Three Essays in Financial Economics

by

Karsten Müller

[redacted]

Thesis

Submitted to the University of Warwick

for the degree of

Doctor of Philosophy

Warwick Business School

July 2018

Contents

Acknowledgements

I would like to thank my supervisors Stuart Fraser and James Mitchell for their continuous help and support. By allowing me to pursue my own path and guiding me diligently, Stuart and James made this dissertation possible. My examiners Steve Roper and Thorsten Beck also provided invaluable guidance.

Without the support of Ben Iverson, Michael Weber, Amir Sufi, Atif Mian, and Adam Geršl my professional life would have taken a very different turn, and I will always be grateful for their trust in my abilities. The conversations I had with Jacopo Ponticelli, Daisuke Ikeda, Matthieu Chavaz, Raghuram Rajan, Luigi Zingales, and Elisabeth Kempf towards the end of the programme were absolutely invaluable to me. I always enjoyed sharing ideas with colleagues and other graduate students who are much brighter than me, especially Jan Keil, Carlo Schwarz, Paymon Khorrami, Philipp Hukal, Ahmed Nofal, David Finer, Evan Fradkin, Kebin Ma, Matthias Karabaczek, Jung Sakong, Matthias Drehmann, Andrea Presbitero, and Lu Liu. Thank you also to Caspar von Schenck – who deserves the title of the world's best roommate – for sharing a flat with me.

During the doctoral programme, I had the immense pleasure of visiting the University of the West Indies, University of Chicago, Bank of England, and the International Monetary Fund for research stints. I would like to thank the great people I have met there – too many to mention – for their gracious hospitality. I have also had the pleasure of presenting my research at many conferences and seminars throughout the years, and would like to thank the participants for their helpful comments.

I am forever indebted to David Samuel, Edoardo Frangi, and Denise Minciacovschi whose kind words of encouragement motivated me to apply for graduate school. I would also like to acknowledge the financial support I have received from the Economic and Social Research Council Doctoral Training Centre and Chancellor's International Scholarship at Warwick.

Above all, I want to thank my family. Thank you to my wonderful parents, Gudrun and Karl-Reiner Müller, for their guidance, patience, and unconditional love. Without them, I would have never undertaken a PhD. Most importantly, I want to say thank you to my lovely wife Kimberley, who always keeps me in high spirits with an admirable ease and graciousness. Thank you also to my wonderful in-laws for their love and support.

Declaration

I declare that all material contained in this thesis is entirely my own work. I further declare that the thesis has not been submitted for a degree at another university.

Karsten Müller
1 July 2018

Abstract

This thesis investigates the long-run development of credit markets around the world and the legal frameworks governing them. Chapter 1 begins by discussing open questions about financial sector development and financial crises, in particular the role of financial regulation and bankruptcy frameworks. One striking pattern that emerges is the relative paucity of comparable cross-country data on the structure of debt markets. Chapter 2 thus introduces a new resource for macro-finance research: a long-run database on the outstanding amount and sectoral allocation of credit in 120 countries all over the globe from 1910. I discuss in detail how I constructed comparable data on the sectoral level from over 600 archival sources and present a range of new stylized facts. Perhaps most strikingly, corporate credit (relative to GDP) stopped expanding all over the world around 1980, while household credit has skyrocketed. Importantly, the rise of household credit is not only driven by mortgages but also consumer credit – particularly in emerging economies. I then test empirically which theories can explain these trends in credit allocation.

A potential policy implication some may want to draw from the stylized facts in Chapter 2 is that regulators should target particular sectors to influence the allocation of credit. To this end, many central banks around the world regularly use macroprudential tools, which are supposed to prevent a build-up of systemic risk. In chapter 3, I show that such targeted policies historically exhibit a large, robust electoral cycle around the world. More precisely, prudential tools are considerably less likely to be tightened, and more likely to be loosened, in pre-election quarters – particularly when upcoming elections are expected to be close. Central bank independence, which is thought to ease political economy constraints, does not appear to be an important moderating factor for the election cycle in prudential regulation; it does, however, eliminate electoral cycles in monetary policy. Taken at face value, these findings suggest that the more immediate effect of prudential policies on the median voter might make them more difficult to implement than previously imagined.

In Chapter 4, I turn to another legal determinant of credit market outcomes: the efficiency of bankruptcy courts. While it is well known that legal frameworks matter for the development of debt markets, much less is known about the implementation of law into practice, i.e. debt enforcement. I study this question using quasi-random exposure of firm borrowers to the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA) in the United States. BAPCPA fundamentally reformed consumer bankruptcies, which resulted in the largest drop of bankruptcy filings recorded in US history, particularly in districts with a historically higher share of consumer cases. Because BAPCPA left bankruptcy rules for firms largely unchanged, I can identify the causal effect of reduced court congestion on financing terms. I find that the drop in caseload per judge was associated with an improvement in interest rate spreads and loan maturities. A back-of-the-envelope calculation implies that the social costs of busy courts are large.

Chapter 5 provides implications of my thesis for policy makers and future research. I also discuss some preliminary results from ongoing research into how credit is allocated during credit booms and their association with subsequent banking crises.

Chapter 1

Introduction

The Great Financial Crisis of 2007-2008 has led to a reevaluation of the role of credit for macroeconomic dynamics. Long thought of as a mere "veil", there is now an emerging consensus that debt contracts are important for understanding economic development in the long-run but also business cycle fluctuations. Despite a wealth of new insights, it has proven extraordinarily difficult for policy makers to derive regulatory frameworks in the post-crisis era, because relatively recent research appears to partially contradict long-held beliefs about the welfare effects of growing financial sectors. In this thesis, I provide new empirical evidence that I hope will contribute to this important debate.

The role of banking and finance has a long tradition in economics, dating back at least to the classic work of Bagehot (1873). One of the early contributors was Schumpeter (1912), who argued that credit creation by banks enables entrepreneurs to engage in innovative projects, and thus drives economic growth. Both Von Mises (1912) and Hayek (1929) saw credit as an important driver of the business cycle. Goldsmith (1969) first documented a positive correlation between the size of the financial sector and economic development. A large body of literature, starting with the seminal work of King and Levine (1993), found empirical evidence for a link between financial development and economic growth on the macro-level, and argue that this partially reflects causality going from finance to growth.[1].

The perhaps most convincing evidence, however, comes from studies using sectoral or firm-level data. Rajan and Zingales (1998) show that manufacturing industries with a higher dependence on external financing grow faster in countries with higher financial development (see also Guiso et al., 2004a). These industries also experience deeper downturns during recessions, particularly when they have fewer tangible assets (Braun and Larrain, 2005). In countries with more developed financial systems, risky borrowers can also pledge less and more firm-specific collateral, which eases financing constraints (Liberti and Mian, 2010). Fisman and Love (2007) and Bekaert et al. (2007) find that financial development and liberalization, respectively, are asso-

[1]Major contributions include Arestis and Demetriades (1997), Rousseau and Wachtel (1998), Beck et al. (2000), Levine et al. (2000), Wurgler (2000), and Guiso et al. (2004b). See Levine (1997), Levine (2005), Beck (2008), and Popov (2017) for excellent surveys. The literature on financial liberalizations has also found growth effects of plausibly more exogenous reforms (e.g. Henry, 2000b,a; Bekaert et al., 2001, 2005; Levchenko et al., 2009). These findings are also related to the literature on the bank lending channel of monetary policy, e.g. Jiménez et al. (2012) and the references therein.

ciated with higher growth in industries with higher growth opportunities. Banerjee and Duflo (2014), Brown and Earle (2017), and Lelarge et al. (2010) use exogenous, policy-induced changes to firms' access to debt financing and show that these have large effects on firm outcomes. A large separate literature on banking deregulation in the United States has shown largely positive effects on borrowers (see Kroszner and Strahan, 2014, for a survey); Bertrand et al. (2004) provide similar evidence for France. This is intuitive, given that such deregulations have been found to be accompanied by large changes to credit markets (see e.g. Keil and Müller, 2018).

There is also evidence on a tight link between finance and innovation, but the results are considerably less clear-cut. Hsu et al. (2014), for example, find that more developed equity markets foster innovation in a sample of 32 countries, but larger debt markets have a detrimental effect. A related paper by Brown et al. (2013) finds that credit markets have an effect on fixed investment, but only equity markets matter for R&D. Also looking across countries, Acharya and Subramanian (2009) find that more creditor-friendly bankruptcy codes that foster financial development (Djankov et al., 2007) are associated with less innovation. These findings are consistent with the existence of pervasive information asymmetries in debt markets (e.g. Stiglitz and Weiss, 1981). Drawing on the US experience, Chava et al. (2013), Cornaggia et al. (2015), Kerr and Nanda (2009b) find that the effect of deregulation on innovation differs across private and public firms, and – depending on the type of regulatory change – may be positive or negative; Amore et al. (2013). Benfratello et al. (2008) find that local banking development boosts process innovation in Italy. Another separate, largely survey-based literature on the financing constraints of small and medium sized enterprises and entrepreneurship has found that such constraints correlate with inferior firm performance (e.g. Beck et al., 2005; Kerr and Nanda, 2009a; Fraser et al., 2015); also see Hall and Lerner (2010) on the financing of R&D and innovation.

However, the latest financial crisis has cast considerable doubt on the unfettered validity of positive effects of debt market development. Recent data efforts by Schularick and Taylor (2012) and Reinhart and Rogoff (2009a) have shown that financial crises across countries and time have been preceded by credit booms (see also Gourinchas and Obstfeld, 2012); a finding that has since been replicated by many other studies. This result is also a consistent finding in the search for early warning indicators of banking crises (e.g. Reinhart and Kaminsky, 1999; Drehmann et al., 2010, 2011; Drehmann and Juselius, 2014). Eichengreen and Mitchener (2003) document the effect of the 1920s credit boom in the context of the Great Depression. Predictively, Lowe and Borio (2002) describe how financial imbalances can build up in an environment of low inflation if credit pushes up asset prices. These findings are consistent with earlier work by Loayza and Ranciere (2006), who find a negative short-run but positive long-run effect of financial development on economic growth.

Additional evidence on what drives short-lived credit booms comes from credit spreads. Mian et al. (2017b) and Krishnamurthy and Muir (2017) show that spreads are relatively low prior to recessions and financial crises accompanied by credit booms, suggesting that credit *supply* rather than demand is the source of these expansions. While these empirical findings may be new, the insight that financial sector disruptions may be an important source of business cycle fluctuations are not: Minsky (1986) and Kindleberger (1978), much cited references in the post-2007 period, argue that the financial system may be inherently unstable and prone to trigger macroeconomic crises endogenously.[2] Indeed, Danielsson et al. (2016) find that unusually low volatility is predictive of financial crises.

But why do financial sectors at times lead to growth in output, and at others to extremely costly crises (Claessens et al., 2009)? How can these apparently contradictory findings be reconciled?

Theory and existing empirical work suggest that the sectoral allocation of credit might be key. In early work, Hume and Sentance (2009) argue that a disproportionate amount of credit extended for the acquisition of existing assets was central to the "growth puzzle" of the 2000s. Beck et al. (2012) find that corporate credit is associated with the well-documented benefits of economic growth and reductions in income inequality, while household credit is not. Similarly, Cecchetti and Kharroubi (2012) and Cecchetti and Kharroubi (2015) argue that credit may crowd out growth if it is allocated to the "wrong" sectors. Chakraborty et al. (2018) find that house price booms lead to a reallocation of credit from firms to households. Mian et al. (2017a) and Di Maggio and Kermani (2017) show that changes to banking regulation can create booms but also subsequent busts, and tend to reallocate resources from the (relatively more productive) tradable to the (relatively more unproductive) nontradable sector (see also Mian and Sufi, 2014a).

The crowding out argument builds on a series of influential papers arguing that credit growth (or financial development) has a non-linear effect (e.g. Arcand et al., 2015; Cecchetti and Kharroubi, 2012, 2015; Gambacorta et al., 2014). In this view, the financial sector does exert a positive influence, but only up to a point, after which it may become detrimanetal to growth. This also meshes with the earlier work of Easterly et al. (2001), who find that higher financial development decreases the volatility of economic growth, but only up to a point.

[2] The phenomenon of excessive lending was already stressed by Wicksell (1898), Von Mises (1912), Hayek (1929), and Fisher (1933).

Credit allocation has also been identified as a factor in the run-up to financial crises. Büyükkarabacak and Valev (2010) differentiate between household and corporate credit, and find that the former plays a larger role for crises. Jordà et al. (2014) and Jordà et al. (2015) use 140 years of data to document that mortgage credit is a significantly better predictor of financial crises than non-mortgage credit. More broadly, Mian et al. (2017b) find that household credit growth predicts recessions in a broad panel of countries and Bahadir and Gumus (2016) show that household credit matters more than firm credit for business cycles in emerging markets.

It seems clear that many of the open questions about macro-financial linkages are hard to address due to a lack of comparable data across countries. While recent efforts by the Bank for International Settlements (Dembiermont et al., 2013), Jordà et al. (2016), and the IMF Global Debt Database (Mbaye et al., 2018) have laid important groundwork, we still lack an understanding of what lies beneath the often-used measure of private credit to GDP. To remedy this limitation, chapter 2 introduces a novel long-run database on sectoral credit for 120 countries, starting in 1910. By substantially extending previous data efforts using more than 600 primary and secondary sources, I am able to document new stylized facts on the evolution of credit in modern banking systems all over the globe. New long-run time series on household lending (including separate series for residential mortgages), and corporate lending by sector, paint a striking picture: household credit has become the dominating business model of credit institutions, while corporate lending has essentially stalled since around 1980 in most of the world.

After addressing what might explain the composition of credit across and within countries over time, chapter 3 turns to a study of the political economy of financial regulation. One potential take-away from the new stylized facts I provide is that regulators should interfere with credit allocation using (macro)prudential tools – an increasingly common practice among central banks (Cerutti et al., 2017a). I show that the use of targeted sectoral tools is subject to a powerful electoral cycle, drawing on the near-universe of democratic elections from 2000 to 2014. Policies that likely directly affect households are less likely to tighten in the quarters preceding elections, particularly when the electoral outcomes are expected to be close.

In chapter 4, I turn to an alternative set of legal parameters: the efficiency of bankruptcy courts. A long literature, starting with the seminal work of La Porta et al. (1997, 1998) has shown that legal frameworks matter for access to finance. While changes to bankruptcy codes and creditor rights are well-studied, surprisingly little is known about the actual implementation of such laws into practice. I thus build on Djankov et al. (2008) who show that measures of debt enforcement are positively correlated with the size of debt markets across countries.

More precisely, I study the effect of court efficiency on financial contracts. Identifying causal effects in this setting is clearly fraught with challenges. First, it is rarely clear what efficiency is and how to measure it. Second, differences in court efficiency across countries is likely correlated with a myriad of other factors, most obviously the design of legal codes themselves. As a result, previous work by Ponticelli and Alencar (2016) and Rodano et al. (2016) find that financial reforms interact with the pre-existing functioning of courts. Third, even within a given country, poorer or more remote areas likely have fewer resources for courts, which makes any outcome inherently endogenous to local conditions.

I attempt to overcome these concerns by using a unique quasi-experiment in the United States: pre-determined geographical exposure to the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). BAPCPA fundamentally reformed US bankruptcy procedures for *consumer* cases, which resulted in the largest drop in bankruptcy filings ever recorded. Business bankruptcies, however, were largely unaffected, which allows me to identify the causal effect of court efficiency on the terms of corporate loan contracts. The US setting also allows me to measure court efficiency in line with what the Judicial Conference of the United States uses: the number of cases per judge, weighted by the typical time different types of cases take to be processed.

The exogenous variation in the drop in caseload per judge following BAPCPA allows me to construct estimates of the aggregate costs of inefficient legal enforcement based on the implied impact on corporate debt service burdens. I find that these costs are larger and much higher than those of hiring additional judges, even in the most conservative approximations. My methodology also helps to uncover the US districts with the highest projected returns to hiring new judges.

Finally, chapter 5 discusses implications of the work laid out in this thesis for future research and policy. I start by discussing preliminary results of work on a detailed assessment of credit booms in the run-up to financial crises and flesh out future directions. Turning to policy, I then discuss the potential costs and benefits of interfering with credit allocation in light of political limitations.

Chapter 2

Credit Markets in the Long-Run, 1910-2014

2.1 Introduction

"Anyone who supposes that financing business is the primary function of banking is mistaken."
– John Kay, *Other People's Money* (2015)

The Great Financial Crisis of 2007-2008 has brought banking and credit back to the forefront of macroeconomic enquiry. By now, a flourishing body of research has produced many new insights into how credit markets work, often drawing on detailed microeconomic data. Yet despite this interest, there is a surprising paucity of long-run historical data on credit markets across countries. In particular, very little is known about how private credit is distributed across industries and households, and whether and how this allocation has changed over time. This makes it difficult to address questions such as: What triggers sudden increases in credit availability? Why do some credit booms end badly, while others do not? Has mortgage lending become the dominant business model of financial institutions?

In this paper, I attempt to close this gap by presenting a large dataset on sectoral credit for 120 countries. I combine data from more than 600 primary and secondary sources – many of which were digitalized for the first time – to offer an in-depth look beyond what constitutes total credit to the private sector. Using previously untapped sources, I construct series on total lending back to 1910, and novel data on credit by sector starting in 1940. The harmonized time series offer a much more detailed, disaggregated view of the business model of modern credit institutions, and lets me trace its trajectory over time.

I make three contributions. *First*, I provide researchers with new harmonized time series on sectoral credit that are consistent with existing aggregate data on private credit. The dataset is the result of an extensive data collection effort, drawing on a wealth of mostly untapped historical publications of statistical agencies, central banks, regulatory authorities, and banking associations around the world. Large parts of the raw data were digitalized from paper or PDF versions, often through library visits. To make them comparable across countries and time, I harmonized the raw series with the generous help of more than 150 individuals working for over 135 national and international organizations, without whom this project would not have been possible. All data sources and adjustments have been documented and cross-validated in detail in an extensive data appendix and spreadsheet collection.

Second, I provide a historical account of changes in the size and composition of credit markets around the world. A simplified standard textbook view of financial intermediaries is that they channel savings from households to firms. Yet, a number of popular accounts have questioned whether this still makes for an accurate description today (Turner, 2015; Foroohar, 2016; Kay, 2015), supported by studies of the United States (Greenwood and Scharfstein, 2013) and a small group of advanced economies (Jordà et al., 2016). But what holds true for the rest of the world?

As a motivating example, consider the loan portfolio of Deutsche Bank – Germany's largest banking entity with around EUR 1.6 trillion in total assets in 2016, amounting to 55% of German GDP. In 1957, around 58% of the company's outstanding loans were extended to industrial companies, a further 27% to retail and wholesale trade, and only 15% to other sectors including households, real estate, and the financial sector. By 2016, these ratios had reversed: only 9% of Deutsche Bank's loans today are to manufacturing and related industries, retail and wholesale trade are a marginal component with 4%, and most of the remaining 87% are exposures to households, real estate firms, and other financial intermediaries. Does this merely reflect changes in a single institution's business model or have banking systems around the world shifted their credit allocation in a similar manner?

The data lends strong support for the latter. Household credit has surged almost uniformly around the world, despite differences in the growth of the financial sector. While it is well known that the ratio of credit to GDP has strongly increased since the 1980s in advanced economies (Schularick and Taylor, 2012), I show that it has remained essentially flat in emerging economies. The share of household credit, however, has increased everywhere: in non-OECD advanced and emerging countries, the average share increased from around 10% in 1960 to around 40% today.

What is perhaps even more striking is the evolution of corporate credit relative to GDP. With the exception of a recent increase in the wake of the 2008-2009 financial crisis, firm lending has stayed essentially flat in advanced economies over the past 35 years. In emerging economies, corporate credit has in fact *decreased* relative to GDP between 1980 and the mid-2000s by some measures. In other words, firms today do not borrow more from domestic institutions than in 1980. This, of course, is not a necessity from the boom in household lending: credit to firms and households appeared to grow more or less in tandem until a strong decoupling in the early 1980s.

Importantly, there are substantial differences in what accounts for this growth in household debt. In advanced economies, residential mortgages account for a relatively stable share of around 60-70% in total household credit, which has seen a slight increase in the run-up and aftermath of the financial crisis. This is consistent with the findings of Jordà et al. (2015), who show that mortgages make up an ever-increasing share of total lending in 17 advanced economies. It also meshes with the finding of Greenwood and Scharfstein (2013) who show that mortgage debt has grown dramatically in the United States from 1980 to 2007. I show that the picture differs for emerging economies: consumer credit, credit cards, and car loans make up a larger and *increasing* fraction of household lending, weighing in at around 55% today. Taken together, the data suggest that a simple textbook narrative based on banks taking deposits from households and lending to firms may be an incomplete characterization of the business of modern financial institutions.

Heterogeneity also matters for corporate lending. While the total amount of firm lending appears to have changed little over the past three decades, its composition certainly has. All over the globe, agriculture and manufacturing make up an ever smaller share of corporate financing, with emerging economies experiencing the largest declines. Apart from the tertiary sector, it appears that construction and real estate has become an increasingly dominant force: even in the reconstruction period following World War II, lending to real estate developers and construction companies only accounted for around 6% of corporate lending in advanced economies. Today, it accounts for more than 20%. But even in developing countries, there has been a noticeable increase in the portfolio share of construction loans, particularly compared to the period 1950-1970.

This range of new stylized facts prompts the question what might explain them. My third contribution is to study the factors shaping the perhaps most profound shift in global private debt portfolios: the rise of household credit. Identifying what *causes* changes in credit allocation, particularly over long stretches of time, is a daunting challenge and beyond the scope of this paper. However, I can test a number of candidate theories using simple correlations, which – while far from conclusive – will hopefully guide more rigorous future work.

I begin by establishing that much of the variation in the household share in total lending, around 60%, can be accounted for by time-invariant country factors. The addition of global factors affecting all countries simultaneously increases the R^2 by another 20% or so; time-varying regional factors do not appear to provide information over and above global factors. This means that, to understand the allocation of credit, we have to understand country-specific aspects and trends spanning the globe.

Next, I consider whether the development of alternative financing sources for firms can explain their decreasing role in domestic lending. I find that they cannot. Cross-border lending, for example, has in fact decreased in emerging markets relative to domestic credit. It also makes up only a tiny fraction of total debt all over the globe, except in tax havens and for non-bank financial institutions in advanced economies. Corporate bonds market have increased in importance, but only since the mid-1990s. And even after adjusting the ratio of corporate credit to GDP for cross-border lending, bonds, and trade credit, it is still similar today compared to the early 1980s. As it turns out, these and other measures of alternative financing sources – such as the ratio of trade, leasing, or foreign direct investment to GDP – are uncorrelated with the household credit share both across and within countries.

Building on the large literature in law and finance following La Porta et al. (1997, 1998), I consider whether legal frameworks play a role in credit composition across countries. Indeed, it appears that (at least across countries), legal origins, insolvency frameworks, and debt enforcement have some explanatory power for the use of household debt. Measures of income, savings, and demographic factors, however, show much stronger correlations. While it is intuitive that richer countries use more household credit (Cerutti et al., 2017b; Badev et al., 2014), I also find a role for the distribution of income. The link between inequality and debt is ex-ante unclear: Beck et al. (2007), for example, find that financial development disproportionally benefits the poor across countries; and related evidence exists for the staggered lifting of US branching restrictions (Beck et al., 2010). An alternative hypothesis is that increases in inequality may be one of the drivers of household debt booms prior to crises (Kumhof et al., 2015; Rajan, 2010; Mian and Sufi, 2011). In the data, I find that higher inequality is associated with a lower household credit share across countries. Conditioning on a country's GDP per capita, however, reverses the correlation; given that the intuition of inequality-fueled credit booms is based on advanced economy experiences, this may not be entirely surprising. Demographic factors have even higher explanatory power. The share of the population living in urban areas and that aged between 30 and 49 – likely the main group taking out mortgages – is associated with household credit within and across countries.

The variables with the highest explanatory power, however, are related to financial deregulation and information sharing institutions. Popular accounts such as Kay (2015), Foroohar (2016), and Turner (2015) have made the argument that changes to banking regulation have led to a "crowding out" of corporate by household credit.

This is also related to empirical work by Mian et al. (2017a) and Di Maggio and Kermani (2017), who show the reallocative effects of boom-bust cycles following deregulation. Chakraborty et al. (2018) also provide some evidence consistent with a crowding out of corporate lending during housing booms. In the data, I find that deregulation has consistently high explanatory power for the household credit share both across and within countries. I also find that household debt is more widely used in countries with higher usage of information sharing institutions, especially private credit bureaus. That is, better information sharing among lenders is not only associated with a higher ratio of credit to GDP (Djankov et al., 2007), but also a shift away from firm to household lending.

My work extends previous efforts by Schularick and Taylor (2012), Jordà et al. (2015), and Jordà et al. (2016) who introduce long-run credit data for 17 advanced economies starting in 1870. It also builds on institutional efforts at the Bank for International Settlements (Dembiermont et al., 2013), World Bank (Cihák et al., 2013), and the International Monetary Fund in providing such data. The database I present differs from their work and other sources by providing considerably more granular estimates of who received credit around the world. To illustrate, the data allow us to compare the composition of manufacturing lending in Austria and Pakistan in the 1960s or trace the trajectory of household credit growth in Switzerland and Peru from the 1940s. Put differently, while existing work was able to shed some light on the *size* of credit markets (e.g. Djankov et al., 2007), my contribution is to study the *allocation* of credit, which was previously impossible due to the lack of cross-country data.

I expect the data to find wide applications for studying the effects of macroeconomic policies on the financial sector, which has largely relied on measures such as value added and sectoral characteristics to proxy for changes in credit allocation (e.g. Rajan and Zingales, 1998; Wurgler, 2000). They might also be helpful in testing models with financial sectors in which sectoral heterogeneity matters (e.g. Schneider and Tornell, 2004; Matsuyama, 2007, 2013; Rancière and Tornell, 2016; Schmitt-Grohé and Uribe, 2016; Bahadir and Gumus, 2016). At the outset, it is worth stressing that compiling time series from at times more than a dozen individual sources per country introduces a margin of error. To be as transparent as possible, the data appendix accompanying this paper lists all sources and adjustment in great detail and compares my data with existing sources. Still, the data have the potential to be improved and expanded as new sources become available.

The paper proceeds as follows. In section 2.2 I introduce the new dataset on sectoral credit in detail and discuss some aspects of its construction and coverage. In section 2.3 I document a range of new stylized facts about credit markets around the world.

Section 2.4 explores determinants of the share of household credit across and within countries over time. Section 2.6 concludes.

2.2 A New Global Dataset on Sectoral Credit

I assemble a novel dataset on credit markets for 120 countries from over 600 individual country sources. The main contribution compared to existing work is that I construct long-run disaggregated data by households, non-bank financial intermediaries, and non-financial corporations, which in turn are broken down by up to 115 individual industries. In addition, I collect data on household credit by purpose, where I can differentiate in many countries between residential mortgages, consumer credit, credit cards, and car loans. I also add data on commercial (i.e. non-residential) mortgages.

To be included, I require countries to have credit data for at least two corporate sub-sectors (e.g. Agriculture and Manufacturing) since 2005. I make four exceptions for countries that do not fulfill this criterion: China, the Netherlands, Luxembourg, Sweden, and the United States.[1] To guarantee that the detailed data are comparable across countries and with existing data sources, I also collect new data on total private credit from national sources. In some cases, these are complemented with existing data from the Bank for International Settlements, the IMF's International Financial Statistics (including old paper versions), the United Nations Statistical Yearbook, statistical publications of the League of Nations, and the advanced economy time series of Jordà et al. (2016).

Given its disaggregated nature, I document the collection and harmonization of the credit data in detail in an extensive data appendix and spreadsheet collection. These data will be made publicly available to researchers for free. The data appendix also acknowledges the diligent and tireless support I received from statisticians and bank supervisors in most of the countries in the sample, without whom this project would not have been possible. The appendix can be downloaded here.

2.2.1 Data Sources and Concepts

Sectoral credit data have been collected and published in most countries for multiple decades, but not on a harmonized basis. As a result, I draw on hundreds of scattered primary and secondary sources to construct these time series.

[1]The Netherlands publish detailed corporate lending data, but I have not been able to construct reliable long-run time series.

To begin, I retrieved data from statistical publications and data appendices published by national central banks and statistical offices. In many cases, I use publications from different organizations even for the same country; much of these are not available online. Large shares of the data were digitalized for the first time and copied by hand, either from PDF or paper documents. Many of the national authorities also shared previously unpublished, non-public data with me via email or mail. In the data appendix, I show a few examples of what the underlying data look like.

A major challenge in working with sectoral credit data is to make them comparable across countries and time to account for changes in the classification of sectors, lending institutions, and debt instruments. For harmonization purposes, I consulted historical meta data in institutional publications and liased closely with all of the national authorities publishing information on sectoral credit via email. The raw data were adjusted for inconsistencies hampering cross-country comparisons and breaks in the time series arising from changes in classifications that are unrelated to fundamentals (such as large scale debt write-offs). The data appendix reports all harmonization procedures and adjustments in the Excel part of the data appendix on the individual time series level.

All data are end-of-period outstanding amounts in national currency. The coverage comprises the broadest set of lending institutions for which data are available; where possible, I include non-bank financial institutions. "Credit" is defined to include all debt contracts (loans or debt securities) denominated in local or foreign currency. In practice, the statistical coverage usually follows the structure of the financial system; countries with high market shares of non-bank lenders usually also report statistics on these, and the same also holds for debt securities.[2] Because a typical country reports time series on loans extended by all monetary financial institutions (MFIs), this makes my data closer to existing sources reporting *bank credit* (such as the World Bank Global Financial Development Database) rather than *total debt* (such as the IMF Global Debt Database). I discuss comparison with other sources below and in the data appendix.

