijt} = \sum_{k \neq 2006q4} \beta_k \text{ affected}_i \times \mathbf{1}[k = t] + \varepsilon_{ijt}, \tag{1.3}$$

[19] The tradable sector includes manufacturing (SIC code 2000-3999), and the non tradable sector includes construction (SIC code 1500-1799), whole and retail services (SIC code 5000-5999) as well as accommodation (SIC code 7000-7099).

where y_{ijt} is log loan issuance provided by bank i to firm j at time t; $\mathbf{1}[k = t]$ is a dummy variable that equals one in quarter t and 0 otherwise. I exclude Q4 2006, the quarter before the framework change happened, to estimate the dynamic effect. Furthermore, I control for firm × quarter fixed effects. In this case, I define $Affected_{05,i}(0/1)$ as an indicator variable that equals one for affected banks, defined as banks which have an above-median share of euro area loan issuance prior to the announcement of the framework change in July 2005. The control group are banks which are less affected by the framework change. The dashed lines represent 90% confidence intervals, adjusted for bank-level clustering.

This parametric test provides evidence that there are no significant different pre-trends in loan issuance between affected and not affected banks. The coefficients before the changeover are insignificant and close to zero. After the announcement, the lending activity of affected banks becomes positive and significant relative to the control group of unaffected banks. By contrast, confounding factors, as long as they impact both types of banks, are canceled out by the difference-in-differences approach.

4.4 Firm-Level: Real Effects

I now turn to the firm-level. I show that firms with a relationship to affected banks increase their probability of obtaining a loan, confirming the results found on the bank-firm-level in Section 4.1. I also provide evidence on real effects of collateral policy, especially in terms of employment and investment.

Table 1.9 presents the results on loan issuance. The dependent variable *pr(Loan)* is an indicator variable equal to one if firm j obtains a bank loan in the respective year t. I restrict the sample to firms with available balance sheet data. Robust standard errors are clustered at the firm-level. Across specifications, the main effect is positive and significant. Column (1) includes firm and country × year fixed effects. The coefficient of interest is positive and significant at the 5% level. In Column (2),

firm as well as industry × year and country × year fixed effects are added to control for unobservable time-variant factors at the industry- and country-year-level. I find a positive treatment effect, significant at the 10% level: Relative to the control group, firms with a relationship to affected banks increase their probability of obtaining a loan by 8.4% post framework change. In Column (3), I add firm-level control variables to refine my treatment and control group; the coefficient remains virtually unchanged. I also take care of a sample bias by showing the results only for firms with multiple bank relations (Column 4). This confirms the results from the bank-firm-quarter-level.

Table 1.10 and Table 1.11 provide the results with respect to employment and investment. In Table 1.10, the dependent variable *ln(Nr of employees)* is the natural logarithm of firm *j*'s number of employees in year *t*. All specifications include firm-level control variables such as the log of total assets and the leverage ratio, lagged by one year. Column (1) does not include any fixed effects, the interaction term of interest is positive but insignificant. The Post07 indicator variable is negative, and highly significant. Both firms with and without a relationship to affected banks experience a decrease in lending after the framework change. Column (2) includes firm and year fixed effects to take care of unobservable firm factors and common time trends. The interaction term of interest turns positive and highly significant. Firms with a relationship to affected banks experience a larger increase in employment than firms without such a relationship. In Column (3) and (4), I split the sample into risky/safe borrowers, defined as firms located in the top/bottom half in terms of their three-year ROA standard-deviation from year *t-3* to *t-1*. In Column (5) and /6), I split the sample of borrowers into firms active in the tradable sector, and in the nontradable sector. Overall, the evidence provided suggests that especially riskier and less productive borrowers with a relationship to affected banks increase their employment, as opposed to firms without such a relationship. In Table 1.11, the dependent variable $ln(investment)$ is the natural logarithm of capital expenditure. The results are somewhat muted: I do not find any significant results for the sample as a whole (Column 1 and 2). I obtain results only when I split the borrowers with

respect to their riskiness and their productivity. Especially risky and less productive borrowers with a relationship to affected banks increase their investment, as opposed to firms without such a relationship. These results are slightly significant, at the 10% level.

Overall, these results seem to suggest positive real effects on the firm-level as a consequence of the framework change, albeit for less-productive, riskier firms.

5 Robustness

In this section, I present further robustness checks to corroborate my main findings. I address confounding factors (Section 5.1), anticipation effects (Section 5.2), different definitions of "affected" (Section 5.6). I also run a falsification test (Section 5.3), and I restrict the sample to lead arrangers (Section 5.5), and to constrained banks (Section 5.4).

5.1 Confounding Factors

First, a threat to the identification is that the framework change in January 2007 coincides with other events that affect bank lending. As long as these events affect both the treatment and control group the same way, they are differenced out by the difference-in-differences setup. If this is not the case, then my results could be biased. The financial crisis in 2008/09 is such a possible confounding event. While I argue that this framework change is a crisis-unrelated policy change, research has however shown that European banks exposed to U.S. sub-prime market cut their lending already well ahead of the start of the financial crisis (Puri, Rocholl and Steffen (2011) Huber (2018)). Since affected banks are the ones more active on the European syndicated loan market, they might also be more active internationally. I try to take care of this by keeping the event window very short, from Q1 2006 to Q4 2007, four quarters before and after the framework change. Table 1.12 presents

the results. Compared to our baseline estimates in Table 1.5, the magnitude and significant of the interaction term remains almost unchanged. In economic terms, a one-standard-deviation increase in a bank's affected ratio (13.75 percentage points) translates now to a increase in bank lending of 6.9% ($0.005 \times 13.80 = 0.0688$). In Column (3) I add bank-level control variables, such as the log of assets, the leverage (equity) ratio, return on equity (ROE), cash, liquidity, and deposits over total assets as a proxy of a banks' liquidity situation, all lagged by 4 quarters. The magnitude of the difference-in-differences estimate increases slightly.

5.2 Anticipation Effect

Second, I check if there is evidence of any anticipation effect at work (Table 1.13). The idea behind it is that banks might have anticipated the framework change as soon as it was announced in July 2005, piling eligible loans on their balance sheet in the run-up to the framework change. If this was the case, the coefficient of interest should be less pronounced. I therefore cumulate the loan issuance from Q1 2004 to Q4 2006, right up to the implementation date. $\text{Affected}_{06,i}$ is equal to one for banks that have an above-median share of euro area loan issuance (excl. domestic) over total loan issuance in the period Q1 2004 to Q4 2006, prior to the implementation date in January 2007. Affected banks increase their lending in a significant and positive way across specifications. In Column (2), I apply the most demanding fixed effects structure, and include bank $\times$ firm, country $\times$ time and firm $\times$ time fixed effects. The interaction term of interest is positive and significant at the 5% level. A one-standard-deviation increase in a bank's $\text{Affected}_{06,i}$ ratio (11.485 percentage points) leads to a increase in bank lending of 8.2% ($0.007 \times 11.49 = 0.0819$). In Column (3), I add bank-controls, and the estimate increases slightly to 0.008. This is similar in magnitude to the baseline estimate in the main table (Table 1.5), and hence evidence that the implementation date was more significant than the announcement date. Similarly, Table 1.14 addresses the concern that the framework change was announced in July 2005, but implemented only one and a

half year later. I modify the post indicator variable, which now takes on the value of 0 up until Q2 2005 and a value of 1 starting from Q3 2005. The sample period is now Q1 2004 to Q4 2007. I find no effect across all specifications for the new interaction term $\text{Affected}_{05,i} \times \text{Post05}$, which suggests that the announcement date did not play a significant role in banks' lending decisions.

5.3 Falsification Test

Third, to corroborate my findings further, I run a falsification test using non euro area lenders (Table 1.15). The reasoning behind this is that any changes in the monetary and collateral policy of the ECB should not have any direct impact on the lending decisions of banks headquartered outside the euro area. To test this, I build a group of "placebo" affected banks, consisting of non euro area banks, which are actively lending to firms located in the euro area. The procedure is similar to the affected variable. I define $Placebo_{05,i}$ *(%)* as the share of euro area loan issuance over total loan issuance by non euro area banks (my placebo banks) in the period Q1 2003 to Q2 2005. Table 1.15 presents the results. In line with this hypothesis, the coefficient is insignificant across specifications. I find no effect.

5.4 Constrained Banks

Fourth, I use a bank's balance sheet strength to account for an overall liquidity need of a bank. According to previous research (Kashyap and Stein (2000), Jiménez, Mian et al. (2019)), the transmission of monetary policy depends on a banks' health. The less pledgeable assets a bank has on its balance sheet (and is hence more "constrained"), the more the collateral framework change plays a role in its lending decisions. Table 1.16 presents the results. For further robustness, I define banks to be constrained in two ways: I take care of the overall amount of eligible assets on a bank's balance sheet by looking at its securities holdings (Column (1) and (2)). I take care of a bank's overall health in terms of Tier1Ratio in Column (3) and (4).

In detail, in Column (1), constrained banks are banks which exhibit a securities to assets ratio below the median. Unconstrained banks are all the other banks in the sample (Column 2). In Column (3) constrained banks are defined as banks with a Tier1Ratio below the median. Unconstrained banks are all the other banks in the sample (Column 4). In line with the literature, I find that the effect of collateral policy on lending is stronger for affected banks with less liquid balance sheets.

5.5 Lead Arranger Sample

Fifth, I show that the results are robust to restricting the sample to lead arranger banks (Table 1.17). Prior literature has highlighted that lead arrangers are in direct contact with the borrower, performing due diligence and monitor the borrower (Ivashina and Scharfstein (2010)). There might hence be a different role of lead arrangers vs participants in a syndicate. Therefore, I restrict the sample to the sample of lead-arranger banks only. Compared to the baseline results in Table 1.5, the difference-in-differences estimate are similar in magnitude and significance across specifications. Column (3) presents the most stringent fixed effects structure, taking care of time-varying differences across banks driven by firms, i.e. loan demand. The interaction term of interest is positive and significant at the 5% level. A one-standard-deviation increase in a lead arranger bank's affected ratio (13.87 percentage points) translates now to a increase in bank lending of 7.2% ($0.005 \times 13.87 = 0.0719$). In Column (4), I add bank-level controls, which decrease the statistical significance of the estimate, but not its magnitude. Hence, the results do not change depending on a bank being lead arranger or participant.

5.6 Variable Definition

Sixth, I provide evidence that my results do not depend on how I define bank i's exposure to euro area loans (excl. domestic) prior to the announcement of the framework change, my key explanatory variable (Tables 1.18 - 1.21). In Table 1.18,

I define *Affected (0/1)* as an indicator variable equal to one for banks which have an above median share of euro area loan issuance in their overall syndicated loan issuance. I find that being affected still significantly increases loan supply after the collateral framework change. In Table 1.19, I define bank i's exposure to euro area loans not in terms of bank i's total issuance, but in terms of a bank i's total assets in the period Q2 2003 to Q2 2005. In this case, the treatment group are small (affected) banks, as opposed to big (unaffected) banks. The estimated interaction term of interest remains positive and significant at the 10% level. In Table 1.20, I define bank i's exposure to euro area loans at one point in time: the quarter prior to the announcement of the framework change in July 2005. Eligible (log) is the log (+ 1) of the amount of euro area loans in bank i's syndicated loan issuance in Q2 2005. This variable definition does not take care of bank size, which might be the underlying explanation why the results are the most significant (at the 1% level) compared to other definitions. In Table 1.21, eligible (0/1) is an indicator variable equal to one if bank i has an above median share of euro area loan issuances in Q2 2005. Overall, none of these alternative definitions change the main finding.

6 Conclusions

This chapter sheds light on collateral policy, an important part of a central bank's policy tool kit. I show that when central banks change their collateral policy, this has an impact on both bank lending and the real economy. Banks with eligible assets on their balance sheet increase their lending in the syndicated loan market by an economically significant 8.3% compared to unaffected banks. Consequently, these banks lend more to riskier and less productive borrowers located in other euro area countries. The borrowers in turn experience growth in employment and investment.

I provide new evidence along several dimensions. First, my results have important lessons concerning the unintended consequences of financial integration. While increasing cross-border capital flows were a welcomed part of the euro area fin-

ancial integration in the 2000s, I find that banks residing in the core of the euro area, instead of increasing loans towards productive sectors, funded riskier and less-productive borrowers in other euro area countries, and especially in the GIIPS. This highlights the trade-off between financial stability and financial deepening, which is at the heart of macro-prudential policies (see also Giannetti and Jang (2020)).

Second, I expand our understanding of the workings of collateral policy. My results show real effects of collateral policy. While this has been shown for both conventional and unconventional policies, I provide evidence that collateral policy, too, impacts impact firm-level outcomes such as employment and investment. This fills a gap in the literature and highlights the importance of considering collateral policy an equally important monetary policy tool.

Some caveats are in order. First, more theoretical work is needed to understand the distortion in the allocation of credit across regions, in the spirit of Bruche and Suarez (2010).[20]. Empirically, my research focuses on the impact of collateral policy in crisis-unrelated times, which is, so far, exceptional for the literature. Both issues would be a profitable avenue for future research.

[20] The authors suggest counter party risk stemming from different deposit insurances in a regionally segmented banking market set-up

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Tables and Figures

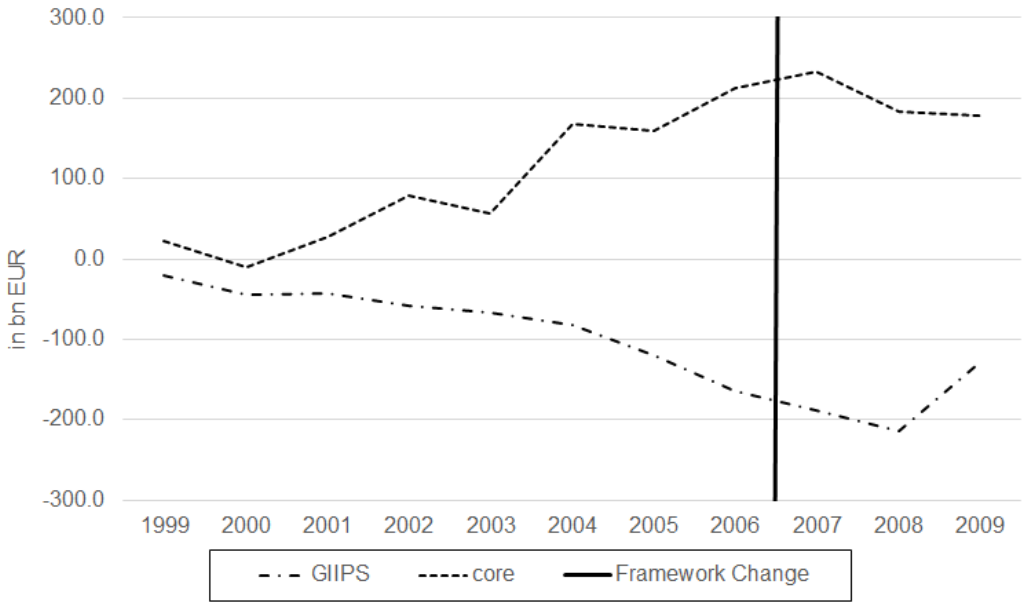

Figure 1.1: Current Account Balance (in bn EUR). This figure plots the current account for the "core" (DE, FR, AT, NL, BE, FI, LU) and the "GIIPS" countries (GR, IT, IE, PT, ES). Source: AMECO online database.

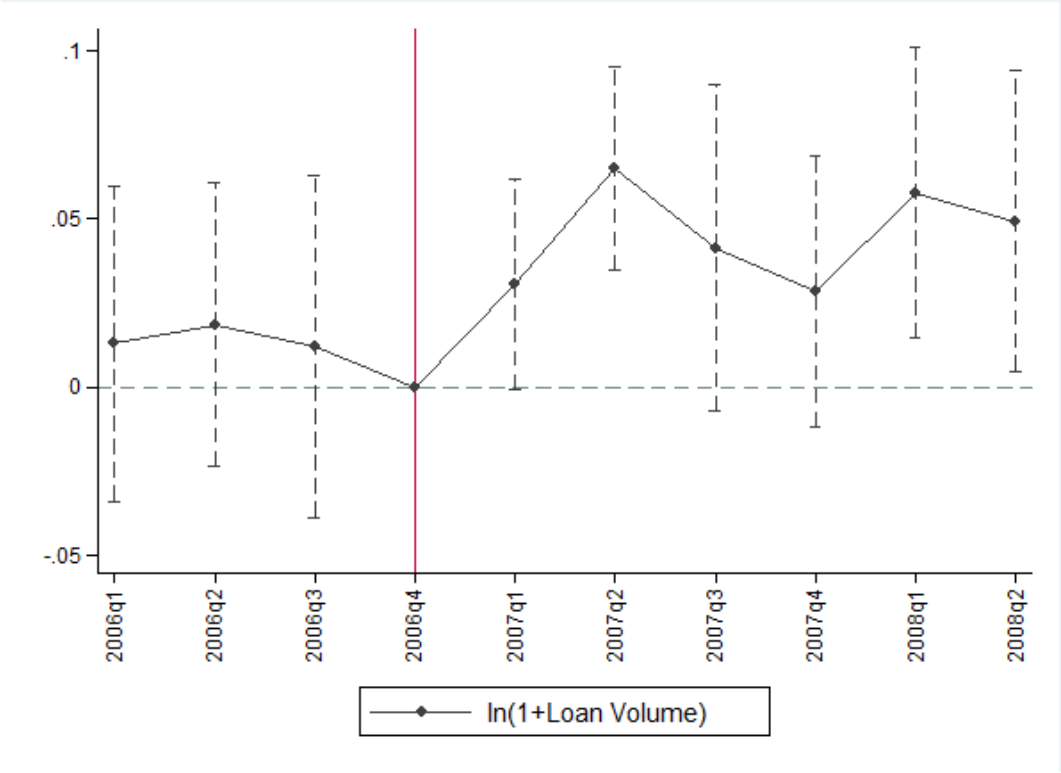

Figure 1.2: Parallel Trends Assumption. The figure is based on the following equation:

$$y_{ijt} = \sum_{k \neq 2006q4} \beta_k \text{ Affected}_{05,i}(0/1) \times \mathbf{1}[k = t] + \varepsilon_{ijt},$$

where y_{ijt} is log loan issuance provided by bank i to firm j at quarter t; $\mathbf{1}[k = t]$ is a dummy variable that equals one in quarter t and 0 otherwise. Q4 2006, the quarter before the framework change happened is excluded to estimate the dynamic effect. The regression includes firm × quarter fixed effects. In this case, $Affected_{05,i}(0/1)$ is an indicator variable that equals one for affected banks, defined as banks which have an above-median share of euro area loan issuance prior to the announcement of the framework change in July 2005. The dashed lines represent 90% confidence intervals, adjusted for bank-level clustering.

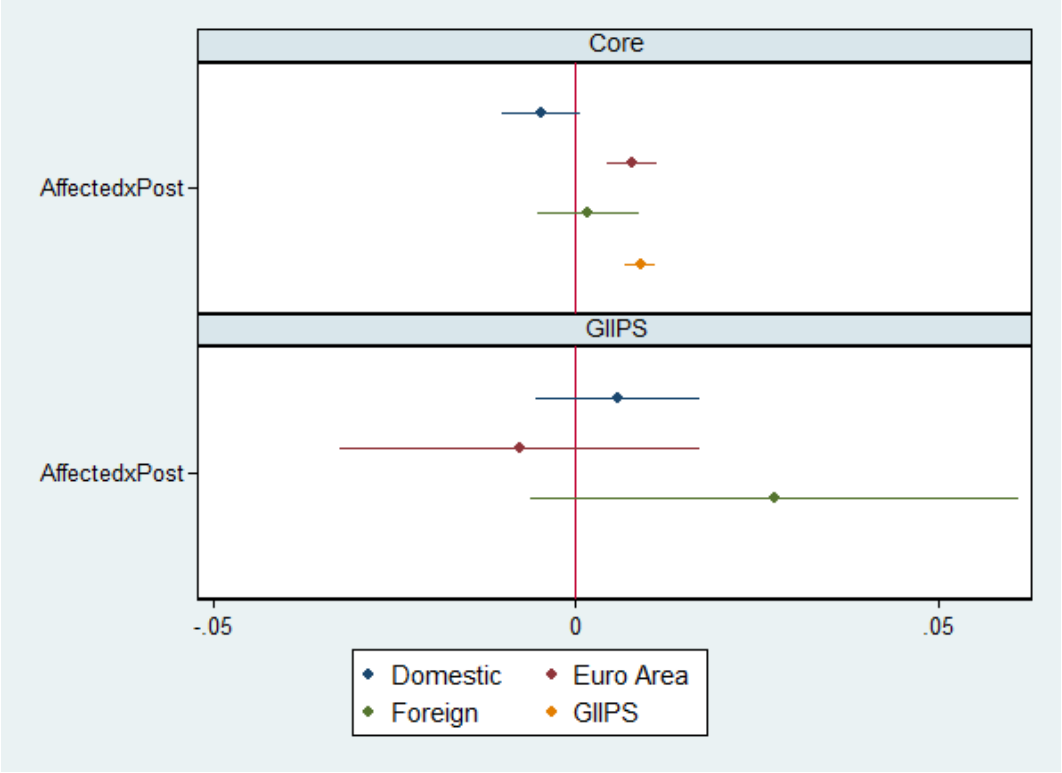

Figure 1.3: Coefficient Plot The figure plots the coefficient estimates of sample splits according to a firms' geographical location, as shown in Table 1.6. In the upper panel, the sample is limited to euro area banks headquartered in the "Core" countries (DE, FR, AT, NL, BE, FI, LU). In the lower panel, the sample is limited to euro area banks headquartered in the GIIPS (GR, IT, IE, PT, ES). "Domestic" includes borrowers headquartered in the same country as the respective bank; "Euro Area" includes borrowers located in other countries of the euro area. "Foreign" includes borrowers located outside the euro area. "GIIPS" includes borrowers located in the GIIPS. The lines present 95% confidence intervals, adjusted for bank-level clustering.

Table 1.1: Summary Statistics: Bank-Loan-Quarter-Level. This table presents summary statistics on the bank-firm-quarter-level. *Loan amount (mn)* are all issued syndicated loans by bank i (as lead or participating bank) to firm j in a quarter t. The sample period is Q1 2006 to Q2 2008. The *all-in-drawn spread* is calculated as the sum of the spread over LIBOR including any annual fees. *Share of lead arrangers* is the number of lead arrangers in all loan issuances. *Share of euro are firms* is an indicator variable equal to one if the firm is headquartered in the euro area. *Domestic over total loan issuance* is the share of domestic loan over total loan issuance, where a domestic loan is a loan to a firm which has the same headquarter location as the bank. *Euro area over total loan issuance* is the share of euro area loan over total loan issuance, excluding domestic loans.

	mean	sd	min	max	count
Loan amount (mn)	171.13	699.11	0.17	16,112.07	1,208
All-in-drawn spread (bps)	120.37	97.99	5.00	600.00	924
Maturity (months)	65.08	48.22	3.00	408.00	1,182
Share of lead arrangers	0.54	0.50	0.00	1.00	1,208
Share of euro area firms	0.45	0.50	0.00	1.00	1,208
Domestic over total loan issuance	24.06	22.77	0.00	100.00	1,208
Euro area over total loan issuance	24.34	17.57	0.00	100.00	1,208

Table 1.2: Summary Statistics: Firm-Level. This table presents summary statistics on the firm-year-level. The sample period is 2006 to 2008. *Affected*$_{06,j}$ *(0/1)* is an indicator variable equal to one if firm j has a relationship with an affected bank in 2006, before the framework change, and zero otherwise. *Ln(Loan amount)* is the natural logarithm plus one of all issued syndicated loans aggregated to firm j in year t. *ln(Total assets)* is the natural logarithm of firm j's total assets. *Leverage* is the ratio of firm j's long term debt to total assets. *Liquidity* is the ratio of firm j's cash flow over total assets. *Employment* is the number of firm j's employees, in thousand. *Ln(Investment)* is the natural logarithm of firm j's capital expenditure.