Household credit comprises all lending to households and non-profit organisations serving households, as in Dembiermont et al. (2013). In most countries, sole proprietorships are not singled out in household credit statistics, so they are not counted as corporate credit to ensure the data remain comparable.[3] I include as non-bank finan-

[2]Note that the issue of lender and debt instrument coverage is typical for data on financial institutions and not a particular feature of the sectoral credit aggregates assembled here. In the few countries where more comprehensive data was available but not included, e.g. Denmark, the rationale is outlined in detail in the data appendix.

[3]This creates some differences with existing data by Jordà et al. (2016), who at times appear to

cial corporations all financial institutions who do not fund themselves with deposits (i.e. non-MFIs); many countries further single out statistics on insurance companies and pension funds.

I also construct time series on "total corporate credit" which equals the sum of non-bank financial and non-financial corporations. This creates a slight difference to the data published by the Bank for International Settlements, for example, who treat total credit as the sum of household and non-financial corporate loans; however, lending to financial institutions makes up a non-negligible part in some countries, which is why I single it out. I exclude credit to national or local governments.[4] Since the overwhelming majority of sources does not differentiate business lending by public or private ownership of the borrower, data on corporate credit in most cases also includes state-owned enterprises; similarly, lending by both private and government banks is included, which includes development banks in some cases.

2.2.2 Coverage and Comparison with Existing Sources

Table 2.1 compares existing datasets with my contribution. I extend previous academic and institutional efforts by the Bank for BIS, World Bank, the IMF, and the Jordà-Schularick-Taylor Macrohistory Database (Jordà et al., 2016). The newly compiled data are an extension along four dimensions: sectors, countries, time, and frequency. First, I collect novel disaggregated data on corporate credit, following the United Nations International Standard Industrial Classification (ISIC Rev. 4). Depending on their availability, the raw data include between 3 and 115 sub-sectors, with an average (median) of 20 (16) sectors per country. 52 countries report data on manufacturing sub-industries at some point. To maximize data availability, I restrict these to four broad sectors in this paper: agriculture, industry (manufacturing and mining), construction and real estate, and others.

The data lend a new level of detail to the analysis of debt markets, as the aggregate credit to non-financial corporations (NFC) includes industries that may differ strongly

count sole proprietorships as corporate credit. Different disaggregation regimes by legal organisation or economic activity further mean that in some countries sectors such as agriculture are largely counted as households. In these cases, I adjusted corporate and household data in consultation with the national authorities to ensure comparability across countries. See the data appendix for more details.

[4] A considerable number of countries reports time series on lending to "public administration and defence; including compulsory social security" (ISIC Rev. 4 section O) as part of disaggregated credit to non-financial corporations. Where it was available, I created additional sub-totals to exclude it explicitly, but in practice the category only makes up for a tiny fraction of the credit market in all sample countries.

in their characteristics. For example, data on NFC credit from existing sources include lending to construction and real estate companies. In many cases, the data reported as "non-financial corporations" also appears to include lending to non-bank financial intermediaries. To my knowledge, I am the first to collect and document systematically data on the latter.

Table 2.1: Comparison with Existing Data Sources on Private Credit

Dataset	Freq.	Countries	Start	Level of disaggregation
WB GFDD	Y	203	1960	–
BIS	Q	40	1940	NFC, Households
IMF FAS	Y	152	2004	Households, SMEs (limited)
IMF GDD	Y	190	1950	NFC, Households
Schularick and Taylor (2012)	Y	14	1870	–
Jordà et al. (2016)	Y	17	1870	Corporate, Households, Mortgages
Müller (2018)	*M/Q/Y*	*120*	*1910/ 1940*	*NFC by industry* *Households:* *Mortgages* *Consumer credit* *Credit cards* *Car loans* *Total mortgage credit* *Non-bank financial institutions*

Notes: The data on total credit in Müller (2018) starts in 1910 and the sectoral data in 1940. WB GFDD stands for the World Bank's Global Financial Development Database (Cihák et al., 2013). BIS refers to the credit to the non-financial sector statistics described in Dembiermont et al. (2013). The IMF FAS and GDD refer to the International Monetary Fund's Financial Access Survey and Global Debt Database. NFC refers to non-financial corporations.

Second, the full database spans across 120 countries. While the country coverage for countries with sectoral data is lower than the near-comprehensive World Bank and IMF GDD data, it considerably expands the coverage of credit to households and non-financial corporates in Dembiermont et al. (2013), which comprises 44 countries in their dataset. One major contribution is thus the collection of long-run series on household debt for many countries. The coverage spans across all major world economies but also includes many small open economies; even at its lowest point in 1940, I have sectoral data for around 50% of world GDP, and almost 90% today.

Third, many countries report data starting in the 1960s, a significant fraction even from after World War II. The time dimension usually goes significantly beyond what has been available before despite the much higher level of detail (see the appendix for more detail). I report sectoral credit data from 1940 to have a meaningful sample size. I also construct new long-run total credit time series for a substantial number of countries going back to 1910.

Fourth, I collect data in higher frequency than previous efforts, in many cases monthly. This increases the size of the full dataset, which contains more than 1.5 million observations. I restrict the data to year-end values for the largely descriptive analysis in this paper.

Figure 2.1: COMPARISON OF PRIVATE CREDIT/GDP WITH EXISTING DATA SOURCES

(a) Comparison with World Bank/IMF **(b) Comparison with BIS**

Note: This graph plots the ratio of total credit to the private sector from the new database against the ratios from the World Bank, IMF Financial Access Survey, and the BIS for overlapping samples. See the online appendix for more details and validation exercises.

My data are consistent with existing sources. Figure 2.1a and 2.1b compares the newly compiled total credit measures with data from the World Bank's Global Financial Development Database (Cihák et al., 2013), the International Monetary Fund's Financial Access Survey, and the BIS data on bank and total credit (Dembiermont et al., 2013) for the respective overlapping samples. In the data appendix, I also compare the data to those recently published in the IMF Global Debt Database and those compiled by Jordà et al. (2016). Reassuringly, the total credit data I constructed are comparable and follow highly similar trends over time for all sources. A natural interpretation of my data is thus that it represents the underlying sectoral structure of total credit to the private sector others have collected, plus an extension of these total credit series. I present much more comprehensive evidence on the comparability and differences of my data with existing sources in the data appendix.

2.2.3 Additional Data

I create a long-run data set on macroeconomic data by combining existing data from the World Bank, Penn World Tables, IMF International Financial Statistics, United Nations, Barro and Ursua (2008), the Maddison Project Database (Inklaar et al., 2018), Jordà et al. (2016), Dincecco and Prado (2013), and national sources. I discuss the construction and sources of these variables in the online appendix. I also add data on financial sector characteristics from a wide range of sources, most prominently the World Bank Global Financial Development Database and the Doing Business Project. The exact sources of these variables are also outlined in the online appendix.

2.3 Loan Portfolios in 120 Countries: Long-Run Evidence

How is credit allocated across different sectors of the economy around the world, and how has this changed over time? In this section, I provide some evidence on the evolution of household and firm lending in 120 economies. I show that household credit has, on average, more than tripled between the early 1950s and 2014. The aggregate trends, however, hide important differences across country groups and sectors. While the mortgage share in household credit has been approximately stable in advanced economies, it is much lower and has in fact *decreased* in the rest of the world; consumer credit, not housing is key to understanding the emerging market household leveraging. I also highlight structural change in corporate financing, which has seen a fundamental shift away from agriculture and manufacturing to construction and real estate and the tertiary sector.

2.3.1 Aggregate Trends

It is instructive to begin with a look at the development of total private credit to GDP around the world, an important indicator of the depth of financial sector activity. The novelty of my data for this exercise is the extension of long-run credit series to the period before 1960. I have data on total lending for 51 countries starting before 1940; 61 countries starting before 1950; and 78 countries before 1960. Figure 2.2 plots the arithmetic mean of the credit to GDP ratio for three country groups: OECD economies, other advanced economies, and emerging economies.[5] While the swift uptick

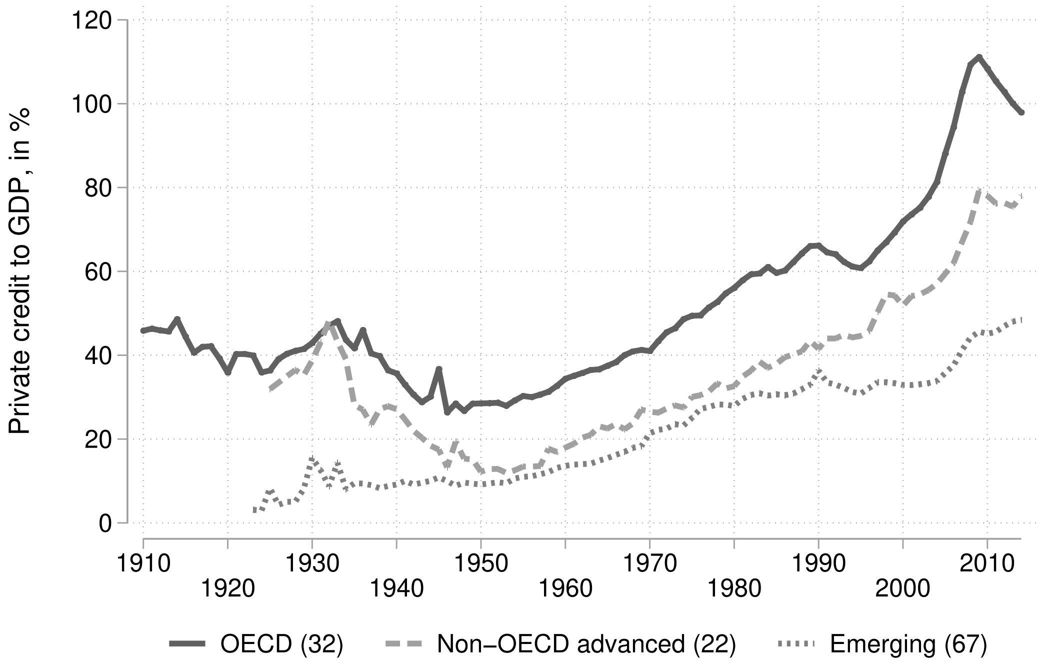
[5] I use the World Bank classification; "emerging" refers to middle and low income countries. All of these are defined as of 2014. A list of the countries in each group can be found in the appendix.

in economy-wide leverage in recent decades has been well-documented for a small group of advanced economies (see e.g. Schularick and Taylor, 2012), it is arguably much less appreciated that a similar "financialization" has *not* taken place to the same extent in emerging economies. While OECD economies in the late 2000s reached an average of more than 100% of private credit to GDP, the figure hovers around 45% for emerging economies.

Next, the newly collected data allows a first glimpse at what sectoral credit allocation look like across countries, and how it has evolved over time. Figure 2.3 visualizes the transformation of credit markets over the 70 year time span in the sample by plotting arithmetic means of sectoral credit to GDP across countries. The compositional changes over time are remarkable. Financial institutions in the mid-1970s used to predominantly lend to non-financial corporations. Starting in the mid-1980s, with the onset of considerable financial deepening, household credit has become increasingly prominent. In fact, the overwhelming bulk of credit growth relative to economic activity has been driven by households, especially since 1990; the ratio of non-financial corporate credit stayed essentially flat between the early 1980s and the mid-2000s. A major trend-break here is the Great Financial Crisis 2007-2008. In the run-up to the crisis, household debt worldwide increased but only picked up in the non-financial corporate sector in 2006 or 2007. As I will show later, this recent growth in corporate lending was primarily driven by lending to the construction and real estate industries. Importantly, the global deleveraging following the crisis was concentrated in corporate lending (both financial and non-financial).

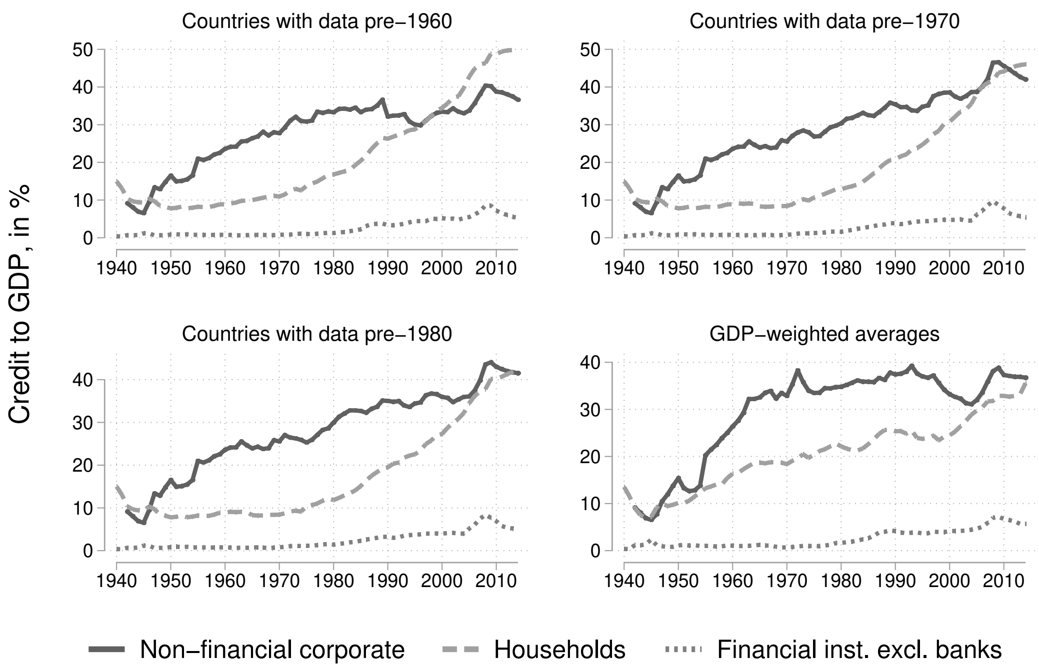
While credit to non-bank financial institutions has also increased, the developments here are surprisingly muted. An important caveat in the case of inter-financial lending, however, is the definition of what constitutes "credit". While the data in many countries include all types of credit contracts – including repos and other often-used instruments of capital market lending – this may lead to important omissions because financial institutions are the main issuers of corporate bonds in many countries (see e.g. Gilchrist and Mojon, 2014). As I will show below, the surprisingly stable ratio of credit to financial institutions may also partially reflect that they are disproportionately more likely to use cross-border credit compared to non-financial corporations, particularly in advanced economies. Taken at face value, the data nevertheless suggest that the financing of (non-bank) financial institutions in fact is not a major part of the domestic lending business of credit institutions around the world, which was one of the central messages in Kay (2015).

Figure 2.2: PRIVATE CREDIT TO GDP (IN %), 1910-2014



Note: Figure 2.2 shows the arithmetic average of total credit to GDP, broken down by country group. Country classification according to the World Bank.

Figure 2.3: GLOBAL SECTORAL CREDIT (IN % OF GDP), 1940-2014

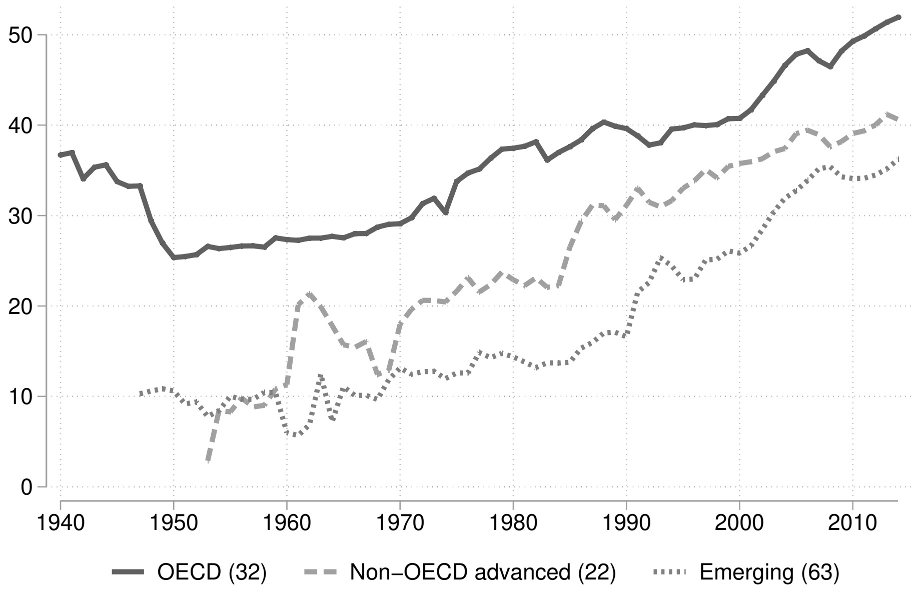


Note: Figure 2.3 plots the average ratio of sectoral credit to non-financial corporations, financial corporations (excl. banks) and households (incl. non-profits and sole proprietorships) to GDP.

2.3.2 The Rise of Household Credit

In the previous section, I have documented that credit markets have expanded mainly in relatively advanced economies and that household loans have been a major driver all over the globe. I next turn to the question what accounts for the increase in household credit by differentiating between country groups and loan purposes.

Figure 2.4: SHARE OF HOUSEHOLD IN TOTAL CREDIT (IN %), BY COUNTRY GROUP



Note: Figure 2.4 shows the yearly average share of household credit in total credit, broken down by country group. OECD and non-OECD advanced economies are the high income countries as classified by the World Bank, and emerging economies all others.

To begin, figure 2.4 plots the arithmetic mean ratio of household in total credit by country groups according to their levels of economic development. The resulting picture is striking. Household credit has taken up an ever increasing share in the loan portfolio of banks all over the world.

While the OECD economies started from a much higher level, household lending has seen a constant increase since the end of World War II, with a noticeable boom in the mid-2000s. The picture here resembles the share of mortgage credit assembled in Jordà et al. (2015), which shows a similar post-WWII drop and significant recovery over time. The data suggest that this pattern also holds true for a broader set of 32 OECD countries.

I establish that household loans have also boomed across the rest of the globe. To my knowledge, I am the first to document this stylized fact. Starting from a much lower base, the share of non-business credit has increased dramatically, from around 10% of total loans up to the late 1960s to around 40% today in both non-OECD advanced and emerging economies. This represents nothing short of a transformation of the lending activity of banking systems.

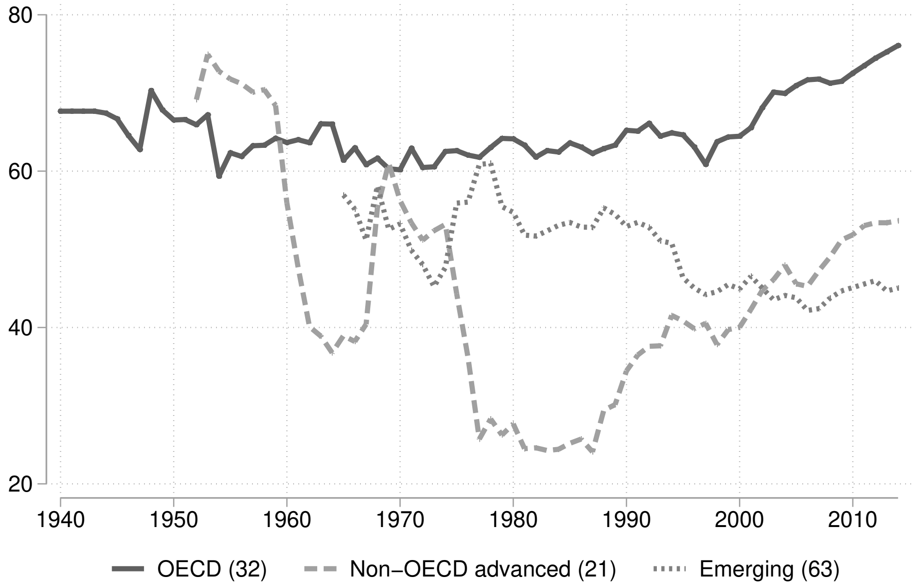
Household lending has become a major part of banks' business model around the world. But what accounts for this staggering rise? In influential work, Jordà et al. (2015) show the importance of mortgage credit for understanding financialization in the major OECD economies. However, the lack of comparable cross-country data has prevented an understanding of what drives household leverage in other countries. In figure 2.5, I thus break down household lending into its mortgage and non-mortgage component. Non-mortgage credit here largely represents consumer credit; technically, it also includes other categories such as loans to households for the purchase of financial instruments or student loans.[6] The cross-country difference are again stark. In OECD economies, residential mortgages have made up a relatively constant fraction of around 60-70% of household loans between 1950 and today. Maybe unsurprisingly, the housing boom of the 2000s can also be seen in the shares of debt taken on by households: the average ratio increased from around 60% in 2000 to 75% today.

The picture looks fundamentally different in non-OECD advanced and emerging economies. In the former, the residential mortgage share has been on a constant increase between 1980 and today, but is still below that of OECD countries at just over 55% in 2014. While I discuss more robustness for these findings below, the data for non-OECD advanced economies is somewhat noisy due to the relatively small number of countries, most of which do not report long-run data. In developing countries, the mortgage share has in fact *decreased* over the past decades and today hovers at just over an average of 40%. Taken together with the evidence in figure 2.4, this implies that consumer credit, not mortgages are at the heart of the emerging market credit boom. In support of this, I show in the online appendix that the share of residential mortgages to GDP has remained almost constant between around 1960 and 2014, while it has doubled in OECD and quadrupled in non-OECD advanced economies. The "great mortgaging" appears to be an advanced economy phenomenon (Jordà et al., 2015).

Given its importance in emerging countries, it is interesting to look at the composition of consumer credit, which is also possible using my data. Unfortunately, these

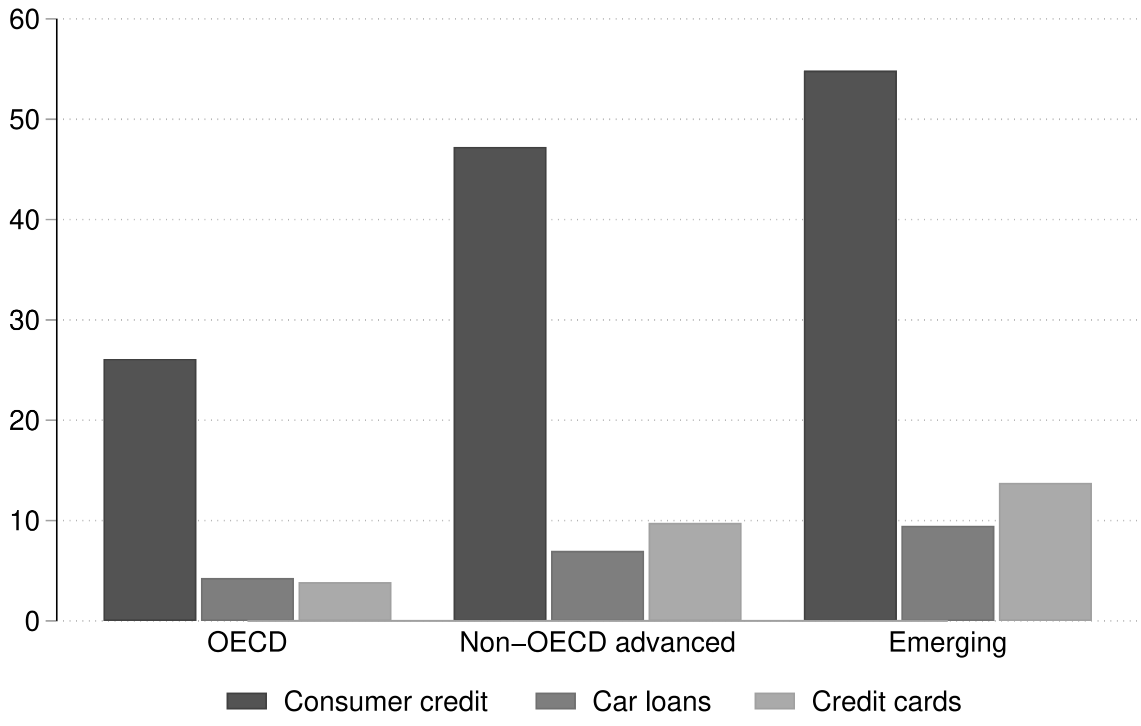
[6] As I discuss in the data appendix, the classification of consumer credit across countries does not follow consistent patterns. As a result, some countries treat all non-mortgage lending to households as "consumer credit", while others only include the financing of durable goods. Using the share of non-mortgages is thus a reasonable approximation of consumer lending.

Figure 2.5: Mortgage Share in Household Credit (in %), by Country Group



Note: Figure 2.5 plots the average ratio of residential mortgages in household credit, broken down by country group.

Figure 2.6: Share of Consumer in Household Credit, in %



Note: Figure 2.6 plots the 2009-2014 average ratio of different consumer credit types in total household credit, broken down by country group.

time series are available for a much smaller number of countries and (except in a few cases) only for a shorter time span. In figure 2.6, I present average ratios of different types of consumer credit in total household lending for the period 2008-2014 only. I calculate these values from 99 countries for total non-mortgage ("consumer") credit and 46 (26) countries for credit cards and car loans, respectively.[7] The pattern confirms the time series trend regarding residential mortgages: the share of consumer credit declines with a country's level of development. Again, the quantities here matter, given the importance of household lending around the world. More than 15% of household credit in emerging economies is accounted for by credit cards alone, and another 10% by car loans. These ratios are much lower for OECD economies.[8]

The patterns presented in this section have important implications. Both theoretical and empirical studies of household finance have, in many cases, focused on mortgage credit (e.g. Badev et al., 2014; Cerutti et al., 2017b). These papers, however, tell us very little about why consumer credit has expanded rapidly in emerging economies. Also, a prominent mechanism for amplification effects between asset prices and credit is that land values boost the liquidation values of housing assets, which in turn increases debt capacity (see e.g. Chaney et al., 2012; Liu et al., 2013). These explanations are, however, silent on the role of unsecured credit. In section 2.4, I explore a few potential explanations for the rise of household credit.

2.3.3 Structural Change in Corporate Credit

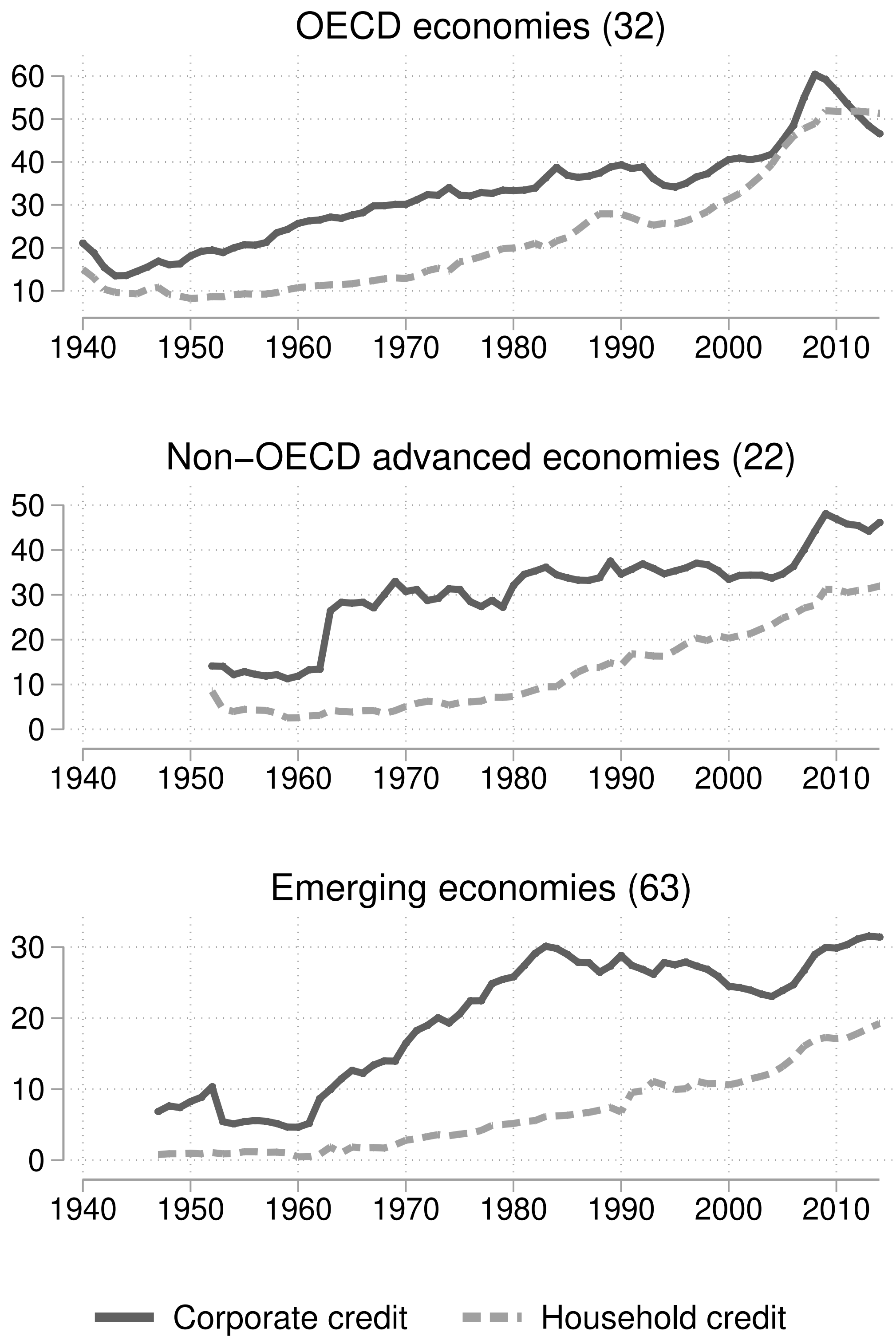
Next, I turn to developments in the corporate credit market. It is a well-known phenomenon that countries undergo economic structural change with increases in development, mainly away from primary sectors towards manufacturing and then service sectors. As such, one would expect to find similar trends in corporate credit. On the other hand, the findings for residential mortgages above suggest an increasing role of the housing sector, at least in advanced economies. Can we detect complementary patterns in the composition of corporate financing?

Figure 2.8 plots the share for four sectors in total corporate lending: agriculture, industry (consisting of manufacturing and mining), construction (which also includes real estate developers), and all other sectors. Again, I differentiate between emerging and advanced (OECD and non-OECD) economies. Consistent with structural change, the share of lending to agriculture and industry has declined consistently since 1950.

[7] The pattern looks almost equivalent if I restrict the sample to countries for which I have either credit card or car loan information or both.

[8] The numbers for car loans in OECD economies should be interpreted with caution because I only have data for the US, Sweden, and Canada.

Figure 2.7: CORPORATE AND HOUSEHOLD CREDIT (IN % OF GDP)



Note: Figure 2.7 shows the yearly average ratio of corporate and household credit to GDP, broken down by country group.

What is striking, however, is just how similar this trend is between country groups. The financing of industry, for example, has not "migrated" from advanced to emerging economies; rather, the decline appears to be relatively uniform, which is somewhat surprising, given the relocation of many manufacturers to developing countries.

The second major trend is that construction and real estate lending have come to make up considerable shares of the aggregate corporate loan portfolio. In OECD economies, which likely faced the highest demand for financing reconstruction after World War II, in fact had a negligible share of construction credit (around 6%) in the 1950s. Today, this share has risen to more than 20%. While the housing boom of the 2000s has clearly played a role, the share had already grown in the 1990s. The trends are almost equivalent in non-OECD advanced economies. Strikingly, a similar pattern also holds true in developing countries. In the 1950s, lending to industry and agriculture accounted for more than 70% of corporate financing. Today, the ratio is closer to 25%. At the same time, construction and real estate has increased from around 5% to almost 20%. The loan portfolio of emerging markets has thus seen a profound shift.

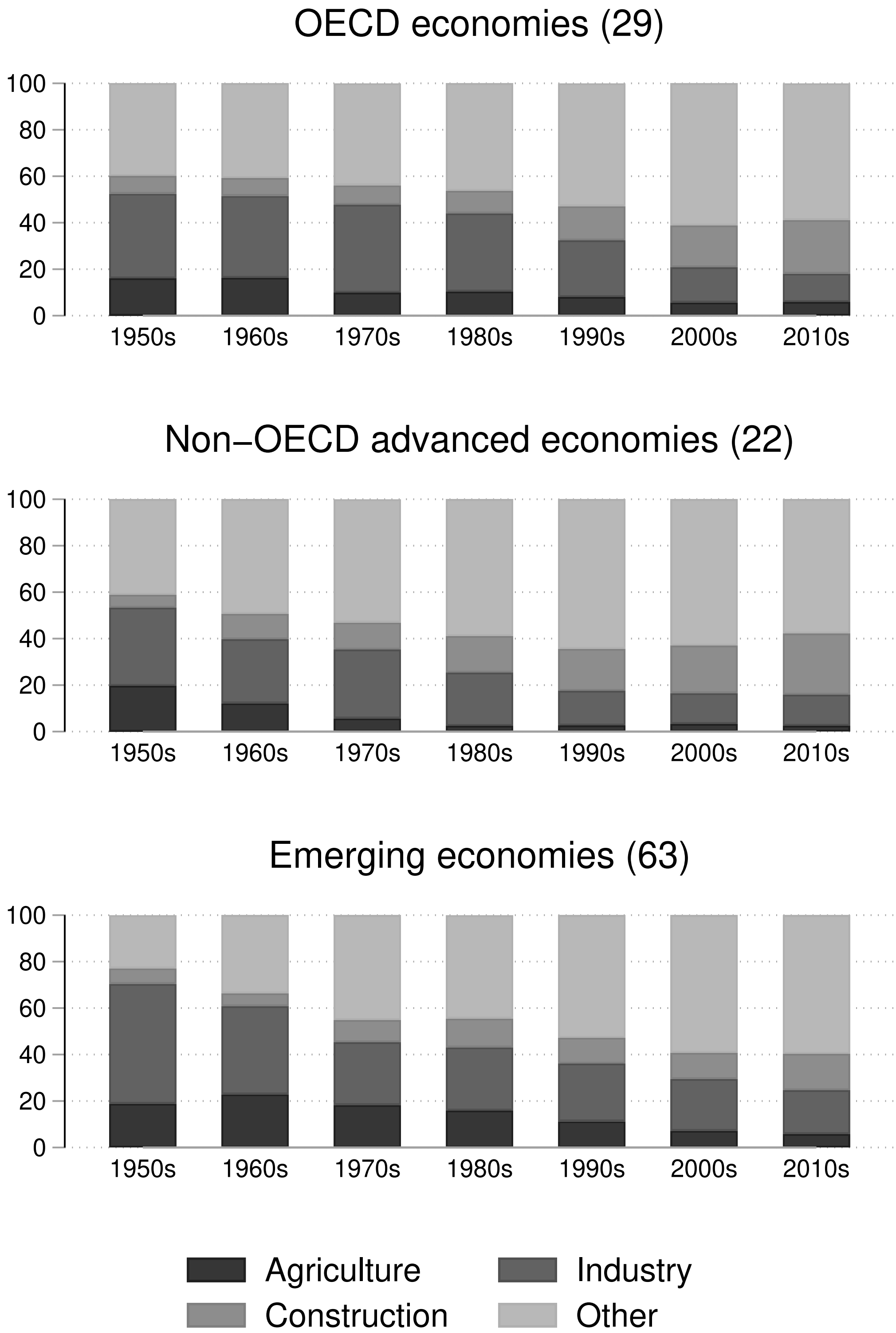
What about other types of lending? Almost all over the globe, the tertiary sector has increased its lending share by a substantial margin. In advanced economies, its share has increased from around 40% in the 1950s to around 60% in recent years. Emerging economies have seen an increase from around 20% to 60% over the same time period. Taken together, these findings suggest that the portfolio composition of Deutsche Bank mentioned in the intro appears to be the rule rather than the exception: the financing of manufacturing and mining industries, the activity perhaps most commonly associated with banking, has come to play only a miniscule role in understanding credit markets.

2.3.4 Robustness Tests

In the online appendix, I challenge the stylized facts I document above to a number of sensitivity analyses. In particular, I present additional figures based on more balanced samples and also allow for weighted averages based on a country's GDP. The latter is to identify whether the shifts I document are driven by the relatively large number of small open economies in my sample, which contribute little to world GDP.

It is worth highlighting a few cases where these adjustments make a difference to the results. First, the long-run evolution of credit to GDP by country groups is influenced by weighting countries by their share in world GDP. China plays a pivotal role here: because it makes up a large fraction of world GDP, and has recently experienced a sustained credit boom, its inclusion in fact pushes the average credit-to-GDP ratio

Figure 2.8: CORPORATE CREDIT COMPOSITION (IN %), 1950-2014



Notes: These figures plot the average composition of corporate credit over time. All values are in percent of total corporate credit. *Industry* refers to the manufacturing and mining sectors, *Construction* includes real estate services where available, and *Other* are all other sectors.

above that of advanced economies. Without China, however, the difference between the financialization of advanced economies and the rest of the world becomes, in fact, considerably larger when weighting by GDP (see figure 1 in the online appendix).

Using balanced samples for countries with data before 1960, 1970, or 1980, as well as GDP-weighting, also creates an even stronger relative increase in the ratio of global household debt to GDP (see figure 2 in the online appendix). For countries with data before 1960, household credit now clocks in at over 50% of GDP alone, followed by non-financial corporate lending at 40%. The other results presented up until now are qualitatively unchanged when allowing for different adjustments and presnted in the online appendix.

2.4 What Explains Higher Shares of Household Credit?

Up to this point in the paper, I have documented how credit markets have evolved in the long-run all over the globe. One of the most striking stylized facts I uncover is the rise of household credit, particularly in emerging economies. But what explains whether countries have more or less household compared to firm credit? In this section, I consider a few candidate theories to gain intuition, and find that some of them are strongly rejected by the data; others, however, are consistent with a number of simple correlations. While far from conclusive, this first set of results will hopefully motivate and guide more rigorous empirical analysis in future work.

2.4.1 Country-specific, regional, or global factors?

Before diving into specific drivers, it is instructive to briefly assess how much of the variation in the household debt share can be explained by time-invariant country factors, as compared to time-varying global or regional factors. These can be neatly summarized by the (adjusted) R^2 of regressing the household credit share on dummies for countries, regions, years, or combinations thereof.

The results in table 2.3 suggest that a large fraction of the variation in household debt is due to country-specific factors that do not change over time ($\approx$ 59%), but adding year fixed effects helps to raise this fraction to $\approx$ 81%. Region-specific time dummies, while informative by themselves, do not add anything above global time factors once time-invariant country factors are taken into account. This suggests that the bulk of what explains the use of household credit is either due to individual country factors that do not change over time or caused by global changes that affect countries more or less similarly over time.

Table 2.3: Explaining the Share of Household Credit - Fixed Effects R^2

Dependent variable: Share of household credit in total credit						
	Region FE	Country FE	Year FE	Region × Year FE	Country + Year FE	Country + Region × Year FE
Adj. R^2	15%	59%	11%	29%	81%	82%

Notes: This table presents the adjusted R^2 for regressions of the share of household in total credit on fixed effects as indicated. The regions correspond to the World Bank classification of East Asia and Pacific, Europe and Central Asia, Latin America and the Caribbean, Middle East and North Africa, North America, South Asia, and Sub-Saharan Africa.

2.4.2 Alternative Sources of External Financing

A straightforward explanation for the relative boom in household credit, particularly in emerging economies, could be that firms have shifted to alternative sources of external financing. Recall that, due to data constraints, my data only cover *domestic* credit and are often – but not always – limited to bank loans. However, it is well known that corporate bond markets and cross-border lending have expanded rapidly in some countries, and globalization has additionally fostered the use of trade credit. Can these factors explain the relatively slow increase visible in the corporate credit data shown here?

The data suggest that alternative sources of external financing cannot explain the rise of household credit. Let us begin by looking at total cross-border loans to the non-bank sector, as reported in the BIS locational banking statistics, for which a long time series is available. Panel A of figure 2.9 plots the arithmetic mean of the ratio of such cross-border loans to total domestic credit from my database over time, by country groups.[9] While emerging economies have seen the most obvious lack of growth in corporate credit over GDP, the share of cross-border lending has in fact *decreased* in these countries since the mid-1980s. In the online appendix, I show that this is remarkably robust with regard to sample composition, the choice of scaling variable, and different methods of collapsing the data. For the years 2013 and 2014, I can also differentiate between cross-border lending to non-financial corporations and financial institutions (excluding banks). The data in panel B of figure 2.9 suggest that only financial institutions in advanced economies use a relatively high share of cross-border loans. Taken together, this makes it quantitatively implausible that corporations in

[9] I exclude a number of tax havens with extraordinarily high ratios of cross-border to domestic loans. Note that I scale over total credit because there is also cross-border lending to households, but this does not drive the results (see below).

emerging economies have substituted domestic for foreign bank loans.

What about the development of bond markets? Figure 2.10 plots the arithmetic mean of the ratio of bonds to non-financial corporations to total corporate credit over time, again split up by development status. Indeed, the data suggest that bond markets have grown in importance, but only since the late-1990s – considerably later than the stalling of corporate (bank) credit. Again, I show in the online appendix that this finding is, again, highly robust. At an average share of 10-15% of domestic loans, however, the volume of outstanding bonds in emerging markets appears too low to account for the shift to household credit.

To get a better understanding of the relative importance of alternative sources of external financing, figure 2.11 plots domestic corporate credit and adds long-run series on outstanding bonds to non-financial corporates, the value of export credits from Berne Union, as well as the recent data on cross-border loans to non-financial corporates.[10] The data show that, even though alternative sources have become more important, they cannot account for the stalling of corporate lending: the sum of domestic loans, bonds, export credits, and cross-border loans today is still below that in the mid-1980s. Weighing these ratios by a country's GDP leads to a slightly larger share of other sources of external financing, but their fraction nevertheless remains small.

Regression evidence also suggests that household credit did not rise disproportionately in countries where alternative financing sources have become more common. Panel A in table 2.4 plots the estimated coefficients of regressing the household debt share on different measures of outside financing and year fixed effects (or year and country fixed effects). This is equivalent to asking whether these sources are correlated with a shift away from domestic corporate lending across and within countries. From bonds and cross-border loans to measures of trade credit, leasing, and FDI, none of the other financing sources are significantly correlated with the increase in household lending, and the (within) R^2 is close to zero in all specifications. I conclude that domestic corporate credit has not been substituted by other lenders.

2.4.3 Law and Finance

An influential body of work, starting with La Porta et al. (1997, 1998), argues that legal systems are crucial to understanding financial sector outcomes. For the case of credit markets, La Porta et al. (1997) show that countries with French or Nordic

[10] I restrict the sample to all country-years for which I have data on domestic lending *and* bonds to avoid changes in composition.

legal origin appear to have lower ratios of debt to GDP. Could it be that legal origins also affect the *composition* of credit? I put this to the test in column (1) of panel B by regressing the share of household credit in a country on legal origin dummies.[11] Note that, because legal origins are time-invariant, both regressions only include year fixed effects. Indeed, I find that French origin is associated with a lower household credit share, and Nordic origin with a higher one; the R^2 in these regressions, however, explains little of the 59% variation due to time-invariant country factors.

One important correlate of legal origin are differences in creditor rights, which Djankov et al. (2007), Qian and Strahan (2007), Haselmann et al. (2010), and Bae and Goyal (2009) find to matter for credit markets across countries. Building on their findings, I regress the household credit share on the creditor rights index from Djankov et al. (2007), which turns out to be statistically insignificant with and without country fixed effects. Using a measure of the strength of insolvency frameworks from the World Bank Doing Business Survey for 2014, however, is positively associated with household lending. The Legal Rights index from the same survey – which measures how conducive collateral and bankruptcy laws are for lending – is also positive and statistically significant. This meshes well with previous cross-sectional evidence in Warnock and Warnock (2008) and Cerutti et al. (2017b), who show correlations of legal rights with the ratio of mortgage debt *to GDP*, but do not show whether this implies higher overall shares of household lending. The index, however, does not have predictive ability for within-country variation in household lending.

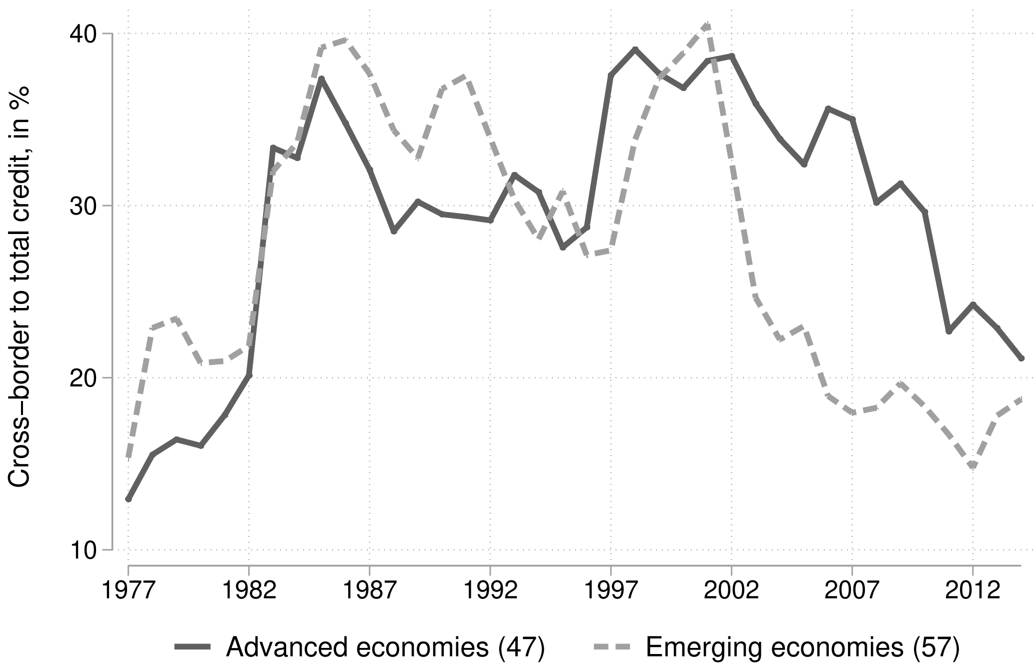
I also consider two indicators of debt *enforcement* (Djankov et al., 2008), also based on World Bank Doing Business data. I find that both higher costs of court claims as well as recovery rates for creditors are associated with household lending. The latter yields a relatively high R^2 of almost 17% and is also informative about the share of household debt *within* countries.[12]

[11]For brevity, I only plot the bivariate correlations for French and Nordic origin, because these are the only ones that are statistically significant in a multivariate regression that also include a dummy for German origin (with Common Law serving as comparison group).

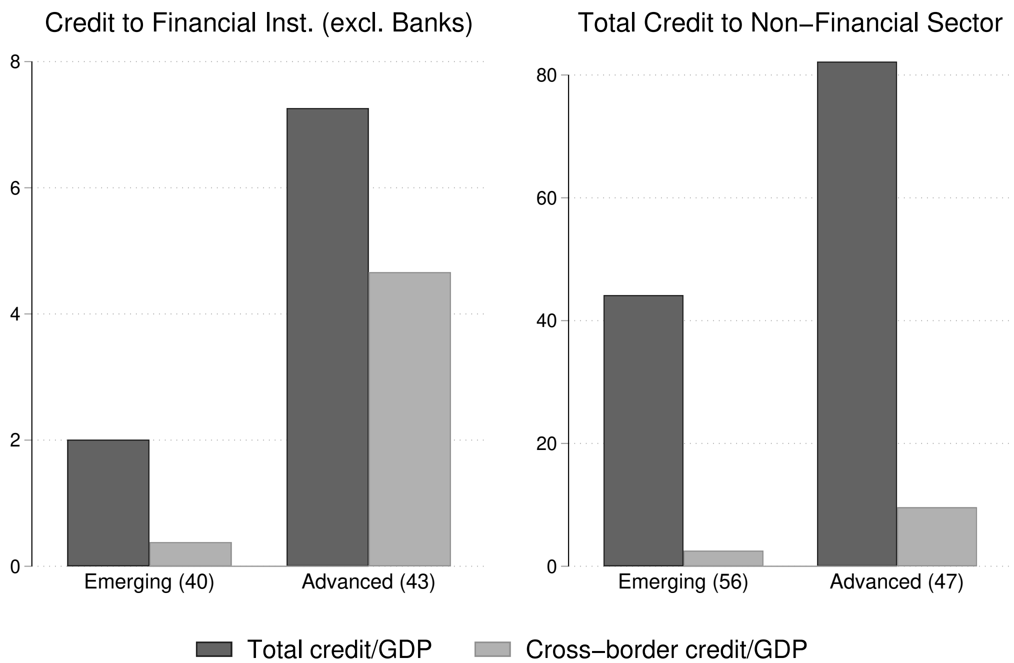
[12]The purely cross-sectional indicator of debt enforcement from Djankov et al. (2008) is also statistically significant when restricting the data to the post-2000 period (unreported).

Figure 2.9: COMPARING DOMESTIC AND CROSS-BORDER CREDIT

Panel A: Ratio of Cross-Border to Domestic Lending (in %)

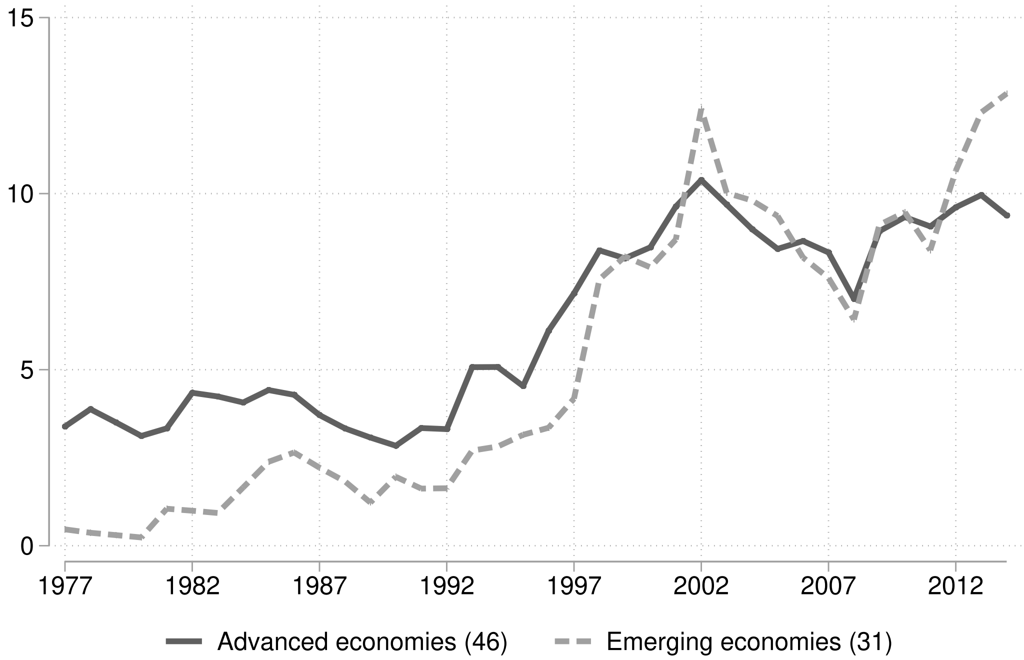


Panel B: Financial vs. Non-Financial Sector (2013-2014, in % of GDP)



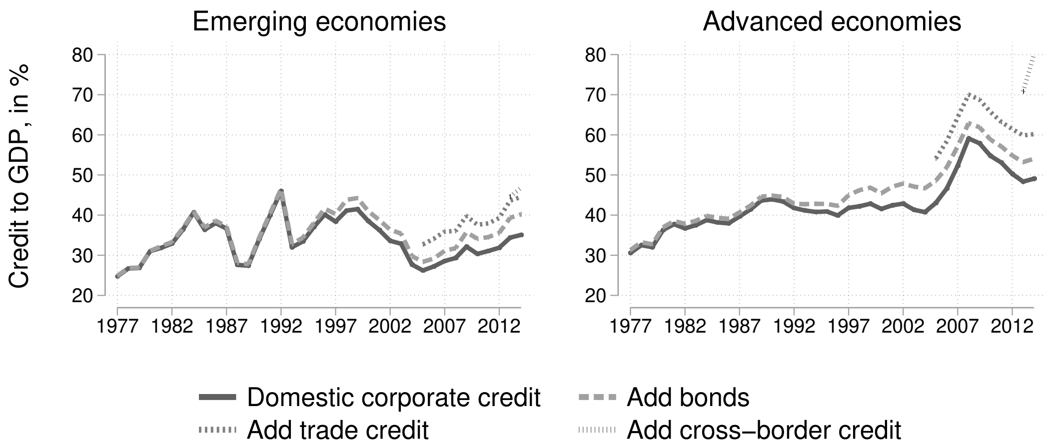
Note: Panel A plots the ratio of cross-border lending to the non-bank sector to total domestic credit to the private sector, broken down by country group. Panel B plots the average share of cross-border and domestic credit to GDP for 2013-2014, where I differentiate between lending to the financial sector (excl. banks) and all other sectors (i.e. households and non-financial corporations). I exclude tax havens (see text and online appendix).

Figure 2.10: SHARE OF BONDS IN CORPORATE CREDIT, IN %



Note: Figure 2.10 plots the ratio of outstanding bonds of non-financial corporations to total corporate credit, broken down by country group. I exclude tax havens (see text and online appendix).

Figure 2.11: COMPARING SOURCES OF EXTERNAL FINANCING, IN % OF GDP



Note: Figure 2.11 plots all sources of external financing as a fraction of GDP for countries that report data on domestic corporate credit and non-financial corporate bonds. I exclude tax havens (see online appendix).

Table 2.4: EXPLAINING THE SHARE OF HOUSEHOLD CREDIT – PANEL REGRESSIONS

Dependent variable: Share of household credit in total credit												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Country FE		Yes		Yes		Yes		Yes		Yes		Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel A: Alternative sources of external financing												
	Ouststanding bonds/GDP		Cross-border loans/GDP		Export credits/GDP		Trade/GDP		Leasing/GDP		Inward FDI/GDP	
Predictor	0.039	-0.033	0.019	-0.004	0.038	0.014	-0.009	0.014	0.428	-0.935	0.012	0.019
	(0.063)	(0.039)	(0.015)	(0.012)	(0.068)	(0.039)	(0.016)	(0.014)	(1.113)	(0.945)	(0.020)	(0.016)
Observations	1,804	1,798	3,054	3,054	1,102	1,102	4,027	4,027	710	702	3,203	3,203
Countries	76	70	110	110	112	112	115	115	66	58	112	112
Adj. R^2	0.029	0.829	0.079	0.811	0.000	0.938	0.111	0.811	0.049	0.861	0.078	0.810
Adj. R^2 (within)	0.000	0.000	0.004	0.000	0.000	0.000	0.000	0.001	0.000	0.011	0.000	0.000
Panel B: Legal frameworks and enforcement												
	Legal origin† French	Legal origin† Nordic	Creditor rights		Insolvency framework		Legal rights		Cost of court claims		Recovery rates	
Predictor	-8.032***	18.689***	0.570	-1.225	0.973**	–	1.326***	0.074	-0.173*	0.028	0.237***	0.142***
	(2.763)	(5.584)	(1.780)	(1.868)	(0.448)		(0.485)	(0.272)	(0.089)	(0.039)	(0.051)	(0.051)
Observations	4,094	4,094	1,375	1,373	109		1,071	1,071	1,161	1,161	1,161	1,161
Countries	116	116	86	84	109		111	111	111	111	111	111
Adj. R^2	0.158	0.184	0.000	0.884	0.053		0.041	0.940	0.052	0.932	0.168	0.934
Adj. R^2 (within)	0.054	0.083	0.000	0.001	0.053		0.042	0.000	0.051	0.001	0.167	0.023

(Continued on next page)

Table 2.4: Explaining the Share of Household Credit – Panel Regressions (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Country FE		Yes		Yes		Yes		Yes		Yes		Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Demographics, income, and savings												
	GDP per capita (log)		Top 10 income share		Consumption/ GDP		Savings/ GDP		Urban pop. share		Population aged 30-49	
Predictor	10.510***	6.478***	-0.404**	-0.001	-33.552***	-9.675*	0.267***	-0.016	0.281***	0.267**	134.758***	50.296**
	(1.261)	(2.336)	(0.200)	(0.164)	(7.955)	(5.544)	(0.084)	(0.053)	(0.070)	(0.112)	(29.273)	(24.545)
Observations	3,933	3,933	1,510	1,507	3,815	3,815	3,351	3,351	3,695	3,695	3,909	3,909
Countries	113	113	102	99	111	111	111	111	113	113	112	112
Adj. R^2	0.398	0.812	0.119	0.793	0.166	0.814	0.121	0.821	0.233	0.820	0.214	0.816
Adj. R^2 (within)	0.331	0.030	0.035	0.000	0.069	0.006	0.036	0.000	0.141	0.017	0.118	0.014
Panel D: Financial regulation and credit information sharing												
	Financial deregulation		Credit market freedom		Private bureau exists		Private bureau coverage		Public registry exists		Public registry coverage	
Predictor	55.213***	4.542	4.829***	1.291***	15.252***	4.415***	0.167***	0.029*	-1.888	4.019*	0.109*	0.006
	(6.839)	(4.845)	(0.671)	(0.387)	(2.012)	(1.459)	(0.033)	(0.017)	(3.316)	(2.121)	(0.056)	(0.027)
Observations	1,595	1,594	1,641	1,639	3,458	3,458	1,180	1,180	3,458	3,458	1,180	1,180
Countries	75	74	105	103	113	113	111	111	113	113	111	111
Adj. R^2	0.337	0.861	0.246	0.834	0.237	0.822	0.167	0.931	0.086	0.820	0.015	0.931
Adj. R^2 (within)	0.329	0.003	0.182	0.032	0.167	0.027	0.166	0.007	0.002	0.016	0.014	0.000

Notes: This table presents the results from individual regressions of the share of household in total credit on a predictor. † indicates regressions with time-invariant predictors, for which I can only include year fixed effects. Heteroskedasticity-robust standard errors in parentheses are clustered by country. See the online appendix for variable definitions and summary statistics. ***, **, and * indicate statistical significance at the 1, 5, and 10% level, respectively.

2.4.4 Demographic Factors, Income, and Inequality

The perhaps most obvious correlation one would expect with the share of household credit is a country's income level, as measured by GDP per capita. Cerutti et al. (2017b) and Badev et al. (2014) , for example, find some evidence that the depth of mortgage markets is higher in richer countries. In my data, I find that this measure alone explains around 33% of the variation in the household *share* in total lending in panel C, and remains highly correlated even within a given country.

In principle, household credit might also depend on how a country's income is distributed between its citizens. Beck et al. (2007), for example, find that financial development disproportionally benefits the poor across countries, and Beck et al. (2010) find similar evidence for the staggered state-level lifting of US branching restrictions. Across countries, I also find that more unequal incomes are associated with a *lower* share of household debt. This contrasts with the argument that one driver of the expansion in household credit in the run-up to the Great Depression and Great Recession may have been an increase in inequality (Kumhof et al., 2015; Mian and Sufi, 2011), also stressed by Rajan (2010). Figure 2.12 shows that a country's income level is an important mediator: once I include the log of GDP per capita as a control variable, a higher income share of the top 10% has a *positive* correlation with household credit.[13] Given that the intuition in the latter group of papers is that households use debt to make up for a decline in real incomes in advanced economies, this may not be completely surprising.

Because income inequality is linked to saving-consumption decisions, the same pattern also holds true for the ratio of consumption to GDP and savings to GDP. Jappelli and Pagano (1994), for example, argue that better access to household credit may decrease the savings rate. In a large cross-section of 111 countries, I indeed find a negative correlation, but only after conditioning on GDP per capita; without that control a higher savings rate is associated with *more* household debt. Because savings and consumption are negatively correlated, I find similar results for consumption to GDP (with the opposite sign).