	Unaffected		Affected	
	mean	sd	mean	sd
Affected$_j$ (0/1)	0.00	0.00	1.00	0.00
ln(Loan amount)	1.70	2.23	2.59	2.83
ln(Total Assets)	7.95	1.62	9.31	2.11
Leverage	0.39	0.19	0.34	0.17
Liquidity (internal finance)	0.01	0.10	0.04	0.07
Market to book ratio	1.33	0.50	1.42	0.62
Return on assets	0.07	0.06	0.08	0.06
Dividend payout ratio	0.22	0.39	0.29	0.33
Employment (th)	12.57	22.46	27.77	41.32
ln(Investment)	4.94	2.07	6.48	2.30

Table 1.3: Summary Statistics: Bank-Level. This table presents summary statistics for all euro area banks included in the sample for the pre-framework period, Q1 to Q4 2006. *Ln(total assets)* is the natural logarithm of total assets plus one. *Loans ratio* is gross loans over total assets. *Equity ratio* is equity over total assets. *ROE* is bank i's return on equity. *Cash ratio* is cash and equivalents over total assets. *Liquidity ratio* is investment securities over total assets. *Deposit ratio* is deposits over total assets. *Affected*$_{05,i}$ *(%)* is bank i's share of loan issuance to euro area borrowers (excl. domestic) over total syndicated loan issuance. *Affected*$_{05,i}$ *(0/1)* is an indicator variable equal to one for banks which have an above median share of loan issuance to euro area (excl. domestic) borrowers in their overall syndicated loan issuance. *Eligible*$_{05,i}$ *(log)* is the natural logarithm of (one plus) bank i's issuance of syndicated loans to other euro area borrowers (excl. domestic) in Q2 2005. *Eligible*$_{05,i}$ *(0/1)* is an indicator variable equal to one for banks which have an above median share of loan issuance to other euro area borrowers (excl. domestic) in Q2 2005.

	mean	min	max	count
ln(total assets)	11.2	9.0	13.7	62
Loans ratio	65.7	38.5	88.4	62
Equity ratio	5.2	1.0	15.2	62
ROE, in (%)	11.3	-0.9	23.8	62
Cash ratio	1.5	0.0	14.4	62
Securities ratio	32.1	8.3	96.5	62
Deposit ratio	42.1	2.2	81.0	62
Affected (%)	20.2	0.0	100.0	62
Affected (0/1)	0.4	0.0	1.0	62
Eligible (mn)	384.2	0.0	3,955.6	62
Eligible (0/1)	0.2	0.0	1.0	62

Table 1.4: Bank-Level: Characteristics of Affected and Unaffected Banks. This table presents the difference-in-mean estimates between affected and unaffected banks. Affected (unaffected) banks refer to banks which exhibit an above (below) median share of loan issuances to euro area borrowers (excl. domestic) over total loan issuance prior to July 2005. All variables are defined in the Appendix A1. The last column shows the absolute value of the t-statistics for a test whether the difference in means between both groups is equal to zero in the period prior to the framework change Q1-Q4 2006.

	Affected		Unaffected			
	Mean	N	Mean	N	Diff.	t-stat.
ln(total assets)	11.12	38	11.33	24	-0.210	-0.652
Equity ratio	5.02	38	5.49	24	-0.470	-0.681
Loans ratio	66.55	38	64.42	24	2.129	0.740
Cash ratio	1.38	38	1.60	24	-0.227	-0.382
Securities ratio	35.33	38	26.87	24	8.459*	2.000
Deposit ratio	44.69	38	37.98	24	6.710	1.207
ROE, in (%)	11.13	38	11.52	24	-0.387	-0.244

Table 1.5: Bank-Firm-Level: Collateral Framework Changes and Credit Supply This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change. The analysis is based on data on the bank-firm-quarter-level. The sample period is Q1 2006 to Q2 2008. *Ln(loan volume)* is the logarithm of (one plus) the loan issuance from bank i (as lead arranger or participant) to firm j at quarter t. *Affected*$_{05}$ *(%)* is the ratio of euro area (excl. domestic) over total loan issuance in the period Q1 2003 to Q2 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. The control variables are lagged by 4 quarters and defined as in the Appendix A1. The regressions further include bank × firm fixed effects, country × time and firm × time fixed effects, as indicated. Country-fixed effects are based on the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
Affected$_{05,i}$(%) × Post07$_t$	0.002	0.006**	0.007**
	(0.006)	(0.002)	(0.003)
ln(Total assets)			-0.323
			(0.222)
Equity ratio			-0.008
			(0.030)
ROE			-0.001
			(0.001)
Cash ratio			-0.013
			(0.010)
Liquidity ratio			-0.003
			(0.003)
Deposit ratio			0.003
			(0.005)
Post07$_t$	0.289*		
	(0.156)		
Observations	1,208	1,208	1,208
R-squared	0.693	0.975	0.975
Bank-level Controls	No	No	Yes
Bank × Firm FE	Yes	Yes	Yes
Country × Time FE	No	Yes	Yes
Firm × Time FE	No	Yes	Yes
Cluster	Bank	Bank	Bank

Table 1.6: Geographical Distribution of Loans. This table provides results of difference-in-differences regressions analyzing the volume of loan issuances of Core and GIIPS banks vis-a-vis domestic, euro area and foreign borrowers pre- and post collateral framework change. The analysis is based on data on the bank-firm-quarter-level. The sample period is Q3 2005 to Q2 2008. *Ln(Loan amount)* is the logarithm of (one plus) the loan issuance in million USD from bank i (as lead arranger or participant) to firm j at time t. *Affected*$_{i,05}$ *(%)* is the ratio of bank i's euro area (excluding domestic) over total loan issuance in the period Q1 2003 to Q2 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. In Column (1) - (4), the sample is limited to euro area bank headquartered in the "core" countries (DE, FR, AT, NL, BE, FI, LU). In Column (1) only domestic borrowers are included, In Column (2) only borrowers located in other countries of the euro area are included. In Column (3) the sample includes only borrowers located in the GIIPS. In Column (4) only foreign borrowers are included, which are located outside the euro area. In Column (5) - (7), the sample is limited to euro area banks headquartered in the "GIIPS" countries (GR, IT, IE, PT, ES). Domestic borrowers are included in Column (5). Borrowers located in other euro area countries are included in Column (6) and outside the euro area in Column (7). The regressions further include bank × firm fixed effects, bank-country × time fixed effects and firm × time fixed effects, as indicated. Bank-country fixed effects are based on the bank's respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Core - domestic	Core - eanotdomestic	Core - giips	Core - foreign	GIIPS - domestic	GIIPS - eanotdomestic	GIIPS - foreign
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
Affected$_{05,i}$(%) × Post07$_t$	-0.005*	0.008***	0.009***	0.002	0.006	-0.008	0.027*
	(0.003)	(0.002)	(0.001)	(0.003)	(0.006)	(0.011)	(0.015)
Observations	440	323	75	1,299	180	38	66
R-squared	0.976	0.989	0.975	0.962	0.976	0.971	0.996
Bank × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Table 1.7: Bank Risk Taking. This table provides results of difference-in-differences regressions analyzing the ex-ante risk taking behaviour of banks pre- and post collateral framework change. The analysis is based on data on the bank-firm-quarter-level. The sample period is Q1 2006 to Q2 2008. The dependent variable is the logged three-year standard-deviation of loan-financed firm j's ROA from year *t-3* to *t-1*. *Affected*$_{05}$ *(%)* is the ratio of euro area (excl. domestic) over total loan issuance in the period Q1 2003 to Q2 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. The regressions further include bank fixed effects, quarter *times* time fixed effects, and country × time fixed effects, as indicated. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)
VARIABLES	$\ln(\sigma(ROA_f)^{5y})$	$\ln(\sigma(ROA_f)^{5y})$	$\ln(\sigma(ROA_f)^{5y})$
Affected$_{05,i}$(%) × Post07$_t$	0.009	0.015**	0.017**
	(0.006)	(0.006)	(0.008)
ln(Total assets)			0.068
			(0.277)
Equity ratio			0.077
			(0.081)
ROE			0.002
			(0.004)
Cash ratio			0.138***
			(0.036)
Liquidity ratio			-0.011
			(0.007)
Deposit ratio			-0.006
			(0.008)
Post07$_t$	-0.526**		
	(0.238)		
Observations	1,367	1,367	1,367
R-squared	0.044	0.375	0.384
Bank FE	Yes	Yes	Yes
Quarter × Year FE	No	Yes	Yes
Country × Year FE	No	Yes	Yes
Cluster	Bank	Bank	Bank

Table 1.8: Borrower Characteristics. This table provides results of difference-in-differences regressions analyzing borrower characteristics pre- and post collateral framework change. The analysis is based on data on the bank-firm-quarter-level. The sample period is Q1 2006 to Q2 2008. *Ln(Loan amount)* is the logarithm of (one plus) the loan issuance in million USD from bank i (as lead arranger or participant) to firm j at time t. *Affected*$_{05}$ *(%)* is the ratio of euro area (excl. domestic) over total loan issuance in the period Q1 2003 to Q2 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. *Tradable* are firms active in tradable industries (SIC code 2000-3999). *Non-tradable* are firms active in non-tradable industries (SIC code 1500-1799, 5000-5999, 7000-7099). The regressions further include bank × firm fixed effects, bank-country × time fixed effects and firm × time fixed effects, as indicated. Bank-country fixed effects are based on the bank's respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)
	Tradable	Non Tradable	Bottom-half Volatility	Top-half Volatility
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
$\text{Affected}_{05,i}(\%) \times \text{Post07}_t$	0.003	0.010**	0.004	0.015*
	(0.005)	(0.004)	(0.004)	(0.008)
Observations	658	420	300	292
R-squared	0.853	0.830	0.861	0.921
Bank × Firm FE	Yes	Yes	Yes	Yes
Country × Time FE	Yes	Yes	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Table 1.9: Firm-Level: Collateral Framework Changes and Credit Supply. This table provides results of difference-in-differences regressions analysing the effects of collateral policy on loan financing. The analysis is based on data on the firm-year-level and the sample period is 2006 to 2008. The dependent variable is *pr(Loan)*, an indicator variable equal to one if firm j obtains a bank loan in the respective year t, and zero otherwise. The treatment group indicator $Affected_j$ equals one if firm j has a relationship with an affected bank in 2006, before the framework change happened, and equals zero otherwise. *Post* equals one after the implementation of the framework change in January 2007, and zero otherwise. The regressions include time-varying firm-level controls [$ln(Total\ assets)_{f,t-1}$, $Leverage_{f,t-1}$, $Liquidity_{f,t-1}$], all lagged by one year. All variables are defined in the appendix. The regressions further include firm fixed effects, year fixed effects, industry × year fixed effects, country × year fixed effects, where indicated. Reported standard errors are in parentheses, clustered at the firm-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	pr(Loan)	pr(Loan)	pr(Loan)	pr(Loan)
$\text{Affected}_{06,j} \times \text{Post07}_t$	0.103**	0.084*	0.084*	0.364**
	(0.043)	(0.050)	(0.050)	(0.174)
$\ln(\text{Assets})_{f,t-1}$			-0.019	
			(0.044)	
$\text{Leverage}_{f,t-1}$			0.036	
			(0.189)	
$\text{Liquidity}_{f,t-1}$			0.296	
			(0.290)	
Observations	1,490	1,490	1,490	170
R-squared	0.706	0.794	0.795	0.568
Multiple Bank Relations	No	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Industry × Year FE	No	Yes	Yes	No
Cluster	Firm	Firm	Firm	Firm

Table 1.10: Firm-Level: Employment. This table provides results of difference-in-differences regressions analysing the effects of collateral policy on firm-level employment. The analysis is based on data on the firm-year-level and the sample period is 2006 to 2008. The dependent variable is *ln(No of employees)*, the natural logarithm of firm j's number of employees in year t. The treatment group indicator $Affected_j$ equals one if firm j has a relationship with an affected bank in 2006, before the framework change happened, and equals zero otherwise. *Post* equals one after the implementation of the framework change in January 2007, and zero otherwise. The sample is limited to borrowers located in the bottom (Column 3) and the top (Column 4) half according to their ROA volatility. The sample is limited to borrowers active in the non-tradable sector (Column 5) or the tradable sector (Column 6). The regressions include time-varying firm-level controls [$ln(Total\ assets)_{f,t-1}$, $Leverage_{f,t-1}$], all lagged by one year. All variables are defined in the appendix. The regressions further include firm fixed effects and year fixed effects, where indicated. Reported standard errors are in parentheses, clustered at the firm-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Risky	Safe	Not tradable	Tradable
VARIABLES	ln(No of employees)	ln(No of employees)	ln(No of employees)	ln(No of employees)	ln(No of employees)	ln(No of employees)
$\text{Affected}_{06,j}(\%) \times \text{Post07}_t$	0.068	0.061***	0.137**	-0.020	0.175***	0.020
	(0.066)	(0.022)	(0.059)	(0.043)	(0.064)	(0.031)
$\text{Affected}_{06,j}$	0.115					
	(0.104)					
Post07_t	-0.179***					
	(0.055)					
Observations	2,012	2,012	471	554	242	758
R-squared	0.363	0.990	0.988	0.994	0.990	0.985
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm

Table 1.11: Firm-Level: Investment. This table provides results of difference-in-differences regressions analysing the effects of collateral policy on firm-level investment. The analysis is based on data on the firm-year-level and the sample period is 2006 to 2008. The dependent variable is *ln(Investment)*, the natural logarithm of firm j's Capital Expenditure (CAPEX) in year t. The treatment group indicator $Affected_j$ equals one if firm j has a relationship with an affected bank in 2006, before the framework change happened, and equals zero otherwise. *Post* equals one after the implementation of the framework change in January 2007, and zero otherwise. The sample is limited to borrowers located in the bottom (Column 3) and the top (Column 4) half according to their ROA volatility. The sample is limited to borrowers active in the non-tradable sector (Column 5) or the tradable sector (Column 6). The regressions include time-varying firm-level controls [$ln(Total\ assets)_{f,t-1}$, $Leverage_{f,t-1}$], all lagged by one year. All variables are defined in the appendix. The regressions further include firm fixed effects and year fixed effects, where indicated. Reported standard errors are in parentheses, clustered at the firm-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Risky	Safe	Not tradable	Tradable
VARIABLES	ln(Investment)	ln(Investment)	ln(Investment)	ln(Investment)	ln(Investment)	ln(Investment)
$\text{Affected}_{06,j}(\%) \times \text{Post07}_t$	-0.004	0.072	0.215*	0.067	0.239*	-0.025
	(0.092)	(0.058)	(0.129)	(0.092)	(0.128)	(0.062)
$\text{Affected}_{06,j}$	0.261**					
	(0.105)					
Post07_t	0.084					
	(0.084)					
Observations	2,459	2,459	557	628	318	924
R-squared	0.792	0.975	0.971	0.978	0.974	0.981
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm

Table 1.12: Robustness: Event Window [Q1 2006 - Q4 2007]. This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the collateral framework change. The analysis is based on data on the firm-bank-quarter-level. The sample period is Q1 2006 to Q4 2007. *Ln(Loan amount)* is the logarithm of (one plus) the loan issuance from bank i (as lead arranger or participant) to firm j at quarter t. *Affected*$_{05}$ *(%)* is the ratio of euro area (excl. domestic) over total loan issuance in the period Q1 2003 to Q2 2005. *Post07*$_t$ is an indicator variable equal to one after the framework change got implemented in January 2007. The control variables are lagged by 4 quarters and defined as in the Appendix A1. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Country fixed effects refer to the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
Affected$_{05,i}$(%) × Post07$_t$	0.004	0.005**	0.006**
	(0.004)	(0.002)	(0.003)
ln(Total assets)			-0.078
			(0.220)
Equity ratio			-0.029
			(0.052)
ROE			-0.001
			(0.002)
Cash ratio			-0.010
			(0.011)
Liquidity ratio			-0.003
			(0.003)
Deposit ratio			0.007
			(0.006)
Observations	812	812	812
R-squared	0.886	0.979	0.979
Bank × Firm FE	Yes	Yes	Yes
Country × Time FE	Yes	Yes	Yes
Firm × Time FE	No	Yes	Yes
Cluster	Bank	Bank	Bank

Table 1.13: Robustness: Anticipation Effect. This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change. The analysis is based on data on the firm-bank-time-level. The sample period is Q1 2006 to Q2 2008. *Affected*$_{06}$ *(0/1)* is an indicator variable equal to one for banks which have an above-median share of euro area loan issuance (excl. domestic) over total loan issuance in the period Q1 2004 to Q4 2006, prior to the implementation date in January 2007. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. The control variables are lagged by 4 quarters and defined as in the Appendix A1. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Country fixed effects refer to the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
Affected$_{06,i}$(%) × Post07$_t$	0.005	0.007**	0.008**
	(0.004)	(0.003)	(0.003)
ln(Total assets)			-0.298
			(0.215)
Equity ratio			-0.009
			(0.030)
ROE			-0.001
			(0.001)
Cash ratio			-0.014
			(0.010)
Liquidity ratio			-0.002
			(0.003)
Deposit ratio			0.003
			(0.005)
Observations	1,208	1,208	1,208
R-squared	0.877	0.975	0.975
Bank × Firm FE	Yes	Yes	Yes
Country × Time FE	Yes	Yes	Yes
Firm × Time FE	No	Yes	Yes
Cluster	Bank	Bank	Bank

Table 1.14: Robustness: Announcement Date vs. Implementation Date. This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change when it was announced in July 2005. The analysis is based on data on the firm-bank-time-level. The sample period is Q2 2004 to Q4 2006, 4 quarter before and 6 quarter after the announcement. *Affected*$_{05}$ *(0/1)* is an indicator variable equal to one for banks which have an above-median share of euro area loan issuance (excl. domestic) over total loan issuance over the period Q1 2003 to Q2 2005. *Post* is an indicator variable equal to one after the framework change got announced in July 2005. The control variables are lagged by 4 quarters and defined as in the Appendix A1. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Country fixed effects refer to the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
$\text{Affected}_{05,i}(\%) \times \text{Post05}_t$	0.001	0.002	-0.001	-0.001
	(0.004)	(0.002)	(0.002)	(0.002)
ln(Total assets)				-0.069
				(0.178)
Equity ratio				-0.010
				(0.032)
ROE				0.000
				(0.001)
Cash ratio				-0.003
				(0.004)
Liquidity ratio				0.002
				(0.002)
Deposit ratio				0.001
				(0.002)
post 2005	0.054			
	(0.086)			
Observations	2,074	2,074	2,074	2,074
R-squared	0.725	0.841	0.976	0.976
Bank × Firm FE	Yes	Yes	Yes	Yes
Country × Time FE	No	Yes	Yes	Yes
Firm × Time FE	No	No	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Table 1.15: Robustness: Falsification Test. This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change for banks located outside the euro area (but inside the EU). The analysis is based on data on the firm-bank-time-level. The sample period is Q1 2006 to Q2 2008. Here, group of "placebo" non-euro area banks is defined as non euro-area banks which are actively lending to euro area borrowers. *Placebo*$_{05,i}$ *(%)* is the share of euro area loan issuance over total loan issuance by this placebo group of banks in the period Q1 2003 to Q2 2005. *Post07* is an indicator variable equal to one after the framework change was implemented in January 2007. The control variables are lagged by 4 quarters and defined as in the Appendix A1. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Country fixed effects refer to the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
Placebo$_{05,i}$(%) × Post07$_t$	0.044	0.067	-0.027
	(0.196)	(0.144)	(0.078)
Post07$_t$	0.336***		
	(0.101)		
Observations	804	804	804
R-squared	0.679	0.890	0.982
Bank-level controls	Yes	Yes	Yes
Bank × Firm FE	Yes	Yes	Yes
Country × Time FE	No	Yes	Yes
Firm × Time FE	No	No	Yes
Cluster	Bank	Bank	Bank

Table 1.16: Robustness: Constrained Banks. This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change. The analysis is based on data on the firm-bank-level. The sample period is Q1 2006 to Q2 2008. *Ln(Loan amount)* is the logarithm of (one plus) the loan issuance from bank i to firm j at time t. *Affected*$_{05}$ *(0/1)* is an indicator variable equal to one for bank which have an above-median share of euro area loan issuance (excl. domestic) over total loan issuance over the period Q1 2003 to Q2 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. In Column (1) the sample is limited to constrained banks, defined as banks which have a securities to total assets ratio below the 50th percentile in 2006. In Column (2), the sample is limited to banks which are unconstrained, which are all the other banks. In Column (3), the sample is limited to constrained banks, defined as banks which have a Tier1Ratio below the 50th percentile in 2006. In Column (4), the sample is limited to banks which are unconstrained, which are all the other banks. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	Securities		Tier1ratio	
	(1)	(2)	(3)	(4)
	Constrained	Unconstrained	Constrained	Unconstrained
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
Affected$_{05}$(0/1) × Post$_{07}$	0.229*	0.120	0.389*	0.010
	(0.125)	(0.101)	(0.186)	(0.114)
Observations	546	431	528	375
R-squared	0.976	0.980	0.983	0.977
Bank × Firm FE	Yes	Yes	Yes	Yes
Country × Time FE	Yes	Yes	Yes	Yes
Firm × Time FE	Yes	Yes	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Table 1.17: Robustness: Lead Arranger Sample. This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change. The analysis is based on data on the firm-bank-time-level, where only banks acting as lead arrangers are considered. The sample period is Q1 2006 to Q2 2008. *Affected*$_{05}$ *(0/1)* is an indicator variable equal to one for banks which have an above-median share of euro area loan issuance (excl. domestic) over total loan issuance over the period Q1 2003 to Q2 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Country fixed effects refer to the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
$\text{Affected}_{05,i}(\%) \times \text{Post07}_t$	0.002	0.006*	0.005**	0.007*
	(0.007)	(0.004)	(0.002)	(0.003)
ln(Total assets)				-0.467
				(0.310)
Equity ratio				-0.039
				(0.037)
ROE				-0.001
				(0.001)
Cash ratio				-0.021
				(0.017)
Liquidity ratio				-0.002
				(0.003)
Deposit ratio				0.006
				(0.008)
Post07_t	0.382*			
	(0.190)			
Observations	834	834	834	834
R-squared	0.641	0.896	0.969	0.970
Bank × Firm FE	Yes	Yes	Yes	Yes
Country × Time FE	No	Yes	Yes	Yes
Firm × Time FE	No	No	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Table 1.18: Robustness: "Affected (0/1)". This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change. The analysis is based on data on the firm-bank-time-level. The sample period is Q1 2006 to Q2 2008. *Ln(Loan amount)* is the logarithm of (one plus) the loan issuance from bank i (as lead arranger or participant) to firm j at time t. *Affected*$_{05}$ *(0/1)* is an indicator variable equal to one for banks which have an above-median share of euro area loan issuance (excl. domestic) over total loan issuance over the period Q1 2003 to Q2 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. The control variables are lagged by 4 quarters and defined as in the Appendix A1. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Country fixed effects refer to the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
$\text{Affected}_{05}(0/1) \times \text{Post}_{07}$	0.067	0.238**	0.271**
	(0.147)	(0.104)	(0.106)
Post07_t	0.295***		
	(0.085)		
Observations	1,208	1,208	1,208
R-squared	0.693	0.975	0.976
Bank-level Controls	No	No	Yes
Bank × Firm FE	Yes	Yes	Yes
Country × Time FE	No	Yes	Yes
Firm × Time FE	No	Yes	Yes
Cluster	Bank	Bank	Bank

Table 1.19: Robustness: "Affected" Defined Over Total Assets (%). This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change. The analysis is based on data on the firm-bank-level. The sample period is Q1 2006 to Q2 2008. *Ln(Loan amount)* is the logarithm of (one plus) the loan issuance from bank i (as lead arranger or participant) to firm j at time t. *Affected over* TA_{05} is the share of bank *i's* euro area loan issuance (excl. domestic) over bank *i's* total assets in the period Q1 2003 to Q2 2005, prior to the announcement of the framework change in July 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. The control variables are lagged by 4 quarters and defined as in the Appendix A1. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Country fixed effects refer to the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
Affected over TA_{05} × post	0.020	0.028*	0.028*
	(0.027)	(0.014)	(0.014)
ln(Total assets)			-0.204
			(0.181)
Equity ratio			-0.035
			(0.046)
ROE			-0.000
			(0.002)
Cash ratio			-0.014
			(0.012)
Deposit ratio			0.002
			(0.005)
Observations	1,059	1,059	1,059
R-squared	0.879	0.974	0.975
Bank × Firm FE	Yes	Yes	Yes
Country × Time FE	Yes	Yes	Yes
Firm × Time FE	No	Yes	Yes
Cluster	Bank	Bank	Bank

Table 1.20: Robustness: Eligible (log). This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change. The analysis is based on data on the firm-bank-time-level. The sample period is Q1 2006 to Q2 2008. *Ln(Loan amount)* is the logarithm of (one plus) the loan issuance from bank i (as lead arranger or participant) to firm j at quarter t. *Eligible*$_{05}$ *(log)* is the log plus one of bank i's euro area loan issuances (excl. domestic) in Q2 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. The control variables are lagged by 4 quarters and defined as in the Appendix A1. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Country fixed effects refer to the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
Eligible$_{05}$ (log)	0.020	0.051*	0.066***	0.066***
	(0.035)	(0.027)	(0.024)	(0.023)
ln(Total assets)				-0.204
				(0.187)
Equity ratio				-0.022
				(0.031)
ROE				-0.001
				(0.001)
Cash ratio				-0.011
				(0.010)
Deposit ratio				0.001
				(0.004)
Post07$_t$	0.206			
	(0.214)			
Observations	1,208	1,208	1,208	1,208
R-squared	0.693	0.877	0.976	0.976
Bank × Firm FE	Yes	Yes	Yes	Yes
Country × Time FE	No	Yes	Yes	Yes
Firm × Time FE	No	No	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Table 1.21: Robustness: Eligible (0/1). This table provides results of difference-in-differences regressions analyzing the volume of loan issuances before versus after the ECB's collateral framework change. The analysis is based on data on the firm-bank-time-level. The sample period is Q1 2006 to Q2 2008. *Ln(Loan amount)* is the logarithm of (one plus) the loan issuance from bank i (as lead arranger or participant) to firm j at quarter t. *Eligible (0/1)* is an indicator variable equal to one if banks have an above median share of euro area loan issuance (excl. domestic) in Q2 2005. *Post* is an indicator variable equal to one after the framework change got implemented in January 2007. The control variables are lagged by 4 quarters and defined as in the Appendix A1. The regressions further include firm × bank fixed effects, country × quarter and firm × quarter fixed effects, as indicated. Country fixed effects refer to the borrowers' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)	ln(1 + loan volume)
$\text{Eligible}_{05}(0/1)$	0.071	0.266*	0.305**	0.304**
	(0.144)	(0.137)	(0.124)	(0.119)
ln(Total assets)				-0.026
				(0.189)
Equity ratio				-0.007
				(0.028)
ROE				-0.001
				(0.001)
Cash ratio				-0.012
				(0.010)
Deposit ratio				0.002
				(0.004)
Post07_t	0.289***			
	(0.089)			
Observations	1,208	1,208	1,208	1,208
R-squared	0.693	0.878	0.976	0.976
Bank × Firm FE	Yes	Yes	Yes	Yes
Country × Time FE	No	Yes	Yes	Yes
Firm × Time FE	No	No	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Appendices

1 Appendix A1

Variable Definitions.