It is also intuitive that demographic factors should play a key role in shaping the use of household credit across countries. This is probably most obvious for financing housing, for which demographic variables are key drivers (e.g. Mankiw and Weil, 1989; Takats, 2012). In the newly collected data, I also find that the household credit share is highly correlated with the share of the population between 30 and 49 – likely

[13] Note that including the log of GDP per capita does not change the results of the other regressions presented here qualitatively.

the main group to take out a mortgage in most countries. Consistent with Badev et al. (2014), urbanization and household debt also appear to go hand-in-hand, both across and within countries. This is likely driven by supply as well as demand factors, e.g. because cities have higher house prices and smaller households, or because creditor rights are easier to enforce where the collateral is located closely to a bank's branches.

2.4.5 Financial Deregulation and Information Sharing

Financial deregulation is perhaps the single most prominent factor featured in popular accounts of changes to the nature of financial intermediation (e.g. Kay, 2015). Easing restrictions on lending, the story goes, have enabled financial institutions to extend ever more household credit – at the expense of businesses serving the "real" economy (Foroohar, 2016; Turner, 2015).

There is some empirical evidence to support this narrative. Mian et al. (2017a) show that US banking deregulation in the 1980s was associated with a boom-bust cycle in household debt and house prices. Relatedly, Favara and Imbs (2015) show that interstate branching deregulation increased mortgage credit and house prices and Di Maggio and Kermani (2017) find similar effects using federal preemptions to local laws against predatory lending. These findings are also consistent with recent models that allow for a relaxation of borrowing constraints to households (e.g. Bahadir and Gumus, 2016; Justiniano et al., 2015), one source of which may be changes to regulation. Chakraborty et al. (2018) argue that, in the United States, mortgage lending crowds out corporate lending during housing booms.

Can changes to regulatory frameworks also help to explain the share of household credit across countries? I take this hypothesis to the data in columns (1) through (4) in panel D, using the financial reform index from Abiad et al. (2010) and the Fraser Institute's measure of credit market freedom as indicators of deregulation. The estimated coefficients are positive and highly statistically significant in 3 out of 4 cases.[14] The regressions also yield the highest R^2s I find outside of the results for GDP per capita, but they do not seem to be driven by differences in income levels: figure 2.12 shows that they are largely unaffected when controlling for the log of GDP per capita. This suggests that the short and medium-term deregulation effects identified in previous

[14]Note that the correlation with the deregulation index from Abiad et al. (2010) is absorbed by the year fixed effects, but is statistically significant at the 1% level with country fixed effects only. This is likely because deregulation occurs in waves, and including time dummies may thus be "overcontrolling" (Mian et al., 2017b), i.e. wash away some of the "true" effect of deregulation. Indeed, a regression of the deregulation index on year fixed effects yields an adjusted R^2 of around 48% in my sample.

work are also reflected in long-term correlations within and across up to 105 countries.

Another critical institutional input for household lending are information sharing institutions (Pagano and Jappelli, 1993; Frame et al., 2001; Petersen and Rajan, 2002). In developing countries in particular, private credit bureaus and public credit registries have been associated with more lending relative to GDP (Djankov et al., 2007), both to firms (Jappelli and Pagano, 2002; Brown et al., 2009) and households (Warnock and Warnock, 2008; Jappelli et al., 2008). However, it is unclear whether better information sharing should lead to a higher *share* of household credit. I put this question to the test in columns (5) to (12), where I differentiate between private bureaus and public registries, as well as between the existence of such institutions (measured by a dummy variable) and their coverage (as % of adults, only available from 2004). The data strongly suggest that household credit is more prominent where private credit bureaus are present across but also within countries. The evidence for public registries is considerably weaker, as in previous papers, which may reflect heterogeneity across country groups (also see Djankov et al., 2007). As in the case of financial deregulation, these findings are unaffected by controlling for GDP per capita (unreported).

2.4.6 Discussion

To summarize, the data reveal a few striking patterns. First, alternative sources of firm financing do not explain higher shares of household credit. Second, legal frameworks are not only associated with the *size*, but also *composition* of debt markets, at least across countries. Third, demographic factors such as the level and distribution of income, as well as aging and urbanization, are highly correlated with household lending. Fourth, financial deregulation and information sharing institutions have the highest explanatory power for the share of household in total private debt, outside of GDP per capita. These correlations should not be taken as definitive answers, but rather as a guide for future work, which should attempt to establish causal relationships using more rigorous empirical designs.

2.5 Implications

After uncovering a new set of stylized facts about credit markets around the globe, it is instructive to consider how this evidence squares with existing theories of financial development and macro-financial linkages. In his classic survey of the financial development literature, Levine (2005) summarizes the functions of financial systems as (1) producing information ex ante about possible investments and allocate capital; (2)

monitoring investments and exert corporate governance after providing finance; (3) facilitating the trading, diversification, and management of risk; (4) mobilizing and pooling savings; and (5) easing the exchange of goods and services. "Financial development", then, is defined as an improvement in how well financial systems perform these functions. It seems like a fair characterization that most of the literature on the role of credit in financial development has at least implicitly focused on these functions in the context of *corporate* financing. For example, Levine (1997) uses the parable of Fred, a hypothetical truck manufacturing entrepreneur, to illustrate how finance affects economic growth. Two of the key insights of his excellent summary are that "production requires capital" and "Fred will require outside funding if he has insufficient savings to initiate his truck project."

A more efficient allocation of resources between firms and industries in particular has also been at the core of many empirical studies (e.g. Rajan and Zingales, 1998; Wurgler, 2000; Bertrand et al., 2007). These papers, however, tell us very little about the massive increase in household credit around the world I have documented here. In fact, the samples studied in many papers, often limited to manufacturing, account for an ever decreasing share of banks' loan portfolio. To pick up Levine's parable, Fred's ability to take out a mortgage or a car loan appears to have become an increasingly important part of what banks do, rather than the provision of working capital. Of course, this does *not* imply that firms necessarily face credit constraints as a result. But it does suggest that many papers neatly identify effects on the *intensive* margin, which have to be interpreted alongside the *extensive* margin of the actual credit allocation across sectors.

Studies of the amplifying effects of leverage on business cycles (starting with the classic papers Kiyotaki and Moore, 1997; Bernanke et al., 1999) have largely focused on interactions between *firm* balance sheets and the financial sector (see e.g. Caballero and Krishnamurthy, 2003; Brunnermeier and Sannikov, 2014). The evidence I present here suggests that models with a focus on *household* balance sheets may be increasingly informative, given the prominence of household lending (see e.g. Justiniano et al., 2015; Greenwald, 2016; Schmitt-Grohé and Uribe, 2016; Martin and Philippon, 2017; Mian et al., 2017b). The striking compositional shifts in corporate credit are also worth highlighting here. In advanced economies, approximately 20% of corporate credit today is extended to construction and real estate companies, and another 20% to financial institutions. This suggests that modeling heterogeneity in sectoral characteristics may be a fruitful endeavour going forward.

A key question raised by my study is which factors are responsible for the boom in household debt all over the world, and for mix of household debt into mortgages and non-mortgages. While I provide some first evidence, this can only been seen as suggestive and as a guidance for future work. It is also unclear whether the stalling of corporate credit, particularly in emerging economies, reflects supply or demand factors. A first striking indication is that external financing sources do not seem to have offset the stalling of corporate credit, despite the fact that GDP and household credit have grown substantially in developing countries; normally, not situations one would characterize as typical for a lack of credit demand. It thus seems plausible that credit supply might at least be partially responsible, which raises the question whether household credit has "crowded out" firm lending – as argued by, among others, Turner (2015), Kay (2015), and Foroohar (2016).

The dataset I present also opens a wealth of opportunities for future research. For one, it remains largely unclear why some credit booms end in crises and others are associated with productivity miracles. Analyzing whether booms can be differentiated depending on the sectors in which they occur is particularly pressing for policy makers. The data may also prove to be a useful laboratory for understanding why different types of credit supply shifts often suddenly, which often results in a boom-bust pattern (Mian and Sufi, 2018). One particular interesting case are episodes of financial liberalization, the effects of which remain somewhat contested. On one hand, evidence on the deregulatory process in the United States has overwhelmingly found beneficial effects (see Kroszner and Strahan (2014) for an excellent survey). On the other hand, there is also some evidence that liberalization is often followed by crises with substantial output costs (e.g. Demirgüç-Kunt and Detragiache, 1998). Going forward, it would be worth investigating whether changes in credit composition can explain these divergent experiences.

2.6 Conclusion

The Great Financial Crisis 2007-2008 has prompted a call to arms to study macro-financial linkages, in particular the role of credit in the macroeconomy. Despite this research interest, the paucity of comparable cross-country data has forced researchers to work either with broad macroeconomic aggregates or draw on usually confidential data from credit registries or proprietary sources. At the same time, analyzing the nature of financial crises and the effects of financial reforms has been hampered by a lack of detailed credit data that cover a country's entire banking system. I present a large novel dataset on sectoral credit for 120 countries to remedy these problems.

The data show that aggregate measures of private credit conceal considerable changes in the *composition* of lending over time and across countries. Indeed, these shifts are nothing short of a transformation of what financial institutions do, both in advanced and emerging economies. I provide a few tentative answers about what can and cannot explain these findings. While only a first step, I show that the stalling of corporate credit is unlikely a result of the development of corporate bond markets, cross-border lending, or other alternative sources of external financing. Rather, higher shares of household debt are associated with demographic factors, the income distribution, financial deregulation, and the development of information sharing institutions. I also find some evidence that legal frameworks play a role. These insights will hopefully guide more empirical work going forward.

The trends I uncover here also present us with major new questions: Why do households in advanced economies use mortgages so much more frequently than those in emerging economies? Has household credit crowded out corporate credit? Do composition shifts in corporate credit reflect industrial change, or are other forces at work? Which types of credit are most important for understanding the relationship between financial development, financial crises, and economic growth? I expect that the data unveiled here will give impetus to new research into a better understanding of credit markets and their interlinkages with macroeconomic dynamics.

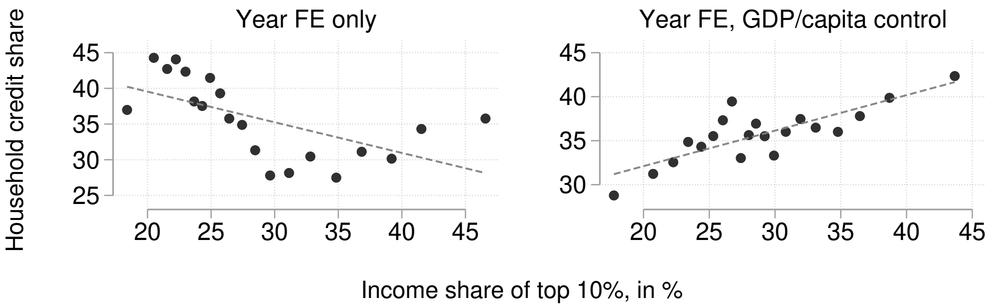
Appendix

Table 2.5: COUNTRY GROUPS, WORLD BANK CLASSIFICATION (2014)

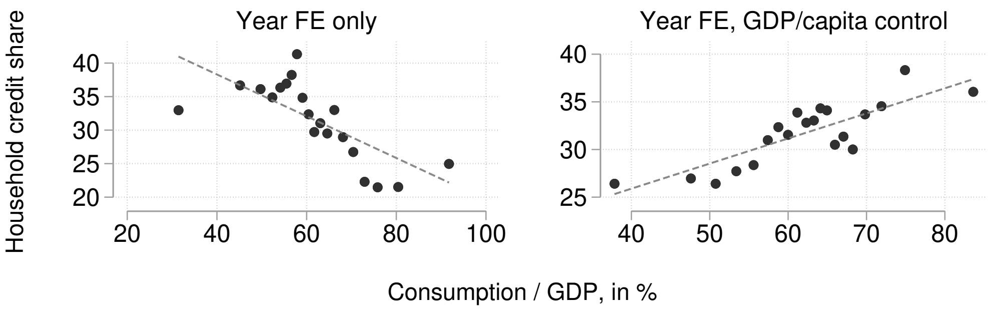
OECD	Non-OECD advanced	Emerging	
Australia	Antigua & Barbuda	Albania	Kenya
Austria	Argentina	Anguilla	Kyrgyz Republic
Belgium	Bahrain	Armenia	Lesotho
Canada	Barbados	Azerbaijan	Macedonia
Chile	Cyprus	Bangladesh	Malawi
Czech Republic	Hong Kong	Belize	Malaysia
Denmark	Kuwait	Bhutan	Maldives
Estonia	Latvia	Bolivia	Mauritius
Finland	Lithuania	Botswana	Mexico
France	Malta	Bulgaria	Mongolia
Germany	Oman	Cambodia	Montserrat
Greece	Qatar	China	Morocco
Hungary	Russian Federation	Colombia	Nepal
Iceland	Saudi Arabia	Costa Rica	Nicaragua
Ireland	Seychelles	Curacao & Sint Maarten	Nigeria
Israel	Singapore	Dominica	Pakistan
Italy	St. Kitts & Nevis	Dominican Republic	Panama
Japan	Taiwan	Egypt	Peru
Luxembourg	Trinidad & Tobago	El Salvador	Philippines
Netherlands	United Arab Emirates	Ethiopia	Romania
New Zealand	Uruguay	Fiji	Sierra Leone
Norway	Venezuela	Georgia	South Africa
Poland		Ghana	Sri Lanka
Portugal		Grenada	St. Lucia
Slovak Republic		Guatemala	St. Vincent
Slovenia		Guyana	Suriname
South Korea		Haiti	Tanzania
Spain		Honduras	Thailand
Sweden		India	Tunisia
Switzerland		Indonesia	Turkey
United Kingdom		Iran	Uganda
United States		Jamaica	Ukraine
		Jordan	Zimbabwe
		Kazakhstan	

Figure 2.12: EXPLAINING THE HOUSEHOLD CREDIT SHARE: INCOME CONTROL

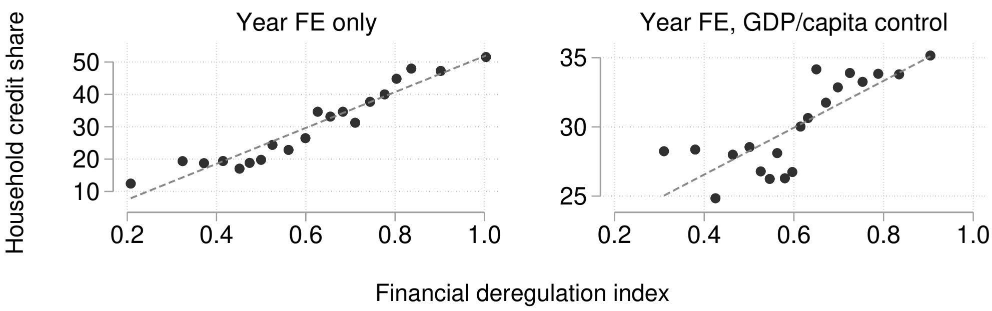
Panel A: Income inequality



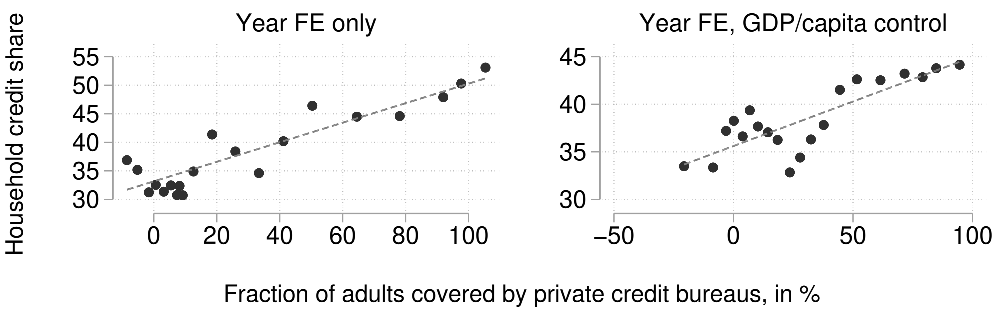
Panel B: Consumption / GDP



Panel C: Financial deregulation



Panel D: Information sharing



Notes: These figures show binned scatter plots of the household credit share and the variables listed in the panel headers. All values were adjusted for year fixed effects; the plots on the right further include the natural logarithm of GDP per capita in constant USD from the Maddison Project as control variable.

Chapter 3

Electoral Cycles in Prudential Regulation

3.1 Introduction

In the wake of the Great Financial Crisis 2008-2009, many countries have made sweeping changes to financial regulation (see, e.g., Duffie, 2017; Yellen, 2017). Because banking crises tend to follow periods of high growth in credit (Kindleberger and Aliber, 2005; Gourinchas and Obstfeld, 2012; Schularick and Taylor, 2012), there has been widespread agreement in policy and academic circles that prudential regulation is necessary to ease the negative externalities of financial sector disruption and "curb the credit cycle" (IMF, 2017; Aikman et al., 2015). A growing empirical literature has established that macroprudential tools in particular can be effective in stabilizing growth in credit and house prices (see e.g. BIS, 2017).[1] As a result, prudential policies have become widely used: out of 119 countries surveyed in Cerutti et al. (2015), 107 regulators reported using at least one macroprudential tool in 2013, compared to 76 in 2000.

A potential issue for implementing prudential regulation is that policies which restrict access to credit may have immediate effects on the median voter, and thus come with political costs (Dagher, 2017). As a result, politicians may have considerable incentives to intervene – formally or informally – with regulatory decision making. Examples of suspicious coincidences abound: In Germany, for example, the parliament blocked the introduction of income-based limits for borrowers (debt-to-income and debt-service-to-income ratios) just eight weeks before the 2017 elections.[2] In many countries, governments have direct representatives on the committees responsible for macroprudential decisions (Edge and Liang, 2017). This might be a binding constraint: In June 2017, for example, the Belgian finance ministry blocked a proposal by the

[1] Among many other country case studies, Jiménez et al. (2017), Aiyar et al. (2016), Aiyar et al. (2014), Gambacorta and Murcia (2017), and Epure et al. (2017) show that prudential policies affect loan and firm-level outcomes. Altunbas et al. (2018) show that macroprudential policy affects bank risk. Ayyagari et al. (2017) find some evidence for effects of macroprudential tools on firm outcomes across countries. Macroeconomic evidence includes IMF (2011), Kuttner and Shim (2016), Akinci and Olmstead-Rumsey (2015), and Cerutti et al. (2015).

[2] See the Annual Report of the German Council of Economic Experts.

National Bank of Belgium to increase the risk weights on mortgages with high loan-to-value ratios to address an increase in systemic risk in the mortgage market (NBB, 2017). Drawing on a plethora of case studies, Dagher (2017) argues that regulatory changes in the run-up to banking crises seem to be the rule rather than the exception.

The risk of political intervention is at times acknowledged in policy circles (e.g. Haldane, 2017). As Horvath and Wagner (2016) put it: "Pressure from the financial industry and politicians will make it difficult for regulators to impose additional capital when excesses start to materialise." This contrasts with a lack of empirical evidence. In this paper, I attempt to fill this void by testing for the existence of electoral cycles in the use of prudential regulation in the run-up to 217 elections in 58 countries between 2000 to 2014.

I find that countries are much less likely to tighten prudential tools, and somewhat more likely to loosen them, in the quarters prior to an election. This finding turns out to be a strikingly clear and robust feature of the data. Consistent with government clout over policy decisions, the election cycle is strongest for the tightening of sector-specific capital buffers that target mortgage and consumer credit – arguably the policies which most directly affect the median voter. Crucially from a policy perspective, such targeted tools have also been found to be the most effective in curbing credit and house price growth (e.g. Akinci and Olmstead-Rumsey, 2015).

I also find strong evidence that policy makers are less tough on financial sector risks in "good times", when memories of previous crises may seem more distant (Reinhart and Rogoff, 2009b; Malmendier and Nagel, 2011; Bordalo et al., 2017). In particular, elections matter more for regulatory decisions when credit and GDP growth are high, and booming stock returns and economic forecasts signal optimism about the future. This may not be coincidental: Antoniades and Calomiris (2018) show that voters punish Presidential candidates for contractions in mortgage credit, but do not reward them during boom times. Of course, such boom times are precisely when prudential policy is supposed to help cushion bank balance sheets against a reversal of fortunes. As such, the electoral cycle weakens efforts to decrease the pro-cyclicality of the financial sector. In contrast, I find no evidence that countries with more concentrated banking sectors - which one would expect to have greater lobbying powers - have a stronger election cycle.

A common view among academics and policy makers alike is that a strengthening of institutions is a potential remedy for political economy dilemmas. In the case of monetary and macroprudential policy, central bank independence is often referred to as a backstop for political interference (Cukierman, 1992; Eijffinger and de Haan, 1996; Crowe and Meade, 2007). To quote the former director of the IMF's Monetary and Capital Markets Department, "... in many countries the central bank is unique in being insulated from lobbying and political pressures, which is important to make macroprudential policy work."[3] On the other hand, many prominent case studies on the political economy of finance are countries with arguably excellent institutions, such as the United States or the United Kingdom (Calomiris and Haber, 2014).

In the data, I find very limited evidence that central bank independence mitigates the electoral cycle in prudential regulation, which at best shows a quantitatively small and inconsistent mediating effect. For example, the electoral cycle in sector-specific capital buffers is still around half as strong for countries in the top tercile of central bank independence compared to the average effect, and in fact more pronounced for the use of other prudential tools. In contrast, central bank independence appears to have a large and consistent effect on electoral cycles in *monetary policy*. While countries with low central bank independence show signs of lower policy rates, higher base money growth, and a strengthening exchange rate prior to elections, the effect changes sign for those with highly independent monetary authorities. Other measures of institutional quality and democratic development also have limited moderating effects on prudential measures. Further, the electoral cycle has not become weaker in the wake of sweeping post-crisis reforms; if anything, I find larger coefficients in the later period of the sample. Taken together, this suggests that the forces intrinsic in the democratic process may create strong incentives for politicians to directly intervene in credit market regulation via targeted tools, even in countries where central bank independence appears to insulate monetary policy.

Identifying electoral cycles empirically is a non-trivial task, because changes to prudential regulation, election dates, and the state of the economy may be jointly determined. Most obviously, elections may be irregular, that is held before or after the dates set by a country's constitution or common practice (Ito, 1990; Alesina et al., 1992). However, endogenous election timing turns out to play little role in the setting I study: the effects I uncover are basically equivalent in the full sample and a sample of "regular" elections. I also find that pre-election periods are strikingly similar to other times in macroeconomic and financial fundamentals. In other words, "treated" time periods (i.e. pre-election quarters) both across and within countries do not differ in observable characteristics from "control" periods (i.e. all other quarters). This is important because there is considerable evidence for electoral cycles in government-controlled bank lending, investment, fiscal policy, as well as other policy levers (e.g. Dinc, 2005; Akhmedov and Zhuravskaya, 2004; Jului and Yook, 2012; Jens, 2017). These cycles, in turn, may have an effect on the stance of prudential regulation.

[3] See the transcript of José Viñals' speech at the Brookings Institution.

I show that my results change little when I control for macroeconomic and financial sector variables in my regressions, including a large number of leads and lags. The quarterly dimension of the data further allow me to introduce a full set of $country \times year$ fixed effects; effectively, this means comparing the quarters surrounding elections within the *same country* in the *same year*. The remaining identifying assumption, then, is that there are no unobserved factors that are (1) sufficiently orthogonal to the large set of quarterly control variables I employ, and (2) systematically correlated with election timing *within a given year in the same country*. I argue that this assumption is likely to hold. Nevertheless, it is important to stress that the cross-country setting I study does not provide random variation in election timing that would be plausibly orthogonal to the state of the economy. As such, I view the results I provide here as a largely descriptive exercise.

This paper is embedded in a growing body of work on the political economy of finance. My first contribution is to show that elections influence discretionary changes to financial regulation, in particular macroprudential (and microprudential) tools. As such, I provide some empirical evidence to suggest that political limitations are a potential weakness of the post-crisis consensus on how to address the build-up of systemic risk. My second contribution is to examine the circumstances in which political interference may impact regulation. I show that stronger central bank independence – and institutional frameworks more broadly – have very limiting moderating effects. At the same time, central bank independence is an important moderator for political cycles in *monetary* policy. The electoral cycle I document is also uncorrelated with proxies of the market power of financial institutions. Taken at face value, this suggests that it is not driven by powerful special interests exerting power over politicians. Consistent with theories of opportunistic regulatory cycles, I find that benign economic conditions characterized by higher economic and credit growth drive an easing of financial regulation prior to elections.

My work is related to three broad strands of the literature. First, it is grounded in the vast literature on electoral cycles in economic policies and outcomes. There is considerable evidence that political incumbents benefit from better economic performance and a classic literature argues incumbents have incentives to manipulate policies to this end (see e.g. Nordhaus, 1975; MacRae, 1977; Rogoff and Sibert, 1988). My focus on elections extends papers documenting political cycles in, among others, fiscal transfers (e.g. Akhmedov and Zhuravskaya, 2004), local tax rates (e.g. Foremny and Riedel, 2014), or monetary policy (e.g. Alesina et al., 1992; Block, 2002; Clark and Hallerberg, 2000). Perhaps most closely related is work by Brown and Dinc (2005), Dam and Koetter (2012), and Behn et al. (2015), who show that bank bail-outs by governments are much less likely prior to elections in emerging economies and Germany, respectively.[4] In contrast to these studies, I focus on changes in prudential regulation, which are much more frequent than bank bailouts. My work also adds directly to Herrera et al. (2014), who show that rises in the popularity of governments (which they call "political booms") predict financial crises, over and above financial and macroeconomic variables. The results I present meshes well with their finding that financial regulation is pro-cyclical with respect to government popularity.

Second, I add to the literature on election cycles on bank lending. Previous work by Sapienza (2004), Dinc (2005), Cole (2009), Carvalho (2014), Halling et al. (2016), and Englmaier and Stowasser (2017) suggests that government ownership of banks is associated with "political lending" during election periods. In contrast, I focus on discretionary policies set by regulators, not the credit decisions of financial institutions.

Third, my paper is also related to work that describes financial sectors outcomes as bargaining between special interest groups (such as bankers or powerful incumbents) and politicians (e.g. Kroszner and Strahan, 1999; Rajan and Zingales, 2003; Braun and Raddatz, 2008; Benmelech and Moskowitz, 2010; Calomiris and Haber, 2014). Among others, Mian et al. (2010), Mian et al. (2013), and Chavaz and Rose (2016) provide evidence how special interests shape legislative decisions in financial regulation. While such studies are concerned with developments in the medium- and long-run, I show that politics also matter for frequent, discretionary decisions taken by regulators in the post-crisis framework.

3.2 Data and Empirical Strategy

3.2.1 Data

I combine three different types of data for the empirical analysis: (1) data on the use of regulatory tools, (2) election dates, and (3) data on macroeconomic and financial sector conditions. I briefly discuss these in turn and refer the interested reader to the online appendix, which outlines the sources and variable construction in more detail, as well as detailed summary statistics.

The basis for the analysis in this paper is the cross-country database on changes in prudential policy instruments compiled by Cerutti et al. (2017a). This dataset comprises quarterly data on changes in the intensity of widely used regulatory tools for 64 countries for 2000 Q1 through 2014 Q4. In particular, the data differentiate between

[4] Liu and Ngo (2014) study bank failures around elections, but do not investigate the actions of policy makers.

capital buffers (both general and sector-specific), limits to interbank exposures, concentration limits, loan-to-value (LTV) ratio limits, and reserve requirements. The total of nine measures is aggregated into two indexes, where one tracks changes in *any* instrument and a second changes in sector-specific capital requirements, where the latter can refer to real estate credit, consumer credit, or other specific requirements. I discuss the use of each tool in more detail in section 3.2.3; Cerutti et al. (2017a) provide more details. Table 3.9 in the appendix plot the total number of tightening and loosening episodes for each tool. In the online appendix table 3.16, I plot the correlations between the individual measures. This exercise shows that only a few of them move in tandem, suggesting that there is considerable variation across time and countries in their use.

I match the data on changes to prudential regulation with election dates in 58 democratic countries. Theories of electoral cycles posit that politicians may attempt to influence key policy variables to target special interest groups or the median voter. As such, identifying such cycles requires countries with reasonably credible elections where domestic policies are likely to bite. In the main estimation sample, I thus only keep countries that have a score of above 0 on the Polity IV democracy score in all sample years from 2000 through 2014. This requirement eliminates five countries from the dataset on prudential regulation (China, Hong Kong, Kuwait, Saudi Arabia, and Vietnam). I further drop Luxembourg, where – given its role as a tiny financial center country – prudential regulation may not predominantly target the domestic economy. In section 3.3.2, I show that the sample selection makes no difference to the results I present throughout the paper.

To identify elections, I start with the Polity IV database, Database of Political Institutions (Beck et al., 2001), Comparative Political Data Set (Armingeon et al., 2017), and the Global Elections Database (Brancati, 2017), which list the month and year of elections throughout the world. I begin by identifying the most relevant elections by country, which are usually centered around the selection of chief executives. In presidential systems such as the United States, power is normally concentrated in the office of the president. In parliamentary systems, prime ministers or premiers are the relevant figures, which are elected in parliamentary elections. Since the classification is unclear in a few cases, I hand-check all elections drawing on additional information from various internet resources, starting with Wikipedia. I also cross-check my classification with that in Jului and Yook (2012). The full list of elections is reported in the online appendix table 3.24. Table 3.23 further reports the number of tightening and loosening episodes across countries, the number of elections, and the type of elections used. Changes in prudential policy stance are widely distributed across countries and thus provide a lot of variation.

The main variable of interest is a dummy E_{it} that takes the value of 1 in quarters *before* an election takes place. I follow previous studies in using the pre-election quarter to identify political cycles in quarterly data (see e.g. Schultz, 1995; Canes-Wrone and Park, 2012). This choice is also motivated by the finding of Akhmedov and Zhuravskaya (2004) that such cycles may have a relatively short length and thus be underestimated in low frequency data. Further, research on the effectiveness of macroprudential policies suggests that these affect the macroeconomy with a lag of about one quarter (e.g. BIS, 2017). In section 3.3.1, I explore how regulation changes from one year before until one year after elections and find that these only vary consistently for pre-election quarters.