Variable	Source	Description
Loan amount (mn USD)	DealScan	Total loan amount in million USD to firm j by bank i in quarter t
All-in-drawn spread (bps)	DealScan	The sum of the spread over LIBOR including annual fees in basispoints (bps)
Maturity (months)	Dealscan	Maturity of syndicated loan in months
Share of lead arrangers	Dealscan	Indicator variable equal to one if a bank acts as lead arranger
Share of euro area firms	Dealscan	Indicator variable equal to one if a firms' headquarter is located in the euro area
Domestic over total loan issuance	Dealscan	Share of domestic loans over total syndicated loans, where "domestic loans" are loans given to a firm which has the same headquarter location as the bank.
Euro area over total loan issuance	Dealscan	Share of euro area loans over total syndicated loans, where "euro area loans" are loans given to a firm which has its headquarters in the euro area.
ln(Total Assets)	CapitalIQ (SNL Financial)	The natural logarithm of total assets plus one.
Loans ratio	CapitalIQ (SNL Financial)	Share of gross loans over total loans
Equity ratio	CapitalIQ (SNL Financial)	Share of equity over total assets (leverage ratio)
ROE, in (%)	CapitalIQ (SNL Financial)	Bank i's return on equity
Cash ratio	CapitalIQ (SNL Financial)	Share of cash and equivalents over total assets
Liquidity ratio	CapitalIQ (SNL Financial)	Share of investment securities over total assets
Deposit ratio	CapitalIQ (SNL Financial)	Share of deposits over total assets
Ln(volume)	Dealscan	Natural logarithm of (one plus) the loan issuance from bank i (as lead arranger or participant) to firm j at quarter t
Constrained - Securities	CapitalIQ (SNL Financial)	Indicator variable equal to one if bank i's securities to total assets ratio is below the 50th percentile in 2006, and zero otherwise
Constrained - Tier1Ratio	CapitalIQ (SNL Financial)	Indicator variable equal to one if bank i's Tier1Ratio is below the 50th percentile in 2006, and zero otherwise
$\text{Affected}_{05,i}$ (%)	Dealscan	Share of bank i's euro area (excluding domestic) over total syndicated loan issuance in the period Q1 2003 to Q2 2005
$\text{Affected}_{05,i}$ (0/1)	Dealscan	Indicator variable equal to one for banks which have an above median share of euro area (excl. domestic) over total syndicated loan issuance in the period Q1 2003 to Q2 2005
Affected over $\text{TA}_{05,i}$ (%)	Dealscan	Share of bank i's euro area (excluding domestic) over total assets in the period Q1 2003 to Q2 2005
$\text{Eligible}_{05,i}$ (log)	Dealscan	Natural logarithm of (one plus) bank i's euro area (excl. domestic) loan issuance in Q2 2005
$\text{Eligible}_{05,i}$ (0/1)	Dealscan	Indicator variable equal to one for banks which have an above median share of euro area (excl. domestic) loan issuance in Q2 2005
Post07_t		An indicator equal to one after the framework change got implemented in January 2007, and zero otherwise
Post05_t		An indicator equal to one after the framework change was announced in June 2005, and zero otherwise
$\ln(\sigma(ROA_f)^{5y})$	Dealscan/Compustat	Logged five-year standard-deviation of loan-financed firm j's ROA from year *t-5* to *t-1*

Tradable	Dealscan/Compustat	Indicator variable equal to one if firm j is active in tradable industries (SIC codes 2000-3999)
Non-tradable	Dealscan/Compustat	Indicator variable equal to one if firm j is active in non-tradable industries (SIC codes 1500-1799, 5000-5999, 7000-7099)
Private	Dealscan/Compustat	Indicator variable equal to one if firm j is a private firm
Public	Dealscan/Compustat	Indicator variable equal to one if firm j is a public firm
Pr(Loan)	DealScan	Indicator variable that equals one if firm j obtains a bank loan in period t, and zero otherwise
$\text{Affected}_{06,j}$ (0/1)	Dealscan	Indicator variable equal to one if firm j has a relationship with an "affected" bank in 2006
ln(Total assets)	Compustat	Natural logarithm of firm j's total assets
Leverage	Compustat	Ratio of firm j's long term debt to total assets
Liquidity	Compustat	Ratio of firm j's cash flow over total assets
Employment	Compustat	Number of firm j's employees, in thousand
ln(Employment)	Compustat	Natural logarithm of the number of firm j's employees
Investment	Compustat	Firm j's Capital expenditure, CAPEX
ln(Investment)	Compustat	Natural logarithm of firm j's capital expenditure

Chapter 2

They Who Call the Piper Pay the Tune: Bank Bailouts and Political Connections of Bank Boards

1 Introduction

The financial crisis following the collapse of Lehman Brothers in September 2008 has led to an unparalleled wave of government interventions in the banking sector worldwide. With billions of tax payer money flowing into ailing banks, the nexus between banks and politics has arguably become more opaque (Sapienza (2004), Duchin and Sosyura (2012), Moon and Schoenherr (2021)). Anecdotal evidence comes from the French bank rescue programme in 2009: after major losses in the common investment arm, Banques Populaires and Caisse d'Epargne merged into one banking group, receiving a total of 7.05 billon EUR from the French government. Consequently, Francois Perol, the former chief economic advisor of then President Sarkozy was appointed chairman and chief executive of the new banking group. Combining two novel datasets on the political ties of bank boards and state aid in the European Union (EU), we shed light on the role of governments in influencing bank board composition after state aid interventions and its impact on bank performance.

We find that banks subject to government aid increase their number of politically connected board members by 21.4%, compared to banks which were not subject to such aid. This holds for banks which are considered too-big-to-fail (TBTF). Next, we look at the type of state aid in influencing board composition. Especially asset relief measures have an impact on the number of politically connected board members. Restricting our sample to bailed-out banks, we then empirically verify if and how these new political ties impact bank performance compared to banks without such new political ties. We provide evidence that following a bailout, banks with such new political ties perform better in terms of market capitalisation and valuation than bailed-out banks without such ties. This evidence suggests a role of political board members in providing valuable information during crisis times.

The empirical analysis focuses on the response of European governments to the financial crisis and the subsequent European debt crisis. We hand-collect government aid given to individual banks for the 28 countries of the European Union, and break

down the information into amount and type of government aid.[1] We identify a bank to be subject to government aid if it has received at least one aid measure between 2008 and 2015. To measure political connections, we hand-collect data on boards of directors of European banks from the CapitalIQ (SNL financial) database.[2] To build a time-varying political connection variable on the bank-level, we first extract the full employment history of all board directors. We then identify directors to be "politically connected board members" if they hold current or former positions in a ministry, a party or a state or local government. Lastly we add balance sheet data for our sample of banks from CapitalIQ (SNL financials) to identify too-big-to-fail banks based on their share of assets in the banking system of the respective country's headquarters.

We use a difference-in-differences approach to tackle the effect of a state aid intervention on the political composition of bank boards. In doing so, we compare the number of politically connected board members of banks subject to state aid (treatment) relative to banks not subject to state aid (control). Our identifying assumption is that banks not subject to state aid provide a counter-factual for the amount of political ties in the absence of a state aid intervention. The most obvious identification challenge is that our treatment is lacking random assignment. Instead, it is based on a variable that affects treated and control group differentially and correlates with the state aid intervention. To tackle such endogeneity concerns, we include bank-level control variables for size, health and leverage. Furthermore, this is less of an issue once we turn to bank performance and focus on the restricted sample of bailed-out banks. The idea here is to distinguish between bailed-out banks, which appoint new political connected board members, compared to bailed-out banks which did not. We differentiate hence between changes of political connections within bailed-out banks. Another possible concern is linked to reverse causality: political connections may increase the probability of receiving a

[1] A recent paper by Acharya et al. (2021) presents a similar dataset. However, the focus lies on a shorter time period, and on euro area banks only.

[2] We follow recent literature on political connections using granular director data, see i.e. Duchin and Sosyura (2012), D. Ferreira, Kirchmaier and Metzger (2010)

bailout in the first place, as highlighted in Duchin and Sosyura (2012). We address this by lagging our explanatory variable of interest for one period. The reasoning behind this is that although current values of bailouts might be endogenous to the number of political board members, it is unlikely that past values of bailouts are subject to the same problem. Lastly, we confirm the parallel trend assumption using parametric tests: the political ties between treated and control group of banks did not differ systematically in the period prior to the state aid interventions.

This chapter relates to the literature in the following ways. First, we add to the corporate literature on boards of directors, which has highlighted their endogenous nature (Hermalin and Weisbach (1998)). Similarly, only limited evidence exists on the causal estimates of the effect of firm characteristics on board structure or viceversa (see D. Ferreira, Kirchmaier and Metzger (2010), D. Ferreira, M. Ferreira and Mariano (2018)). Recent work by Baltrunaite and Karmaziene (2021) however provides evidence on how the supply side of potential candidates affects board appointments in private firms in Italy. Our chapter relates closely to D. Ferreira, M. Ferreira and Mariano (2018), who show that the number of independent directors on corporate boards increase after a financial covenant violation. In our case, we provide evidence that political ties of bank boards increase following a bailout by the government. This confirms theoretical work done by Aghion and Bolton (1992): governments influence bank board compositions as a way to secure control rights after a state aid intervention.

Second, we also add to literature on state aid to the banking sector. The financial crisis prompted a set of theoretical contributions on how to set-up state aid interventions in the best possible way. Two papers dealing with the debt overhang problem and state aid interventions are Philippon and Schnabl (2013) and Bhattacharya and Nyborg (2010). Both analyse state aid in terms of equity injections, able to eliminate debt overhang, while minimizing subsidies to bank's equity holders. Bruche and Llobet (2014) suggest asset buy-backs as means to end inefficient

gambling for resurrection by banks. Recent empirical contributions focused mostly on the effects of the Troubled Asset Relief Programme (TARP) enacted in October 2008 in the US.[3] Several paper shed light on the impact of state aid: Li (2013) finds that TARP funding indeed increased the supply of lending. By contrast, Duchin and Sosyura (2014) find that TARP banks approved riskier loans, while there is no evidence for an increase in the credit supply. Black and Hazelwood (2013) find that the risk of loans originated increased for large TARP banks, but decreased for small TARP banks. Turning to Europe and different bailout programmes, a recent paper by Acharya et al. (2021) shows that the type of state aid depends on the financial constraints of governments: financially constrained ones provided banks with guarantees instead of fully-fledged recapitalizations, which resulted in zombie lending. Duchin and Sosyura (2012) highlight the political dimension of state aid and provides evidence that existing political links increase the probability of obtaining state aid in the first place. Similarly, Koetter and Noth (2016)) uses political links of banks to proxy for bailout probabilities. We provide a more nuanced picture by abstracting from pre-existing political links and show that the political composition of bank boards depends on the different types of state aid measures applied to the respective banks.

More in general, we add to the literature on the impact of political links on bank performance: Sapienza (2004) finds that the interest rate charged by government-owned banks in Italy reflect the power of the party that controls the bank, resulting in preferential access to capital for party-affiliated borrowers. Similarly, Khwaja and Mian (2005) investigates rents to politically connected banks in Pakistan and finds similar distorting results, especially in election times. Indeed, a range of papers has highlighted a electoral credit cycle in political banks (Dinç (2005), Halling, Pichler and Stomper (2016), Bircan and Saka (2019)). Duchin and Sosyura (2012) look at the performance of connected and unconnected banks subject to TARP and find that the former underperform. We find that within bailed-out banks, banks with a newly appointed political board member perform better in terms of market capit-

[3] For an overview of the full literature, see Calomiris and Khan (2015).

alisation and valuation compared to their bailed-out peers without such new board members. This suggests a role of political board members in providing valuable information during crisis times (Downs (1957)). However, it is important to highlight that this analysis does not put into question the long term distortions of political links discussed in the literature so far.

The rest of the Chapter is organized as follows. Section 2 sets the scene. Section 3 describes data, variables and the empirical strategy. Section 4 presents the results. Section 5 gives some robustness checks. Section 6 concludes.

2 Institutional Setting

2.1 State Aid in the European Union

With the start of the financial crisis in 2008, significant amounts of public money have been granted to the EU financial sector. Although the measures varied in amount and complexity in the various member states the European Commission identifies four main aid types: bank recapitalisation, asset relief measures, as well as guarantees on liabilities and other liquidity support.

Figure 2.1 shows the amount in EUR billion of each aid instrument over the years 2008 to 2015. State aid in the EU happened in two waves: one following the fallout from the financial crisis in 07/08, and one following the European debt crisis as of 2011/12. Guarantees make up by far the most used instrument - however, the numbers here relate to guarantees pledged, as it is impossible to obtain any information on guarantees effectively *used* on a bank-level basis. Recapitalisation measures are the second most used instrument, followed by other aid. While all aid instruments have been used across the sample period, especially asset relief measures increase in importance with the outbreak of the European debt crisis. Figure 2.2 shows the amounts broken down by country, this time excluding guarantees from the calcu-

lations. The United Kingdom has by far spent most money on its ailing banking sector, followed by Germany, Spain, Ireland and Greece. We will now discuss the single aid instruments in more detail:

Recapitalisation Measures in the form of capital injections aim at improving bank capital ratios. Governments participate in the capital of a distressed bank through non-voting or voting, preferred or ordinary shares. Similarly, some governments used the issuance of contingent convertible subordinated bonds (CoCos) to shore up banks tier1 balance sheets.

Asset Relief Measures aim at relieving banks from assets which are considered toxic or impaired. To do so, governments resort to asset purchases and asset guarantees, or a mix of both. Asset purchase measures consist in transferring impaired assets from the balance sheet of the beneficiary bank to another entity. This can take the form of a special purpose vehicle (SPV), fully or partially sponsored and/or guaranteed by a government. Examples are the establishment of the Irish National Asset Management Agency (NAMA) in 2009, the German FMS Wertmanagement (FMS) in 2010 and the Spanish Management Company for Assets Arising from the Restructuring of the Banking Sector (Sareb) in 2012. By contrast, with asset guarantee measures the portfolio of impaired assets remain on the balance sheet of the troubled bank, but the potential losses are guaranteed by the state.

Guarantees on Liabilities aim at supporting bank funding and reducing the risk premia which banks have to pay in money markets. Some governments announced guarantees to the banking system as a whole. The most common case was however guaranteeing individual portions of a banks' balance sheet.

Other Aid is a residual which contains minor direct liquidity support measures.

2.2 Bank Boards and Control Rights

The formative work by Aghion and Bolton (1992) theorize a pecking order theory of governance structures, by first pointing out that any minimum degree of incompleteness in financial contracts raises issues of control allocation between an entrepreneur and an investor. They then elaborate on the optimal control allocation and find that

> "it is always best to start first with entrepreneur control if that is feasible. If, however, entrepreneur control does not sufficiently protect the investor's claims, one should go for contingent control. Finally if that is still not enough to protect the investor's interests, one wants to give full control to the investor. This ordering of governance structures corresponds to the following ordering of financial contracts: first, try non-voting equity; if that doesn't work, try to share ownership by issuing some but not all voting shares to outside investors and/or issue debt; finally, give away all the control rights to the investor by raising all funds in return for voting-equity. *(page 491-492, Aghion and Bolton (1992))*"

We can map this to our cases of government interventions in the banking sector. With recapitalisation measures, the state (= investor) participates in a bank through a capital injection and gets in turn voting or non-voting equity. How does this influence the governance structure of the bank? Following what was said before, entrepreneur control happens through the allocation of *non-voting* equity. Hence, the state in this case does not have the power to elect the board of directors and therefore, we should not expect an increase in political board members after the recapitalisation measures. By contrast, investor control happens through the allocation of *voting* equity. In this case, the state, among others, has the power to elect the board of directors, and hence we should expect an increase in the number of political board members on bank boards subject to recapitalisation measures. Contingent control allocation depend on states of the nature, and can be a mix between some but not all voting shares and/or issue debt.[4] A similar mapping can be done for the remaining types of state aid. Asset relief purchases for example are very often

[4] Due to data limitations, we cannot distinguish between recapitalisation measures through voting or non-voting equity in our state aid sample, but only if the measure applied is a recapitalisation. This implies a lower bound: if all recapitalisation measures were implemented using non voting equity, the political board composition would not change.

done by transferring the impaired assets to a new entity. In case this new entity is fully or partially sponsored by the state, voting equity is allocated to the state and outside investors. Here, the state has the power to elect the board of directors, and hence we should expect an increase in the number of political board members on those boards. Asset relief in the form of guarantees, as well as other forms of guarantees and other liquidity support do not imply any transfer of control rights to the state in the form of equity or convertible debt issuance. Hence, they should not have any impact on the political connections of bank boards.

Such a set-up helps also understanding ex-post bank performance. Bank receiving government support through the means of non-voting equity and guarantees will be under the control of the entrepreneur, while banks receiving government support through the means of voting equity will be under the control of the investor. So the question becomes: What do these new politically appointed directors do? A first scenario might be that they enable a constant flow of information between the government and the bank. This builds on the idea of Downs (1957), who argues that political connections can mitigate the information asymmetry between government officials and firms. This might especially be important in times of crises. In this case, we expect the bailed-out banks with a newly appointed political board member (connected) to outperform those of their politically unconnected bailed-out peers ex-post. A second scenario might be that the new political board members cater to their private interest. This builds on the theories of politics of government ownership and investment (Shleifer and Vishny (1994)), which raise the point that public enterprises pursue political goals; politicians only cater for themselves to secure electorate votes, fund election campaigns, and to extract personal benefits from corporate lobbying. Under this hypothesis, connected bailed-out banks are likely to trail their unconnected bailed-out peers in terms of performance.

3 Data and Empirical Strategy

3.1 Data

Measure of Political Connections To construct our measure of a board members' political connection, we hand-collect data on boards of directors of our sample banks from the CapitalIQ (SNL financial) database. First, we extract the full employment history of all board members of a bank. We then identify a board member to be "politically connected" if they hold current or former positions in a ministry, a party or a state or local government. At the same time, we collect the period of active board membership of the board member in question. Combining the two gives us the number of active politically connected board members of bank i in a certain year t. Similarly, we also identify the number of board members without political links, and define them as "other board members" of bank i in a certain year t. We obtain a sample of 112 too-big-to-fail banks for which we have data on their political connections: 73 are politically connected, and 39 are not.

In Table 2.1, we split the sample into banks subject to state aid (Bailout Sample) vs. banks not subject to state aid (No Bailout Sample). The average number of connected board members is 0.60 in the bailout sample, and 0.43 in the no bailout sample. The probability of getting a new politically connected board member between 2008-2012 in the bailout sample is 0.98 compared to 0.67 in the no bailout sample. Not surprisingly, the average number of other board members is slightly smaller in the bailout sample (4.24) compared to the no bailout sample (5.21). Banks in the bailout sample have more political board members in the period between 2008-2012 (0.98) compared to banks not in the bailout sample (0.67).

State Aid We hand-collect state aid given to individual banks for the 28 countries of the European Union, and break down the information into amount and type of state aid.[5] Our primary source for state aid to financial institutions is the online

[5] A recent paper by Acharya et al. (2021) presents a similar dataset. However, the focus lies on a shorter time period, and on euro area banks only.

database on state aid provided by the European Commission. It contains all state aid cases which occurred in the European Union, and in particular, it contains state aid within the meaning of Article 107(1) TFEU[6] granted to financial institutions.

Four aid instruments can be distinguished on a bank-level basis: (i) Recapitalisation measures, such as capital injection, aimed at improving a bank's capital ratios; (ii) Guarantees on liabilities, aimed at supporting bank funding and reducing the risk premia which banks have to pay in money markets; (iii) impaired asset measures, aimed at relieving banks from assets which are considered toxic or impaired and often linked to the establishment of a good/bad bank; (iv) other aid, which is a residual including various liquidity support measures. It is important to highlight that we only have information about *implemented* state aid, involving the aid actually used and given to financial institutions. We identify a bank to be subject to government aid, if it has received at least one of the four state aid measures. From this we construct a time-varying indicator variable, which takes the value of one when a bank i is subject to government aid at year t, and stays one thereafter.

Of the 112 banks in our full sample, 42 banks received state aid and 70 banks did not (Table 2.1). Recapitalisation measures seem to be the most important measure implemented by the respective governments, followed by other aid, guarantees and asset relief measures. Nationalisation and liquidation are the least used.[7]

Financial Variables We download bank balance sheet data from CapitalIQ (SNL financial) to control for banks' financial condition and performance. In Table 2.1, we present summary statistics both for the bailout and the no bailout sample. Bailed-out banks are slightly bigger than not bailed-out banks in terms of their asset size (312 mn EUR vs 229 mn EUR). The two groups are similar in terms of loans, debt and deposits (around 63%. 42% and 44% for both groups). On average, bailed-out banks have slightly worse revenues (1.46% vs 1.86%), tier1ratios (10.07%

[6] Treaty on the Functioning of the European Union

[7] Note that a single bank can be subject to more than one of the listed government aid measures.

vs 12.09%), loan losses (-2.53% vs -1.95%), and bank performance indicators such as ROA (-0.03% vs 0.43%) and ROE (-2.37% vs 5.88%). Banks in the bailed-out sample rely more on short term debt (12.69%) than the no bailout sample (9.37%).

3.2 Empirical Strategy

We argue that in crisis times, governments are likely to influence the bank board composition to secure control rights after a state aid intervention. This line of argument follows from the theoretical contribution of Aghion and Bolton (1992) on the contingent allocation of control rights: outside investors (= the state in our case) require control rights to safeguard themselves from potential conflicting choices of action by the entrepreneur (= the bank in our case). By contrast, banks *not* subject to such government interventions should not be subject to such dynamics.

We take the European governments' decisions to bail-out their respective banks during the crisis years 2008-2012 as a proxy for a governments' window of opportunity to influence banks board composition. We construct a post indicator variable able to capture the starting date of state aid interventions, which vary across EU countries. We use (whenever applicable) the implementation dates of national state aid packages in the respective countries to define our post indicator, valid for both banks subject to state aid, and banks not subject to it. As an example, Germany announced its state aid scheme to banks in February 2009. The post indicator for German banks (both subject and not subject to government aid) is therefore equal to zero up to the announcement year 2009, and stays equal to one from 2009 onwards. Consequently, ($Post_{ct}$) varies on a country-level. Overall, we examine how the number of politically connected board members change in bank boards subject to government aid compared to banks not subject to it. The null hypothesis is that government interventions are irrelevant for the political composition of bank boards.

To account for the heterogeneous introduction across EU member states, we implement our test using a staggered difference-in-differences (DiD) regression framework.

We focus our analysis on banks which are too-big-to-fail (TBTF)[8], which have been at the center of government support measures given their systemic relevance. We estimate the following equation:

$$Y_{it} = \beta_1 \; Bailout_i \times Post_{ct} + \beta_2 \; Bailout_i + \beta_3 \; Post_{ct} + \gamma \mathbf{X'}_{it} + \phi_i + \phi_{ct} + \varepsilon_{it} \quad (2.1)$$

where the dependent variable Y_{it} is the log (plus one) of the number of politically connected board members (PC_{it}) for bank i in year t. As a robustness check, we change our dependent variable Y_{it} to the log (plus one) of the number of other board members ($noPC_{it}$) for bank i in year t. $Bailout_i$ is an indicator variable equal to one if bank i received a bailout and 0 otherwise. $Post_{ct}$ is an indicator variable equal to one when country c announced government aid to its banking system in year t, and 0 before. $\mathbf{X'}_{it}$ is a vector of bank-level controls (total assets, leverage ratio, tier1ratio), ϕ_i are bank fixed effects, ϕ_{ct} are country $\times$ time fixed effects. The standard errors are clustered at the bank-level.

In the first specification, the set-up is a log-linear one. The interaction term of interest β_1 reflects the change in politically connected board members, when bank i receives government aid compared to when it does not receive government aid. Following our reasoning beforehand, we expect $\beta_1 > 0$ as governments are likely to influence board composition to secure control rights.[9] A sub-hypothesis is linked to the type of state aid. Asset relief measures done through asset purchases are often linked to the establishment of a new entity co-sponsered by the state. We expect the number of politically connected board members to increase even more. At the same time, asset relief in the form of guarantees, as well as other forms of guarantees do

[8] To get a proxy for systemic relevance, we first calculate the median of country c's total banking assets in 2007. We then define bank i to be TBTF (not TBTF) if its total assets are above (below) the median of their respective country c's total banking assets. As a robustness check, we estimate the same regression for banks which are not too big to fail, and do not find significant results.

[9] As a robustness check, we look at the number of politically unconnected board members and in line with expectations, we cannot reject the null hypothesis.

not imply any transfer of control rights to the state. Hence, they should not have any impact on the political connections of bank boards. We test this through the means of a horse-race of the different types of state aid.

In a second step, we investigate what happens to bank performance after these new political directors get appointed. In this case, we restrict the sample to banks subject to government aid. We first create a *Newly appointed connected board member* (NPC_i) indicator variable which takes on the value of one if, during 2008-2012, there has been an increase in the number of politically connected board members, and zero if the was no increase. Second, we create an indicator variable $Post_{it}$ equal to 1 for the years 0, 1, 2, 3 and 4 (defined as the period after the bailout of bank i), and equal to 0 for the years -4, -3, -2 and -1 (defined as the period before the bailout of bank i).[10] We estimate the following equation:

$$Y_{it} = \beta_1 \; NPC_i \times Post_{it} + \beta_2 \; NPC_i + \beta_3 \; Post_{it} + \gamma \mathbf{X'}_{it} + \phi_i + \phi_t + \varepsilon_{it} \qquad (2.2)$$

where y_{it} is a bank outcome, $Post_{it}$ is an indicator variable, which takes on a value of one after bank i was subject to government aid at time t; NPC_i is an indicator variable equal to one if bank i experienced a newly appointed political connected board member after the government aid, and zero otherwise; $\phi_i + \phi_t$ are bank and year fixed effects respectively, and $\mathbf{X'}_{it}$ is a vector of bank-level variables such as total assets. Our key variable of interest is the interaction term between the newly appointed board member and the Post Bailout indicator variable. The coefficient β_1 on this term captures the difference in performance between bailed-out banks with a newly appointed political board member and their bailed-out unconnected peers. The interpretation is similar to that of a difference-in-differences estimator, except that the "treatment" here - an increase in political board members - is endogenous, which means that the estimated coefficient should not be interpreted as a causal

[10] In the corporate finance literature, D. Ferreira, M. Ferreira and Mariano (2018) use a similar approach when analysing the role of new directors on firm boards after covenant violations.

effect. We cluster the standard errors at the bank-level.

4 Main Results

This section presents our main empirical results. We split our analysis into three parts. In Section 4.1, we examine the role of state aid in influencing the political ties of bank boards. We show that the number of politically connected members of bank boards increase for banks subject to state aid compared to banks not subject to it. Turning to the type of state aid, the number of political board members increase especially for banks subject to asset relief measures. Section 4.2 discusses the impact of new political board members on bank performance measures. Lastly, Section 4.3 provides evidence on the validity of the parallel trend assumption using a parametric test.

4.1 Effect of State Aid on Political Connections

Univariate Analysis Table 2.3 examines potential differences in political connections of boards and financial characteristics between banks subject to state aid, and banks not subject to state aid. In other words, between our treatment and control group, respectively. Column (1) and (3) report mean values for the period after the state aid announcement in the respective country. Column (5) reports the difference in means and Column (6) the respective t-statistic.

We find that the average number of politically connected board members in the bailed-out group is two times higher than in the not bailed-out group (0.77 to 0.39). The probability of having a new political board member in the period between 2008-2012 is also almost two times higher in the bailed-out group than in the not bailed-out group. Both differences are statistically significant at the 5% level. Also, the probability of being politically connected is slightly higher for banks subject to

a bailout than for banks not subject to a bailout (0.79% to 0.61%). This result is significant at the 10% level. Also, bailed-out banks have a lower revenues-to-assets (1.02% to 1.55%), more loan losses (-3.10% to -1.61%) and underperform in terms of ROA (-0.41% to 0.20%) and ROE (-10.33% to 2.70%). The threat to our identification does not, however, come from cross-sectional differences across bailed out and not-bailed out banks, as these are taken care of with our difference-in-differences approach. Instead, the threat comes from time-varying differences. In Section 4.3, we will turn to a test of the parallel trend assumption to address this concern.

Multivariate Analysis Table 2.4 provides formal regression evidence for Equation (2.1): the number of politically connected members of bank boards increase for banks subject to state aid compared to banks not subject to it.[11] Each column includes more stringent levels of fixed effects. The dependent variable is the log (plus one) of the number of politically connected bank board members. In Column (1) we do not include any fixed effects, the interaction term of interest is positive but insignificant. We start with including bank-fixed effects in Column (2), which compares the number of political board members of bailed-out banks versus not bailed-out banks before and after the bailout was announced in their respective home countries. Our interaction term of interest β_1 is positive and statistically significant. In Column (3), we include country $\times$ time fixed effects to take care of time-varying differences across banks driven by their home countries. We find again a positive treatment effect, significant at the 5% level: bailed-out banks increase their number of politically connected board members after a country's announcement of state aid. In Column (3), we include time-varying variables on the bank-level (total assets, tier1 ratio, leverage ratio) to refine our treatment and control group. The coefficient of interest remains significant at the 5% level and even increases in magnitude. A bank subject to a bailout increases its number of politically connected board members by $(exp(0.194) - 1)$ 21.4%. Economically speaking, this implies an increase

[11] We focus our analysis on TBTF banks. As a robustness check, we look at banks which are not TBTF, and do not find any significant results.

of (0.19 × 0.58) 0.11 politically connected board members, evaluated at the (full) sample average of the number of politically connected board members. This evidence provides support for our argument that governments influence bank boards to secure control rights after a state aid intervention, as opposed to banks not subject to state aid.

Next, Table 2.5 looks at the different types of state aid and their impact on political bank boards. In line with our sub-hypothesis, we find that asset relief measures have a twice as high an impact on the number of politically connected board members compared to recapitalization measures. At the same time, we do not find significant results for guarantees.[12] In sum, we show that the number of political board members depends on the type of state aid intervention.

4.2 Effect of Political Connections on Bank Performance

We now turn to the impact of these newly appointed political board members on bank's operating decision, and in particular, on bank performance. In the previous analysis we have shown that banks subject to a bailout experience an increase in their politically connected board members as opposed to banks not subject to a bailout. Similarly, we have shown that the number of political board members depends on the type of state aid applied. The question now is, if these newly appointed political board members act differently from their unconnected peers when it comes to bank performance. To answer this, we shift our focus to the bailed-out sample, and distinguish between connected bailed-out banks, which appointed new political connected board members, compared to unconnected bailed-out banks which did not. We differentiate hence between changes of political connections within our bailout sample. When examining bank performance, it is important to highlight

[12] We do not include other aid measures in this analysis, as it is a residual category and does not allow a general interpretation.

that, as opposed to the US Troubled Assets Relief Programme (TARP) where all benefiting banks were charged the same dividend rate on the investment done by the US Treasury, the European approach differed from country to country in terms of measures taken and conditions attached. However, we argue that at least in terms of recapitalisations, each bank subject to such a measure, must have been charged some kind of unfavourable dividend rate. In this case, the mechanism, through which the taxpayers money would be safeguarded, is still the same and depends on the stock market performance of the respective bank. Therefore, following Duchin and Sosyura (2012), we focus on stock returns and market-based valuation measures, on top of accounting-based measures.[13]

We start out by showing results for Equation 2.2 on accounting-based measures in Table 2.6. The dependent variables are: revenue-to-assets ratio (Column 1), deposits-to-assets ratio (Column 2), loans-to-assets ratio (Column 3), debt-to-asset ratio (Column 4), ROA (Column 5) and a measure of bank i's ex-ante risk taking, $ln(\sigma(ROA_f)^{3y})$, the five-year standard-deviation of loan-financed firm j's ROA from year t-5 to t-1 (Column 6).[14] Detailed definitions for each measure can be found in the caption of Table 2.6 and in the Appendix 1.12. Our analysis is on the bank-year-level. Each column includes bank and country $\times$ time fixed effects, where country fixed effects refer to the banks' respective headquarters. Standard errors are clustered at the bank-level.

We find that connected bailed-out banks decrease their deposits by 7.6% (Column 2) and increase their debt ratios by 11.1% compared to their unconnected bailed-out peers (Column 4). Next, we look at different measures of risk, and find that banks with a newly appointed political board member experience an increase in both their ROA (Column 5) and their risk taking (Column 6), compared to the control group. By contrast, looking at revenues (Column 1) or loan issuance (Column 2), we do not find any significant differences.

[13] Given that not all banks in our sample are quoted on the stock exchange, we restrict our analysis to 22 banks in the bailout sample, for which we were able to obtain stock market data.

[14] This definition of risk taking behaviour follows Heider, Saidi and Schepens (2019).

Table 2.7 presents the results for Equation 2.2 on market-based measures. The dependent variables are: net debt issues (Column 1), net equity issues (Column 2), market capitalization (Column 3) and Tobin's Q (Column 4). Detailed definitions for each measure can be found in the caption of Table 2.7 and in the Appendix 1.12. Again, our analysis is at the bank-year-level. Each column includes bank and country × time fixed effects. Standard errors are clustered at the bank-level.

Contrary to what we have found in the accounting based analysis, we do not find any difference in terms of net debt issuance (Column 1) and net equity issuance (Column 2). However, in terms of market capitalisation (Column 3) and Tobin's Q (Column 4), our coefficient of interest is positive and statistically significant. This suggests that investment in politically connected banks outperformed those of unconnected banks. The difference is also economically significant: Banks with a newly appointed political board member increase their market capitalisation by exp(0.368) - 1 = 44.5 % compared to their bailed-out unconnected counterparts. Similarly, banks with such new political ties increase their Tobin's Q by exp(0.103) - 1 = 10.8% compared to their bailed-out unconnected counterparts.[15]

In sum, bailed-out banks subject to state aid with a newly appointed political board member seem to exceed the performance of their bailed-out peers which are unconnected, confirming Downs (1957) work on valuable information-exchange between government officials and firms.

4.3 Parallel Trends Assumption

Addressing the parallel trends assumption in our staggered Difference-in-Differences set-up is slightly complex. Normally, we would set up a coefficient plot showing the impact of a bank being subject to state aid on the number of politically connected board members over time. This allows analysing if the number of political board

[15] As long as a newly vs. no newly appointed political board member reflects the choice of recapitalisation measures vs. guarantees, our findings are in line with Acharya et al. (2021)

members between banks subject to state aid and banks not subject to it did differ systematically in the period prior to the government support (the treatment).

However, as we do not have a single treatment date, the concept of pre-treatment becomes fuzzy. To evaluate the pre-treatment dynamics between a treatment and control group with differential timing, we will follow what was done in Miller, Johnson and Wherry (2019), and estimate a regression model that includes treatment leads and lags.[16] In our case, we examine the impact of a bank being subject to state aid on the number of politically connected board members. We include treatment leads or lags, where treatment (government support) occurs at time 0 in country c. Again, we focus on too-big-to-fail banks and estimate the following equation:

$$Y_{it} = \sum_{\tau=-q}^{-1} \gamma_\tau \; Bailout_{i\tau} + \sum_{\tau=0}^{m} \delta_\tau \; Bailout_{i\tau} + \beta \mathbf{X'}_{it} + \phi_i + \phi_t + \varepsilon_{it} \tag{2.3}$$

where Y_{it} is the log plus one of the number of politically connected board members of bank i. $Bailout_{i\tau}$ is an indicator variable equal to one if bank i received a bailout and 0 otherwise at event time τ. Treatment occurs in event year 0 and we include q leads effects and m lags or post-treatment effects.

Figure 2.3 plots the estimates of a bank being subject to state aid on the number of politically connected board members using leads and lags. For the pre-treatment period, the estimated coefficients are close to zero and statistically insignificant. This changes once state aid gets implemented - being a banks subject to government aid has a significant and positive impact on the number of politically connected board members. This provides support for the validity of the parallel trends assumption underlying our difference-in-differences framework.

[16] taken from Cunningham, Scott (2021) Causal Inference, Yale University Press

5 Robustness Checks

In this section, we present further robustness checks to corroborate the main findings. Section 5.1 sheds light on the role of being TBTF and our sample selection. Section 5.2 looks at the effect of state aid on other board members and does not find any significant results. Section 5.3 addresses the reverse causality problem by including lagged variables.

5.1 Too-Big-To-Fail Sample

Our analysis so far has focused on TBTF banks, as their systemic importance makes them preferred recipients of government aid, as opposed to banks which are not systemically relevant.[17] To substantiate this claim further, we first run Equation 2.1 on a restricted sample of banks which are not TBTF. Table 2.8 presents the results. Our coefficient of interest is significant and negative across specifications, but becomes insignificant and close to zero when we add bank-level control variables. Hence, banks which are not systemically relevant exhibit opposing dynamics in terms of political composition of board members.

Next, as a more rigorous test, we introduce a triple interaction term, $TBTF_i \times Bailout_i \times Post_{ct}$ and set up the following equation on the bank-year-level:

$$\begin{aligned} Y_{it} = \beta_1\ TBTF_i \times Bailout_i \times Post_{ct} + \beta_2\ TBTF_i \times Bailout_i + \\ +\beta_3\ TBTF_i \times Post_{ct} + \beta_4\ Bailout_i \times Post_{ct} + \beta_5\ TBTF_i + \\ +\beta_6\ Bailout_i + \beta_7\ Post_{ct} + \gamma \mathbf{X'}_{it} + \phi_i + \phi_{ct} + \epsilon_{it} \end{aligned} \tag{2.4}$$

where the dependent variable $Y_{i,t}$ is the log (plus one) of the number of politically connected board members (PC_{it}) for bank i in year t. $TBTF_i$ is an indicator variable equal to one if bank i's total assets are above the median of their respective country c's total banking assets in 2007, and zero otherwise. $Bailout_i$ is an indicator variable

[17] For a thorough discussion of the challenges of TBTF, see Veron and Goldstein (2011)

equal to one if bank i received a bailout and 0 otherwise. $Post_{ct}$ is an indicator variable equal to one when country c announced government aid to its banking system in year t, and 0 before. $\mathbf{X'}_{it}$ is a vector of bank-level controls (total assets, leverage ratio, tier1ratio), ϕ_i are bank fixed effects, ϕ_{ct} are country $\times$ time fixed effects. The standard errors are clustered at the bank-level.

The coefficient of interest is β_1 on the triple interaction term, which captures if our two-way interaction differs if a bank is TBTF or not. We expect this to be the case, as TBTF banks are more likely to receive government support due to their systemic relevance, compared to banks which are not systemically relevant. By contrast, an insignificant estimate indicates that being TBTF does not affect the number of political board members on a bank board, subject to government aid.

Table 2.9 presents the results. Our coefficient of interest is highly significant and positive across specifications. If we were to look at the full sample, and our two-way interaction term, we would get opposing and insignificant results. In other words, the interaction among a bank being bailed-out with the post indicator variable is different across the level of a bank being TBTF. This evidence supports our focus on the TBTF sample.

5.2 Unconnected Board Members

In Table 2.10 we re-run Equation 2.1 on other board members, defined as the log (plus one) of the number of politically unconnected board members ($noPC_{it}$). We do not find any effect, both for banks in the TBTF sample, and in the not TBTF sample. This provides further evidence that governments influence bank board composition through the election of political board members, as opposed to unpolitical ones.

5.3 Reverse Causality

In this section we address the potential reverse causality problem: political connections may increase the probability of receiving a bailout in the first place, as

highlighted in Duchin and Sosyura (2012). We address this by leading our dependent variable of interest for one period.[18] The reasoning behind this is that although current values of bailouts might be endogenous to the number of political board members (i.e. the probability of getting a bailout depends on the number of political board members), it is unlikely that current values of bailouts depend on the future number of political board members.

Table 2.11 presents the results. The dependent variable is the log (plus one) of the number of politically connected bank board members for bank i in year $t+1$. We start with including bank-fixed effects in Column (2), which compares the number of political board members of bailed-out banks versus not bailed-out banks before and after the bailout was announced in their respective home countries. Our interaction term of interest β_1 is positive, but not statistically significant. In Column (3), we include country $\times$ time fixed effects to take care of time-varying differences across banks driven by their home countries. We find a positive treatment effect, significant at the 10% level: bailed-out banks increase their number of politically connected board members one period ahead after a country's announcement of state aid. In Column (4), we include time-varying variables on the bank-level (total assets, leverage ratio, tier1 ratio) to refine our treatment and control group. The coefficient of interest remains significant at the 10% level and even increases in magnitude. A bank subject to a bailout increases its number of politically connected board members one period ahead by $(exp(0.216) - 1)$ 24.1%.

6 Conclusion

In this chapter, we investigate the relationship between political connections of bank boards and bank performance in times of banking crises. We find that the number of politically connected board members increases by 21.4% following government

[18] Using lead dependent variables with contemporaneous independent variables is essentially the same as using contemporaneous dependent variable with lagged independent variables, if the sample size is held constant.

support. Bailed-out banks with such new political ties perform better in terms of market capitalisation and valuation than bailed-out banks without such ties.

We provide new evidence along several dimensions. First, we confirm theoretical work done by Aghion and Bolton (1992) on the incompleteness in financial contracting and control allocation. Applying their pecking order theory of government structures to our bailouts setting, we show that governments indeed influence bank board compositions as a way to secure control rights after a bailout. Moreover, by by abstracting from pre-existing political links, we provide a more nuanced picture of the government-bank interplay. We find that the political composition of bank boards depends on the different types of state aid measures applied to the respective banks. Second, the political composition of bank boards depends on the different types of state aid measures applied to the respective banks. Second, we try to give an answer to what these new political directors do, once they are appointed. We find that they improve bank performance in terms of market capitalisation and valuation, as opposed to the prevailing argument in the literature that politicians only cater to their private interest (Shleifer and Vishny (1994)). This finding highlights the role of political board members in providing valuable information during crisis times (Downs (1957)), and highlight the need for further theoretical work on the optimal bailout schemes and the role of political board members.

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Tables and Figures

Figure 2.1: Types of State Aid, 2008-2012 This figure plots the four main types of state aid (recapitalisation, guarantees, asset relief, other aid) in EUR billion for all member states of the European Union over 2008-2012. Based on own calculations.

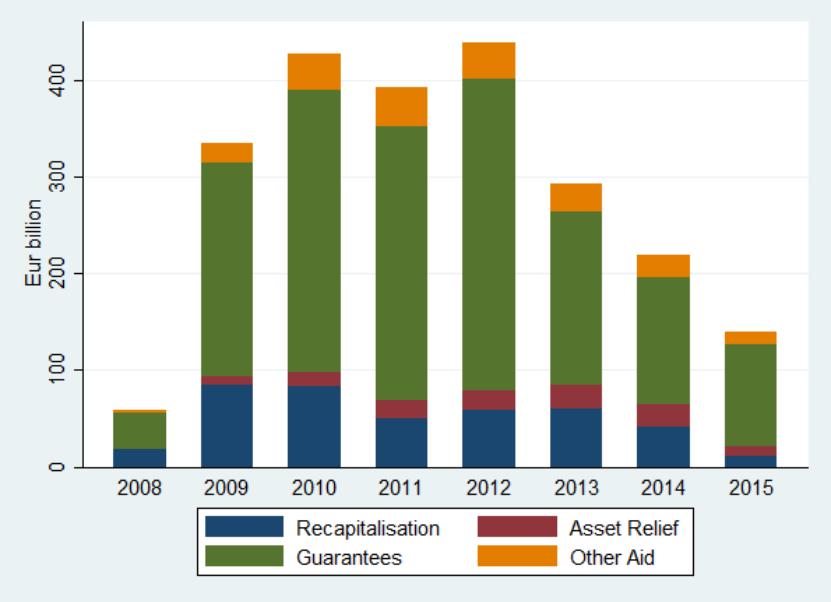


Figure 2.2: State Aid Over 2008-2012, by EU Member State This figure plots the total amount of state aid (recapitalisation, asset relief, other aid) in EUR billion for each member states of the European Union over 2008-2012. Based on own calculations.

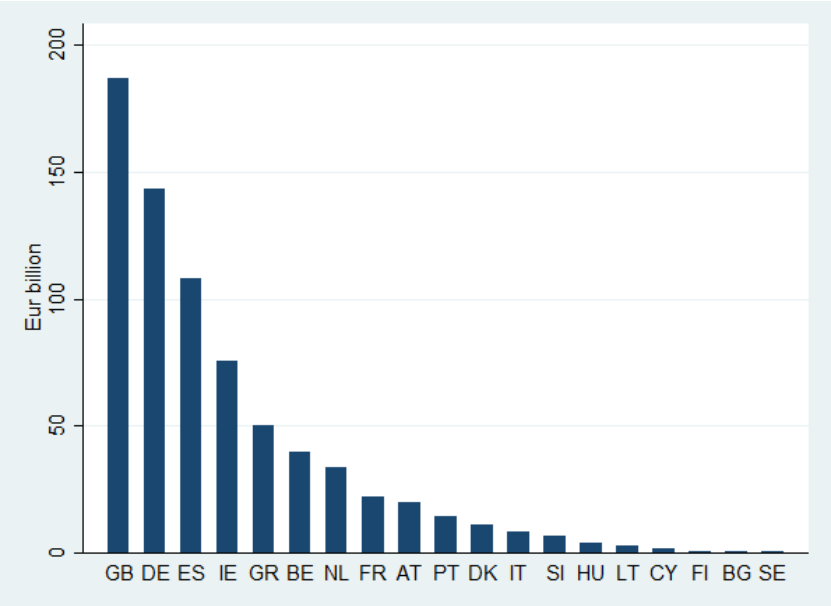


Figure 2.3: Parallel Trends Assumption. This figure plots the sequence of coefficient estimates from Equation 2.3, showing the dynamic effect of being subject to government aid on the share of politically connected board members for the event years -3 to 3 (where event year 0 = year the state aid was announced in respective country c). $Log(PC)$ is the log (plus one) of the number of politically connected board members for bank i in event year t. The estimations include bank-level controls and bank and year fixed effects. Error bars represent 90% confidence intervals.

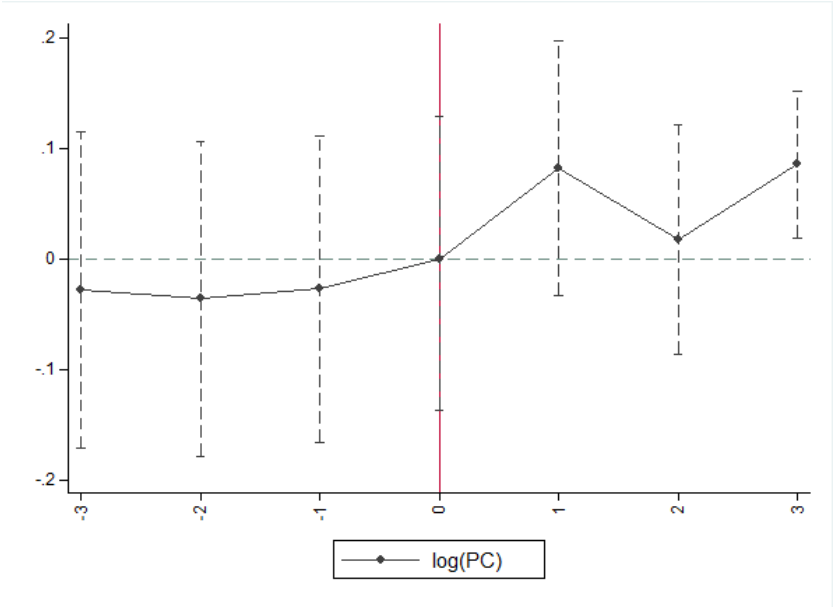


Table 2.1: Summary Statistics: Bank-Level (Full Sample) This table presents summary statistics for all too-big-to-fail banks located in the European Union included in the sample. We report our measures of political connections, government aid, financials and split the sample into banks subject to state aid, and banks not subject to it. *Nr of political board members* is the absolute number of politically connected members on a banks' board. *NPC (0/1)*, new political board member, is an indicator variable equal to one if bank i experienced an increase in its political board members in the period between 2008-2012. *Number of other board members* is the absolute number of politically unconnected board members. *Bailout (0/1)* is an indicator variable equal to one if a bank was subject to state aid and 0 otherwise. *Recap / Assetrelief / Guarantees / Other Aid / Nationalisation / Liquidation* is an indicator variable equal to one if bank i received a recapitalization / asset relief / guarantees / other aid / was nationalized / was liquidated, and 0 otherwise. Detailed definitions for each measure can be found in the Appendix 1.12. All bank-level variables are calculated using annual balance-sheet data for the year 2010.