Theory predicts that political cycles should be particularly strong when incumbent governments are uncertain about the electoral outcome. I thus also differentiate elections by whether they can be regarded as relatively "close", in the sense of their outcome being hard to predict. Because the uncertainty about electoral outcomes is unobservable, I follow Jului and Yook (2012) and Canes-Wrone and Ponce de Leon (2018) in using *within-country* variation in actual election results as a proxy. For parliamentary systems, I consider elections where the government achieves a margin of victory below the country-specific median as relatively close, based on the share of parliamentary seats. For presidential systems, I use elections that are below the median in the last-round presidential vote share.[5]

The third group of variables are macroeconomic and financial sector controls. Since changes in regulation likely depend on the state of the economy and the financial sector in particular, I control for key quarterly macroeconomic variables taken from the International Monetary Fund's International Financial Statistics and the OECD, as well as annual financial sector data from the World Bank's Global Financial Development database. I discuss the exact variable construction of these and a few additional variables for cross-sectional tests in the online appendix table 3.11. Table 3.10 in the online appendix provides descriptive statistics for the entire estimation sample.

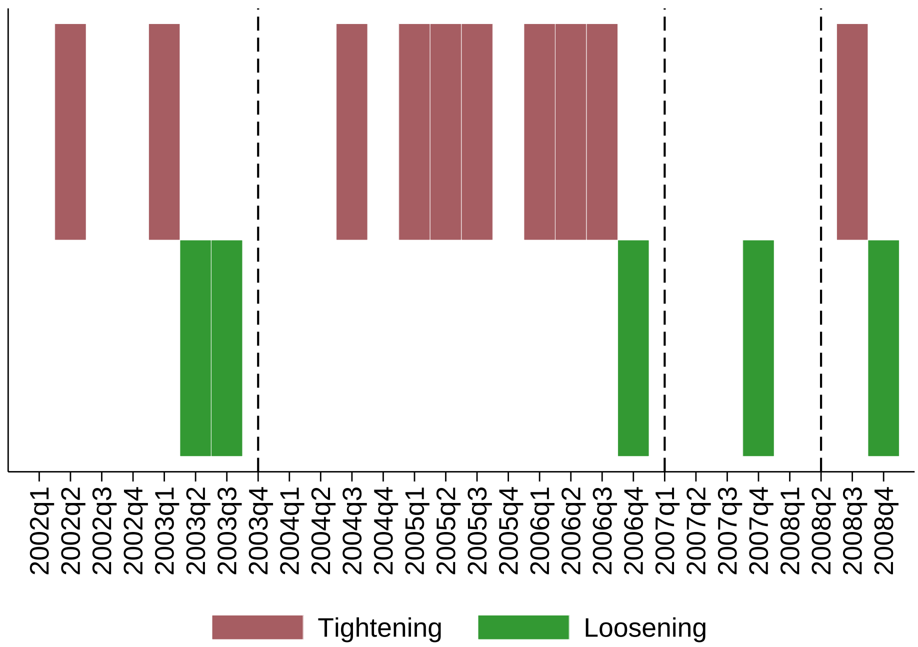
3.2.2 A Case Study of Serbia

Do political considerations play a role in the implementation of prudential regulation? For illustration, consider the particular case of Serbia between 2002 and 2008. Serbia is a parliamentary republic, where the president holds a largely ceremonial post. The

[5] The results are qualitatively similar if I instead define close elections based on the margin of victory in vote shares between the government and opposition or their strongest parties.

modern-day Republic of Serbia was founded in 1992 as a union with Montenegro after the breakup of the Socialist Federal Republic of Yugoslavia in 1992. After the tumultuous Yugoslav and Kosovo wars of the 1990s, president Milošević was overthrown after the disputed 2000 presidential elections amid demonstrations. In its aftermath, the Democratic Opposition of Serbia won an absolute majority in the first free parliamentary elections.

Figure 3.1: ELECTIONS AND PRUDENTIAL REGULATION – THE SERBIAN CASE



This figure plots tightening and loosening episodes in the prudential regulation index in Serbia for 2002 through 2008. The dashed vertical lines indicate the dates of parliamentary elections.

Throughout the particularly volatile first years of the newly democratic Serbia, there was a striking pattern of changes to prudential regulation around parliamentary elections (see figure 3.1). Both before the vote in December 2003 – in the wake of reformist prime minister Đinđić's assassination – and the elections in early 2007, the National Bank of Serbia eased prudential regulation, mainly through reserve requirements.[6] A similar picture can be seen prior to the early 2008 elections, which were held after prime minister Koštunica's government dissolved as a result of Kosovo's unilateral declaration of independence. For the latter, however, the onset of the Global Financial Crisis may also play a role. In both the 2003 and 2008 election, there was

[6]Note that, until July 2003, Serbia and Montenegro's central bank was the National Bank of Yugoslavia.

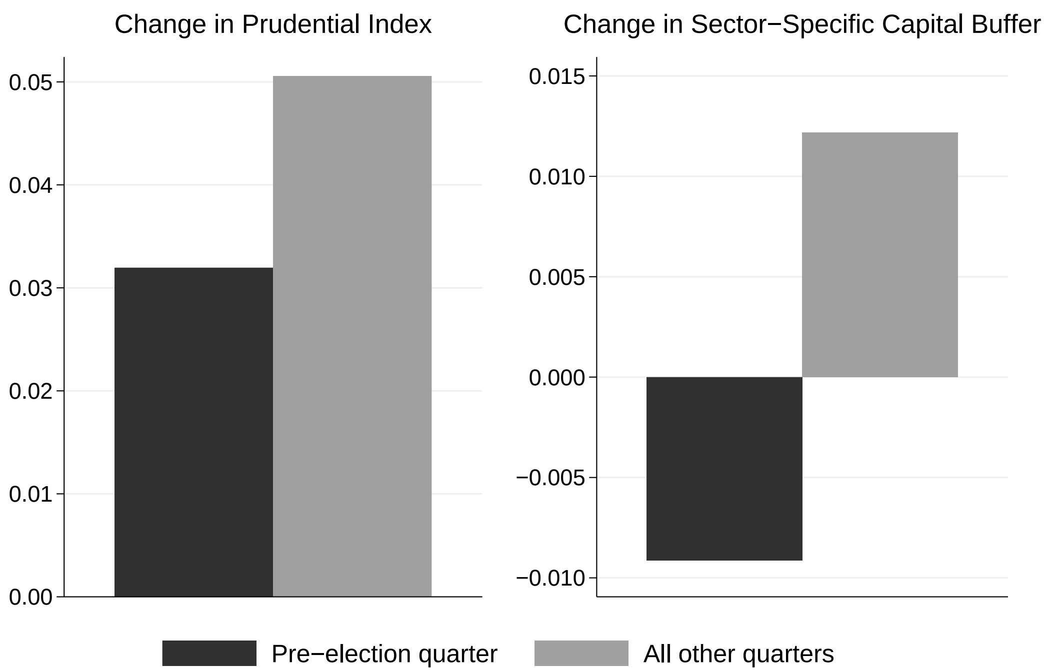
further a loosening in close proximity *after* the vote, which can be suggestive of overly lax policy prior to an election (Alesina et al., 1992).

In all three elections, the incumbent Democratic Party of Serbia (DSS) and its predecessor, the Democratic Opposition of Serbia (DOS), fought for re-election in years with a relative slow-down in economic growth. While there may be many alternative explanations, the pattern of regulatory changes around elections in Serbia could also reflect political considerations. The hypothesis I test in the next section is whether the pattern of an easing of regulation prior to elections is limited to the admittedly special case of Serbia, or rather a general feature of the data.

3.2.3 Basic Correlations

Do regulators systematically loosen or tighten prudential measures prior to elections? In this section, I use data on 58 countries from 2000 through 2014 to provide some basic correlations as an introductory exercise. At this stage, these should not be interpreted as causal effects; rather, the goal is to illustrate the patterns in the raw data.

Figure 3.2: **CHANGES IN (MACRO)PRUDENTIAL POLICY, BY PRE-ELECTION QUARTER**



This figure plots simple averages of changes in the prudential regulation index and sector-specific capital buffers in pre-election quarters and all other quarters. Positive values indicate a tightening, negative values a loosening of regulation.

Figure 3.2 plots the average change in the prudential regulation index and the index of sector-specific capital buffers for all quarters for which I have data, depending on whether it is prior to an election. The bar chart suggests that regulation is, on average, tightened in every quarter, but less so in pre-election quarters. For sector-specific tools, the effect is almost diametrically opposed: regulation is loosened before elections and tightened in all other periods.

Table 3.16 plots these changes and those in all macroeconomic and financial sector control variables. It also shows the difference between pre-election and all other quarters and challenges these to a simple two-sided t-test for statistical significance. The data confirm that the pattern shown in 3.2 is significantly different from zero for the sector-specific buffer and 3 out of the 9 individual regulatory tools, with p-values close to the 10% threshold for 2 other measures. In particular, pre-election quarters saw a loosening of regulation for real estate capital buffers sectoral capital tools (and no changes to other sectoral buffers); concentration limits (also aimed at loan composition); and interbank exposures. I find no effect for *general* capital requirements, loan-to-value limits, or reserve requirements.

Importantly, there seems to be no systematic difference in financial and macroeconomic conditions between the election and non-election periods. For the vast majority of variables, the p-values for statistical significance are in excess of 0.5 – clearly, pre-election periods do not seem to be particularly different in regard to the state of the economy on average. Even for those variables with p-values closer to thresholds of statistical significance, the differences are quantitatively small. This will become important for causal inference later, because a valid caveat to the observation of softer regulation before elections is that it may be driven by differences in the broader economic climate.

3.2.4 Empirical Strategy

The backbone of the empirical analysis are standard fixed effects dynamic panel regressions of the type:

$$\Delta R_{it} = \alpha_i + \alpha_t + \beta E_{it} + \gamma C_{it} + \psi(L) R_{it-1} + \varepsilon_{it}, \tag{3.1}$$

where i and t index countries and year-quarters, respectively, and α_i and α_t refer to a full set of fixed effects. As discussed later, I also consider specifications with $country \times year$ fixed effects, α_{iy}.

E_{it} is the pre-election dummy, taking the value of 1 if an election takes place in

Table 3.1: VARIABLE MEANS, BY PRE-ELECTION QUARTER

Note: This table presents simple averages of the variables used in the empirical analysis, divided by whether a country held an election in the following year-quarter. The sample is the estimation sample including all controls, as in columns (2) and (7) of table 3.2. The *p*-value in the right column is based on a two-sided *t*-test that allows for unequal variances of the samples under the null hypothesis that the difference between the variables is zero.

	Pre-election quarter	Other quarters	Difference	Pr(Diff.) = 0 (p-value)
Index measures				
Δ Prudential regulation index	0.043	0.052	0.008	0.816
Δ Sector-specific capital buffer	-0.007	0.014	0.021	0.014
Individual regulatory tools				
Δ Real estate capital buffer	-0.007	0.009	0.016	0.044
Δ Consumer credit capital buffer	0.000	0.002	0.002	0.157
Δ Other capital buffer	0.000	0.003	0.003	0.144
Δ General capital requirement	0.036	0.022	-0.014	0.396
Δ Concentration limit	-0.012	0.018	0.030	0.018
Δ Interbank exposure	0.000	0.021	0.021	0.000
Δ Loan-to-value ratio	0.080	0.024	-0.056	0.259
Δ Reserve requirements (FC)	0.007	0.005	-0.002	0.905
Δ Reserve requirements (LC)	-0.022	-0.008	0.014	0.576
Bank variables				
Bank capitalisation (%)	8.251	8.273	0.022	0.945
Lending concentration	66.746	66.886	0.140	0.935
Cost to income ratio (%)	56.931	57.166	0.235	0.842
Non-performing loans (%)	4.901	5.065	0.164	0.714
ROA	0.908	1.112	0.203	0.251
Z-score	10.058	10.234	0.176	0.760
Foreign bank share (%)	35.799	35.505	-0.293	0.893
Macro variables				
Government exp./GDP	0.177	0.172	-0.005	0.241
Money market rate	4.398	4.443	0.045	0.932
Base money growth	0.163	0.133	-0.030	0.323
Real credit growth	0.070	0.084	0.015	0.278
Real GDP growth	0.036	0.032	-0.004	0.484
Δ Current account/GDP	-0.001	-0.001	0.001	0.856
Total trade/GDP	0.888	0.865	-0.023	0.608
Investment/GDP	0.230	0.228	-0.002	0.648
Consumption/GDP	0.576	0.580	0.004	0.635
Inflation	0.038	0.039	0.001	0.760
Exchange rate (US$)	220.720	319.186	98.467	0.363

country i in year-quarter $t+1$, and 0 otherwise. In principle, election timing may be endogenous to changes in financial regulation; I address this in section 3.3.1 by restricting the sample to "regular" elections that are held within the limit implied by a country's constitution or regular practice. Because most elections in my sample are indeed regular (see table 3.15 in the online appendix), this adjustment makes no difference to any of the results presented in the paper.[7]

C_{it} is a vector of macroeconomic variables that describe the state of the economy and the financial sector; for the full set of controls, I use 11 quarterly macro and 7 annual banking sector variables.[8] $(L)\Delta R_{it-1}$ is a vector of lags of the dependent variable up to account for autocorrelation in regulatory decisions; in the baseline regressions, I set the lag polynomial (L) to 4.[9] ε_{it} is an error term that is assumed to be well-behaved. Standard errors are clustered by country.

I estimate all baseline regressions using ordinary least squares (OLS). In some specifications, I transform the dependent variables into dummies to indicate a tightening or loosening of prudential policy, which suggests the use of maximum likelihood estimators such as logit or probit in many applications. In my setting, however, this is infeasible because of the combination of two-way fixed effects, interaction terms, and completely separated variables.[10] However, I replicate all regressions with dummy dependent variables using standard logit regressions as well as the bias-corrected logit estimator for two-way fixed effects described in Fernández-Val and Weidner (2016)

[7]In unreported regressions, I also tried instrumenting election dates using "predictable" elections by exploiting the fact that fixed legislative periods generate plausibly exogenous variation in the exact election timing from the perspective of the pre-sample period. This approach yields estimates very similar to those obtained via OLS.

[8]These include government spending/GDP, a money market interest rate, the growth of central bank reserves, real credit growth, real GDP growth, current account/GDP, total trade/GDP, investment/GDP, private consumption/GDP, CPI growth, the nominal USD exchange rate, bank capitalization, a measure of banking sector concentration, banks' cost-to-income ratio, the NPL ratio, bank return on assets, bank Z-score, and the share of foreign banks. See the online appendix for the exact variable descriptions.

[9]Lag selection tests using the Bayesian or Akaike information criteria suggest autocorrelation of between 1 and more than 8 quarters. I use 4 lags as a baseline compromise. As I show in section 3.3.2, the exact number of lags makes virtually no difference to the results. The results are also unaffected by Nickell bias, because they also hold using specifications without lagged dependent variables (see section 3.3.2); this is unsurprising, given that I have 56 quarters of observations per country.

[10]Complete separation of a variable arises when a variable perfectly an outcome. This is the case in my setting because, for example, out of the 51 changes in sector-specific capital requirements in the sample, *none* occur in pre-election quarters. Models with such variables cannot be estimated using maximum likelihood because the likelihood of no change in sector-specific capital requirements in pre-election quarters is infinity by definition.

in section 3.3.2.

The goal of estimating equation 3.1 is for the coefficient $\hat{\beta}$ to capture the likelihood that a change in the regulation measure R is influenced by an upcoming election. The estimation abstracts from time-invariant country-specific factors that may have an impact, e.g. how often regulation is generally changed in country i, as well as time-specific factors in quarter t, e.g. the Great Financial Crisis 2007-2008. Next, I investigate to which extent pre-election periods differ in their underlying economic fundamentals from other times.

3.2.5 Are Pre-Election Quarters Different?

A potential challenge for identifying electoral cycles in regulation is that the macroeconomic variables in vector C_{it} may themselves be subject to an electoral cycle. If, for example, financial conditions prior to elections are relatively gloomy, regulators may see less reason to interfere or loosen existing measures. In the data, we may thus observe a negative correlation of E_{it} and ΔR_{it} even in the absence of a true causal effect.

Whether *observable* macroeconomic or financial sector fundamentals are different in the run-up to elections is an empirical question. In section 3.2.3, I already showed that there is no evidence that macroeconomic conditions in my sample differ between pre-election quarters and other periods *on average*. Another way to think about this is that the "treatment group" (pre-election quarters) and "control group" (all other quarters) are randomly assigned across observable differences.

I explore this issue in more detail in table 3.17 in the online appendix, where I regress each of the macroeconomic and financial sector controls on the pre-election quarter dummy. Whether one includes fixed effects or not, *none* of the variables consistently exhibits an electoral cycle. In other words, pre-election quarters appear to be highly similar in macroeconomic fundamentals compared to other quarters.

The remaining identifying assumption is that unobserved time-varying country factors that are sufficiently orthogonal to the variables in vector C_{it} do not have a pronounced independent election cycle affecting R_{it}, conditional on C_{it}. I argue that this assumption is likely to hold in practice, given the relatively large number of 18 baseline control variables in C_{it}.[11]

[11] In robustness checks, I further use up to 4 lags and leads of each of these variables, which yields a total of $18 + 4 \times 18 + 2 \times 18 = 126$ covariates; alternatively, I use the first principal components of 20 financial sector and the 11 macroeconomic controls. In unreported results, I also experimented with including interactions of all control variables with the pre-election dummy, which considerably increases the point estimate of $\hat{\beta}$ and leaves statistical significance intact.

To further address the issue of potential unobserved factors, I also allow for a specification of equation 3.1 that includes $country \times year$ fixed effects by replacing the term α_i with α_{iy} where y indexes years. Effectively, I only compare the quarters around elections in the *same country* in the *same year*. This considerably eases the identifying assumption I have to make. In particular, there would have to be an omitted factor that affects prudential regulation at high frequency and is sufficiently orthogonal to the quarterly control variables in C_{it} to bias $\hat{\beta}$ consistently above zero. While this seems like a rather mild assumption to make, I want to stress that in the absence of random variation in elections across countries, the results presented here should be interpreted as descriptive.

3.3 Elections and Prudential Regulation

3.3.1 Baseline Results

Table 3.2 shows the main results of running equation 3.1. I start by differentiating between the broad prudential regulation index and the index of sector-specific capital requirements, which particularly target real estate and consumer credit. I then turn to the impact on individual tools and graphical evidence below.

Looking at aggregate tightening and loosening jointly, the coefficients for the broad prudential regulation index presented in panel A always attract a negative sign, but lack statistical significance in some specifications. The negative sign implies less stringent changes to prudential regulation in the run-up to elections. The p-values of 13% and 11% in columns (1) and (4), respectively, are closest to conventional levels of statistical significance. When adding the full set of macroeconomic and financial sector controls including 4 lags and 2 leads of each variable in column (5), the coefficient become significant at the 5% level. One explanation for this result may be the clearly much smaller sample in column (5), which could inflate the coefficient estimate. Alternatively, it may be crucial to appropriately control for the current, past, and (expected) future economic environment to isolate the effect of upcoming elections on changes to prudential regulation.

However, as we saw in section 3.2.3, many of the index changes are due to changes in general capital requirements. In panel B, I thus turn to changes in the index of sector-specific tools. In column (1), I begin by running the OLS fixed effects regression without controls. The coefficient of -0.021 is highly statistically significant at the 5% level. It is also remarkably large: since the mean change of the sector-specific capital buffer is 0.049, pre-election quarters explain almost half of the average regulatory de-

Table 3.2: MAIN RESULTS – ELECTIONS AND PRUDENTIAL REGULATION

Note: This table shows coefficients from estimating equation 3.1. The dependent variable is the change in the prudential index in panel A and the change in sector-specific capital buffers in panel B. All estimations include four lags of the dependent variable as covariates and other controls as indicated. See text for variable descriptions. Standard errors are clustered by country in columns (1) through (3) and heteroskedasticity-robust in columns (4) and (5) due to small cluster size, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively. The † refers to a p-value of 0.104.

	Only FE (1)	Add controls (2)	Add country × year FE (3)	Add lagged controls (4)	Add lead controls (5)
Panel A: Δ Prudential regulation index					
Pre-Election Quarter	-0.043	-0.024	-0.037	-0.065†	-0.102**
	(0.028)	(0.037)	(0.035)	(0.040)	(0.041)
Countries	58	51	51	49	46
Observations	3,190	2,213	2,199	1,674	1,274
R^2	0.123	0.133	0.481	0.204	0.309
Panel B: Δ Sector-specific capital buffer					
Pre-Election Quarter	-0.021**	-0.026***	-0.025**	-0.025**	-0.023*
	(0.009)	(0.010)	(0.012)	(0.010)	(0.013)
Countries	58	51	553	49	46
Observations	3,190	2,213	2,199	1,674	1,274
R^2	0.046	0.072	0.489	0.134	0.204
AR(4)	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	–	Yes	Yes
Country-Year FE			Yes		
Time FE	Yes	Yes	Yes	Yes	Yes
Controls (18)		Yes	Yes	Yes	Yes
4 lags of controls (72)				Yes	Yes
2 leads of controls (36)					Yes

cision. Next, I introduce the vector of 18 macroeconomic and financial sector control variables to account for potential differences in fundamentals in column (2). Because of the relatively large number of controls, this reduces the number of countries from 58 to 51; the point estimate, however, is now even more precisely estimated and increases to -0.026. This model specification will serve as the baseline model for the rest of the paper. In column (3), I add $country \times year$ effects, which effectively means comparing the electoral cycle within the *same country* in the *same year*. As outlined above, this makes it highly unlikely that unobservables are driving the result and yields an almost unchanged coefficient of -0.025. The fact that conditioning on a full set of country-year dummies does little to the point estimate is not surprising because, I will discuss momentarily, the entire effect on regulation is concentrated in the quarter prior to the election.

Could it be that I am not sufficiently conditioning on *past* fundamentals? After all, the state of the economy in the previous quarters may explain both current regulation and election timing. In column (4), I introduce the contemporaneous controls as well as 4 lags of each variable, which again slightly decreases the sample size. The point estimate and its statistical precision, however, remain unaffected, despite the inclusion of $18 + 4 \times 18 = 90$ covariates.[12] As a last check, I further add 2 leads of each of the control variables, yielding a total of $18 + 4 \times 18 + 2 \times 18$ controls. The idea is that, while I do not have data on forecasts of each variable, future realizations may serve as a proxy for expected changes in these fundamentals. This may be important in studying policy changes where the expected future state of the economy is key, which creates a "foresight problem" when they are not controlled for (Ramey, 2016). Again, this leaves the point estimate largely unchanged at -0.023 and still significant at the 10% level. This is rather remarkable given the drop in degrees of freedom from including 126 covariates in the regression besides the election dummy.[13] The fact that the coefficient on sector-specific capital buffers is almost unchanged across specifications suggests that differences in economic or financial conditions, e.g. due to electoral cycles in these variables, are unlikely to drive the regulatory easing prior to elections.

Next, I investigate differences in the timing and competitiveness of elections in table 3.3. Again, panel A begins by plotting the results for the prudential regulation index. Column (1) reproduces the baseline specification (as in column (2) from table

[12]Note that for this model and that in column (5) I use heteroskedasticity-robust standard errors, because the lags leave me with insufficient observations to cluster standard errors by country.

[13]Note that the point estimates are virtually unchanged when including more than two leads (or additional lags), but the diminished degrees of freedom then push the *p*-values slightly above the 10% threshold.

Table 3.3: Main Results – Differences across Elections

Note: This table shows coefficients from estimating equation 3.1. The dependent variable is the change in the prudential index in panel A and the change in sector-specific capital buffers in panel B. All estimations include four lags of the dependent variable as covariates and other controls as indicated. In column (2), I restrict the pre-election dummy to "regular" elections, defined as those that were not held late or prematurely (while tolerating one quarter of difference); column (3) uses the remaining "irregular" elections. In column (4), I restrict elections to those that are relatively "close", as defined in the text; column (5) uses the relatively less close elections. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively. The † refers to a p-value of 0.107.

		Election timing		Election outcome	
	Baseline	Regular	Irregular	Close	Not close
	(1)	(2)	(3)	(4)	(5)
Panel A: Δ Prudential regulation index					
Pre-Election Quarter	-0.024	-0.046	0.033	-0.098†	-0.006
	(0.037)	(0.047)	(0.041)	(0.059)	(0.046)
Countries	51	51	51	43	48
Observations	2,213	2,213	2,213	672	1,393
R^2	0.133	0.133	0.133	0.306	0.143
Panel B: Δ Sector-specific capital buffer					
Pre-Election Quarter	-0.026***	-0.030**	-0.015	-0.067**	-0.006
	(0.010)	(0.012)	(0.009)	(0.029)	(0.009)
Countries	51	51	51	43	48
Observations	2,213	2,213	2,213	672	1,393
R^2	0.072	0.072	0.071	0.175	0.101
AR(4)	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Controls (18)	Yes	Yes	Yes	Yes	Yes

3.2) for convenience. Similar to the results shown before, the results on election timing in columns (2) and (3) lack statistical precision. Taken at face value, they suggest the presence of an electoral cycle only during "regular" elections, defined as those that are held within the time frame specified in a country's constitution or established as regular practice.[14] In column (4), I restrict the sample to close elections, defined as those that are more competitive than the median election in a given country. The point estimate is now -0.098 with a p-value of 0.107, considerably larger than the baseline of -0.024; less contested elections in column (5) attract a coefficient of -0.006 and thus do not have an effect on prudential regulation. Despite the lack of statistical power for the prudential index, this is already a first indication that the electoral cycle is driven by periods when incumbents expect election outcomes to be uncertain – and thus have stronger incentives to intervene.

Panel B paints a much clearer picture for the sector-specific capital buffer. Regulators are twice as likely to forgo tightening sectoral buffers in the run-up to regular compared to irregular elections. This is intuitive from a political economy angle, because incumbent politicians may be able to influence policy more when the timing of elections is predictable. Also recall that table 3.15 showed that three quarters of all elections in my sample can be considered regular. As such, the baseline coefficient of -0.026 is naturally closer to that of -0.030 for regular elections in column (2) than that of -0.015 for irregular elections. This also suggests that endogenous election timing does not appear to be major factor in my setting; I will thus use the baseline pre-election dummy for the remainder of the paper.[15]

In columns (4) and (5), I again split the sample by whether pre-election dummy precedes election outcomes that are more or less likely to be predictable upfront. The point estimate for close elections in column (4) now jumps to -0.067 (statistically significant at the 5% level), while that for elections that are not contested is an order of magnitude smaller and close to 0 in column (5). This suggests that the electoral cycle in sector-specific capital buffers, similar to that for prudential tools on average, is limited periods where incumbents face uncertainty about election outcomes.

As a next exercise, I differentiate between tightening and loosening episodes for the prudential index and sector-specific capital buffers in table 3.4. The dependent variable is now a dummy equal to 1 for the respective changes, and 0 otherwise. Because

[14] I allow one quarter deviation from the exact quarter of the previous election, which is unlikely to reflect severe meddling with election timing. The results, however, are virtually unchanged if I do not make this correction.

[15] All results I present in the following are almost unchanged if I use the "regular elections" dummy instead. However, this comes at the cost of reducing the number of elections, which may yield a less general result.

Table 3.4: REGRESSIONS BY PRUDENTIAL TOOL AND TIGHTENING/LOOSENING EPISODES

Note: This table shows coefficients from estimating equation 3.1 with and without *country* $\times$ *year* fixed effects (α_{iy}, where the dependent variable is a dummy for a tightening or loosening of the indicated prudential instrument. Each cell represents an individual regression, where I only plot the estimated coefficient of the pre-election quarter dummy $\hat{\beta}$. All estimations include four lags of the dependent variable as covariates but no other control variables. The coefficient on the loosening of general capital requirements cannot be estimated because these requirements are never loosened in the sample (as in Cerutti et al. (2017a)). Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Tightening		Loosening		
	$\hat{\beta}^{\alpha_i}$	$\hat{\beta}^{\alpha_{iy}}$	$\hat{\beta}^{\alpha_i}$	$\hat{\beta}^{\alpha_{iy}}$	N
Δ Prudential regulation index	-0.011	-0.002	0.035**	0.035**	3,190
Δ Sector-specific capital buffer	-0.015***	-0.016**	0.003	0.002	3,190
Δ Real estate capital buffer	-0.011***	-0.011*	-0.001	-0.003	3,190
Δ Consumer credit capital buffer	-0.002**	-0.004	0.003	0.004	3,190
Δ Other capital buffer	-0.005**	-0.004	-0.001	0.001	3,190
Δ Capital requirements	-0.001	-0.004	–		2,970
Δ Concentration limit	-0.017***	-0.013	0.008	0.008	1,773
Δ Interbank exposure	-0.022**	-0.018**	-0.001	0.000	1,013
Δ Loan-to-value ratio	0.039	0.069**	0.027	0.044*	960
Δ Reserve requirements (FC)	-0.016*	-0.010	-0.002	-0.002	3,190
Δ Reserve requirements (LC)	-0.004	-0.001	0.020	0.022	3,190
Country FE	Yes	–	Yes	–	
Country × Year FE		Yes		Yes	
Time FE	Yes	Yes	Yes	Yes	

the sample size is somewhat smaller for concentration limits, interbank exposure limits, and LTV ratio limits in particular, I focus on OLS regressions without controls. I report the coefficients from the baseline specification 3.1 $\hat{\beta}^{\alpha_i}$ and that of a regression including $country \times year$ fixed effects, $\hat{\beta}^{\alpha_{iy}}$.[16]

The findings suggest that electoral cycles in prudential regulation are pervasive across a wide range of tools. While there is little evidence of an average effect on the regulation index, the results here suggest that regulators are 3.5% more likely to *loosen* constraints in the run-up to an election for both the baseline and country-year FE specification. This is a large effect, given that the unconditional probability of a loosening in the estimation sample is 5%. A lower likelihood of tightening of regulation prior to elections can be observed for all the sector-specific capital requirements, but also concentration limits and interbank exposures. Similar to the patterns in the raw data, the effect sizes imply that almost none of the tightening episodes occur in pre-election quarters, even after adjusting for country and time fixed effects.