	Bailout Sample					***No Bailout Sample***				
	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N
Nr. of political board members	0.60	0.89	0.00	3.00	42	0.43	0.69	0.00	3.00	70
New political board members (0/1)	0.98	1.02	0.00	4.00	42	0.67	0.79	0.00	3.00	70
Nr. of other board members	4.24	3.12	0.00	12.00	42	5.21	3.13	0.00	14.00	70
Bailout (0/1)	1.00	0.00	1.00	1.00	42	0.00	0.00	0.00	0.00	70
Recapitalisation (0/1)	0.95	0.22	0.00	1.00	42	0.00	0.00	0.00	0.00	70
Asset Relief (0/1)	0.38	0.49	0.00	1.00	42	0.00	0.00	0.00	0.00	70
Guarantees (0/1)	0.52	0.51	0.00	1.00	42	0.00	0.00	0.00	0.00	70
Other Aid (0/1)	0.62	0.49	0.00	1.00	42	0.00	0.00	0.00	0.00	70
Nationalisation (0/1)	0.31	0.47	0.00	1.00	42	0.00	0.00	0.00	0.00	70
Liquidation (0/1)	0.12	0.33	0.00	1.00	42	0.00	0.00	0.00	0.00	70
Total Assets (in mn EUR)	312.41	426.42	2.52	1,593.53	41	229.06	318.22	2.03	1,579.20	64
Loans-to-Assets Ratio	62.55	15.47	22.20	84.51	39	63.84	16.86	16.97	89.83	59
Revenue-to-Assets Ratio	1.46	1.23	-1.72	5.52	39	1.86	1.26	-0.96	4.87	59
Tier1ratio	10.07	2.47	4.29	16.50	33	12.09	3.83	6.70	22.38	41
Equity-to-Assets Ratio	5.29	2.53	1.89	13.38	38	6.55	3.72	1.59	17.35	58
Debt-to-Assets Ratio	41.97	20.06	4.96	92.04	39	42.86	24.03	6.31	92.53	58
Cash-to-Assets Ratio	2.64	2.64	0.08	10.51	39	3.02	5.14	0.00	21.47	58
Deposits-to-Assets Ratio	44.18	19.70	1.29	85.91	38	43.75	24.05	1.29	83.03	58
Loan-Losses-to-Assets Ratio	-2.53	1.95	-8.23	-0.38	39	-1.95	2.04	-10.50	-0.04	53
ST Debt-to-Assets Ratio	12.69	10.93	0.36	35.99	39	9.37	8.26	0.16	35.99	59
ROA (%)	-0.03	0.71	-2.70	1.21	41	0.43	0.44	-0.62	1.88	63
ROE (%)	-2.37	16.10	-63.26	14.79	41	5.88	7.16	-26.62	17.92	63

Table 2.2: Summary Statistics: Bank-Level (Bailout Sample) This table presents summary statistics for all too-big-to-fail banks located in the European Union subject to government aid. We report book and market data and split the sample into bailed-out banks which appointed new political board members between 2008-2012 and bailed-out bank which did not appoint such political board members. *NPC (0/1)*, new political board member, is an indicator variable equal to one if bank i experienced an increase in its political board members in the period between 2008-2012. *Tobin's Q* is bank i's ratio of total assets - equity + market capitalisation divided by lagged total assets. *Marketcap* is bank i's value on the stock market, the log plus one of the total number of shares $\times$ present share price. *Net Debt Issues*, *Net Equity Issues*, *Dividends*, *Capex* are all scaled by lagged total assets. Detailed definitions for each measure can be found in the Appendix 1.12. All bank-level variables are calculated using annual balance-sheet data for the year 2010.

	No NPC			NPC		
	mean	sd	count	mean	sd	count
NPC (0/1)	0.00	0.00	13	1.00	0.00	16
Total Assets (in mn EUR)	537.43	582.68	12	269.75	370.91	15
Revenue-to-Assets Ratio	1.74	0.88	12	1.74	1.63	15
Loans-to-Assets Ratio	55.69	17.20	11	63.03	14.50	15
ST Debt-to-Assets Ratio	12.30	11.68	12	10.94	10.27	14
Debt-to-Assets Ratio	30.18	12.83	12	31.77	12.55	15
ROA (%)	0.23	0.27	13	0.02	0.79	14
Tobin's Q	4.69	0.07	12	4.69	0.16	14
Marketcap	8.19	2.95	13	3.63	4.44	16
Net Debt Issues	-0.27	0.83	12	-0.07	3.23	14
Net Equity Issues	0.04	0.12	12	0.69	1.45	14
Dividends	0.08	0.11	12	0.02	0.03	14
Capex	0.09	0.06	12	0.37	0.37	14

Table 2.3: Univariate Evidence This table presents difference-in-means estimates for all too-big-to-fail banks located in the European Union and included in the sample. For each measure of political connection and bank characteristics, we divide our sample into banks not subject to state aid (NO) and banks subject to state aid (YES) during the period 2008-2012. The sample consists of 70 banks. The last column shows the absolute value of the t-statistic for a test whether the difference in means between both groups is equal to zero. ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	NO		YES			
	Mean	N	Mean	N	Diff.	t-stat.
Nr. of political board members	0.39	36	0.77	34	-0.373**	-2.024
New political board members (0/1)	0.61	36	1.15	34	-0.536**	-2.452
Nr. of other board members	5.51	36	4.90	34	0.608	1.071
PolCon (0/1)	0.61	36	0.79	34	-0.183*	-1.680
Total Assets (in mn EUR)	244.16	36	309.10	34	-64.937	-0.773
Loans-to-Assets Ratio	63.87	36	62.67	34	1.202	0.323
Revenue-to-Assets Ratio	1.55	36	1.02	34	0.525**	2.078
Tier1Ratio	10.82	36	10.12	34	0.692	1.118
Equity-to-Assets Ratio	5.48	36	5.16	34	0.321	0.527
Debt-to-Assets Ratio	41.74	36	43.25	34	-1.505	-0.323
Cash-to-Assets Ratio	2.13	36	2.59	34	-0.454	-0.657
Deposits-to-Assets Ratio	43.41	36	42.63	34	0.780	0.172
Loan-Losses-to-Assets Ratio	-1.61	36	-3.10	34	1.492***	2.816
ST Debt-to-Assets Ratio	10.20	36	12.72	34	-2.520	-1.246
ROA (%)	0.20	36	-0.41	34	0.612***	4.358
ROE (%)	2.70	36	-10.33	34	13.037***	4.126

Table 2.4: Effect of State Aid on Banks' Political Board Composition This table provides results of a regression analyzing the effect of a bank being subject to government aid on the number of politically connected board members. The analysis is based on data on the bank-year-level and includes all too-big-to-fail banks located in the European Union. The sample period is 2008 to 2012. The dependent variable $log(PC)$ is the log (plus one) of the number of politically connected board members for bank i in year t. $Bailout_i$ is an indicator variable equal to one if bank i received a bailout and 0 otherwise. $Post_{ct}$ is an indicator variable equal to one when country$c$ announced government aid to its banking system, and 0 before. Bank-level controls (Total assets, Tier One Ratio, Leverage Ratio) are lagged by one period and defined as in Appendix 1.12. The regressions further include bank fixed effects and country × time fixed effects, as indicated. Country fixed effects refer to the banks' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

VARIABLES	(1) log(PC)	(2) log(PC)	(3) log(PC)	(4) log(PC)
Bailout (0/1) × Post_{ct}	0.210	0.177**	0.169**	0.194**
	(0.136)	(0.080)	(0.078)	(0.096)
ln(Total Assets)				0.007
				(0.044)
Tier One Ratio				-0.005
				(0.011)
Leverage Ratio				-0.009
				(0.024)
Bailout (0/1)	-0.138			
	(0.126)			
Post_{ct}	-0.090	0.003		
	(0.095)	(0.045)		
Observations	510	510	510	379
R-squared	0.010	0.788	0.825	0.827
Bank FE	No	Yes	Yes	Yes
Country × Year FE	No	No	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Table 2.5: Effect of Different Types of State Aid on Banks' Political Board Composition This table provides results of a regression analyzing the effect of a bank being subject to different types of government aid on the number of politically connected board members. The analysis is based on data on the bank-year-level and includes all too-big-to-fail banks located in the European Union. The sample period is 2008 to 2012. The dependent variable $log(PC)$ is the log (plus one) of the number of politically connected board members for bank i in year t. Recap_i / Assetrelief_i / Guarantees_i / National_i is an indicator variable equal to one if bank i received a recapitalization / asset relief / guarantees / was nationalized and 0 otherwise. $Post_{ct}$ is an indicator variable equal to one when country c announced government aid to its banking system, and 0 before. Bank-level controls (Total assets, Tier One Ratio, Leverage Ratio) are lagged by one period and defined as in the Appendix 1.12. The regressions further include bank fixed effects and country × time fixed effects, as indicated. Country fixed effects refer to the banks' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

VARIABLES	(1) log(PC)	(2) log(PC)	(3) log(PC)
Recap × Post_{ctry}	0.194**	0.148*	0.173**
	(0.096)	(0.087)	(0.083)
Assetrelief × Post_{ctry}		0.658***	0.723***
		(0.051)	(0.183)
Guarantees × Post_{ctry}			-0.077
			(0.188)
Observations	379	379	379
R-squared	0.827	0.830	0.830
Bank Controls	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Country x Year FE	Yes	Yes	Yes
Cluster	Bank	Bank	Bank

Table 2.6: Effect of New Political Board Member on Bank Performance (Bailout Sample, Accounting Data). This table provides results of regressions analysing the effect of a new political board member after government aid on several accounting-based measures of bank performance. The sample includes all too-big-to-fail banks located in the European Union subject to government aid. NPC_i is an indicator variable equal to one if bank i experienced a new politically connected board member in the years following a bailout. $Post_{it}$ is an indicator variable which takes on a value of one after bank i was subject to government aid in year t, and zero before; The bank-level control variable is bank i's lagged log (plus one) total assets. Detailed definitions for each measure can be found in the Appendix 1.12. The sample includes the years -4, -3, -2, -1, before the bailout, and 0, 1, 2, 3, 4 after the bailout. Reported standard errors are in parenthesis, clustered at the bank-level. ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Revenue-to-Assets Ratio	Deposits-to-Assets Ratio	Loans-to-Assets Ratio	Debt-to-Assets Ratio	ROA (%)	$\ln(\sigma(ROA_b)^y)$
$NPC_i(0/1) \times Post_{it}$	-0.138	-7.572*	-2.341	11.123**	0.454*	0.124***
	(0.332)	(4.295)	(4.270)	(4.699)	(0.268)	(0.036)
$Post_{it}$	0.000	5.091	5.255	-0.729	0.269	-0.030*
	(0.273)	(3.617)	(3.920)	(6.149)	(0.482)	(0.017)
Observations	307	278	274	308	308	136
R-squared	0.893	0.921	0.841	0.859	0.712	0.962
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Bank	Bank	Bank	Bank	Bank	Bank

Table 2.7: Effect of New Political Board Member on Bank Performance (Bailout Sample, Market Data). This table provides results of regressions analysing the effect of a new political board member after government aid on several market-based measures of bank performance. The sample includes all too-big-to-fail banks located in the European Union subject to government aid. NPC_i is an indicator variable equal to one if bank i experienced a new politically connected board member in the years following a bailout. $Post_{it}$ is an indicator variable which takes on a value of one after bank i was subject to government aid in year t, and zero before. The bank-level control variable is bank i's lagged log (plus one) total assets. *Tobin's Q* is bank i's ratio of total assets - equity + market capitalisation divided by lagged total assets. *Marketcap* is bank i's value on the stock market, defined as the log (plus one) of the total number of shares × the present share price. *Net Debt Issues*, *Net Equity Issues* are all scaled by lagged total assets. Detailed definitions for each measure can be found in the Appendix 1.12.The sample includes the years -4, -3, -2, -1, before the bailout, and 0, 1, 2, 3, 4 after the bailout. Reported standard errors are in parenthesis, clustered at the bank-level. ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	Net Debt Issues	Net Equity Issues	Marketcap	TobinsQ
$NPC_i(0/1) \times Post_{it}$	0.368	0.382	0.368*	0.103**
	(1.486)	(0.344)	(0.187)	(0.045)
Observations	94	94	62	94
R-squared	0.627	0.730	0.987	0.864
Bank FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Table 2.8: Robustness: Effect of State Aid on Banks' Political Board Composition (not TBTF sample) This table provides results of a regression analyzing the effect of a bank being subject to government aid on the number of politically connected board members. The analysis is based on data on the bank-year-level. The sample period is 2008 to 2012 and includes all banks which are not too-big-to-fail in Europe. The dependent variable $log(PC)$ is the log (plus one) of the number of politically connected board members for bank i in year t. $Bailout_i$ is an indicator variable equal to one if bank i received a bailout and 0 otherwise. $Post_{ct}$ is an indicator variable equal to one when country c announced government aid to its banking system, and 0 before. Bank-level controls (Total assets, Tier One Ratio, Leverage Ratio) are lagged by one period and defined as in the Appendix 1.12. The regressions further include bank fixed effects and country × time fixed effects, as indicated. Country fixed effects refer to the banks' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

VARIABLES	(1) log(PC)	(2) log(PC)	(3) log(PC)	(4) log(PC)
Bailout (0/1) × Post_{ct}	0.105	-0.056**	-0.031*	0.014
	(0.079)	(0.023)	(0.018)	(0.021)
ln(Total Assets)				-0.165*
				(0.087)
Tier One Ratio				0.015**
				(0.007)
Leverage Ratio				-0.024*
				(0.013)
Bailout (0/1)	-0.111			
	(0.070)			
Post_{ct}	-0.083	0.037*		
	(0.063)	(0.019)		
Observations	605	605	605	260
R-squared	0.015	0.849	0.890	0.920
Bank FE	No	Yes	Yes	Yes
Country × Year FE	No	No	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Table 2.9: Robustness: Triple Difference-in-Differences This table provides results of a regression analyzing the effect of a bank being subject to government aid and TBTF on the number of political board members. The analysis is based on data on the bank-year-level and includes all too-big-to-fail banks located in the European Union. The sample period is 2008 to 2012. The dependent variable $log(PC)$ is the log (plus one) of the number of board members with political connections for bank i in year t. $TBTF_i$ is an indicator variable equal to one if bank i's total assets are above the median of their respective country c's total banking assets in 2007, and zero otherwise. $Bailout_i$ is an indicator variable equal to one if bank i received a bailout and 0 otherwise. $Post_{ct}$ is an indicator variable equal to one when country c announced government aid to its banking system, and 0 before. Bank-level controls (Total assets, Tier One Ratio, Leverage Ratio) are lagged by one period and defined as in the Appendix 1.12. The regressions further include bank fixed effects and country × time fixed effects, as indicated. Country fixed effects refer to the banks' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

VARIABLES	(1) log(PC)	(2) log(PC)	(3) log(PC)	(4) log(PC)
TBTF × $Bailout$ × Post_{ct}	-0.035	0.220***	0.245***	0.302**
	(0.154)	(0.079)	(0.087)	(0.131)
TBTF × Post_{ct}	0.086	-0.034	-0.045	-0.053
	(0.104)	(0.049)	(0.050)	(0.080)
Bailout (0/1) × Post_{ct}	0.140*	-0.056**	-0.069*	-0.101
	(0.081)	(0.023)	(0.036)	(0.078)
TBTF × Bailout	0.136			
	(0.139)			
Bailout (0/1)	-0.147**			
	(0.072)			
Post_{ct}	-0.118*	0.037*		
	(0.066)	(0.019)		
TBTF	0.101			
	(0.097)			
Observations	1,180	1,180	1,180	697
R-squared	0.086	0.828	0.848	0.840
Bank Controls	No	No	No	Yes
Bank FE	No	Yes	Yes	Yes
Country × Year FE	No	No	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Table 2.10: Robustness: Effect of State Aid on Banks' Other Board Composition This table provides results of a regression analyzing the effect of a bank being subject to government aid on the number of politically unconnected ("other") board members. The analysis is based on data on the bank-year-level and includes banks located in the European Union, split between banks which are TBTF and banks which are not TBTF. The sample period is 2008 to 2012. The dependent variable $log(noPC)$ is the log (plus one) of the number of board members without political connections for bank i in year t. $Bailout_i$ is an indicator variable equal to one if bank i received a bailout and 0 otherwise. $Post_{ct}$ is an indicator variable equal to one when country c announced government aid to its banking system, and 0 before.Bank-level controls (Total assets, Tier One Ratio, Leverage Ratio) are lagged by one period and defined as in the Appendix 1.12. The regressions further include bank fixed effects and country × time fixed effects, as indicated. Country fixed effects refer to the banks' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	TBTF	TBTF	TBTF	not TBTF	not TBTF	not TBTF
VARIABLES	log(no PC)	log(no PC)	log(no PC)	log(no PC)	log(no PC)	log(no PC)
Bailout (0/1) × Post_{ct}	0.331**	0.215	0.278	-0.010	-0.065	-0.343
	(0.155)	(0.146)	(0.245)	(0.103)	(0.109)	(0.240)
Post_{ct}	0.096			0.053		
	(0.063)			(0.069)		
Observations	510	510	379	605	605	260
R-squared	0.860	0.898	0.832	0.881	0.912	0.918
Bank Controls	No	No	Yes	No	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Country x Year FE	No	Yes	Yes	No	Yes	Yes
Cluster	Bank	Bank	Bank	Bank	Bank	Bank

Table 2.11: Robustness: Reverse Causality This table provides results of a regression analyzing the effect of a bank being subject to government aid on the number of political board members, leaded by one period. The analysis is based on data on the bank-year-level and includes too-big-to-fail banks located in the European Union. The sample period is 2008 to 2012. The dependent variable $log(PC)$ is the log (plus one) of the number of board members with political connections for bank i in year $t+1$. $Bailout_i$ is an indicator variable equal to one if bank i received a bailout and 0 otherwise. $Post_{ct}$ is an indicator variable equal to one when country c announced government aid to its banking system, and 0 before. Bank-level controls (Total assets, Tier One Ratio, Leverage Ratio) are lagged by one period and defined as in the Appendix 1.12. The regressions further include bank fixed effects and country × time fixed effects, as indicated. Country fixed effects refer to the banks' respective headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

VARIABLES	(1) log(PC)$_{t+1}$	(2) log(PC)$_{t+1}$	(3) log(PC)$_{t+1}$	(4) log(PC)$_{t+1}$
Bailout (0/1) × Post$_{ct}$	0.128	0.105	0.183*	0.216*
	(0.138)	(0.092)	(0.094)	(0.124)
ln(Total Assets)				-0.030
				(0.047)
Tier One Ratio				0.017
				(0.013)
Leverage Ratio				-0.015
				(0.024)
Bailout (0/1)	-0.067			
	(0.132)			
Post$_{ct}$	-0.050	0.068		
	(0.090)	(0.049)		
Observations	510	510	510	379
R-squared	0.005	0.785	0.829	0.844
Bank FE	No	Yes	Yes	Yes
Country × Year FE	No	No	Yes	Yes
Cluster	Bank	Bank	Bank	Bank

Appendices

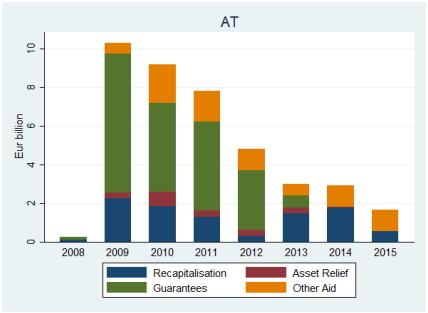
1 Appendix A1

Variable Definitions.

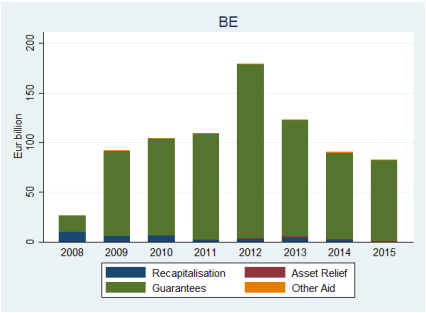
Variable	Source	Description
Nr. of political board members	CapitalIQ (SNL Financial)	Absolute number of politically connected board members
log(PC)	CapitalIQ (SNL Financial)	is the log (plus one) of the number of board members with political connections for bank i in year t
NPC (0/1)	CapitalIQ (SNL Financial)	indicator variable equal to one if bank i experienced an increase in its political board members in the period between 2008-2012
PolCon (0/1)	CapitalIQ (SNL Financial)	indicator variable equal to one if bank i has political connections and 0 otherwise
Nr. of other board members	CapitalIQ (SNL Financial)	Absolute number of other, politically unconnected board members
log(noPC)	CapitalIQ (SNL Financial)	is the log (plus one) of the number of board members without political connections for bank i in year t
Total Assets	CapitalIQ (SNL Financial)	Bank i's total assets (in mn EUR)
TBTF	CapitalIQ (SNL Financial)	indicator variable equal to one if bank i's total assets are above the median of its respective country c's total banking assets in 2007 and 0 otherwise
Loans-to-Assets Ratio	CapitalIQ (SNL Financial)	Share of gross loans over total assets
Loan-Losses-to-Assets ratio	CapitalIQ (SNL Financial)	Share of loan losses over total assets
Revenue-to-Assets Ratio	CapitalIQ (SNL Financial)	Share of revenue over total assets
Tier1ratio	CapitalIQ (SNL Financial)	Share of core tier 1 capital over total risk-weighted assets
Equity-to-Assets Ratio	CapitalIQ (SNL Financial)	Share of equity over total assets
Debt-to-Assets ratio	CapitalIQ (SNL Financial)	Share of long term debt over total assets
ST Debt-to-Assets ratio	CapitalIQ (SNL Financial)	Share of short term debt over total assets
Cash-to-Assets ratio	CapitalIQ (SNL Financial)	Share of cash and equivalents over total assets
Deposits-to-Assets ratio	CapitalIQ (SNL Financial)	Share of deposits over total assets
ROA (%)	CapitalIQ (SNL Financial)	Bank i's Return on Assets (in %)
ROE (%)	CapitalIQ (SNL Financial)	Bank i's Return on Equity (in %)
Tobin's Q	Bloomberg	Bank i's Tobin's Q
Marketcap	Bloomberg	Bank i's Marketcapitalisation
Net Debt Issues	Bloomberg	Bank i's Net Debt Issues
Net Equity Issues	Bloomberg	Bank i's Net Equity Issues
Dividends	Bloomberg	Bank i's Dividends
Capex	Bloomberg	Bank i's CAPEX
Bailout (0/1)	State Aid Dataset	indicator variable equal to one if bank i received state aid and 0 otherwise
Recap (0/1)	State Aid Dataset	indicator variable equal to one if bank i received a recapitalization and 0 otherwise
Assetrelief (0/1)	State Aid Dataset	indicator variable equal to one if bank i received asset relief and 0 otherwise
Guarantees (0/1)	State Aid Dataset	indicator variable equal to one if bank i received guarantees and 0 otherwise
Other Aid (0/1)	State Aid Dataset	indicator variable equal to one if bank i received other aid and 0 otherwise
Nationalisation (0/1)	State Aid Dataset	indicator variable equal to one if bank i was nationalized and 0 otherwise
Liquidation (0/1)	State Aid Dataset	indicator variable equal to one if bank i was liquidated and 0 otherwise
Post_{ct}	State Aid Dataset	indicator variable equal to one when country c announced government aid to its banking system and 0 before

2 Appendix A2

Type of State Aid by EU Member State (2008-2015)



(a) Austria



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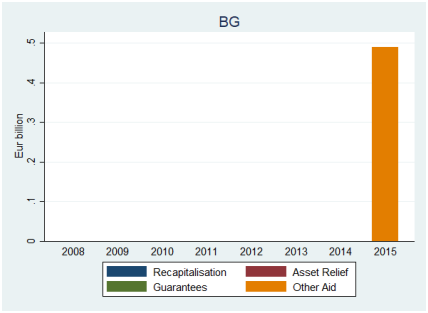

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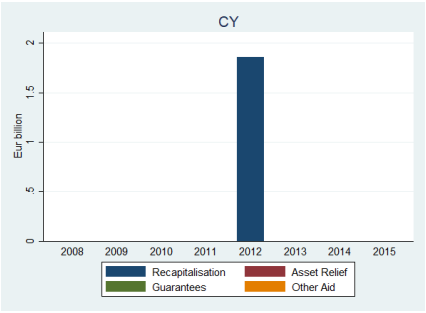
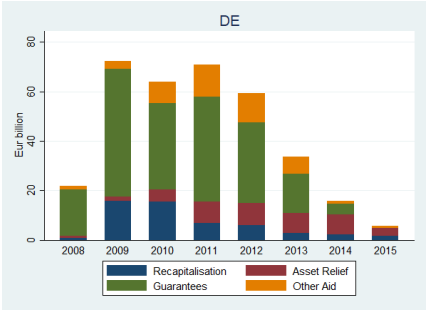
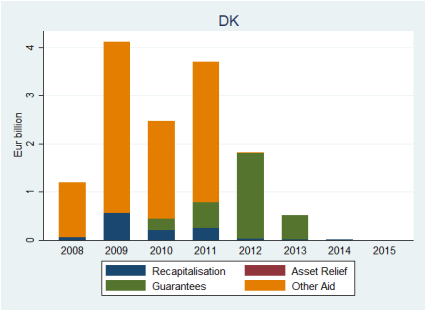


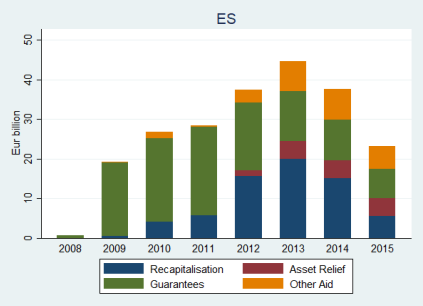
(d) Cyprus



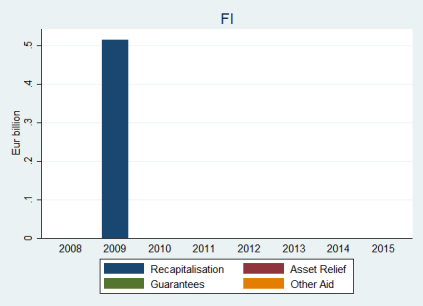
(e) Germany



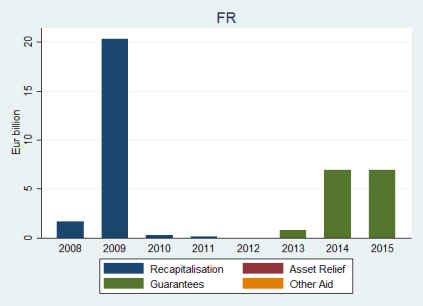
(f) Denmark



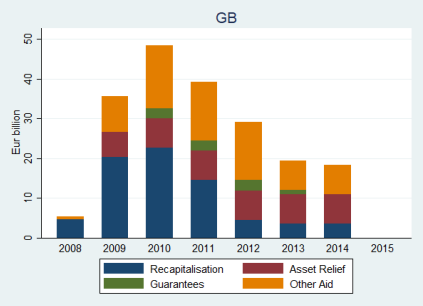
(g) Spain



(h) Finland



(i) France



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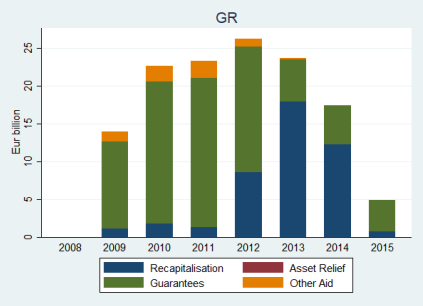
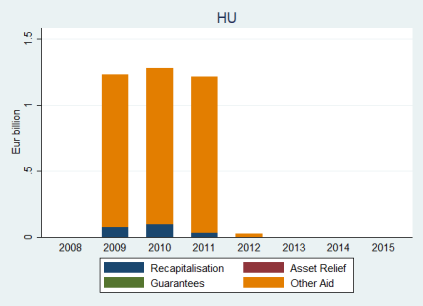


(k) Greece



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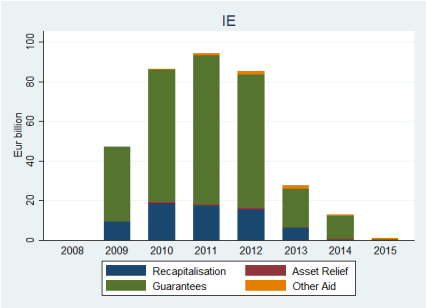

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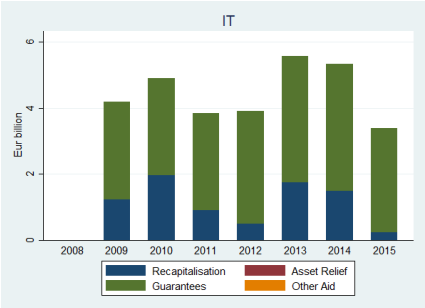
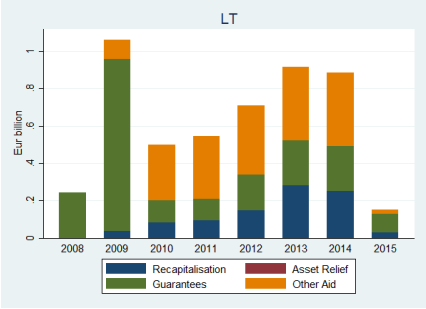
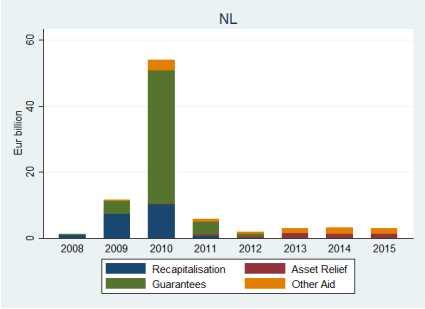


(n) Italy



(o) Lithuania



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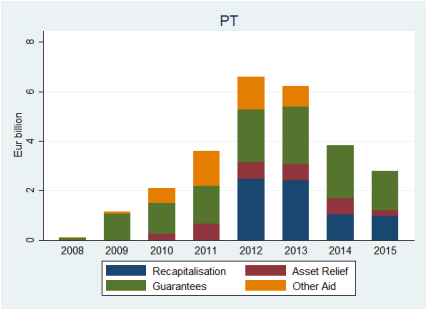

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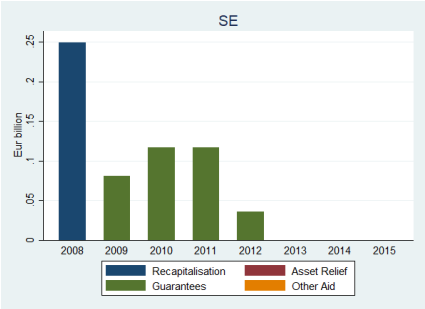

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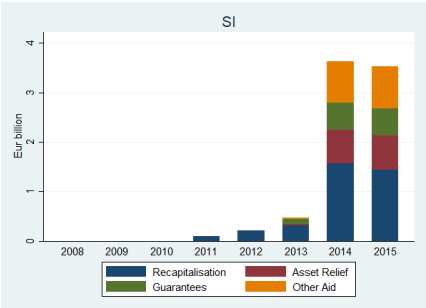

(s) Slovenia

Chapter 3

When Credit Turns Radical: Evidence From the Spanish Financial Crisis

1 Introduction

"Populism is the true legacy of the global financial crisis."

Financial Times, August 2018

Since the start of the 2008 financial crisis, anti-establishment sentiment is on the rise again (Rodrik (2018), Algan et al. (2017)). Financial crises, as opposed to other types of crises, are often at the heart of this radicalisation (Mian, Sufi and Trebbi (2014)). However, the specific channels behind these dynamics have remained a black box this far. Doerr et al. (2021) suggest antisemitism in Germany's banking crisis of the 1930s as a key driver behind the rise in Nazi votes, Gyongyosi and Verner (2021) finds a debtor-creditor conflict at the heart of the far-right support in Hungary. In this Chapter, we provide causal evidence in support of the model of Guiso et al. (2017): Rising economic insecurity leads to higher support for populist parties. Using data on bank-firm connections and electoral outcomes during the financial crisis in Spain, we argue that credit crunches lead to political radicalisation through rising unemployment risk.[1]

As a first step, we examine the effects of distressed banks on the real economy. Using bank-firm connections during the financial crisis in Spain, we confirm what has been found in the literature so far. Firms in a relationship with weak banks experience a bigger reduction in loan supply compared to firms without such relationships (Chodorow-Reich (2014), Acharya and Steffen (2015)). We define weak banks as banks that received a bailout from their respective government. Here, one major identification challenge is related to reverse causality between the health of the banking sector and the economy. Given that Spain experienced a housing bubble, it could well be that failing borrowers decreased loan demand, which led to banks cutting credit to these borrowers. To address this concern, we focus on *foreign* weak banks active in Spain. In other words, we focus on banks that are active in Spain, but

[1] Spain is an ideal setting for our empirical analysis. Its entire banking sector suffered from the bursting of a housing bubble. At the same time, populist parties running on anti-elite, and anti-corruption platforms reached new heights in terms of approval and voting results.

received a bailout in their respective home countries other than Spain. The troubles of these banks are likely to be exogenous to the performance of the Spanish loan portfolio, but might nevertheless lead to a contraction of credit supply to Spanish borrowers (Giannetti and Laeven (2012)). Consequently, this credit crunch leads to a reduction of firm-level employment, as shown also in Bentolila, Jansen and Jiménez (2018), increasing the employee's risk of becoming unemployed. In our analysis, we cannot measure unemployed risk directly. Instead, we use a measure which influences the costs of becoming unemployed: the local labour market tightness. In tight labour markets, it is easier for employees to find new jobs, so the cost a worker incurs when getting unemployed is relatively low because search costs are lower and the expected duration of unemployment is shorter.

As a second step, we estimate the causal effects of a credit crunch on radical voting. First, we empirically verify the impact of labour market tightness on voting behaviour. We find that the effect on radicalisation is higher in areas with lower labour market tightness. In this econometric set-up, we face an identification challenge related to an omitted variable bias. Unobserved city-specific time-varying factors might affect labour market tightness and voting at the same time. Immigration could be an example of this bias, driving both labour market tightness as well as political radicalisation. More specifically, there might exist reverse causality between voter radicalisation and labour market tightness: Radical voting could be used to install local governments that shelter local labour markets from immigration-induced competition. In order to address this concern in the most rigorous way, we set up a two-stage least squares (2SLS) estimation, using our firm-level exposure to foreign weak banks measure as an instrument for labour market tightness. The reasoning behind is the following: As credit supply shocks have an adverse impact on firm-level employment, we use it as an shock for labour market competition on the local level which is unrelated to immigration. Our identification relies on a number of assumptions. First, the loan demand from Spanish firms is negligible from the foreign

bank's perspective.[2] Second, the local exposure to foreign weak banks has no effect on the number of immigrants in the locality.[3]

This Chapter relates to the following strands of literature. First, we add to the emerging political economy literature which links economic shocks to sharp ideological shifts in voting behaviour. When looking at different types of crises such as financial, currency, inflation, and debt crises, Mian, Sufi and Trebbi (2014) provide evidence that, especially after financial crises, government vote shares decrease and voters become more ideologically extreme. Similarly, using a historical dataset spanning 140 years and 800 elections, Funke, Schularick and Trebesch (2016) find that far-right parties increase their vote shares significantly after a financial crisis. However, the literature on well-identified microeconomic evidence on the impact of credit contraction is limited. Notable exceptions are Braggion, Manconi and Zhu (2020) and Doerr et al. (2021), who show that credit rationing has an impact on social unrest in 1930s China, and on the votes for the Nazi Party in 1930s Germany. In this Chapter, we bring this set-up to modern day data, and provide causal evidence for the impact of financial crises on the radicalisation of voting in Spain.

By identifying unemployment risk as one channel behind radicalisation, we contribute to the literature on the drivers behind rising populism. Autor et al. (2020) identifies Chinese import shocks as a driver behind rising populism in the US; Becker, Fetzer and Novy (2017) highlights the role of cuts to government spending in the Brexit vote; Gyongyosi and Verner (2021) the foreign currency composition of household debt on Hungarian far-right votes, and Sartre, Daniele and Vertier (2021) public finance mismanagement on the entry of populist politicians. Doerr et al. (2021) suggest antisemitism in Germany's banking crisis of the 1930s as a key driver behind the rising Nazi votes. For Europe, Algan et al. (2017) document a link between in-

[2] This assumption would reasonably be violated when we would look at local Spanish banks instead of foreign banks.

[3] A positive correlation is unlikely. If immigrants would strategically chose their destination to increase expected future income, they would optimally chose not to migrate to cities which experienced a financial shock.

creases in unemployment and voting for populist parties during the Great Recession. Using our city-level exposure to foreign weak banks as an instrument, we establish causality between labour market tightness, our measure of unemployment risk, and the radicalisation of votes.

Third, we also add to the literature on the real effects of banking crises. Chodorow-Reich (2014)'s important work on firm-bank relationships during crises reveals that firms with a relationship to banks affected by the Lehman crisis suffered more in terms of employment than firms without such a connection. Huber (2018) moves beyond firm-level evidence and shows that credit contractions also indirectly depress economic activity in the regions most exposed to such lending cuts. For Spain, Bentolila, Jansen and Jiménez (2018) find that the solvency of Spanish banks caused the highest employment losses. We extend this literature by looking at election outcomes as another real effect, suggesting that credit contractions do not only impact firm performance and employment and economic output, but also shape voter behaviour.

The remainder of the Chapter is as follows. Section 2 describes the data, the variable construction, and the empirical strategy. Section 3 presents the results. Section 4 reports robustness tests and Section 5 concludes.

2 Data and Empirical Strategy

2.1 Data

Lending Data We obtain bank-firm relationships from the syndicated loan market. In this market, different banks form a syndicate to then jointly lend to a single borrower. The lending syndicate includes one lead bank and a number of participating banks. Lead arrangers are those members of a syndicate typically responsible for traditional bank duties, including negotiating the conditions of the deals, due diligence, and monitoring (Ivashina and Scharfstein (2010)). Participants are usually not in direct contact with the borrower, but merely supply credit. We therefore consider only banks acting as lead arrangers. Similarly, we restrict the sample to loans by banks to non-financial firms and consider lending only by commercial, savings, cooperative, and investment banks. We decompose syndicated loan deals into loan portions provided by each lender to obtain granular loan-level data. Whenever Dealscan provides information on lending shares of each bank, we use this information to split loan volume accordingly. In other cases, we follow Schwert (2018) and estimate lending shares via a tobit estimation using information on the facility amount, the number of participants, and borrower and lender sales. In doing so, we obtain bank i's loan issuance to firm j in year t, which we define as a bank-firm observation. Total loan volume in a given year is the sum of all new loans issued by bank i to firm j. Hence, we only account for transactions happening when a syndicated loan is issued, disregarding its maturity profile. We hence only account for flows on the bank-firm-level.

Firm and Bank Variables To control for bank characteristics, we match the banks included in the DealScan database with bank balance sheet data from CapitalIQ (SNL financial). To control for firm-characteristics, we obtain annual firm accounting data for Spanish firms from CapitalIQ. We aggregate the Dealscan bank-firm-quarter to the firm-year-level, to match borrowers in Dealscan with firms in

Compustat, and later CapitalIQ, based on Chava and Roberts (2008), updated in April 2018. Combining those two databases reduces observations, since not all firms have balance sheet data available on CapitalIQ, especially the smaller ones. Variables are winsorized at the 1st and 99th percentile. Financial firms (SIC codes 6000-6999) are dropped.

State Aid We hand-collect state aid given to individual banks for the 28 countries of the European Union, and break down the information into amount and type of state aid. Our primary source for state aid to financial institutions is the online database on state aid provided by the European Commission. It contains all state aid cases which occurred in the European Union, and in particular, it contains state aid within the meaning of Article 107(1) TFEU[4] granted to financial institutions. It is important to highlight that we only have information about implemented state aid, involving the aid actually used and given to financial institutions as opposed to the state aid planned. We identify a bank to be subject to government aid, if it has received at least one of four state aid measures, as indicated in the documents (recapitalisation, asset relief, liquidity support, and guarantees). In doing so, we can identify 70 banks active in the European Union, which were subject to government aid between 2008 and 2015.

Electoral Data We hand-collect data of the Spanish parliamentary elections on the constituency (city)-level, which took place in 2011, 2015, and 2016.[5] This allows us to obtain the electoral results of 8127 constituencies with respect to 56 parties. The electoral data also allows extracting data on population and voter turnout on a constituency-year-level. "Voter turnout" is defined as the ratio between total votes and the electoral census. "Population" is the log of the total population in constituency c in year t.

[4] Treaty on the Functioning of the European Union
[5] Available at: Spain Ministerio del Interior www.infoelectoral.mir.es/infoelectoral/min/

Political Orientation Data We download the political orientation of European parties from Chapel Hill, which is widely used in the literature.[6] This database allows classifying parties with respect to eight political ideology categories: far right, conservatives, liberal, socialist, far left, greens, regional and no family.

2.2 Variable Construction

2.2.1 Weak Bank Measure

Firm-Bank-Level Combining the bank-firm relationship data with the bank-level state aid data allows constructing the weak bank indicator variable on the firm-level. We divide the sample into two groups depending on firm j's relationship with a weak bank i. In our case, a "weak" bank is a bank which has received a government aid (WB_i).

We then construct our firm-level bank dependence variable $Dependence_{jt}$, using the loan issuance to firm j by bank i, depending on bank i being a weak or a healthy bank. Mathematically speaking:

$$Dependence_{jt} = \sum_{\forall j} Volume_{ijt} * WB_i \tag{3.1}$$

where $Volume_{jit}$ is bank i's loan issuance to firm j's at year t in the syndicated loan market. Finally, we define the firm-level bank dependence variable to be its three year backward looking moving average.[7]

Table 3.1 presents summary statistics on the firm-level. Eventually, we obtain a sample of 796 firms, 361 of which do not have an exposure to weak banks, and 435 which have not. The average firm with a weak bank exposure has more total assets (5.7% vs. 4.9%), lower profits (25.0% vs 29.1%) and equity (5.5% vs 8.7%), com-

[6] Available at: www.chesdata.eu

[7] We are interested in the shadow cost of acquiring a new loan. This rolling average captures the importance of the set of weak banks that lent to firm j in the past three years. The assumption is, if bank i is in distress, firm j's shadow cost of acquiring a new loan by this bank i increases.

pared to firms without such an exposure.

Aggregation to the City-Level Next, our goal is to construct a measure that describes the overall exposure of firms to weak banks on the level where the elections take place, i.e. the electoral constituencies (cities). We therefore combine the voting data with the data on firm-level exposure to weak banks. Our idea is that through the firms' exposure to foreign weak banks, their employees face higher (perceived) risks of unemployment, which has an effect on the employees' voting behavior.

When creating this measure, we exploit information on the geographical coordinates of the firms' headquarters and match (exposed) firms to electoral constituencies (cities)[8].

To identify the headquarter of the respective firms, we use the firm information provided in the syndicated loan dataset. Dealscan provides both the city name and the zip, or missings thereof. Whenever one of the two is missing, we combine the available data with zip or cityname data for Spain.[9] Whenever none of the two is available, we identify the headquarter manually through CapitalIQ searches based on the firm name. Eventually, we identify 796 firms located in 199 cities across Spain. 435 of which have a relationship to a weak bank and 361 do not have such a relationship in end-2010.

We then compute geodetic distances between each electoral constituency, c, and every Spanish municipality, m.[10] For every c, m pair we define a dummy variable, D_{cm}, which takes the value of one if the physical distance between electoral con-

[8] We use the geographical coordinates of the district capital municipality as a proxy for the location of the electoral constituencies. By doing so, we implicitly assume that the location of a firm's headquarter is correlated with the locations of the facilities where the firm's employees are employed. This assumption might seem unrealistic but is standard in the literature.

[9] Available at: www.geonames.org

[10] That is the length of the shortest curve between two points along the surface of a mathematical model of the earth. We follow the methodology proposed by Vincenty, T. (1975) Direct and inverse solutions of geodesics on the ellipsoid with application of nested equations, Survey Review 22(176): 88-93. Available from: http://www.ngs.noaa.gov/PUBS_LIB/inverse.pdf

stituency c and municipality m is smaller than 20 kilometers; and zero else. This dummy variable flags municipalities which are in the vicinity of a certain electoral constituency.

In order to create a constituency-level measure, we aggregate the firm-level exposure to weak banks, $Dependence_{jt}$, over all firms which are in the vicinity of the electoral constituency.

$$Exposure_{mt} = \sum_{\forall j} D_{c(j)} \times Dependence_{jt} \tag{3.2}$$

where $D_{c(j)}$ equals one if firm j is within 20 km of constituency c.

Table 3.2 presents summary statistics. Out of 8127 Spanish municipalities (cities), we obtain 4657 cities, out of which 2,691 have a weak bank exposure, and 1,966 do not have such an exposure. The average city with a weak bank exposure has more overall population (9.2% vs. 4.2%), albeit a similar unemployment rate (21%) compared to cities without weak bank exposure. In terms of electoral outcomes, people in the average city with weak bank exposure vote less conservative (32.8% vs. 36.4%), and less socialist (24.2% vs. 26.1%), but more far-left (12.8% vs. 10.2%).

2.2.2 Political Radicalization Measure

We want to identify the share of votes to parties with anti-establishment orientation in the Spanish parliamentary elections. To do this, we combine two datasets on the city-level: one on electoral data and one on the political orientation of parties. This allows classifying 26 parties out of the 56 Spanish parties running in the parliamentary elections. The ones not classified are fringe parties, and those of which achieved a electoral result of only 1% of overall votes or below are dropped. Eventually, we obtain the vote share of the respective parties and their political orientation on the city-year-level. Importantly, on the discussion of changes vs. levels when it comes to voting data, we follow the reasoning highlighted by Rodrik (2021) on the matter:

> "The relative importance one ascribes to economics versus culture depends crucially on whether we are interested in a question about levels or about changes that is, whether we ask why so many people voted for a populist candidate or why the populist vote share increased so much." *(Margalit (2019)).*

We hence look at changes and define radical votes as the votes going to new parties which collocate themselves on the extremes, and have advertised themselves as radical alternatives, on the back of major corruption scandals of the more established parties.[11] Our political orientation variable ($Radical_{ct}$) is the logarithmic growth rate, change in votes, going to the radical left and right, where c is city and t is year:

$$\Delta ln(\text{Radical}_{ct}) = \Delta ln(\text{Radical Left}_{ct} + \text{Radical Right}_{ct}). \tag{3.3}$$

We then define centralist votes as the votes going to established, traditional parties, both of which have shaped Spanish politics over the last decades. We define our political orientation variable ($Central_{ct}$), as the logarithmic growth rate, the change in votes going to the conservatives and the social democrats, where c is city and t is year:

$$\Delta ln(Central_{ct}) = \Delta ln(\text{Conservatives}_{ct} + \text{Social Democrats}_{ct}). \tag{3.4}$$

Lastly, we define our preferred measure of $\text{Radicalisation}_{ct}$ as the logarithmic growth rate, the change in $Radical_{ct}$ minus the change in $Central_{ct}$, where c is city and t is year:

$$\text{Radicalisation}_{ct} = \Delta ln(\text{Radical}_{ct}) - \Delta ln(\text{Central}_{ct}). \tag{3.5}$$

[11] We add up radical left and radical right, as the newly established radical right party VOX only enters national elections as of 2015.

2.3 Identification and Empirical Strategy

Our aim is to estimate the causal effects of credit crunch on radical voting. The idea underlying the channel we have in mind is the following: During the Great Recession, many banks were subject to financial pressure which limited their ability to grant new loans to their existing corporate customers (Chodorow-Reich (2014)). This constraint led to increased lay-offs at the banks' corporate customers (Bentolila, Jansen and Jiménez (2018)) and, thus, their employees' risk of becoming unemployed rises. One potential consequence of unemployment risk is that the employees radicalize politically (Urdal (2006)). We are specifically interested in the effect a credit crunch has on political radicalization through the channel of unemployment risk. However, since we are unable to observe unemployment risk directly, we need to proxy an employees' costs of becoming unemployed. Assuming that employees' geographical mobility is limited, it is relatively easy for employees to find a new job when local labour markets are tight. Therefore, the costs associated with becoming unemployed are relatively low in tighter labour markets. We proxy unemployment risk by labour market tightness.

As a first step, we need to establish that foreign weak banks curtailed credit to firms more than other banks (Section 2.3.1). Here, we face a major identification challenge related to reverse causality between the health of the banking sector and the economy (Reinhart and Rogoff (2009)). Weak banks might be weak (i.e. subject to a bailout in our case) because their loan portfolio performs poorly. Given that Spain experienced a housing bubble, it can well be that failing borrowers decreased loan demand, which led to banks cutting their credit to these borrowers. To address this concern, we focus on *foreign* weak banks active in Spain. In other words, we focus on banks active in Spain, but subject to a bailout in their respective home countries. The troubles of these banks are likely to be exogenous to the performance of the Spanish loan portfolio, but might nevertheless lead those banks to reduce their exposure to Spanish borrowers (Giannetti and Laeven (2012)). Hence, firm-level exposure to foreign weak banks is our proxy for exposure to a credit crunch.

We analyse its impact before and after the start of the European debt crisis in May 2010.[12]

Second, we turn to the main research question and estimate the effects of a credit supply shock on the radicalisation of votes. We first empirically verify the impact of labour market tightness on voting behaviour (Section 2.3.2). We expect the effect of radicalisation to be higher in areas with lower labour market tightness. However, in this set-up, we face a major identification challenge related to an omitted variable bias; unobserved city-specific time-varying factors might affect labour market tightness at the same time as the voting. For example, the number of immigrants is a plausible candidate for an omitted variable, driving both labour market tightness (through their addition to the local labour force) as well as political radicalisation (through xenophobia) on the city-level.