I find more limited evidence for reserve requirements or *general* capital requirements, which do not target particular sectors. The picture is also unclear for changes to loan-to-value limits, which however are only available for a small subset of the dataset in Cerutti et al. (2017a); this may explain why I find some (however inconsistent) evidence for both a tightening *and* loosening prior to elections. Taken together, these results suggest that (1) there is considerable heterogeneity in the political cyclicality of prudential regulation, and (2) sector-specific tools, broadly speaking to include interbank and concentration limits, appear to react particularly strong to upcoming elections. Judging by the highly similar coefficients in the specification with and without $country \times year$ effects, these findings are unlikely to be driven by omitted factors.

I next tease out the full dynamic path of regulatory reaction to elections in figure 3.3, where I differentiate between close and other elections.[17] Since there is only consistent evidence for a cycle in the loosening of the regulation index and a tightening of sector-specific capital requirements, I focus on these two measures in the graphical analysis. To get a glimpse of how elections impact regulation, I estimate equation 3.1 with four lags and leads of the election dummy E_{it} as well as the full control variable vector, and plot the estimated coefficients. Figure 3.3 paints a clear picture: the electoral cycle in prudential regulation is highly concentrated in the quarters prior to elections, and is only statistically significant for contested, close elections – precisely

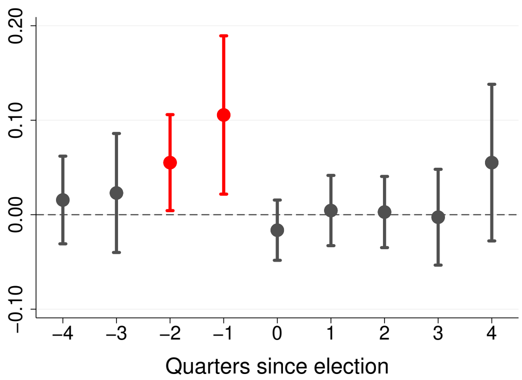
[16] All results presented here are almost unchanged when including the full vector of controls (available upon request).

[17] In 3.4 in the online appendix, I show that the results in fact look very similar for the entire sample of elections.

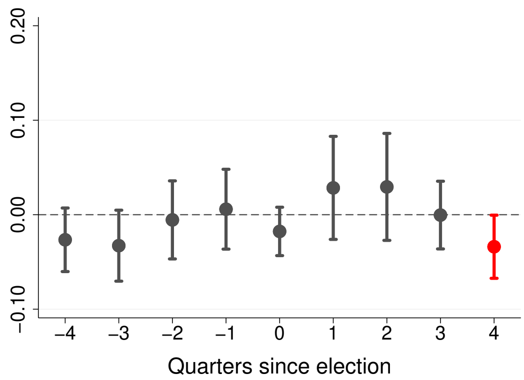
Figure 3.3: Elections and Prudential Regulation – Graphical Evidence

Panel A: Loosening of Prudential Regulation Index

(A.1) Close Elections

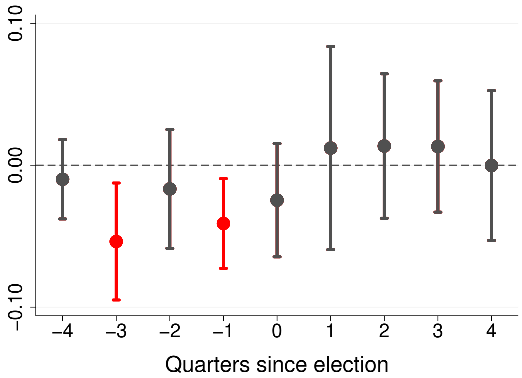


(A.2) Not Close Elections

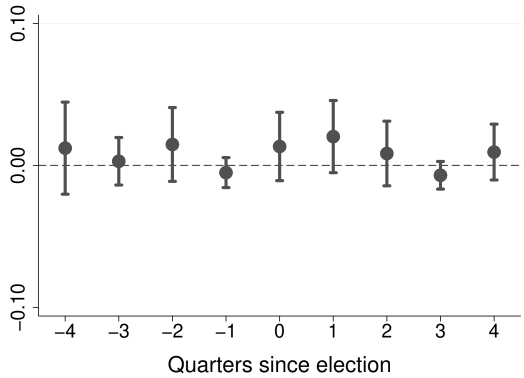


Panel B: Tightening of Sector-Specific Capital Buffers

(B.1) Close Elections



(B.2) Not Close Elections



Notes: These figures show the dynamic effect of elections on the loosening of the prudential regulation index (Panel A) and the tightening of sector-specific capital buffers (Panel B). For each estimation, the sample is divided into close elections and all other elections, as defined in the text. The plotted coefficients are the $\sum_{t=-4}^{4} \hat{\beta}_t$ estimated using the OLS regression $R_{it} = \alpha_i + \alpha_t + \sum_{t=-4}^{4} \beta_t E_{it} + \gamma C_{it} \varepsilon_{it}$, where R_{it} is one of the outcome dummies. The regressions also include four lags of the dependent variable. Standard errors are clustered by country.

those situations when governments may believe to benefit from easily flowing credit. This confirms findings in the previous literature that electoral cycles are often short-lived and thus require data at relatively high frequency (Akhmedov and Zhuravskaya, 2004). It is also consistent with evidence that the effect of macroprudential tools kicks in after one or two quarters; the election quarter itself always attracts a statistically insignificant coefficient. I do not find statistically significant rebalancing in the post-election period, which could be a systematic reaction to a too loose policy stance before (Alesina et al., 1992). The lack of such rebalancing suggests that – on average – foregone tightening or excessive loosening in the election run-up have permanent effects.

The implied magnitudes of the estimates are also worth highlighting: For the aggregate prudential regulation index in figure (A.1), the estimates suggest that a loosening is around 10% more likely in the quarter prior to elections. Sector-specific capital buffers have an around 5% lower likelihood to be tightened between 1 and 3 quarters prior to the election in figure (B.1). These are large effects.

3.3.2 Robustness

In the previous section, I established some evidence for an electoral cycle in the use of prudential regulatory tools. A clear concern for the type of cross-country panel regression I use is that it may not be robust to changes in estimation technique or model specification; sample composition; or different sets of control variables. In table 3.5, I thus present a wide range of validity exercises to showcase the robustness of the coefficient estimates $\hat{\beta}$ while taking into account potential non-linearities between tightening and loosening episodes.

I begin by addressing concerns regarding the exact model specification and estimation technique in Panel A. It turns out that the coefficients I find are remarkably stable, independent of the included set of fixed effects or lags of the dependent variable. In unreported regressions, I further experimented with including additional lags and found that the results did not change. They also hold when using the mean group estimator (Pesaran and Smith, 1995) to account for heterogeneous effects across countries. This is an important result because it suggests that there is little underlying heterogeneity in the pooled main estimate I report in table 3.4. In the online appendix figure 3.5, I also plot the distribution of coefficients estimated *by country*, which are negative for the sector-specific capital buffer in virtually all sample countries.

Since the dependent variables used here are all dummies, it is common practice to use non-linear models such as logit regressions. In the case here, however, I run into the problems of (1) complete separation, and (2) bias in the use of two-way fixed effects

Table 3.5: ELECTIONS AND PRUDENTIAL REGULATION – ROBUSTNESS

Note: This table shows coefficients from estimating equation 3.1 using OLS, where the dependent variable is a dummy for a tightening or loosening of the indicated prudential instrument (the prudential regulation index or sector-specific capital buffer). Each cell represents an individual regression, where I only plot the estimated coefficient of the pre-election quarter dummy $\hat{\beta}$. All estimations include four lags of the dependent variable as covariates as well as country and year-quarter fixed effects, unless otherwise indicated. Any control variables that were included are indicated in the first column. The coefficient on the loosening of general capital requirements cannot be estimated because these requirements are never loosened in the sample (as in Cerutti et al. (2017a)). Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Tightening		Loosening	
	Δ Index	Δ SSCB	Δ Index	Δ SSCB
Panel A: Model specification				
No FE	-0.002	-0.016***	0.017	0.004
Time FE only	-0.013	-0.015***	0.023	0.005
AR(0)	-0.011	-0.015***	0.024	0.004
AR(1)	-0.011	-0.015***	0.028*	0.004
Mean Group Estimator, no FE	0.004	-0.016***	0.023	0.003
Logit, no FE	-0.051	—	0.586**	0.538
Logit, time FE only	-0.179	—	0.762**	1.019
Logit, both FE	-0.150	—	0.775**	1.024
Panel B: Sample selection				
Consensus democracies only	-0.015	-0.014***	0.039**	0.004
No militaristic leaders	-0.004	-0.016***	0.042**	0.004
Drop Africa	-0.012	-0.016***	0.037**	0.004
Drop Asia	-0.007	-0.012***	0.048**	0.004
Drop Americas	-0.020	-0.016***	0.034*	-0.000
Pre-crisis only	-0.003	-0.012**	0.048	0.010
Post-crisis only	-0.006	-0.020***	0.022	-0.002
Panel C: Additional controls				
Only bank controls	0.002	-0.016***	0.023	-0.001
20 bank controls	-0.007	-0.017***	0.028	0.002
Only macro controls	-0.008	-0.019***	0.035*	0.007
Factor controls	-0.014	-0.018***	0.031	0.003
Regulation $\times$ Time FE	-0.022	-0.016***	0.048**	0.004
Region $\times$ Time FE	-0.019	-0.014**	0.031	-0.001
Development $\times$ Time FE	-0.001	-0.014***	0.043**	0.005

because of the incidental parameter problem. Since *none* of the tightening episodes of the sector-specific capital buffer occur in pre-election quarters, it is not possible to estimate the maximum likelihood in a standard logit framework, which is infinity by definition. However, the bias-corrected estimator of Fernández-Val and Weidner (2016) enables the estimation of panel logit regressions with two-way fixed effects. This yields somewhat higher statistical significance of the pre-election dummy for the loosening of the prudential regulation index throughout.

In Panel B, I deal with concerns regarding sample selection. I start by dropping all countries that, at any point in the sample, are not defined as an (electoral) democracy by both Polity IV *and* Freedom House. As an alternative proxy for authoritarianism, I drop countries where either the chief executive or defense minister is a military officer. If anything, this increases both the point estimates and statistical significance. I next drop the continents of Africa, Asia, and the Americas in turn to validate that the findings are not driven by a particular region. I find that they are not. Similarly, I divide the sample into the pre-crisis (up to 2006) and post-crisis (from 2007) period and find similar results, with somewhat larger coefficients on the sectoral capital buffer tightening (but smaller coefficient on the index loosening).

Finally, I deal with the issue of cherry-picking of control variables in Panel C. I start by including only bank controls or macro controls, or alternatively controlling for 20 (instead of 7) financial system controls from the World Bank's Global Financial Development database. This makes only tiny differences to the point estimates. I also address the fact that the control variables are likely to be highly collinear. To overcome this issue, I separately take the first principal component of the 10 quarterly macro variables and 20 indicators of financial conditions, and control for these in the fourth row of Panel C; this also makes no difference to the results. Next, I control for the number of macroprudential tools in a given year via Cerutti et al. (2015), interacted with year-quarter dummies, which does not make a difference, either. At last, I control for detailed region $\times$ time or World Bank development level $\times$ time dummies, which also leaves the estimates unchanged.

Overall, it seems fair to conclude that a lower likelihood of a regulatory tightening of sectoral capital buffers – and to a lesser extend, higher likelihood of a general loosening – is a highly robust feature of the data.

3.4 Exploring Heterogeneity

The results in the previous sections suggest the existence of an electoral cycle in the use of prudential regulation. But what drives it? Theories of cycles in economic policy usually suggest that they are the result of either (1) powerful special interest groups attempting to influence policies in their favor (e.g. Kroszner and Strahan, 1999; Dagher, 2017), or (2) incumbent politicians attempting to influence policies to increase their chance of re-election (e.g. Nordhaus, 1975; Tufte, 1980; Canes-Wrone and Ponce de Leon, 2018). In the first case, we would expect that measures of the market power of the financial industry should influence the correlation between elections and prudential regulation. In the latter case, we would expect that the economic outlook plays a particular role. If incumbent politicians can claim that the current state of the economy is good because of their competency, this might give them a strong incentive to urge regulators not to interfere with the financial sector; this also meshes well with the finding of Antoniades and Calomiris (2018) that voters punish governments for a credit crunch, with no reward during a boom.[18] A potential third explanation is economic policy uncertainty, which has been found to spike around close elections (Baker et al., 2016): if regulators await voting outcomes similarly to what can be observed for firm investment (Jului and Yook, 2012), this may explain a more cautious stance prior to elections.

In this section, I test how well these theories fit the data in the case of regulatory cycles. I implement these tests by introducing an interaction term of the pre-election quarter dummy with measures of market power, economic outlook, and uncertainty in the baseline regression 3.1. Since the evidence for a cycle in the aggregate prudential regulation index is limited, I focus on sector-specific capital buffers.[19]

Table 3.6 plots the results. In columns (1) through (3), I attempt to test for heterogeneity in the power of financial institutions over governments. The theoretically most sound variable would be an index of interlinkages between politicians, regulators, and bankers; unfortunately, I am not aware of such measures on a cross-country basis. I thus begin by using two variables which are likely correlated with such linkages and included in the controls. The first is a measure of banking sector concentration, where one would expect that more concentrated sectors with few powerful institutions are able to wield larger lobbying powers.[20] The second is the market share of foreign banks. The latter is based on the intuition developed in Rajan and Zingales (2003) that foreign bank ownership decreases the sway of politicians over financial institutions

[18]An alternative hypothesis is that interference is more likely when the economic situation is relatively poor. However, as we will see below, the evidence appears to be more consistent with interference in good times – which is also consistent with my main finding of a reduced *tightening* prior to elections.

[19]In unreported regressions, I find that interactions of the pre-election dummy with the variables presented here are all insignificant in predicting changes in the prudential regulation index.

[20]The results here are similar when using other measures of competition, namely the Boone indicator, H-statistic, or Lerner index instead (available upon request).

(see also Calomiris and Haber (2014)).

Table 3.6: ELECTIONS AND SECTOR-SPECIFIC CAPITAL BUFFERS – HETEROGENEITY

Note: This table shows coefficients from estimating equation 3.1 using OLS. The dependent variable is the change in the sector-specific capital buffers index. $Measure$ refers to the measure of market power, economic outlook, or uncertainty listed in the top row. All estimations include four lags of the dependent variable as covariates and the baseline control variables as described in the text. "Connected firms" in column (3) is the share of firms with political connections from Faccio (2006) (by market capitalization). "Campaign financing" in column (4) is an index of legal restrictions on campaign financing constructed from the IDEA Political Finance Database. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Market power				Economic outlook				Uncertainty
	Bank conc. (1)	Foreign bank share (2)	Connected firms (3)	Campaign financing (4)	GDP forecast (5)	GDP growth (6)	Bank ROA (7)	Δ Credit-to-GDP gap (8)	EPU index (9)
Pre-election quarter	-0.018	-0.039*	-0.035**	-0.038	-0.012	-0.018**	-0.018*	-0.033**	-0.049
	(0.030)	(0.021)	(0.014)	(0.036)	(0.011)	(0.008)	(0.009)	(0.012)	(0.036)
Measure	-0.017	-0.146	–	–	0.009*	0.129	0.002	0.004	0.001
	(0.051)	(0.098)			(0.005)	(0.108)	(0.003)	(0.004)	(0.020)
Pre-election × Measure	-0.013	0.035	0.016	0.087	-0.006**	-0.235**	-0.008**	-0.011**	0.022
	(0.050)	(0.042)	(0.042)	(0.220)	(0.002)	(0.111)	(0.003)	(0.006)	(0.034)
Observations	2,213	2,213	1,766	2,213	2,125	2,213	2,213	1,593	1,600
R^2	0.072	0.072	0.077	0.072	0.076	0.072	0.072	0.084	0.095
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Strikingly, I find that the interaction terms with these variables are statistically indistinguishable from zero at reasonable levels of confidence. In column (3), I use the share of politically connected firms (by market capitalization) constructed by Faccio (2006) as an indicator for linkages between private firms and the government. This measure is arguably more closely related to the theoretical notion of revolving doors, but is unfortunately not specific to the financial sector. Again, the interaction term is close to zero and has the "wrong" sign, indicating that higher connections mitigate the cycle. As a last check, I use legal limits to campaign financing rules as a measure of lobbying in column (4). More precisely, I construct a country-level index based on bans and limits on private income; regulations of spending; and reporting, oversight and sanctions from the Political Finance Database of the Institute for Democracy and Electoral Assistance. Again, the interaction term is clearly insignificant. Based on these admittedly imperfect proxies, the regulatory cycle thus does not appear to be more pronounced in countries where individual institutions are likely to have higher market power. One interpretation of this result is that financial sector lobbying may not be the driving force behind the cycle.

Next, I introduce measures that are designed to capture optimism about the current state of the economy in the quarter prior to elections. In column (5), I add the interaction term with the World Bank GDP forecast for the year of the election. The interaction term has negative coefficient of -0.006 and highly statistically significant. This implies that prudential regulation is more likely to ease prior to elections when forecasts about the future state of the economy are more optimistic. The coefficient implies a large economic effect: a one standard deviation increase in the growth forecast (about 3.05) implies a total decrease in sector-specific regulatory tightness of $-0.015 - 0.006 \times 3.05 \approx -0.033$, which is more than twice the mean of the dependent variable in the estimation sample of 0.012.[21] Next, I use the actual year-on-year GDP growth in column (6) as an optimism proxy. I find similar results. Because the health of the financial sector is highly procyclical, positive economic developments are also associated with higher bank profitability and credit growth. I thus introduce interactions with the banking system's return on asset (ROA) and changes in the credit-to-GDP gap (as calculated by the BIS) in columns (7) and (8). Again, the interaction terms on both are highly statistically significant and negative.

As a last hypothesis, I test whether the results can be explained by uncertainty. Jului and Yook (2012), for example, find that firm investment is lower in the run-up to elections, especially those with uncertain outcomes. Through a plethora of tests, they

[21] I find highly similar results using stock returns as an indicator of expectations about future economic activity (results available upon request).

show that this is most likely to reflect uncertainty about future policies. In principle, it is possible that this uncertainty also affects regulator decisions. In column (9), I thus introduce an interaction term with the standardized index of economic policy uncertainty (Baker et al., 2016).[22] The interaction term, however, introduces a *positive* sign and is far from conventional levels of statistical significance. If anything, this would imply that regulators are *more* likely to tighten prior to elections when uncertainty is high, which is inconsistent with the "waiting out" hypothesis. In the online appendix table 3.22, I show that this result is robust across a wide variety of specifications of the EPU index.

Taken together, the findings presented here suggest that the electoral cycle is highly procyclical: prudential regulation is less likely to tighten prior to elections when the state of the economy and its projected future path look bright. Particularly important is that these periods tend to coincide with increases in economy-wide leverage as measured by the credit-to-GDP gap, the very measure regulators are expected to pay considerable attention to. This is consistent with the interpretation that political pressures can deter regulators from reigning in the pro-cyclicality of financial sector risk taking. Uncertainty does not appear to play a role in mediating the regulatory cycle.

3.5 Does Central Bank Independence Reign in the Electoral Cycle?

A natural question that arises from documenting an electoral cycle in regulatory measures is whether better institutions play a mitigating role. In the case of monetary policy, a broad consensus holds that central bank independence can ease political economy concerns for policy makers (see e.g. Cukierman, 1992; Eijffinger and de Haan, 1996; Crowe and Meade, 2007). On the other hand, many of the cautionary tales regarding political pressures in the design of financial regulation are countries with arguably excellent institutions in international comparison, such as the United States, the United Kingdom, Germany, or Spain (Dagher, 2017; Calomiris and Haber, 2014). The latter would suggest that better institutions may be insufficient to "tame" regulatory cycles.

[22]Because the economic policy uncertainty index is only available for a sub-group of countries, I assign the value of the aggregate European EPU to EU countries for which I do not have data. I also assign the values for China to Taiwan. By increasing the sample size, this stacks the odds against finding a null result, but admittedly introduces noise. Table 3.22 shows that, in practice, this adjustment makes no difference to the results.

I challenge these competing hypotheses to an empirical test by introducing interaction terms with the central bank independence measure from Garriga (2016) in table 3.7. Strikingly, the results in columns (1) and (3) suggest that both for changes in the prudential index as well as sector-specific capital buffers, higher central bank independence does not consistently mitigate the electoral cycle. The point estimates are far from statistically significant at conventional levels ($t = -0.36$ and $t = 1.43$, respectively) In fact, the estimate of the interaction term $Pre-election\ quarter \times CBI$ for the prudential index is *negative*, indicating that countries with more independent central banks are less likely to tighten regulation in the run-up to elections.

A potential explanation of this finding could be that central banks differ markedly in their decision making power over (macro)prudential policy. I thus restrict the sample to countries where the central bank has at least 50% voting power for macroprudential decisions in columns (2) and (4), according to the classification in Cerutti et al. (2015). As it turns out, this yields very similar estimates. If anything, the *negative* interaction term for the prudential index is now even more precisely estimated ($t = -1.38$); for the sector-specific capital buffer, the t-statistic drops to 1.02.

Could it be that central bank independence has no effect in my sample at all? As a counter-factual test, I build on previous studies (e.g. Block, 2002) and replace the dependent variable with three simple measures of monetary policy: the policy rate, base money growth, and the change in the exchange rate. This is equivalent to asking the question whether, in the same sample, central bank independence is a moderating factor for political cycles in *monetary* policy.[23] This builds on evidence in Clark and Hallerberg (2000) for electoral cycles in monetary policy in OECD economies (see also Garriga (2016)). Importantly, recall the finding from 3.2.3 that the measures of monetary policy I use here clearly do *not* exhibit an electoral cycle on average.[24]

[23]Note that the Eurozone countries are excluded for the regressions of the central bank rate and exchange rate, because national authorities do not have influence over these in a monetary union. I do not exclude the Eurozone countries for base money growth, because central bank reserves are still under some control of the national authorities, e.g. through the use of reserve requirements (Cerutti et al., 2015). However, the point estimate is almost equivalent when excluding the Eurozone countries (unreported).

[24]More accurately, 3.2.3 shows that there is no electoral cycle in base money growth, the exchange rate, and a money market interest rate (which are used as control variables throughout). Because the money market interest rate and central bank policy rate have a Pearson correlation coefficient of 0.83, I do not find an average electoral cycle for the policy rate, either ($t = 0.38$). Also note that excluding the money market rate as a control variable makes no difference to the results in table 3.7 or 3.8.

Table 3.7: DOES CENTRAL BANK INDEPENDENCE MODERATE THE ELECTORAL CYCLE?

Note: This table shows coefficients from estimating equation 3.1 using OLS. The dependent variable is the change in the prudential index in columns (1) and (2), the change in sector-specific capital buffers in columns (3) and (4), the central bank rate in column (5), base money growth in column (6), and change in the exchange rate in column (7). "High CB Power" refers to countries where the central bank has at least a 50% decision share in macroprudential policy decisions as classified by Cerutti et al. (2015). All estimations include four lags of the dependent variable and all other control variables as described in the text. The † in columns (6) and (7) indicates that base money growth and the exchange rate are excluded from the control set, respectively. The sample is restricted to countries outside of the Euro-zone in columns (5) and (7), because the national central banks have no power over the policy and exchange rate in a monetary union. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Δ Prudential Index		Δ Sector-specific Capital Buffer		Monetary Policy Measures		
	Full Sample (1)	High CB Power (2)	Full Sample (3)	High CB Power (4)	CB Rate (5)	Base Money Growth (6)	Δ Exchange Rate (7)
Pre-election quarter	0.029	0.255	-0.104*	-0.071	-0.545*	0.111***	0.044**
	(0.160)	(0.180)	(0.060)	(0.048)	(0.313)	(0.040)	(0.021)
CBI	0.319	0.443	0.348***	0.400***	0.539	0.029	0.010
	(0.218)	(0.334)	(0.109)	(0.131)	(0.681)	(0.074)	(0.040)
Pre-election quarter × CBI	-0.081	-0.343	0.118	0.073	1.193*	-0.134**	-0.074**
	(0.228)	(0.249)	(0.082)	(0.071)	(0.679)	(0.065)	(0.035)
Observations	2,213	1,230	2,213	1,230	1,719	2,173	1,727
R^2	0.133	0.185	0.078	0.103	0.971	0.581	0.795
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes†	Yes†

Columns (5) through (7) present the results of this exercise. Strikingly, the interaction term $Pre-election\ quarter \times CBI$ is statistically significant in all three specifications. The coefficient signs suggest that central bank independence decreases electoral pressures for monetary policy. For the policy rate, the estimate of -0.545 for the pre-election dummy suggests that central banks are more likely to ease monetary policy prior to elections; however, the interaction term of 1.193 suggests that this effect is fully mitigated by a two standard deviation shift in central bank independence. Similarly, base money growth is higher before elections, but much less so in countries with highly independent central banks. Finally, the results in column (7) suggest that exchange rates are more likely to strengthen in quarters before elections, but considerably less so when central banks are more independent. Taken together, these results suggest that while central bank independence is effective in moderating *monetary* policy, it is not so for *prudential* policy.

Could it be that the model specification in 3.7 is introducing a downward bias in the interaction terms by assuming linear effects? I investigate this question in 3.8, which instead splits the estimation sample into terciles of central bank independence. This yields highly similar results: again, I find little evidence for a systematic mitigating effect of central bank independence on the electoral cycle in prudential regulation. Even in the top tercile of independence, the coefficient on $Pre-election\ quarter$ still has a coefficient of -0.01, which is around half of the average effect documented in 3.2. In contrast, measures of *monetary* policy clearly exhibit large differences in outcomes across the distribution of central bank independence. In the bottom tercile, upcoming elections are associated with significantly higher base money growth and appreciating exchange rates, while the estimated coefficient turns negative and close to zero in the top tercile.

In the online appendix, I conduct further validity checks. 3.18 splits the sample into terciles but drops all control variables and re-runs regressions of the individual prudential tools on the pre-election dummy. As above, this yields highly inconsistent coefficients across regulatory tools. The only consistent results appear to be that real estate capital buffers exhibit a *weaker* and concentration limits a *stronger* electoral cycle in countries with higher central bank independence. Table 3.19 uses a plethora of alternative measures of central bank independence from Garriga (2016) and Crowe and Meade (2007) (on which Garriga's data is based). Across all specifications, I find that the interaction term of pre-election quarters with central bank independence is statistically indistinguishable from zero.

In table 3.20, I further investigate whether differences in macroprudential policy institutions as classified by Edge and Liang (2017) are a source of heterogeneity. I

Table 3.8: Central Bank Independence – Tercile Sample Splits

Note: This table plots the estimated coefficients from regressions of different measures of prudential regulation and monetary policy on the pre-election quarter dummy, split by terciles of central bank independence. The columns under "Low = High?" report a χ^2-test for the equality of coefficients between the coefficient on the pre-election dummy in the first and third tercile, and the associated p-value. All regressions include the full set of controls introduced above, except for panel D which omits base money growth and panel E which omits the exchange rate (which are the respective dependent variables). All models further include country and year-quarter fixed effects as well as four lags of the dependent variable. The sample is restricted to countries outside of the Eurozone for the policy rate (panel C) and exchange rate (panel E), which are outside of the national central bank's control in a monetary union. Standard errors are clustered by country, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% level, respectively.

	CBI Tercile			Low = High?	
	Low	Medium	High	χ^2	*p-value*
Panel A: Prudential regulation index					
Pre-election quarter	-0.005	0.030	-0.066	0.673	*0.412*
	(0.069)	(0.071)	(0.046)		
Observations	684	703	826		
R^2	0.096	0.041	0.051		
Panel B: Sector-specific capital buffer					
Pre-election quarter	-0.065*	-0.022	-0.010	3.511*	*0.061*
	(0.031)	(0.023)	(0.008)		
Observations	684	703	826		
R^2	0.076	0.033	0.039		
Panel C: Central bank policy rate					
Pre-election quarter	-0.121	0.090	0.363	4.484**	*0.034*
	(0.116)	(0.104)	(0.271)		
Observations	504	523	692		
R^2	0.886	0.928	0.925		
Panel D: Base money growth					
Pre-election quarter	0.062**	0.008	-0.014	4.521**	*0.033*
	(0.024)	(0.056)	(0.026)		
Observations	684	703	786		
R^2	0.653	0.446	0.555		
Panel E: Δ Exchange rate					
Pre-election quarter	0.020**	0.006	-0.011	3.690*	*0.055*
	(0.008)	(0.009)	(0.008)		
Observations	504	523	700		
R^2	0.590	0.611	0.669		

find that the effects are strikingly homogenous across countries. The only noteworthy difference I find is that in countries where the Ministry of Finance has *no* vote over macroprudential decisions, the electoral cycle is *stronger*. If anything, this goes against the intuition that central bank independence mitigates political interference.

As a last exercise, I investigate whether other institutional variables matter. In particular, I draw on a standard set of variables that describe a country's institutional quality and level of democracy, and again interact these with the pre-election dummy. For brevity, the results are presented in table 3.21 in the online appendix. Similar to the effects of central bank independence, I find very limited evidence that institutions mitigate the systematic correlation of upcoming elections and the stance of prudential policy. For the sector-specific capital buffer, the results in columns (1) through (3) suggest that political stability, voice and accountability, and government effectiveness are helpful in mitigating electoral cycles. The implied magnitudes, however, are fairly small. Even for the most precisely estimated interaction, it takes almost a three standard deviation increase in the level of voice and accountability to undo the main effect of upcoming elections ($3 \times 0.018 \times 0.664 \approx 0.036$). The interaction terms on the prudential index are *negative* (albeit imprecisely estimated), if anything suggesting an adverse effect. Other standard measures of institutions or democracy do not have moderating power for regulatory cycles.

Overall, the results presented in this section suggest that better institutions – including higher central bank independence – do not appear to sufficiently insulate countries from electoral cycles in prudential regulation. At the same time, the estimates presented here suggest that central bank independence does mitigate cycles in *monetary* policy, consistent the previous literature (e.g. Eijffinger and de Haan, 1996).

3.6 Conclusion

Since the Great Financial Crisis of 2008-2009, central banks and financial regulators have focused on prudential measures to limit the build-up of systemic risk with the goal of preventing costly banking crises or soften their adverse impact. In this paper, I show that such prudential regulation exhibits a striking electoral cycle: regulatory tools were much less likely to be tightened (and somewhat more likely to be loosened) in the quarters preceding 207 elections across 58 countries between 2000 and 2014.

This electoral cycle in prudential tools is remarkably stable across countries and institutional regimes. Importantly, central bank independence does *not* appear to be an important moderating factor. This does not mean that central bank independence is irrelevant: monetary policy reacts much less strongly to upcoming elections in countries where monetary authorities are relatively independent.

The effect I document is not driven by strategic timing of elections and unlikely to be a relict of political cycles in other economic or financial conditions. Pre-election quarters do not appear to be systematically different from other periods in observable characteristics; further, the results are almost unchanged when conditioning on $country \times year$ effects. Importantly, I find that the correlation with upcoming elections is among the highest for the tightening of sector-specific capital buffers for residential mortgages and consumer credit that are likely to most directly affect the median voter. If this reflects the influence of politicians over regulators, it may be an effective way to sway voters: Antoniades and Calomiris (2018) show that voters punish Presidential candidates for a contraction in mortgage credit, but do not reward them for booms.

I also find that higher economic growth and future growth prospects, as well as an increase in the credit-to-GDP gap and bank profitability, all exacerbate the electoral cycle. Of course, the very point of macroprudential tools is to cushion banks against the build-up of risk during these boom times. Taken at face value, this suggests that political pressures can prevent efforts to decrease the pro-cyclicality of the financial sector.

The findings presented here call for more research into potential political limitations of current financial stability frameworks. It has been widely assumed that central bank independence would shield regulators from the most severe political pressures. This rests on the finding that independence leads to less politically-sensitive monetary policy decisions, a finding I confirm here. Could it be that things are different for targeted prudential tools that arguably affect the median voter much more directly? If corroborated by more evidence, this would raise the question whether stricter rules (rather than discretion) are the right recipe for the design of macroprudential policy.

Another potential policy implication might be that time-invariant limits to the business model of financial institutions – rather than time-varying changes – could serve as a backstop. To the extent that changing such limits require legislation that is considerably more involved and time-consuming than the calibration of prudential tools, they are likely to be more resilient to short-term political pressures. Haldane (2017) stresses that, in light of political economy questions, “there is a debate to be had ... about the appropriate degree of discretion to confer on regulators”. I hope that the empirical evidence presented here is a useful step in informing this debate.

3.7 Appendix

Table 3.9: TIGHTENING AND LOOSENING EPISODES, BY PRE-ELECTION QUARTER

Note: This table plots the number of tightening and loosening episodes for all prudential tools in the dataset of Cerutti et al. (2017a) that overlap with the election data criteria described above.

	Tightening episodes			Loosening episodes		
	Total	Pre-election quarters	Other quarters	Total	Pre-election quarters	Other quarters
Index measures						
Δ Prudential regulation index	327	22	305	165	15	150
Δ Sector-specific capital buffer	51	0	51	16	2	14
Individual regulatory tools						
Δ Real estate capital buffer	35	0	35	13	1	12
Δ Consumer credit capital buffer	8	0	8	2	1	1
Δ Other capital buffer	15	0	15	5	0	5
Δ General capital requirement	88	7	81	0	0	0
Δ Concentration limit	30	0	30	1	1	0
Δ Interbank exposure	24	0	24	1	0	1
Δ Loan-to-value ratio	45	9	36	17	3	14
Δ Reserve requirements (FC)	84	3	81	49	2	47
Δ Reserve requirements (LC)	106	7	99	146	11	135

Table 3.10: Descriptive Statistics of Estimation Sample

Note: This table presents descriptive statistics for the main variables used throughout the paper. The sample is the estimation sample from table 3.2, columns (2) and (7).

	Observations	Mean	Median	Std. Dev.
Index measures				
Δ Prudential regulation index	3190	0.057	0	0.392
Δ Sector-specific capital buffer	3190	0.012	0	0.188
Δ Individual regulatory tools				
Δ Real estate capital buffer	3190	0.006	0	0.123
Δ Consumer credit capital buffer	3190	0.002	0	0.059
Δ Other capital buffer	3190	0.004	0	0.094
Δ General capital requirement	2970	0.032	0	0.176
Δ Concentration limit	1793	0.015	0	0.131
Δ Interbank exposure	1043	0.019	0	0.144
Δ Loan-to-value ratio	1061	0.032	0	0.262
Δ Reserve requirements (FC)	3190	0.010	0	0.261
Δ Reserve requirements (LC)	3190	-0.008	0	0.324
Bank variables				
Bank capitalization (%)	2958	8.353	7.900	3.350
Lending concentration	3001	67.458	68.078	20.486
Cost to income ratio (%)	3121	56.837	56.732	14.976
Non-performing loans (%)	2951	5.394	3.200	5.854
ROA	3128	0.885	1.096	4.446
Z-score	3132	10.399	8.357	7.491
Foreign bank share (%)	2912	35.345	33.000	24.811
Macro variables				
Government exp./GDP	2898	0.174	0.183	0.047
Money market rate	3095	5.027	3.595	6.966
Base money growth	3052	0.427	0.100	15.506
Real credit growth	2892	0.086	0.057	0.194
Real GDP growth	2898	0.029	0.031	0.108
Δ Current account/GDP	2916	0.000	0	0.147
Total trade/GDP	2934	0.879	0.740	0.549
Investment/GDP	2886	0.227	0.222	0.045
Consumption/GDP	2886	0.582	0.578	0.086
Inflation	3082	0.044	0.030	0.063
Exchange rate (US$)	3058	311.945	3.631	1333.133

3.8 Online Appendix

3.8.1 Variable Construction

Details on Control Variables

The bank control variables come from the World Bank's Global Financial Development database, which in turn compiles data from many different sources. The sources of the macroeconomic control variables are the International Monetary Fund's (IMF) International Financial Statistics and the OECD (see table 3.11). Since the data availability is highly heterogeneous across different transformations of the variables, I combine these to maximize sample size. Because the coverage of the IMF is much broader, I start with their most commonly available variables, which are then further enriched with other versions as available. I then add the OECD data using the same procedure. For Argentina, I further obtain data on the ratio of consumption and gross fixed capital formation to GDP, as well as CPI growth, from the website of the Instituto Nacional de Estadística y Censos (indec).

Table 3.11: SOURCES OF CONTROL VARIABLES

	Description	Source
Bank Variables		
Bank capitalization (%)	Ratio of bank capital and reserves to total assets. Capital includes tier 1 capital and total regulatory capital.	World Bank GFD
Lending concentration	The asset share of a country's three largest banks, divided by total bank assets.	World Bank GFD
Cost to income ratio (%)		World Bank GFD
Non-performing loans (%)	The ratio of a country's non-performing loans to total outstanding loans.	World Bank GFD
ROA	The banking system's pre-tax return on assets.	World Bank GFD
Z-score	The Z-score captures the probability of default of a country's banking system by comparing its buffer (capitalization and returns) with the volatility of those returns.	World Bank GFD
Foreign bank share (%)	Percentage of the total banking assets that are held by foreign banks.	World Bank GFD
Macroeconomic Variables		
Government exp./GDP	Government expenditure scaled over GDP.	IMF IFS, OECD
Money market rate	A typical short-term money market interest rate.	IMF IFS, OECD
Base money growth	The year-on-year growth of central bank reserves or base money, a measure of monetary policy.	IMF IFS, OECD
Real credit growth	The inflation-adjusted year-on-year growth in financial sector claims on the private sector.	IMF IFS
Real GDP growth	The year-on-year growth in gross domestic product, adjusted for inflation.	IMF IFS, OECD
Δ Current account/GDP	The ratio of the current account to GDP.	IMF IFS, OECD
Total trade/GDP	The sum of total exports and imports, scaled over GDP.	IMF IFS, OECD
Investment/GDP	The ratio of gross fixed capital formation to GDP.	IMF IFS, OECD
Consumption/GDP	The ratio of private household consumption to GDP.	IMF IFS, OECD
Inflation	The year-on-year growth in a country's consumer price index.	IMF IFS, OECD
Exchange rate (US$)	A country's exchange rate vis-à-vis the US dollar.	IMF IFS, OECD
Central bank rate	The central bank's official policy rate or the market rate explicitly targeted by the central bank.	IMF IFS, BIS, National central banks

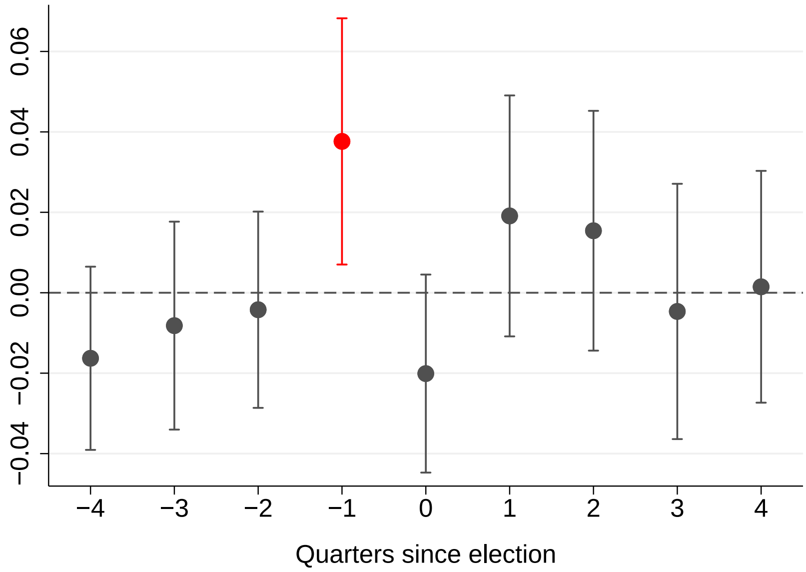
Table 3.13: SOURCES OF ADDITIONAL VARIABLES

	Description	Source
Connected firms	The share of politically connected firms (by market capitalization).	Faccio (2006)
Campaign financing	An index of legal limits on campaign financing. I construct the index as the sum of bans and limits on private income, regulations of spending, and reporting, oversight and sanctions in a given country. In particular, I count all regulations that are unambiguous ("Yes" in the raw data).	IDEA Political Finance Database
GDP Forecast	The World Bank's GDP forecast for the current year (technically, a "nowcast" of a country's current economic growth).	World Bank
Stock Return	The past year's annual stock market return, as calculated by the World Bank.	World Bank GFD
Δ Credit-to-GDP gap	The year-on-year change in a country's credit-to-GDP gap as calculated by the Bank for International Settlements. For the Eurozone countries, I add data provided by the ECB.	BIS, ECB
CBI (time-varying)	Time-varying measure of central bank independence. In the baseline analysis, I use the unweighted measure; for robustness, I also use the unweighted score. Original data is available only until 2013; I extend the series to 2014 by assuming no change in central bank independence in that year (using the data only until 2013, if anything, makes the results shown here stronger).	Garriga (2016)
CBI (time-invariant)	A measure of central bank independence corresponding to the year 2003. In the baseline analysis, I use the unweighted measure; for robustness, I also use the unweighted score.	Crowe and Meade (2007)
Macroprudential Committee	A dummy variable equal to 1 if a country has a macroprudential committee consisting of multiple members, and 0 otherwise.	Edge and Liang (2017)
CB in Charge of CCYB	A dummy variable equal to 1 if a country's national central bank is in charge of implementing changes in the countercyclical capital buffer (CCYB), and 0 otherwise.	Edge and Liang (2017)
MoF has Vote	A dummy variable equal to 1 if a country's Ministry of Finance has a vote on the macroprudential committee, and 0 otherwise.	Edge and Liang (2017)
High Central Bank Power	A dummy variable equal to 1 if a country's national central bank has at least a 50% decision share over macroprudential tools.	Cerutti et al. (2015)
Economic Policy Uncertainty	The index of economic policy uncertainty for all countries available at . I re-scale all country-level indeces to 1 in 2008q1. For EU countries that I do not have data on, I assign the aggregate European index. For Taiwan, I use the China index.	Baker et al. (2016)

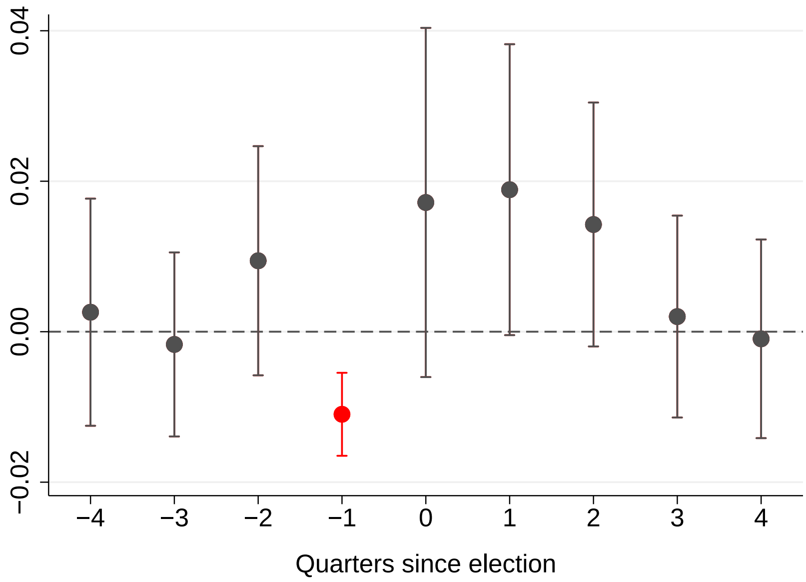
3.8.2 Additional Figures

Figure 3.4: ELECTIONS AND PRUDENTIAL REGULATION – ALL ELECTIONS

PANEL A: LOOSENING OF PRUDENTIAL REGULATION INDEX



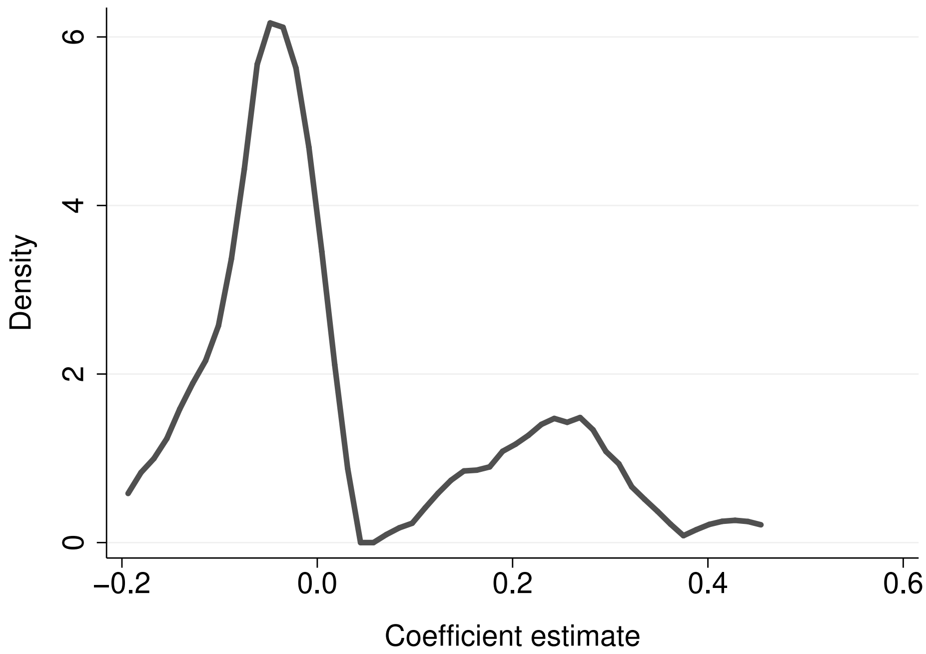
PANEL B: TIGHTENING OF SECTOR-SPECIFIC CAPITAL BUFFERS



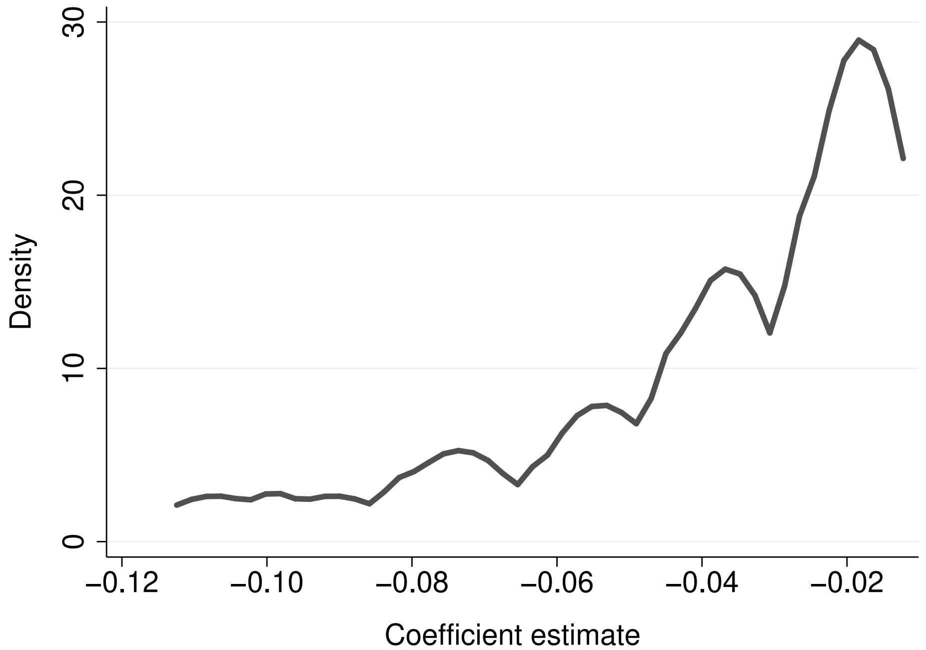
These figures show the dynamic effect of elections on the loosening of the prudential regulation index (Panel A) and the tightening of sector-specific capital buffers (Panel B). They plot the estimated OLS coefficients $\hat{\beta}_t$ of the regression $\Delta R_{it} = \alpha_i + \alpha_t + \sum_{t=-4}^{4} \beta_t E_{it} + \varepsilon_{it}$, where ΔR_{it} is one of the outcome dummies. The regressions also include four lags of the dependent variable. Standard errors are clustered by country.

Figure 3.5: Kernel Density of Pre-Election Dummy Estimation by Country

Panel A: Loosening of Prudential Regulation Index



Panel B: Tightening of Sector-Specific Capital Buffers



These figures show the kernel density distribution of the coefficient estimates $\hat{\beta}$ from running time series regressions of the type $\Delta R_t = \alpha + \beta E_t + \varepsilon_t$ for each country that has at least one change in the policy variable. ΔR_t refers to one of the outcome dummies.

3.8.3 Additional Tables

Table 3.15: Cross Tabulation of Regular and Irregular Elections

Notes: This table shows the proportion of elections that are regular. Elections are defined as "regular" if they are held within the time frame specified in a country's constitution or by legislative practice.

	Pre-Election Quarter	Other Quarters	Total
Irregular	54	2,983	3,073
Regular	153	0	153
Total	207	2,983	3,190

Table 3.16: CORRELATION MATRIX OF PRUDENTIAL TOOLS

Note: This table plots pairwise Pearson correlation coefficients of the prudential tools used in the sample. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1)	Δ Prudential regulation index	1										
(2)	Δ Sector-specific capital buffer	0.3110*	1									
(3)	Δ Real estate capital buffer	0.2811*	0.7854*	1								
(4)	Δ Consumer credit capital buffer	0.1286*	0.5338*	0.2144*	1							
(5)	Δ Other capital buffer	0.1743*	0.6402*	0.1311*	0.1672*	1						
(6)	Δ General capital requirements	0.3565*	-0.0101	0.0057	-0.0046	-0.0253	1					
(7)	Δ Concentration limit	0.2515*	-0.0072	-0.0068	-0.0043	-0.0028	0.0054	1				
(8)	Δ Interbank exposure	0.3317*	0.0648	0.0418	0.0059	0.0885*	0.0463	0.1575*	1			
(9)	Δ Loan-to-value ratio	0.5839*	0.0122	0.0169	-0.0021	-0.0052	0.0084	0.0918	-0.0056	1		
(10)	Δ Reserve requirements (FC)	0.4296*	0.0104	0.0077	0.0194	-0.0013	-0.0061	-0.0176	0.0143	-0.0059	1	
(11)	Δ Reserve requirements (LC)	0.5970*	0.0860*	0.0547*	0.0487*	0.0699*	-0.0564*	-0.0217	0.0083	0.0047	0.3509*	1

Table 3.17: TESTING FOR ELECTORAL CYCLES IN OTHER VARIABLES

Note: This table shows coefficients from estimating panel regressions of the type $C^k_{it} = \alpha + \beta E_{it} + \varepsilon_{it}$, where C^k_{it} is one of the control variables in vector C_{it} (shown in the left column). To make the estimated coefficients comparable, the dependent variable in each regression is standardized to have a mean of 0 and standard deviation of 1 and the sample is the estimation sample including all controls, as in columns (2) and (7) of table 3.2. The independent variable is the pre-election quarter dummy. The coefficients in the right column are from regressions that also include country and year-quarter fixed effects. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	No FE		Country, Time FE	
	$\hat{\beta}$	S.E.	$\hat{\beta}$	S.E.
Bank variables				
Bank capitalisation (%)	-0.006	(0.057)	0.002	(0.017)
Lending concentration	-0.007	(0.042)	-0.045*	(0.023)
Cost to income ratio (%)	-0.016	(0.041)	-0.043	(0.036)
Non-performing loans (%)	-0.027	(0.034)	-0.023	(0.029)
ROA	-0.047	(0.032)	-0.040	(0.034)
Z-score	-0.024	(0.032)	-0.025	(0.018)
Foreign bank share (%)	0.012	(0.035)	-0.006	(0.007)
Macro variables				
Government exp./GDP	0.097*	(0.055)	-0.011	(0.034)
Money market rate	-0.007	(0.031)	0.008	(0.018)
Base money growth	0.002	(0.002)	0.002	(0.002)
Real credit growth	-0.070	(0.047)	-0.050	(0.046)
Real GDP growth	0.035	(0.039)	0.058*	(0.034)
Δ Current account/GDP	-0.003	(0.012)	-0.002	(0.015)
Total trade/GDP	0.043	(0.030)	0.014	(0.011)
Investment/GDP	0.045	(0.082)	0.086	(0.066)
Consumption/GDP	-0.043	(0.060)	-0.007	(0.030)
Inflation	-0.017	(0.026)	0.001	(0.021)
Exchange rate (US$)	-0.074	(0.064)	0.004	(0.005)

Table 3.18: Central Bank Independence and the Electoral Cycle – by Tool

Note: This table shows coefficients from estimating equation 3.1 using OLS. The dependent variable is the change in the (macro)prudential tool on the left column. Each cell represents the estimated coefficient on the pre-election quarter dummy for each of the terciles of the time-varying central bank independence measure from Garriga (2016), extended to 2014 by assuming no change between 2013 and 2014 (which, however, makes no difference to the results). All regressions include country and year-quarter fixed effects, but no controls. Results are almost equivalent when excluding controls (unreported). Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	CBI Tercile			Low = High?	
	Low	Medium	High	χ^2	*p-value*
Δ Prudential regulation index	-0.007	-0.072	-0.030	0.175	*0.676*
Δ Sector-specific capital buffer	-0.053**	-0.012	-0.003	4.340**	*0.037*
Δ Real estate capital buffer	-0.030*	-0.007	0.004	4.170**	*0.041*
Δ Consumer credit capital buffer	-0.019	0.003	-0.002	1.418	*0.234*
Δ Other capital buffer	-0.002	-0.007	-0.004	0.052	*0.820*
Δ Capital requirements	-0.006	0.032	-0.016	0.384	*0.536*
Δ Concentration limit	-0.004	-0.014	-0.045**	4.015**	*0.045*
Δ Interbank exposure	-0.005	-0.005	-0.032	0.929	*0.335*
Δ Loan-to-value ratio	0.088	-0.105**	0.062	0.077	*0.782*
Δ Reserve requirements (FC)	-0.005	-0.051	-0.006	0.000	*0.987*
Δ Reserve requirements (LC)	0.027	-0.073	-0.002	0.679	*0.410*

Table 3.19: Central Bank Independence and the Electoral Cycle – Robustness

Note: This table shows coefficients from estimating equation 3.1 using OLS. The dependent variable is the change in the prudential index in panel A and the change in sector-specific capital buffers in panel B. All estimations include four lags of the dependent variable and all other control variables as described in the text. *Garriga* refers to the time-varying central bank independence measure taken from Garriga (2016), extended to 2014 by assuming no change between 2013 and 2014 (which, however, makes no difference to the results). *Crowe-Meade* refers to the time-invariant central bank independence measure from Crowe and Meade (2007), which refers to the year 2003. Columns (1) through (4) are using the full sample, while columns (5) through (8) are restricted to the sample of countries where the central bank has at least 50% decision rights over macroprudential tools as identified by Cerutti et al. (2015). Results are almost equivalent when excluding controls (unreported). Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Full Sample				High Central Bank Decision Share			
	Garriga CBI		Crowe-Meade CBI (2003)		Garriga CBI		Crowe-Meade CBI (2003)	
	Unweighted (1)	Weighted (2)	Unweighted (3)	Weighted (4)	Unweighted (5)	Weighted (6)	Unweighted (7)	Weighted (8)
Panel A: Prudential regulation index								
Pre-election quarter	0.029	0.041	0.062	0.066	0.255	0.273	0.157	0.166
	(0.160)	(0.141)	(0.100)	(0.109)	(0.180)	(0.192)	(0.132)	(0.136)
CBI	0.319	0.380**	–	–	0.443	0.740**	–	–
	(0.218)	(0.160)			(0.334)	(0.293)		
Pre-election quarter × CBI	-0.081	-0.096	-0.134	-0.143	-0.343	-0.352	-0.209	-0.227
	(0.228)	(0.189)	(0.134)	(0.151)	(0.249)	(0.247)	(0.168)	(0.179)
Observations	2,213	2,213	2,163	2,163	1,230	1,230	1,180	1,180
R^2	0.133	0.134	0.132	0.132	0.185	0.188	0.186	0.186
Panel B: Sector-specific capital buffer								
Pre-election quarter	-0.104*	-0.102	-0.061	-0.068	-0.071	-0.086*	-0.065*	-0.070*
	(0.060)	(0.063)	(0.038)	(0.041)	(0.048)	(0.045)	(0.037)	(0.038)
CBI	0.348***	0.184	–	–	0.400***	0.288	–	–
	(0.109)	(0.139)			(0.131)	(0.232)		
Pre-election quarter × CBI	0.118	0.110	0.050	0.064	0.073	0.089	0.063	0.071
	(0.082)	(0.083)	(0.047)	(0.053)	(0.071)	(0.062)	(0.046)	(0.049)
Observations	2,213	2,213	2,163	2,163	1,230	1,230	1,180	1,180
R^2	0.078	0.075	0.074	0.074	0.103	0.099	0.099	0.099

Table 3.20: Do Macroprudential Policy Institutions Moderate the Electoral Cycle?