As we want to establish a *causal* relationship between labour market tightness and voter behaviour, we propose an instrumental variable approach based on our firm-level exposure measure to foreign weak banks (Section 2.3.4).[13] To do so, we aggregate our exposure measure to the city-level. Our identification strategy relies on two assumptions. First, the city-level exposure to foreign weak banks affects local unemployment and, therefor, the tightness of the local labour market. Second, bank bailouts affect voting only via the risk that employees, i.e. voters, become unemployed. One might argue that the second identifying assumption is not sensible, as bailouts were indeed drivers of voting.[14] For this reason, the use of city-level expos-

[12] Similar to Drechsler et al. (2016), we define the start of the European debt crisis as May 2, 2010, the day the European Union and the IMF agreed on the first bailout-package to Greece. The crisis subsequently put into question the credit-worthiness of other euro area member states, most notably Spain and Italy, and prompted the ECB to intervene in the sovereign bond markets through the Securities Markets Programme (SMP) in May of the same year.

[13] Algan et al. (2017) study the impact of unemployment on the radicalisation of votes in Europe during the Great Recession. They suggest the use of pre-crisis share of construction as a Bartik-style instrument for unemployment. However, the pre-crisis share of construction is very much related with housing prices, an potential omitted variable driving the results.

[14] Bailouts have always been very much politicized for the sheer amount of tax payer's money flowing into financial sector bailout programmes.

ure to *foreign* weak banks as a valid instrument seems reasonable.[15] Our exclusion restriction is that foreign banks in Spain do not grant mortgage loans. Given that the syndicated loan market is restricted to corporate lending, this seems reasonable to assume.

2.3.1 Credit Supply Shock

First, we want to test the hypothesis that foreign weak banks curtail credit to firms more than other banks after the start of the European debt crisis in 2010. We estimate the following equation on the bank-firm-quarter-level:

$$y_{ijt} = \delta_1 \text{ Foreign WB}_i \times \text{Post10}_t + \delta \, \mathbf{X}'_{i,t-1} + \mu_{jt} + \epsilon_{ijt}, \tag{3.6}$$

where y_{ijt} is the logarithm of (one plus) the loan issuances in million USD to firm j provided by bank i (as lead or participating bank) at quarter t. Foreign WB$(0/1)_i$ is an indicator variable equal to 1 if a foreign bank received a bailout, and equal to zero otherwise. Post10$_t$ is equal to zero up to Q1 2010, the start of the European debt crisis, and 1 afterwards. $\mathbf{X}'_{i,t-1}$ is a vector of bank-level controls for size, equity ratio, cash ratio, liquidity ratio, and deposits ratio, all lagged by two quarters. μ_{jt} denote country or firm $\times$ year fixed effects, where country fixed effects represent the the country of origin of the respective bank.

The coefficient of interest δ_1 measures how firm relationships with foreign weak banks impacts loan supply before and after the start of the European debt crisis in Q2 2010. We expect $\delta_1 < 0$, as foreign weak banks are the banks most in trouble, and curtail credit more than other banks. This is along the lines of Chodorow-Reich (2014), who finds that firms with a relationship to banks hit by the Lehman shock suffer most in terms of credit contraction and employment.

[15] Unobserved geographical heterogeneity, for example in voting behavior or home-ownership, or general macro-economic time trends are controlled for by the use of city-level fixed effects and year fixed effects.

To strengthen our results further, we rerun the specification on the firm-level. We are interested in the effect on loan and employment growth if a firm is in a relationship with a weak bank, as opposed to firms without such a relationship. We estimate the following equation:

$$y_{jt} = \gamma_1 \text{ Foreign WB}(0/1)_j + \gamma \, \mathbf{X}'_{j,t-1} + \mu_r + \mu_t + \epsilon_{jt}, \tag{3.7}$$

where y_{jt} is the logarithmic growth rate of the loan supply of firm j in year t, and zero otherwise. Foreign WB$(0/1)_j$ in this case is an indicator variable equal to one if a firm is in a relationship with a foreign weak bank, and equal to 0 if it is not. $\mathbf{X}'_{j,t-1}$ is a vector of firm-level controls (Total Assets, ROA, Sales, and CAPEX), all lagged by 1 year. μ_r denote industry, and μ_t year fixed effects.

The coefficient of interest γ_1 measures the effect on the loan growth if firm j has a lending relationship with a foreign weak bank. We expect $\gamma_1 < 0$, reflecting the negative coefficient estimate on the bank-firm-level.

Next, following Bentolila, Jansen and Jiménez (2018), we explore the impact of the credit supply shock on on firm-level outcomes, and in particular on employment. We substitute the dependent variable in Equation 3.7 with the logarithmic growth rate of the number of full time employees at firm j at year$t$. In this case, we expect $\gamma_1 < 0$, as firms with a relationship to foreign weak banks decrease their employment more compared to firms without such a relationship.

2.3.2 Unemployment Risk and the Radicalisation of Voters (OLS)

We empirically verify the impact of labour market tightness on voting behaviour. Labour market tightness (LMT_{ct}) tell us how easy it is for a worker to find a new

job.[16] In areas with low labour market tightness, the voter (worker) has higher costs finding a new job. Bentolila, Jansen and Jiménez (2018) show that the credit crunch causes a reduction in firm-level employment in Spain. At the same time, Algan et al. (2017) highlight that unemployment is the main factor contributing to the ideological radicalisation of voters during the European debt crisis. Therefore, we suspect the effect of radicalization to be higher in areas with low labour market tightness.

To test this, we set up the following equation on the city-year-level:

$$Vote_{ct} = \beta_1 LMT_{ct} + \beta_2 \mathbf{X}'_{ct} + \phi_c + \phi_t + \epsilon_{ct} \tag{3.8}$$

where $Vote_{ct}$ is the logarithmic growth of the votes going to a certain party in city c at election in year t; LMT_{ct} is one minus city c's unemployment rate at year t. X'_{ct} is a vector of city-level controls (log population and voter turnout). ϕ_c are city fixed effects and ϕ_t year fixed effects. The sample period comprises the parliamentary elections of 2011, 2015, and 2016.

We expect our coefficient of interest $\beta_1 < 0$, as unemployment is associated with lower costs in cities with high labour market tightness. This causes cities with low labour market tightness to experience an stronger increase in radical votes, compared to cities with high labour market tightness.

[16] Because we lack information on the local number of job vacancies, instead of vacancies/unemployment we define the measure by 1/unemployment. We, therefore, implicitly assume that there are no different trends in the number of vacancies across different cities. It is plausible, that the number of vacancies and unemployment are negatively correlated following a credit shock, i.e. an affected firm fires employees and cuts back on hiring temporarily. Therefore, if anything, we underestimate the effect of the credit shock on local labour market tightness.

2.3.3 Credit Supply Shock and the Radicalization of Voters (Reduced Form)

Our main hypothesis is that credit constraints impact voting behaviour. To test this, we estimate the following equation on the city-year-level:

$$Vote_{ct} = \beta_1 \; Exposure_{ct}^{for} + \beta_2 \; \mathbf{X'}_{ct} + \phi_c + \phi_t + \epsilon_{ct} \tag{3.9}$$

where $Vote_{ct}$ is the logarithmic growth of the votes going to a certain party in city c at election in year t; $Exposure_{ct}^{for}$ is city c's exposure to foreign weak banks previously defined on the firm-level. This is our main explanatory variable of interest. We include $\mathbf{X'}_{ct}$, a vector of city-level controls (log population and voter turnout). To further control for unobservable factors at the city-level, we include city fixed effects ϕ_c. We also take care of time-trends common to all cities by including year fixed effects ϕ_t. The sample period comprises the parliamentary elections of 2011, 2015 and 2016.

We expect $\beta_1 > 0$, as cities with a higher exposure to firms borrowing from weak banks see a stronger contraction in bank lending, and an increase in the radicalization of votes. Our identifying assumptions is that cities with firms borrowing more from weak banks are hit harder by the troubles of those banks.

However, β_1 in Equation 3.9 only measures the correlation between the two variables and does not allow us to make a causal statement about the effect of a credit crunch on the radicalisation of votes. We turn to an instrumental variable approach in the next section.

2.3.4 Instrumental Variable Approach

To establish a causal relationship between labour market tightness and voter behaviour, we propose an instrumental variable based on our city-level exposure measure

to weak banks. In a standard two stage least square (2SLS), we first regress LMT on Foreign Exposure

$$LMT_{ct} = b \text{ Exposure}_{ct}^{for} + \gamma \mathbf{X'}_{ct} + \epsilon_{pt} \tag{3.10}$$

and use the prediction of LMT_{ct}, $\widehat{LMT_{ct}}$ as regressor in the second stage regression:

$$Vote_{ct} = \beta' \widehat{LMT_{ct}} + \gamma \mathbf{X'}_{ct} + \epsilon_{ct} \tag{3.11}$$

The b coefficient estimated from Equation 3.10, the first stage, measures the relationship between labour market tightness (LMT_{ct}) and the city-level exposure to foreign weak banks ($Exposure_{ct}^{for}$). The reasoning behind this is the following: Foreign bank bailouts affect employment on the firm-level, and hence labour market tightness. If this was not the case, b would be equal to zero, and our instrument would be weak. If foreign bank bailouts instead do play a role for domestic labour markets, b is larger than zero. We cluster our standard errors at the city-level.

Consequently, the β' coefficient estimated from Equation 3.11, the second stage, allows for causal interpretation of the effect of labour market tightness on the change in radical voting.

3 Main Results

This section presents the empirical results. The analyses in Section 3.1 to 3.3 are on the city-year-level. This allows including city and year fixed effects in all specifications, which is a rigorous way of absorbing time-invariant factors at the city-level as well as common time trends. We first show that cities with lower labour market tightness experience a stronger increase in the radicalization of voters. We then set up a IV estimation, and provide causal evidence on the effect of labour market tightness on the radicalisation of votes. In Section 3.4 we first demonstrate on the firm-bank-quarter-level that foreign weak banks curtail credit to firms more than other banks. We then reconfirm these findings on the firm-year-level in Section 3.5. Firms with a relationship to foreign weak banks experience a drop in loan and employment growth.

3.1 City-Level: OLS Results

Table 3.3 presents the results of labour market tightness on electoral outcomes. All estimations include year as well as year and city fixed effects. Robust standard errors are clustered at the city-level. We start by looking at the logarithmic growth, or changes, in votes for radical parties (Column 1-2). In Column (1), the coefficient of interest is negative and significant. Once we include year fixed effects to take care of common macro trends, the coefficient turns insignificant. A different picture emerges when looking at changes in votes for central parties (Column 3-4): the coefficient of interest is positive and highly statistically significant across specifications, less so economically. Column 5 and 6 report the results for our preferred measure of "Radicalisation", the logarithmic growth of radical minus the logarithmic growth of central. In other words, it allows grasping which of the two components grew more/less. The coefficient is highly significant and negative in Column 5 and stays that way once we include year fixed effects in Column 6. Cities with lower (higher)

labour market tightness experience an increase (decrease) in radicalisation. This result is marginally economically significant. A one-standard-deviation decrease in labour market tightness translates into a 0.9 percentage point increase in the radicalisation of votes.

3.2 City-Level: Reduced Form Results

Table 3.4 presents the results of credit constraints on electoral outcomes. All estimations include control variables on the city-level: log population and voter turnout. Robust standard errors are clustered at the city-level. $\text{Exposure}_{ct}^{for}$ is city c's exposure to foreign weak banks previously defined on the firm-level at year t. We start by looking at the logarithmic growth, in votes for radical parties (Column 1-2). Column (1) includes city fixed effects. The coefficient of interest is positive and significant at the 1% level. Once we add year fixed effects to absorb any common trends across cities (Column 2), the coefficient stays positive and highly significant but halves in magnitude. Cities with a higher exposure to weak banks experience an growth in votes for radical parties. This result is slightly economically significant. A one standard deviation increase in exposure translates into a 0.5 percentage increase in radical voting. We turn to the change in votes for centrist parties in Column (3) and (4). Without year fixed effects (Column 3), our exposure measure has a negative and highly significant impact on centralist voting. Once we include year fixed effects, the coefficient of interest decreases in magnitude but stays significant (Column 4).

Lastly, we look at our preferred measure "Radicalization", the logarithmic growth of radical minus the logarithmic growth of central. Column (5) includes only city fixed effects, while Column (6) includes both year and city fixed effects. In both specifications, our coefficient of interest is highly significant. When we include year fixed effects in Column (6), the coefficient decreases in magnitude, but stays significant at the 1% level. Cities with a higher exposure to foreign weak banks experience an increase in radicalisation. In economic terms, a one standard deviation increase in

exposure translates into a increase our measure of radicalization of 0.6% points.

3.3 City-Level: IV Results

Given the endogeneity of labour market tightness in the previous regressions, we introduce an instrumental variable approach. This allows us to establish a causal relationship between labour market tightness, our proxy for unemployment risk, and changes in voting behaviour.

Table 3.5 reports the results of the two stage least squares estimation (2SLS). We use our preferred measure "Radicalisation" as dependent variable across all specifications. Column (1) presents again the OLS estimates, while Column (2) presents again the estimates from the reduced form. Column (3) gives the results of the first stage, and Column (4) the results of the second stage. [17] In Column (3), we empirically test if our instrument $Exposure_{ct}^{for}$ has an impact on labour market tightness. We find a highly significant and strong negative relationship. This evidence suggest that our instrument is indeed a relevant instrument. Column (4) reports estimates for the second stage regression as defined in Equation (3.11). Compared to the OLS estimate in Column (1), the IV coefficient in Column (4) gains both in magnitude and significance. We find a strong negative relationship. Economically speaking, a one-standard-deviation increase in instrumented labour market tightness leads to a 7 % point increase in vote radicalisation. This effect is significant at the 1% level.

3.4 Firm-Bank-Level Results

Table 3.6 presents the results of the estimation on the firm-bank-quarter-level. The dependent variable is the logarithm of (one plus) the loan issuances to firm j provided by bank i at quarter t, conditional on the firm j receiving a loan. Foreign WB_i is an

[17] The results differ slightly from what we have discussed in the sections before, as we condition on the sample of the IV across specifications here.

indicator variable equal to one if bank i is a foreign weak bank, that is, was subject to government aid in its home country, and equal to zero otherwise. $Post10_t$ is equal to zero up to Q1 2010, the start of the European debt crisis, and 1 afterwards. Each column includes bank-level controls, such as the log of total assets, the equity ratio, the cash ratio, the liquidity ratio, and the deposits ratio. Robust standard errors are clustered at the bank-level, which is the level of the treatment.

Column (1) does not include any fixed effects, the coefficient on the interaction term is negative, but insignificant. Once we include bank fixed effects, as well as industry × quarter and firm × quarter fixed effects in Column (2), the coefficient of interest is negative and significant at the 1% level. To refine our comparison between treatment and control group, we add time-varying bank-level controls in Column (3). The coefficient of interest remains virtually unchanged: Foreign weak banks decrease their lending by exp(0.302) - 1 = 35.3% to the same firm compared to other banks after the start of the European debt crisis.

It is important to highlight that our identification strategy relies on the absence of differential pre-2010 trends in terms of loan issuance for banks in the treatment and control groups. We test this parallel trends assumption graphically in Figure **??**, showing the quarterly coefficients of loan volume between Q1 2008 and Q4 2012. The coefficient is not significantly different from zero before Q1 2010, and turns negative after Q1 2010. This provides evidence that loan issuance between the foreign weak banks and the healthy banks did not differ systematically in the period prior to Q1 2010.

3.5 Firm-Level Results

Next, to corroborate our findings further, we re-run the estimations on the firm-level. Due to data limitation, we now run our analyses on the year instead of the quarter level. We define a new treatment indicator variable on the firm-level, Foreign WB_j, equal to 1 if firm j has a relationship with a foreign weak bank, and zero otherwise.

Table 3.7 presents the results. The dependent variable is $\Delta ln(Loans)$, the logarithmic growth rate of loans for firm j in year t. Robust standard errors are clustered at the firm-level. The coefficient of interest remains highly significant and negative as we add more stringent fixed effects across specifications. In Column (2), we add year fixed effects, to take care of common time trends across firms. The coefficient is significant at the 1% and negative. A firm with a relationship to a foreign weak bank, experiences a decrease in its loan growth, compared to firms without such a relationship. In Column (3) we add industry fixed effects, to take care of differences across firms driven by firms operating in different industries. The coefficient increases slightly in magnitude and stays negative and significant. Once we add time-varying firm-level controls, such as log of total assets, ROA, Sales, and CAPEX, all lagged by one year, the coefficient of interest decreases in magnitude, but remains negative and significant at the 5% level. Relative to the control group, firms with a relationship to foreign weak banks experience a substantial decrease in their loan growth.

Next, Table 3.8 provides evidence that the observed credit supply shock on the firm-level has an impact on employment growth. The dependent variable is the logarithmic growth of full-time employees at firm j in year t. Robust standard errors are clustered at the firm-level. In Column (2) we include both year and industry fixed effects, and find that firms with a relationship to foreign weak banks experience a decrease in their employment growth compared to firms without such a relationship. Once we add firm-level controls such as the log of total assets, Capex, ROA and Sales, all lagged by one year, the coefficient of interest remains negative, but is only significant at the 13% level.

To sum up, this evidence re-confirms what we have found so far on the firm-bank-level: Firms with a relationship to foreign weak banks experience a credit supply shock. Also, they experience a drop in employment growth, which confirms negative real effects of banking crises highlighted so far in the literature (Chodorow-Reich

(2014), Acharya and Steffen (2015), Bentolila, Jansen and Jiménez (2018)).

4 Robustness

To substantiate our findings, we run a set of robustness checks.

Distance Measures First, we vary the distance measures in our geo-matching exercise (see Equation (3.2)). Table 3.9 presents the IV results when D is equal to 50 km. We find that such an alternative definition does not alter the significance of our main results.

Different Moving Averages Second, our preferred definition of our exposure measure is its three-year moving average. Table 3.10 presents regressions both with two-year moving averages ($Exposure2_{ct}$) and no smoothing ($ExposureNO_{ct}$). Our results do not depend on how our explanatory variable is smoothed.

Single Parties Third, instead of grouping the vote shares to our political orientation variables, we run our baseline specification on the vote shares going to the single parties present in Spain. Table 3.11 presents the results: The effect of an increase in exposure to foreign weak banks is strongest for votes going to the radical parties.

5 Conclusions

We provide causal evidence on the effect of credit crunches on political radicalisation. We combine data on bank-firm connections and on electoral outcomes on the

city-level during the Spanish financial crisis. First, we show that firms in a relationship with weak banks experience a reduction in loan supply. Next, we estimate the effects of unemployment on voting behaviour. We construct an instrument for unemployment based on the city-level exposure to foreign weak banks. We find that a one standard deviation increase in instrumented unemployment translates into a 7 percentage point increase in the radicalisation of votes.

This Chapter expands our understanding of the channels through which financial crises radicalize voters. Our results confirm the model of Guiso et al. (2017): Rising economic insecurity leads to higher support for populist parties. We find that credit supply shocks lead to political radicalisation through rising unemployment risk.

Some caveats are in order. We only focus on unemployment risk as a channel, which however does not exclude other channels discussed in the literature so far (i.e. cultural traits, import competition, austerity, debtor-creditor conflicts, and public mismanangement). Studying in more detail the relative strength of the different channels would be a profitable avenue for future research.

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Tables and Figures

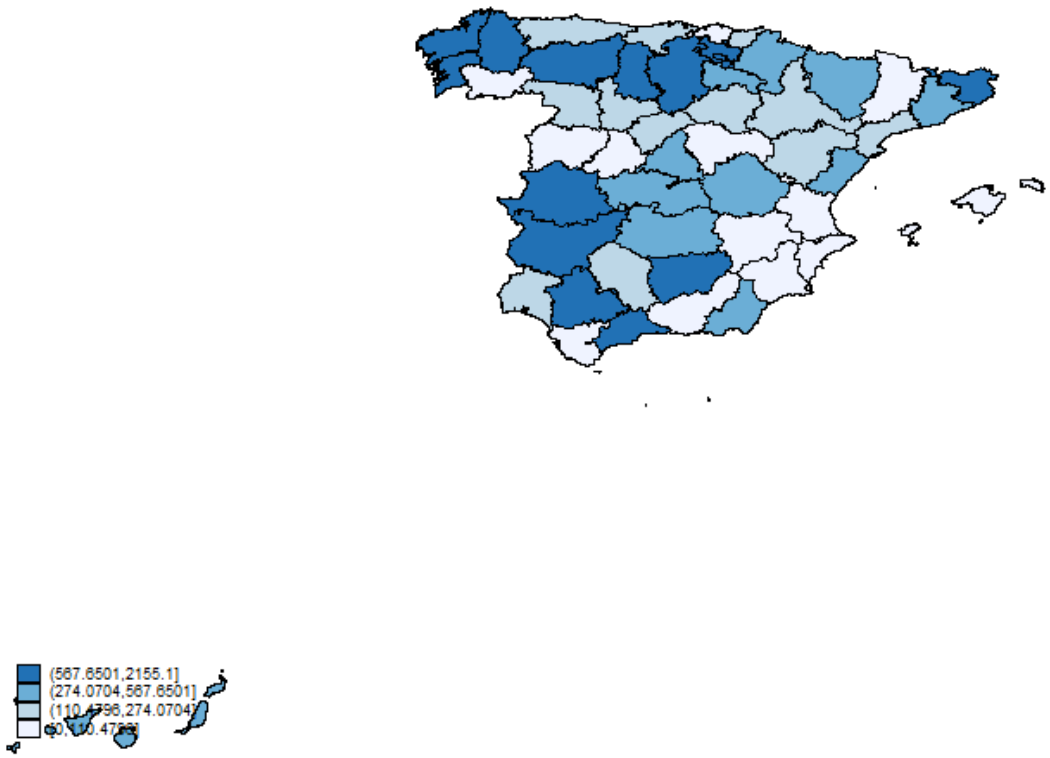

Figure 3.1: Exposure to Weak Banks by Spanish Provinces. This figure plots the city-level exposure measure to weak banks for 51 Spanish provinces end 2015. Source: Own calculations and GADM.

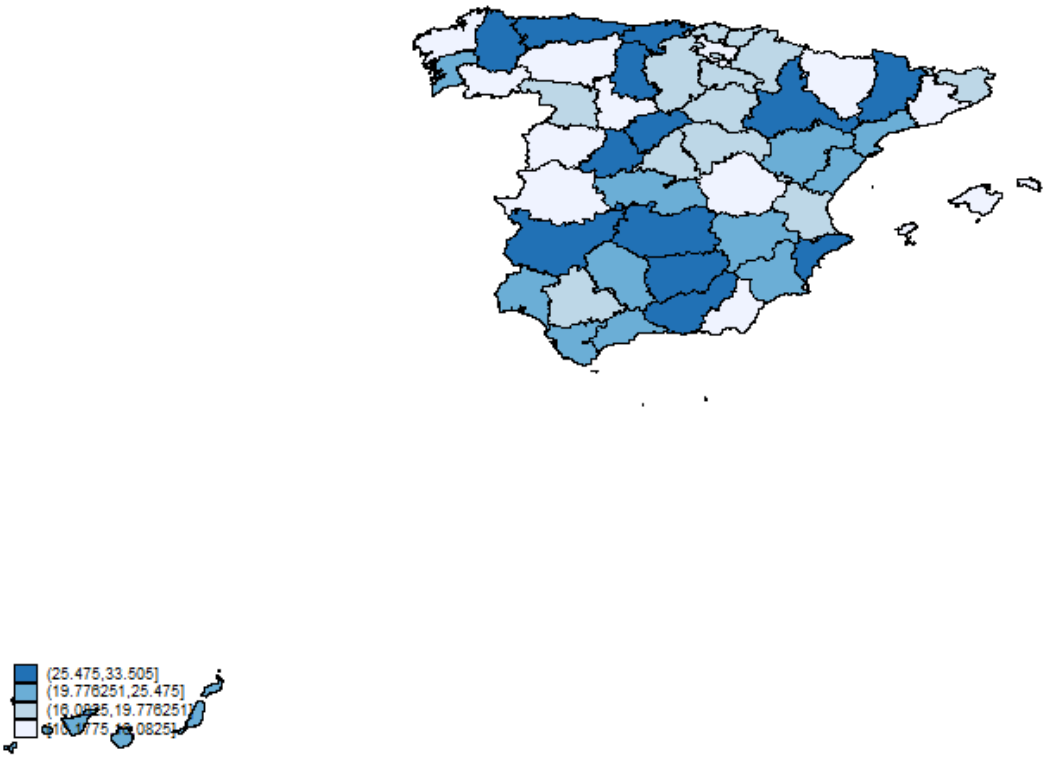

Figure 3.2: Unemployment Rate by Spanish Provinces. This figure plots the unemployment rate for 51 Spanish provinces in 2015. Source: Instituto Nacional de Estadística (INE) and GADM.

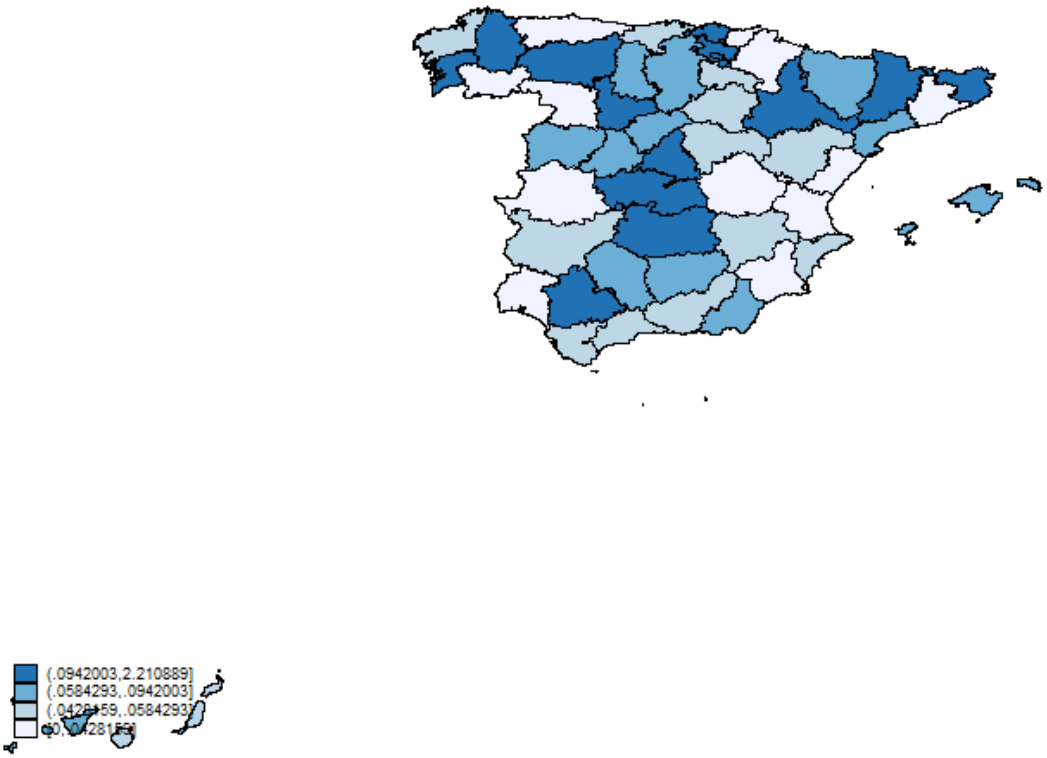

Figure 3.3: Radical Votes by Spanish Provinces. This figure plots the votes going to radical right and left-wing parties in the 2015 parliamentary elections for 51 Spanish provinces. Source: Ministerio del Interior and GADM.

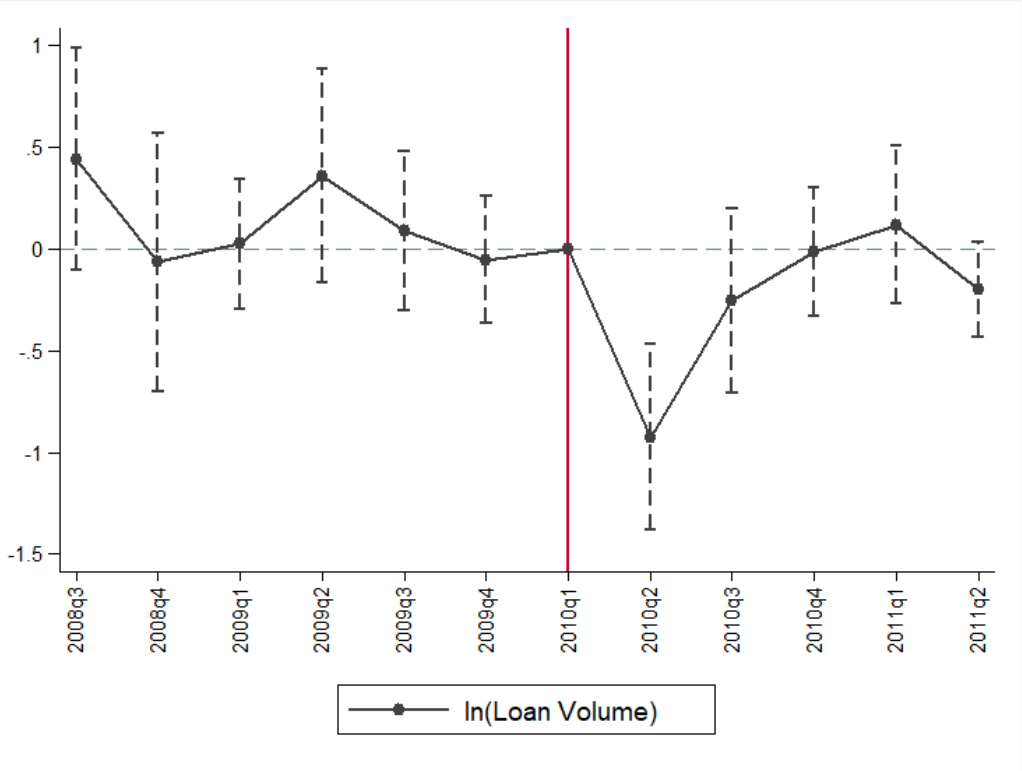

Figure 3.4: Parallel Trends Assumption. The figure is based on the following equation:

$$y_{ijt} = \sum_{k \neq 2010q1} \beta_k \text{ Foreign WB}_i(0/1) \times \mathbf{1}[k = t] + \varepsilon_{ijt},$$

where y_{ijt} is log loan issuance provided by bank i to firm j at quarter t; $\mathbf{1}[k = t]$ is a dummy variable that equals one in quarter t and 0 otherwise. Q1 2010 is excluded to estimate the dynamic effect. The regression includes bank fixed effects, country $\times$ and firm $\times$ quarter fixed effects. In this case, Foreign WB_i (0/1) is an indicator variable that equals one for banks headquartered outside Spain that received government aid. Country fixed effects refer to the respective banks' headquarters. The dashed lines represent 90% confidence intervals, adjusted for bank-level clustering.

Table 3.1: Summary Statistics: Firm-Level. This table presents summary statistics on the firm-level. The sample period is end 2010. *Exposure$_j$ (0/1)* is an indicator variable equal to one if firm j has a relationship with a weak bank, and zero otherwise. *ln(Total assets)* is the natural logarithm of firm j's total assets.*Profits-to-Assets-Ratio* is firm j' EDIBTA over total assets. *LTDebt-to-Assets-Ratio* is firm j's long term debt to total assets. *STDebt-to-Assets-Ratio* is firm j's short term debt to total assets. *Equity-to-Assets-Ratio* is firm j's equity ratio to total assets. *Leverage-Ratio* is firm j's total liabilities to total assets.

	No Exposure			Exposure		
	mean	sd	count	mean	sd	count
Exposure (0/1)	0.00	0.00	361	1.00	0.00	435
ln(Total Assets)	4.92	1.65	154	5.71	1.93	209
Profits-to-Assets-Ratio	8.67	10.51	145	5.46	8.69	198
LTDebt-to-Assets-Ratio	30.75	24.30	132	32.14	23.88	176
STDebt-to-Assets-Ratio	9.68	12.90	130	10.75	16.42	174
Equity-to-Assets-Ratio	29.10	22.09	154	24.95	21.60	209
Leverage-Ratio	71.25	21.75	153	75.05	21.60	209

Table 3.2: Summary Statistics: City-Level. This table presents summary statistics on the constituency(city)-year-level. The sample period is 2015. The sample is split between cities with *No Exposure* and cities with *Exposure*. *No Exposure*$_c$ are cities without exposure to weak-bank connected firms. *Population* is the total population of constituency c in thousands. *Voter turnout* is the ratio of total votes and the electoral census. *Unemployment rate* is ratio of the number of unemployed people over total labour force on the city-level. *Vote Share "k"* is the ratio of votes going to party "k" over total votes in city c .

	No Exposure			Exposure		
	mean	sd	count	mean	sd	count
Population (in thousands)	4.24	20.97	1,966	9.21	46.00	2,691
Unemployment Rate	20.95	5.43	1,966	21.05	5.90	2,691
Voter Turnout	0.75	0.06	1,966	0.75	0.06	2,691
Vote Share Far-Right	0.14	0.37	1,966	0.12	0.37	2,691
Vote Share Conservatives	36.39	16.78	1,966	32.76	17.48	2,691
Vote Share Liberals	10.34	5.60	1,966	9.85	5.53	2,691
Vote Share Socialist	26.14	12.82	1,966	24.21	13.92	2,691
Vote Share Greens	0.00	0.00	1,966	0.00	0.00	2,691
Vote Share Far-Left	10.22	7.75	1,966	12.79	8.46	2,691

Table 3.3: OLS: Effect of Labour Market Tightness on Electoral Results. This table provides results of a OLS regression analyzing the effect of labour market tightness on electoral results. The analysis is based on data on the city-year-level. The sample period are the election years 2011, 2015 and 2016. The dependent variable is $\Delta ln(Radical)$, the logarithmic growth of the votes going to the radical left and radical right in city c at year t in Column (1) and (2); $\Delta ln(Central)$ in Column (3) and (4), the logarithmic growth of the votes going to the conservatives and the social democrats in city c at year t; *Radicalisation* in Column (5) and (6), equal to $\Delta ln(Radical) - \Delta ln(Central)$. Labour market tightness LMT_{ct} is 1 over city c's unemployment rate in year t; The control variables on the city-level are *Population*, the log of the total population of city c in thousands and *Turnout*, the ratio of city c's total votes over the electoral census. All variables are defined in the Appendix 1.1. The regressions further include city and year fixed effects, as indicated. Reported standard errors are in parentheses, clustered at the city-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Δ log(Radical)	Δ log(Radical)	Δ log(Central)	Δ log(Central)	Radicalisation	Radicalisation
LMT_{ct}	-0.024***	-0.002	0.089***	0.006***	-0.113***	-0.009**
	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)	(0.004)
Observations	8,179	8,179	8,179	8,179	8,179	8,179
R-squared	0.267	0.281	0.386	0.762	0.261	0.413
City-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes	No	Yes
Cluster	City	City	City	City	City	City

Table 3.4: Reduced Form: Effect of Credit Supply Shock on Electoral Results. This table provides results of a OLS regression analyzing the effect of credit constraints on electoral results. The analysis is based on data on the city-year-level. The sample period are the election years 2011, 2015 and 2016. The dependent variable is $\Delta ln(Radical)$, the logarithmic growth of the votes going to the radical left and radical right in city c at year t in Column (1) and (2); $\Delta ln(Central)$ in Column (3) and (4), the logarithmic growth of the votes going to the conservatives and the social democrats in city c at year t; *Radicalisation* in Column (5) and (6), equal to $\Delta ln(Radical) - \Delta ln(Central)$. $Exposure_{ct}$ is city c's exposure to weak banks previously defined on the firm-level at year t; The control variables on the city-level are *Population*, the log of the total population of city c in thousands and *Voter turnout*, the ratio of total votes and the electoral census of city c. All variables are defined in the Appendix 1.1. The regressions further include city and year fixed effects, as indicated. Reported standard errors are in parentheses, clustered at the city-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Δ log(Radical)	Δ log(Radical)	Δ log(Central)	Δ log(Central)	Radicalisation	Radicalisation
$\text{Exposure}_{ct}^{for}$	0.011***	0.005***	-0.003***	-0.001*	0.015***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Observations	11,677	11,677	11,677	11,677	11,677	11,677
R-squared	0.347	0.356	0.191	0.736	0.230	0.423
City-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes	No	Yes
Cluster	City	City	City	City	City	City

Table 3.5: IV Results: Votes and Labour Market Tightness. This table reports the regressions of the effect of labour market tightness (LMT_{ct}) on votes. Column 1 presents the OLS regression of Radicalisation on LMT. Column 2 presents the Reduced From regression of Radicalisation on Foreign Exposure. Column 4 presents the estimates of a two stage least squared (2SLS) fixed effects panel regression. The first stage (Column 3) uses $Exposure_{ct}^{for}$, the log of city c's exposure to foreign weak banks in year t as an instrument for labour market tightness (LMT_{ct}). We report the Angrist-Pischke F test statistic of the excluded instruments in the first stage regression. *Radicalisation* is equal to $\Delta ln(Radical) - \Delta ln(Central)$. The control variables on the city-level are *Population*, the log of the total population of city c in thousands and *Voter turnout*, the ratio of total votes and the electoral census of city c. All variables are defined in the Appendix 1.1. Reported standard errors are in parentheses, clustered at the city-level. All specifications include city fixed effects. ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1) Radicalisation	(2) Radicalisation	(3) LMT	(4) Radicalisation
LMT	-0.01*			-0.07***
	(0.005)			(0.012)
Turnout	-0.64***	0.25***	0.47***	-0.58***
	(0.081)	(0.073)	(0.182)	(0.079)
Population	-0.17***	-0.08***	-0.06	-0.16***
	(0.029)	(0.024)	(0.104)	(0.028)
Exposure^{for}		0.02***	-0.19***	
		(0.002)	(0.010)	
N	6063	6063	5768	5768
APFtest			352.044	

Table 3.6: Firm-Bank-Level: Credit Supply Shock This table provides results of a OLS regression analyzing the volume of loan issuances when banks are weak banks before and after the start of the European debt crisis in May 2010. The analysis is based on data on the firm-bank-quarter-level. The sample period is Q2 2008 to Q1 2012. *Ln(loan volume)* is the logarithm of (one plus) the loan issuance from bank i (as lead arranger) to firm j at quarter t. *Foreign* WB_i is an indicator variable equal to one if bank i is a foreign weak bank, and zero otherwise. $Post10_t$ is an indicator variable equal to one after Q1 2010, the start of the European debt crisis, and equal to zero otherwise. Bank controls are bank i's log of total assets, the leverage ratio, the cash ratio, the liquidity ratio and the deposits ratio, lagged by two periods. The regressions further include country × time and firm × time fixed effects, as indicated. Country fixed effects refer to the respective banks' headquarters. Reported standard errors are in parentheses, clustered at the bank-level . ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)
VARIABLES	ln(1 + Loan Volume)	ln(1 + Loan Volume)	ln(1 + Loan Volume)
Foreign $WB_i \times Post10_t$	-0.171	-0.312***	-0.302***
	(0.205)	(0.069)	(0.067)
Total Assets			0.068
			(0.104)
Equity Ratio			-0.004
			(0.036)
Cash Ratio			-0.006
			(0.018)
Liquidity Ratio			-0.003
			(0.006)
Deposits Ratio			0.000
			(0.003)
Foreign WB_i	0.509**		
	(0.231)		
$Post10_t$	-0.095		
	(0.094)		
Observations	1,313	1,313	1,313
R-squared	0.017	0.929	0.929
Bank FE	No	Yes	Yes
Country × Time FE	No	Yes	Yes
Firm × Time FE	No	Yes	Yes
Cluster	Bank	Bank	Bank

Table 3.7: Firm-Level: Loan Growth This table provides results of a OLS regression analyzing the loan growth when a firm has a relationship with a weak foreign bank. The analysis is based on data on the firm-year-level. The sample period is 2008 to 2012. The dependent variable is $\Delta ln(Loans)$, the logarithmic growth rate of loan issuance to firm j in year t. *Foreign* WB_j is an indicator variable equal to one if firm j has a relationship with a foreign weak bank, and zero otherwise. Firm controls are firm j's log of total assets, ROA, Sales and CAPEX, all lagged by one year. The regressions further include year fixed effects and industry × year fixed effects, as indicated. Reported standard errors are in parentheses, clustered at the firm-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	$\Delta ln(Loans)$	$\Delta ln(Loans)$	$\Delta ln(Loans)$	$\Delta ln(Loans)$
Foreign WB_j	-0.340***	-0.353***	-0.373***	-0.286**
	(0.073)	(0.071)	(0.111)	(0.114)
Total Assets				-0.033
				(0.038)
Capex				0.002*
				(0.001)
ROA				0.071
				(0.043)
Sales				0.028
				(0.027)
Observations	1,438	1,438	1,438	1,438
R-squared	0.003	0.005	0.027	0.030
Year FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Cluster	Firm	Firm	Firm	Firm

Table 3.8: Firm-Level: Employment Growth This table provides results of a OLS regression analyzing the employment growth when a firm has a relationship with a weak foreign bank. The analysis is based on data on the firm-year-level. The sample period is 2008 to 2012. The dependent variable is $\Delta ln(Employees)$, the logarithmic growth rate of full-time employees at firm j in year t. *Foreign* WB_j is an indicator variable equal to one if firm j has a relationship with a foreign weak bank, and zero otherwise. Firm controls are firm j's log of total assets, lagged by one year. The regressions further include year fixed effects and industry × year fixed effects, as indicated. Reported standard errors are in parentheses, clustered at the firm-level . ***, **, *, $^+$ denote significance at the 1, 5, 10% level, respectively.

	(1)	(2)	(3)
VARIABLES	$\Delta ln(Employees)$	$\Delta ln(Employees)$	$\Delta ln(Employees)$
Foreign WB_j	-0.039	-0.106**	-0.075
	(0.046)	(0.050)	(0.049)
Total Assets			-0.015
			(0.011)
Capex			-0.000
			(0.001)
ROA			0.000
			(0.013)
Sales			-0.011
			(0.023)
Observations	1,438	1,438	1,438
R-squared	0.000	0.044	0.045
Year FE	No	Yes	Yes
Industry FE	No	Yes	Yes
Cluster	Firm	Firm	Firm

Table 3.9: Robustness: Distance Measure (50 km). This table reports the regressions of the effect of labour market tightness (LMT_{ct}) on votes. Column 1 presents the OLS regression of Radicalisation on LMT. Column 2 presents the Reduced From regression of Radicalisation on Foreign Exposure. Column 4 presents the estimates of a two stage least squared (2SLS) fixed effects panel regression. The first stage (Column 3) uses $Exposure^{for}_{ct}$, the log of city c's exposure to foreign weak banks in year t as an instrument for labour market tightness (LMT_{ct}). We report the Angrist-Pischke F test statistic of the excluded instruments in the first stage regression. *Radicalisation* is equal to $\Delta ln(Radical) - \Delta ln(Central)$. The control variables on the city-level are *Population*, the log of the total population of city c in thousands and *Voter turnout*, the ratio of total votes and the electoral census of city c. All variables are defined in the Appendix 1.1. Reported standard errors are in parentheses, clustered at the city-level. All specifications include city fixed effects. ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1) Radicalisation	(2) Radicalisation	(3) LMT	(4) Radicalisation
LMT	-0.01***			-0.07***
	(0.004)			(0.015)
Turnout	-0.85***	0.04	0.61***	-0.82***
	(0.065)	(0.060)	(0.137)	(0.065)
Population	-0.23***	-0.09***	-0.03	-0.22***
	(0.029)	(0.024)	(0.075)	(0.028)
Exposure^{for}		0.04***	-0.17***	
		(0.002)	(0.008)	
N	10012	10012	9692	9692
APFtest			461.847	

Table 3.10: Robustness: Different Moving Averages This table provides results of a OLS regression analyzing the effect of credit constraints on electoral results. The analysis is based on data on the city-year-level. The sample period are the election years 2011, 2015 and 2016. The dependent variable is *Radicalisation*, equal to $\Delta ln(Radical) - \Delta ln(Central)$, where c is city and t is year. $ExposureNO_{ct}$ is city c's exposure to weak banks previously defined on the firm-level; $Exposure2_{ct}$ is the two-year moving average of city c's exposure to foreign weak banks previously defined on the firm-level; $Exposure4_{ct}$ is the four-year moving average of city c's exposure to weak banks previously defined on the firm-level. The control variables on the city-level are *Population*, the log of the total population of city c in thousands and *Voter turnout*, the ratio of total votes and the electoral census of city c. All variables are defined in the Appendix 1.1. The regressions further include city and year fixed effects, as indicated. Reported standard errors are in parentheses, clustered at the city-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	Radicalisation	Radicalisation	Radicalisation	Radicalisation
ExposureNO$_{ct}$	0.014***	0.012***		
	(0.001)	(0.001)		
Exposure2$_{ct}$			0.015***	0.005***
			(0.001)	(0.001)
Observations	13,734	13,734	13,734	13,734
R-squared	0.201	0.382	0.210	0.378
City Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes
Cluster	City	City	City	City

Table 3.11: Robustness: Single Parties This table provides results of a OLS regression analyzing the effect of credit constraints on electoral results. The analysis is based on data on the city-year-level. The sample period are the election years 2011, 2015 and 2016. The dependent variable are the growth rates of different parties: Radical (radical right and left), Conservatives, Liberals, Socialists, Greens, Regionalists, where c is city and t is year; $Exposure_{ct}$ is city c's exposure to weak banks previously defined on the firm-level; The control variables on the city-level are *Population*, the log of the total population of city c in thousands and *Voter turnout*, the ratio of total votes and the electoral census of city c. All variables are defined in the Appendix 1.1. The regressions further include city and year fixed effects, as indicated. Reported standard errors are in parentheses, clustered at the city-level . ***, **, * denote significance at the 1, 5, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	$\Delta Radical$	$\Delta Conservative$	$\Delta Liberal$	$\Delta Socialist$	$\Delta Greens$	$\Delta Regionalist$
$\text{Exposure}_{ct}^{for}$	0.010***	-0.004***	0.001**	-0.002***	0.001***	-0.002
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)
Observations	13,734	13,734	13,734	13,734	13,734	13,734
R-squared	0.295	0.743	0.728	0.638	0.125	0.509
City-Controls	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	City	City	City	City	City	City

Appendices

1 Appendix A1

Variable Definitions.

Variable	Source	Description
Foreign WB_j		indicator variable equal to one if firm j has a relationship with a foreign weak bank, and zero otherwise
$\Delta ln(Loans)$	Dealscan	the logarithmic growth rate of loan issuance to firm j in year t.
$\Delta ln(Employees)$	CapitalIQ	the logarithmic growth rate of full-time employees at firm j in year t.
Total assets	CapitalIQ	the natural logarithm of firm j's total assets
Profits-to-Assets-Ratio	CapitalIQ	firm j' EDIBTA over total assets
LTDebt-to-Assets-Ratio	CapitalIQ	firm j's long term debt to total assets
STDebt-to-Assets-Ratio	CapitalIQ	firm j's short term debt to total assets
Equity-to-Assets-Ratio	CapitalIQ	firm j's equity ratio to total assets
Leverage-Ratio	CapitalIQ	firm j's total liabilities to total assets
CAPEX	CapitalIQ	firm j's Capital Expenditures to total assets
ROA	CapitalIQ	firm j's Return on Assets
Sales	CapitalIQ	firm j's Sales to total assets
Ln(loan volume)	Dealscan	the logarithm of (one plus) the loan issuance from bank i (as lead arranger) to firm j at quarter t.
Foreign WB_i	Dealscan	is an indicator variable equal to one if bank i is a foreign weak bank, and zero otherwise.
Post10_t		is an indicator variable equal to one after Q1 2010, the start of the European debt crisis, and equal to zero otherwise.
Total Assets	CapitalIQ (SNL Financials)	bank i's log of total assets
Cash-to-Assets ratio	CapitalIQ (SNL Financial)	Share of cash and equivalents over total assets
Deposits-to-Assets ratio	CapitalIQ (SNL Financial)	Share of deposits over total assets
Liquidity-to-Assets ratio	CapitalIQ (SNL Financial)	Share of investment securities over total assets
Exposure_{ct}		city c's exposure to weak banks previously defined on the firm-level at year t
$\text{Exposure}_{ct}^{for}$		city c's exposure to foreign weak banks previously defined on the firm-level at year t
Voter Turnout	Spanish Interior Ministry	ratio of total votes and the electoral census in constituency (city) c
Vote Share "k"	Spanish Interior Ministry	is the ratio of votes going to party "k" over total votes in constituency (city) c
Population	Spanish Interior Ministry	log of the total population of city c in thousands
Unemployment Rate	Spanish Statistical Office INE	is ratio of the number of unemployed people over total labour force on the city-level
Labour market tightness LMT_{ct}		equals one over city c's unemployment rate in year t
$\Delta ln(Radical)$		logarithmic growth of the votes going to the radical left and radical right in city c
$\Delta ln(Central)$		the logarithmic growth of the votes going to the conservatives and the social democrats in city c
$Radicalisation$		equal to $\Delta ln(Radical) - \Delta ln(Central)$

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Declaration on Co-authors, Own Contribution, and Publication Status

Chapter	Paper Title	Names of Co-Authors	Declaration of Own Contribution	Publication Status (When/Where)
Chapter 1	Flight Abroad within the Euro Area: Evidence From a ECB Collateral Framework Change		single-authored	SSRN Electronic Journal, available at "https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3963809"
Chapter 2	They Who Call the Piper Pay the Tune: Bank Bailouts and Political Connections of Bank Boards	Philipp Schaz	each section was joint work	working paper
Chapter 3	When Credit Turns Radical: Evidence From the Spanish Financial Crisis	Simon Baumgartner	each section was joint work	working paper