Note: This table shows coefficients from estimating equation 3.1 using OLS. The dependent variable is the change in the prudential index in panel A and the change in sector-specific capital buffers in panel B. All estimations include four lags of the dependent variable. The sample is split using data from Edge and Liang (2017) depending on whether a country has a macroprudential committee (columns 1 through 3); the central bank is in charge of a counter-cyclical capital buffer (columns 4 through 6); and whether the country's Ministry of Finance has a vote in macroprudential decisions (columns 7 through 9). The columns "Diff. (p-value)" report the p-value of a χ^2-test of the equality of coefficients for the pre-election quarter dummy across the respective models. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Macroprudential Committee			Central Bank in Charge of CCYB			Ministry of Finance has Vote		
	Yes	No	Diff. (p-value)	Yes	No	Diff. (p-value)	Yes	No	Diff. (p-value)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Prudential regulation index									
Pre-election quarter	-0.031	-0.069	*0.585*	-0.057	-0.049	*0.900*	-0.050	-0.038	*0.838*
	(0.034)	(0.065)		(0.049)	(0.038)		(0.038)	(0.046)	
Observations	2,035	825		1,430	1,485		1,705	1,155	
R^2	0.127	0.229		0.147	0.130		0.125	0.193	
Panel B: Sector-specific capital buffer									
Pre-election quarter	-0.020*	-0.019	*0.955*	-0.022	-0.026	*0.822*	-0.008	-0.041*	*0.156*
	(0.011)	(0.019)		(0.018)	(0.017)		(0.006)	(0.023)	
Observations	2,035	825		1,430	1,485		1,705	1,155	
R^2	0.057	0.127		0.091	0.054		0.073	0.092	

Table 3.21: Other Institutions and the Electoral Cycle in Prudential Regulation

Note: This table shows coefficients from estimating equation 3.1 using OLS. The dependent variable is the change in the prudential index in Panel A and the change in sector-specific capital buffers in column Panel B. All estimations include four lags of the dependent variable as covariates and all other control variables as described in the text. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Political stability (1)	Voice and accountability (2)	Government effectiveness (3)	Economic freedom (4)	Democracy (Polity) (5)	Civil liberties (6)	Political rights (7)
Panel A: Δ Prudential regulation index							
Pre-election quarter	-0.011	-0.001	-0.011	0.333	0.084	-0.037	-0.025
	(0.043)	(0.076)	(0.069)	(0.416)	(0.142)	(0.081)	(0.075)
Measure	0.093**	0.046	0.048	0.026	0.005	-0.038	-0.016
	(0.044)	(0.078)	(0.073)	(0.055)	(0.006)	(0.025)	(0.022)
Pre-election quarter × Measure	-0.020	-0.023	-0.009	-0.049	-0.015	0.008	0.001
	(0.040)	(0.069)	(0.049)	(0.055)	(0.015)	(0.043)	(0.044)
Observations	2,096	2,096	2,096	2,213	2,101	2,213	2,213
R^2	0.137	0.135	0.135	0.132	0.138	0.132	0.132
Panel B: Δ Sector-specific capital buffer							
Pre-election quarter	-0.024***	-0.035***	-0.032***	-0.119	-0.036	-0.013	-0.019
	(0.007)	(0.010)	(0.010)	(0.079)	(0.023)	(0.018)	(0.014)
Measure	-0.017	-0.045	-0.064*	-0.017	-0.001	0.015	0.007
	(0.030)	(0.036)	(0.036)	(0.023)	(0.002)	(0.015)	(0.013)
Pre-election quarter × Measure	0.012*	0.018**	0.012*	0.013	0.001	-0.007	-0.004
	(0.007)	(0.009)	(0.007)	(0.011)	(0.003)	(0.006)	(0.005)
Observations	2,096	2,096	2,096	2,213	2,101	2,213	2,213
R^2	0.073	0.073	0.074	0.072	0.075	0.072	0.072
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.22: ECONOMIC POLICY UNCERTAINTY AND THE ELECTORAL CYCLE IN PRUDENTIAL REGULATION

Note: This table shows coefficients from estimating equation 3.1 using OLS. The dependent variable is the change in the prudential index in Panel A and the change in sector-specific capital buffers in column Panel B. The data of economic policy uncertainty is from ()Baker2016. In columns (3) and (4), I exclude the EU countries and Taiwan, for which I assign the aggregate European and Chinese EPU in the baseline estimation, respectively. All estimations include four lags of the dependent variable as covariates and all other control variables as described in the text. Standard errors are clustered by country, with ***, **, and * denoting statistical significance at the 1%, 5%, and 10% level, respectively.

	Baseline EPU		Baseline EPU No EU/TWN		Log(EPU)		Δ EPU	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Prudential regulation index								
Pre-election quarter	-0.054	0.091	-0.025	0.079	-0.047	-0.043	-0.019	0.671
	(0.130)	(0.115)	(0.161)	(0.149)	(0.043)	(0.043)	(0.559)	(0.571)
EPU	0.002	0.001	0.018	-0.005	-0.007	-0.008	0.003	-0.003
	(0.028)	(0.042)	(0.031)	(0.046)	(0.024)	(0.039)	(0.030)	(0.037)
Pre-election quarter × EPU	0.003	-0.161	-0.031	-0.147	-0.031	-0.060	-0.007	-0.154
	(0.132)	(0.129)	(0.168)	(0.167)	(0.063)	(0.069)	(0.117)	(0.123)
Observations	2,121	1,600	1,021	832	2,105	1,588	2,121	1,600
R^2	0.125	0.129	0.145	0.171	0.127	0.132	0.125	0.130
Panel B: Sector-specific capital buffer								
Pre-election quarter	-0.054*	-0.049	-0.049	-0.053	-0.027**	-0.028**	-0.099	-0.186
	(0.028)	(0.036)	(0.032)	(0.042)	(0.013)	(0.012)	(0.137)	(0.130)
EPU	-0.002	0.001	-0.002	0.001	0.011	0.007	-0.010	-0.011
	(0.010)	(0.020)	(0.008)	(0.018)	(0.014)	(0.022)	(0.019)	(0.027)
Pre-election quarter × EPU	0.030	0.022	0.011	0.020	0.002	-0.002	0.015	0.033
	(0.022)	(0.034)	(0.026)	(0.042)	(0.014)	(0.024)	(0.029)	(0.026)
Observations	2,121	1,600	1,021	832	2,105	1,588	2,121	1,600
R^2	0.064	0.095	0.127	0.189	0.064	0.096	0.064	0.095
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls (18)		Yes		Yes		Yes		Yes

Table 3.23: Types and Number of Elections, by Country

	Tightening		Loosening		Type of	Number of
Country	$\Delta Index$	$\Delta SSCB$	$\Delta Index$	$\Delta SSCB$	Election	Elections
Argentina	9	0	13	2	Presidential	3
Australia	5	1	1	1	Legislative	5
Austria	2	0	2	0	Legislative	4
Belgium	1	0	1	0	Legislative	4
Brazil	15	5	9	4	Presidential	3
Bulgaria	11	2	5	1	Legislative	4
Canada	6	0	2	0	Legislative	5
Chile	1	0	2	0	Presidential	3
Colombia	3	0	2	0	Presidential	4
Croatia	11	2	5	0	Legislative	4
Czech Rep.	2	0	2	0	Legislative	4
Denmark	4	0	2	0	Legislative	4
Estonia	5	1	5	2	Legislative	3
Finland	2	0	1	0	Legislative	3
France	6	0	2	0	Presidential	3
Germany	3	0	0	0	Legislative	4
Greece	1	0	1	0	Legislative	5
Hungary	3	0	6	0	Legislative	4
Iceland	3	0	6	0	Legislative	4
India	17	6	9	2	Legislative	3
Indonesia	9	0	1	0	Presidential	3
Ireland	4	2	1	0	Legislative	3
Israel	8	4	0	0	Legislative	4
Italy	2	0	1	0	Legislative	4
Japan	2	0	0	0	Legislative	5
Latvia	9	0	9	3	Legislative	4
Lebanon	4	0	2	0	Legislative	4
Lithuania	3	0	3	0	Legislative	4
Malaysia	7	2	2	0	Legislative	3
Malta	2	0	3	0	Legislative	3
Mexico	3	0	0	0	Presidential	3
Mongolia	4	0	1	0	Legislative	4
Netherlands	6	0	1	0	Legislative	5
New Zealand	2	0	0	0	Legislative	5
Nigeria	6	1	2	0	Presidential	3
Norway	5	1	2	2	Legislative	4
Peru	15	1	6	0	Presidential	4
Philippines	13	1	6	0	Presidential	2
Poland	6	3	2	0	Legislative	4

Table3.23: Types and Number of Elections, by Country (continued)

	Tightening		Loosening		Type of	Number of
Country	$\Delta Index$	$\Delta SSCB$	$\Delta Index$	$\Delta SSCB$	Election	Elections
Portugal	3	0	2	0	Legislative	4
Romania	9	0	8	0	Legislative	4
Russia	14	1	5	0	Presidential	4
Serbia	15	2	11	1	Legislative	6
Singapore	10	0	0	0	Legislative	3
Slovakia	2	0	5	0	Legislative	4
Slovenia	3	1	1	0	Legislative	5
South Africa	2	0	0	0	Legislative	3
South Korea	10	1	4	0	Presidential	3
Spain	3	1	4	0	Legislative	4
Sweden	8	2	0	0	Legislative	4
Switzerland	5	3	0	0	Legislative	3
Taiwan	8	0	4	0	Presidential	4
Thailand	7	4	3	0	Legislative	5
Turkey	16	3	4	0	Legislative	3
Ukraine	4	0	8	0	Presidential	2
United Kingdom	3	0	0	0	Legislative	3
United States	2	0	0	0	Presidential	4
Uruguay	5	1	3	0	Legislative	2
Total	349	51	180	18		217

Table 3.24: **LIST OF ELECTIONS IN ESTIMATION SAMPLE**

Country	Quarter	Country	Quarter	Country	Quarter	Country	Quarter
Argentina	2003q4	Germany	2002q3	Malaysia	2013q2	Singapore	2001q4
Argentina	2007q4	Germany	2005q3	Malta	2003q2	Singapore	2006q2
Argentina	2011q4	Germany	2009q3	Malta	2008q1	Singapore	2011q2
Australia	2001q4	Germany	2013q3	Malta	2013q1	Slovakia	2002q3
Australia	2004q4	Greece	2000q2	Mexico	2000q3	Slovakia	2006q2
Australia	2007q4	Greece	2004q1	Mexico	2006q3	Slovakia	2010q2
Australia	2010q3	Greece	2007q3	Mexico	2012q3	Slovakia	2012q1
Australia	2013q3	Greece	2009q4	Mongolia	2001q2	Slovenia	2000q4
Austria	2002q4	Greece	2012q2	Mongolia	2005q2	Slovenia	2004q4
Austria	2006q4	Hungary	2002q2	Mongolia	2009q2	Slovenia	2008q3
Austria	2008q3	Hungary	2006q2	Mongolia	2013q2	Slovenia	2011q4
Austria	2013q3	Hungary	2010q2	Netherlands	2002q2	Slovenia	2014q3
Belgium	2003q2	Hungary	2014q2	Netherlands	2003q1	South Africa	2004q2
Belgium	2007q2	Iceland	2003q2	Netherlands	2006q4	South Africa	2009q2
Belgium	2010q2	Iceland	2007q2	Netherlands	2010q2	South Africa	2014q2
Belgium	2014q2	Iceland	2009q2	Netherlands	2012q3	Spain	2000q1
Brazil	2002q4	Iceland	2013q2	New Zealand	2002q3	Spain	2004q1
Brazil	2006q4	India	2004q1	New Zealand	2005q3	Spain	2008q1
Brazil	2010q4	India	2009q2	New Zealand	2008q4	Spain	2011q4
Bulgaria	2001q2	India	2014q2	New Zealand	2011q4	Sweden	2002q3
Bulgaria	2005q2	Indonesia	2004q3	New Zealand	2014q3	Sweden	2006q3
Bulgaria	2009q3	Indonesia	2009q3	Nigeria	2003q2	Sweden	2010q3
Bulgaria	2013q2	Indonesia	2014q3	Nigeria	2007q2	Sweden	2014q3
Canada	2000q4	Ireland	2002q2	Nigeria	2011q2	Switzerland	2003q4
Canada	2004q2	Ireland	2007q2	Norway	2001q3	Switzerland	2007q4
Canada	2006q1	Ireland	2011q1	Norway	2005q4	Switzerland	2011q4
Canada	2008q4	Israel	2003q1	Norway	2009q3	Taiwan	2000q1
Canada	2011q2	Israel	2006q1	Norway	2013q3	Taiwan	2004q1
Chile	2005q4	Israel	2009q1	Peru	2000q2	Taiwan	2008q1
Chile	2009q4	Israel	2013q1	Peru	2001q2	Taiwan	2012q1
Chile	2013q4	Italy	2001q2	Peru	2006q2	Thailand	2001q1
Colombia	2002q2	Italy	2006q2	Peru	2011q2	Thailand	2005q1
Colombia	2006q2	Italy	2008q2	Philippines	2004q2	Thailand	2007q4
Colombia	2010q2	Italy	2013q1	Philippines	2010q2	Thailand	2011q3
Colombia	2014q2	Japan	2000q2	Poland	2001q3	Thailand	2014q1
Croatia	2000q1	Japan	2003q4	Poland	2005q3	Turkey	2002q4
Croatia	2003q4	Japan	2005q3	Poland	2007q4	Turkey	2007q3
Croatia	2007q4	Japan	2009q3	Poland	2011q4	Turkey	2011q2
Croatia	2011q4	Japan	2012q4	Portugal	2002q1	Ukraine	2004q4
Czech Rep.	2002q2	Korea	Rep.	Portugal	2005q1	Ukraine	2010q1
Czech Rep.	2006q2	Korea	Rep.	Portugal	2009q3	United Kingdom	2001q3
Czech Rep.	2010q4	Korea	Rep.	Portugal	2011q2	United Kingdom	2005q2
Czech Rep.	2013q4	Latvia	2002q4	Romania	2000q4	United Kingdom	2010q2
Denmark	2001q4	Latvia	2006q4	Romania	2004q4	United States	2000q4
Denmark	2005q1	Latvia	2010q4	Romania	2008q4	United States	2004q4
Denmark	2007q4	Latvia	2011q3	Romania	2012q4	United States	2008q4
Denmark	2011q3	Lebanon	2000q3	Russia	2000q1	United States	2012q4
Estonia	2003q1	Lebanon	2005q2	Russia	2004q1	Uruguay	2004q4
Estonia	2007q1	Lebanon	2009q2	Russia	2008q1	Uruguay	2009q4
Estonia	2011q1	Lebanon	2010q2	Russia	2012q1		
Finland	2003q1	Lithuania	2000q4	Serbia	2000q4		
Finland	2007q1	Lithuania	2004q4	Serbia	2003q4		
Finland	2011q2	Lithuania	2008q4	Serbia	2007q1		
France	2002q2	Lithuania	2012q4	Serbia	2008q2		
France	2007q2	Malaysia	2004q1	Serbia	2012q2		
France	2012q2	Malaysia	2008q1	Serbia	2014q1		

Chapter 4

Busy Bankruptcy Courts and the Cost of Credit

4.1 Introduction

"Our judicial resources are strained. And the cost to society of an overburdened bankruptcy system ... is enormous."

– U.S. District Judge Barbara Lynn[1]

It is well known that legal frameworks play an important part in shaping the cost of financing across countries.[2] Creditor and property rights, in particular, have been associated with the development of larger and more sophisticated financial sectors.[3] Yet, surprisingly little is known about the implementation of such laws into practice, and how important the enforcement of legal frameworks is for financial contracts.

In this paper, I use exogenous changes to the caseload of US bankruptcy courts to evaluate the effect of judicial efficiency on ex-ante contracting. The main variation stems from the introduction of the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). BAPCPA was a substantial reform to consumer bankruptcy law in the US and made it considerably harder for individuals to default on their debts. In the two years following its implementation, the US bankruptcy system saw the largest drop in the number of bankruptcy filings since the first records in 1899: a drop in bankruptcy filings of around 55% between 2005 and 2007.[4] This drop considerably reduced the number of cases per bankruptcy court, and thus the workload

[1]As reported in the National Law Journal (http://www.nationallawjournal.com/id=1202431566285/Judiciary-Asks-Congress-for-More-Bankruptcy-Judges?slreturn=20170902154758).

[2]See the seminal work of La Porta et al. (1997), La Porta et al. (1998), Levine (1999), and Levine et al. (2000).

[3]This is documented by, among others, Djankov et al. (2007), Qian and Strahan (2007), Bae and Goyal (2009), Davydenko and Franks (2008), Haselmann et al. (2010), and Acharya et al. (2011).

[4]This assessment is based on the bankruptcy case data from the Federal Judicial Center (FJC), available at https://www.fjc.gov/history/courts/caseloads-bankruptcy-cases-1899-2016. There was one slightly larger year-on-year decrease in 1944, which was likely war-related. See figure 4.1.

of bankruptcy judges. Importantly, BAPCPA left the legal framework and filing of business bankruptcies largely unaffected, which provides a relatively clean setting to isolate a role for judicial efficiency.

Estimating the causal effect of judicial efficiency empirically is a challenging task. As an example, consider the cross-sectional relationship between loan terms and the caseload per judge across bankruptcy districts in the United States in figure 4.6. A lower caseload is correlated with lower spreads and higher maturities, consistent with previous findings in Djankov et al. (2008) and Jappelli et al. (2005). However, it is impossible to infer from this correlation whether judicial efficiency has a causal effect on credit contracts, because areas with busy courts likely also differ in other aspects. This is particularly true for studies using cross-country variation, where more efficient enforcement and better loan outcomes are likely jointly determined by differences in bankruptcy codes (e.g. Favara et al., 2017). As a result, it remains fundamentally unclear whether more efficient courts themselves matter *in isolation*, or whether they are merely an outcome of other factors.

Compared to the previous literature, I am able to plausibly assess the quantitative importance of judicial efficiency under mild identifying assumptions by making use of exogenous variation within and across US bankruptcy districts in a difference-in-differences set up. By combining quasi-random exposure to BAPCPA with predetermined borrower characteristics, I can further assess to which extent this is driven by the supply-side factors rather than potentially unobserved local demand. In my setting, I find that the entire effect of an exogenous drop in bankruptcy court caseload works through an outward shift in credit supply.

To identify the effect of BAPCPA, I exploit cross-sectional exposure to the reform generated by the share of non-business bankruptcies that courts handled prior to its enactment. Because BAPCPA almost exclusively targeted consumer cases, there is a strong linear relationship between the share of non-business filings in a bankruptcy district's total caseload and the subsequent drop in the caseload per judge (Iverson, 2016). The share of consumer cases is highly persistent and largely driven by the concentration of economic activity across states. As a result, the non-business caseload share is orthogonal to observable borrower and county characteristics *within* states, and shows only a handful of significant geographical correlates *across* states. This creates exogenous variation in the impact of BAPCPA across bankruptcy districts under mild identifying assumptions.

I find that busy bankruptcy courts matter substantially for the cost and maturity structure of credit contracts. In the baseline specification, a drop of 91 hours of workload per year (or two work-weeks) for the average bankruptcy judge decreases interest rate spreads by around 16 basis points and increases loan maturities by around 7%. Because BAPCPA led to the largest recorded drop in bankruptcy filings, my estimates imply that the reform decreased spreads by 40 basis points and increased maturities by 13%. In the data, the effects occur contemporaneously with the actual drop in court caseload in the beginning of 2006 when the uncertainty over the impact of BAPCPA was lifted. This is consistent with creditors pricing in higher expected recovery values on loan contracts in real time with observable changes to court efficiency. Other legislative steps towards the reform had no noticeable effect on loan terms; changes to court backlog, not the reform itself, are driving my results.

Importantly, the methodology I employ lets me rule out a host of alternative explanations. First, my estimates are unlikely to be driven by relaxed credit conditions in the run-up to the 2007-08 financial crisis. A bankruptcy district's exposure to BAPCPA is largely uncorrelated with exposure to the housing boom, particularly within states. I also find that the change in loan terms persisted with a similar magnitude until the end of the sample in 2012: this suggests that the boom and bust in mortgage credit is unlikely to be the driving factor. Excluding possibly securitized (CLO) loans and the construction and nontradable industries also makes little difference. Second, I exploit heterogeneity in firm characteristics before the reform, which allows me to compare loan contracts within the *same* bankruptcy district in the *same* year. If borrowers cannot credibly pledge future cash flows as collateral, judicial efficiency should affect ex-ante contract terms by increasing higher expected recovery values to creditors in case a borrower defaults. Consistent with this intuition, I find that the post-BAPCPA caseload drop had substantially larger effects for borrowers with higher default risk and lower expected liquidation values. The findings are also unlikely to reflect an unobserved improvement in borrower fundamentals: a higher share of non-business cases has no predictive power for the number of firm bankruptcies following the implementation of BAPCPA, or during the Great Recession. Third, my results are not driven by minor provisions of the reform that were aimed at corporate bankruptcies. A careful examination of these legal changes allow me to eliminate potentially affected firms from the sample; if anything, I find more pronounced effects among firms who saw no change in their bankruptcy framework.

The relatively clean setting I study enables me to back out a rough estimate of the social costs of judicial inefficiency and the returns associated with hiring new judges. While it requires a set of simplifying assumptions, changes to interest rate spreads (a price term) and loan maturities translate naturally into a macroeconomic cost: the US-wide interest burden of non-financial corporations. Higher spreads arising from judicial inefficiency benefit neither creditors (who use it to compensate for lower expected returns) nor borrowers (who have to devote more of their income to interest payments); as such, they are a classic case of a deadweight loss. Drawing on work on debt servicing costs from the Bank for International Settlements (Drehmann et al., 2015), I calculate the savings and costs associated with resolving "excessive" court backlog.

This exercise suggests that the historic drop in the caseload per judge following BAPCPA saved US non-financial corporates between \$8.1 and \$15.3 billion in interest payments per year. Using two simple approaches, I further show that reducing the burden on bankruptcy judges may still be worthwhile today: the estimated social costs of busy courts are at least \$670 million per year. These costs could be reduced by adding a few judgeships in key districts, for which my approach uncovers the highest return to judges. The average implied fiscal multipliers of hiring additional judgeships are above 100, which suggests that judicial efficiency presents a potentially sensible area for government spending.

My paper builds on a large literature that highlights the importance of legal frameworks in economic outcomes. First, and most directly, my work extends a few papers that focus on the enforcement of existing laws, rather than legal reforms. Djankov et al. (2008) use a representative bankruptcy case for 88 countries and show that higher debt enforcement efficiency is associated with higher credit market development. Jappelli et al. (2005) show a negative correlation of judicial efficiency and interest rate spreads for Italian provinces. However, the purely cross-sectional nature of their empirical analysis makes it difficult to infer how important enforcement is in a quantitative sense compared to other predictors. I contribute to answering this question by using plausibly exogenous variation in judicial efficiency. In a recent paper, Favara et al. (2017) show that riskier firms invest and grow less in countries with better debt enforcement, and have higher equity volatility. Most directly, I build on the insight of Iverson (2016) that BAPCPA constituted a shock to the US bankruptcy courts with the highest pre-reform share of non-business bankruptcies. Iverson shows that the reduction in court congestion decreased the time firms spent in court (as a function of firm size) and lowered bank charge-offs for business lending. However, he does not address the question whether this affected ex-ante contracting or consider the social costs. Broadly related is also the work by Schiantarelli et al. (2016), who show that poor enforcement increases the incentive of borrowers to default.

Second, I add to recent papers that analyze how the effect of bankruptcy reforms depends on pre-existing judicial efficiency. Ponticelli and Alencar (2016) and Rodano et al. (2016) show in the context of Brazil and Italy, respectively, that financial reforms have interactions with the *ex-ante* functioning of court systems. Their findings, however, do not tell us whether judicial efficiency *per se* has an independent effect. Because court backlog is partially an outcome of existing legal frameworks, major reforms should have a larger impact where judges are particularly overburdened even if the backlog itself had no effect.

Third, this paper adds to the literature on legal determinants of credit contracts more broadly. Qian and Strahan (2007), Bae and Goyal (2009), and Laeven and Majnoni (2005) show that better property rights, creditor rights, and legal institutions are associated with lower loan spreads. Vig (2013) studies a legal reform in India which increased creditor rights and decreased the delay between default and liquidation. More broadly, my work is also related to a large literature highlighting that bankruptcy frameworks have heterogeneous effects (e.g. Gropp et al., 1997; Acharya and Subramanian, 2009; von Lilienfeld-Toal et al., 2012; Giovanni et al., 2012; Hackbarth et al., 2015; Cerqueiro et al., 2017; Haselmann et al., 2010). This paper also speaks to papers looking at how bankruptcy law is enforced in practice, such as Bris et al. (2006).

The remainder of the paper is structured as follows. In section 4.2, I discuss the institutional background governing bankruptcy in the United States generally and the Bankruptcy Abuse and Protection Act of 2005 in particular. Section 4.3 introduces the data and variables. Section 4.4 introduces the identification strategy. In section 4.5, I show the main results. Section 4.6 provides a rough estimation of the implied social costs of judicial inefficiency. Section 4.7 concludes.

4.2 Background: Bankruptcy Courts in the United States

4.2.1 Courts and Judges

Bankruptcy courts in the United States are units of the district courts operating across 90 judicial districts in the mainland US.[5] 27 states only have a single bankruptcy district, usually due to their smaller size or low population density. The decision-making power over bankruptcy cases lies with the bankruptcy judge. As of September 2012, there were 350 bankruptcy judgeships in the United States, out of which 34 were temporary. Judges have full authority over their cases, e.g. whether debtors are eligible to file for bankruptcy in a district or should receive debt relief. In the case of Chapter 11 filings, bankruptcy judges are responsible for confirming or disapproving plans of reorganization and thus whether firms should be reorganized or liquidated. In particular, motions to dismiss cases or convert them to a Chapter 7 liquidation are a crucial

[5] In Arkansas, the Western and Eastern districts share bankruptcy judges, so I treat them as a single district.

decision for the fate of debtors firms. Bankruptcy judges in the United States are appointed for 14 years by the respective court of appeals in their district, as governed by 28 U.S.C. §152.

4.2.2 Measuring Judicial Efficiency

The number of bankruptcy judgeships in the United States has increased by 53% between 1980 and 2010. This strongly contrasts with the staggering increase of 381% in the total number of bankruptcy filings over the same time period. As a result, the number of cases the average bankruptcy judge had to handle in 2010 is 3.1 times the number of 1980. One of the reasons for this trend is that the creation of additional judgeships requires the passage of a bill determined by the House of Representatives as well as the Senate. The last change to the number of *permanent* bankruptcy judgeships was implemented through the Bankruptcy Judgeship Act of 1992. In October 2017, President Trump officially signed the Bankruptcy Judgeship Act of 2017, which effectively adds four permanent judgeships and extends a number of temporary ones. In 2005, as part of BAPCPA, 28 additional temporary judgeships were created, one of which was discontinued in 2010.

The calculation above, however, does not take into account that different types of bankruptcy cases differ markedly in the time they take to process. Consumer bankruptcies in particular are usually settled without the debtor appearing in court, especially in the case of Chapter 7 cases. Naturally, this leads to a considerably lower time effort on part of the bankruptcy judge. To accommodate these differences, the Judicial Conference of the United States makes use of a weighting system to determine the caseload of a bankruptcy district. These generic case weights are based on a 1989 study by Bermant et al. (1991) and are still in use today.

Table 4.8 in the appendix shows the approximated number of hours judges spend on different types of consumer and business filings, which are the weights used in the calculation of court caseload. Chapter 11 filings are clearly the most time-intensive, with the average case taking around 8 hours. On the other extreme, consumer Chapter 7 cases only take an average of 6 minutes. While there is considerable heterogeneity underlying these averages, these numbers reflect the best practice of the Judicial Conference in evaluating the caseload of US bankruptcy courts. In the remainder of the paper, I thus use the time-weighted number of cases filed per bankruptcy judge in a district as a measure of judicial efficiency.

The relative share of different bankruptcy types changes very little within districts, a fact I exploit in the identification strategy introduced below. An intuitive interpretation of the weighted caseload variable is the number of hours per year a judge spends on bankruptcy cases alone. Note that this estimate does not include other tasks, such as adversary proceedings, court administration, and travel. According to Bermant et al. (1991), work on cases and proceedings make up only 56.5% of a judge's time; 29% are spent on court administration, work-related travel, and other judicial activities, and 14.5% on personal time during the work day.

Figure 4.1 plots the development of the weighted caseload per judge over time. In 1980, judges had an average workload of 503 hours per year. This increased to 1,102 hours per year by the end of 2004. For most of the period, the caseload hovered around 1,000 hours, before BAPCPA led to an unprecedented drop just before the Great Recession. Due to the magnitude of the economic downturn, cases spiked in the aftermath of the 2007-2008 financial crisis, leading to a sharp increase in judge workload. The high workload is a well-known fact in the judiciary and yields yearly recommendations by the Judicial Conference to create additional judgeships.[6] When asked about the court caseload by the Huffington Post, one district judge summarized the situation as follows:

"For the most part, we've just resigned ourselves that this is our fate and there's nothing we can do about it. We've complained. We've begged. We've cajoled. We've done everything you can humanly do to try to get additional judgeships."[7]

A major reason why these requests have not been granted is that political polarization in Congress has made it extraordinarily difficult to pass the required bills. BAPCPA, which I discuss in more detail in the next section, is a prime example.

4.2.3 Bankruptcy Abuse Prevention and Consumer Protection Act of 2005

On April 20, 2005 President George W. Bush signed into law the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005, which made it considerably more difficult for consumers to file for bankruptcy under Chapter 7. The bill had a long legislative history: originally drafted in 1997, the law was de-facto vetoed by President Clinton in 2000 and then introduced in each Congress, only to be shelved over disagreements between Republicans and Democrats. BAPCPA was introduced in the senate on February 1, 2005; and passed Senate on March 10 (and the House of Representatives on April 14). President Bush signed the bill shortly after on April 20, 2005. However, most provisions only applied to cases filed on or after October 17, 2005.

[6] See the US Courts website for news releases on the Judicial Conference recommendations.

[7] As reported here by the Huffington Post online.

Support for the act came mainly from banks and credit card companies, who expected to benefit from "a stronger hand in recovering unpaid consumer debt".[8] BAPCPA introduced a number of amendments to steer debtors with incomes above the state median from Chapter 7 towards Chapter 13, which required them to pledge future income and allowed for less debt forgiveness. If a filer's income is above the state median, she is now subject to a "means test". A major amendment was that post-BAPCPA, filings can be dismissed based on a "presumption of abuse", depending on the outcome of the means test or through a finding of bad faith. BAPCPA also changed various other aspects of consumer bankruptcy, e.g. by increasing the minimum time between bankruptcy filings and limiting homestead exemptions, which allow debtors to exempt the value of their homes from creditors. In addition, BAPCPA made bankruptcy considerably more expensive: a report by the United States Government Accountability Office estimates that attorney fees for Chapter 7 cases increased by approximately 50% and also increased filing fees (USGAO, 2008).

These changes led to a dramatic effect of BAPCPA on the caseload of the US bankruptcy courts. Because the bill was signed in April but applied in large parts only from October, many individuals attempted to make use of the "old" law in a rush to file for bankruptcy. To illustrate, an American Banker article at the time reported that "[p]eople were lined up around the block Friday at the U.S. Bankruptcy Court for the Southern District of New York, hoping to file the last business day before the new rules took effect."[9] This spike can clearly be seen in the data on consumer case filings (see figure 4.2). More importantly, the number of consumer bankruptcies collapsed in the first quarter of 2006, once BAPCPA took effect and the courts had digested the filing frenzy. As a result, caseload per judge more than *halved*, dropping to unprecedented levels in the ballpark of what had last been seen around 1980. During 2006 and 2007, there was only a slight upward correction, leaving bankruptcy judges with a considerably lower caseload. The average caseload per judge in 2006 and 2007 was around 566 hours, the largest ever percentage change reduction of court workload in peace-time from an average of 1,059 hours in 2004 and 2005.

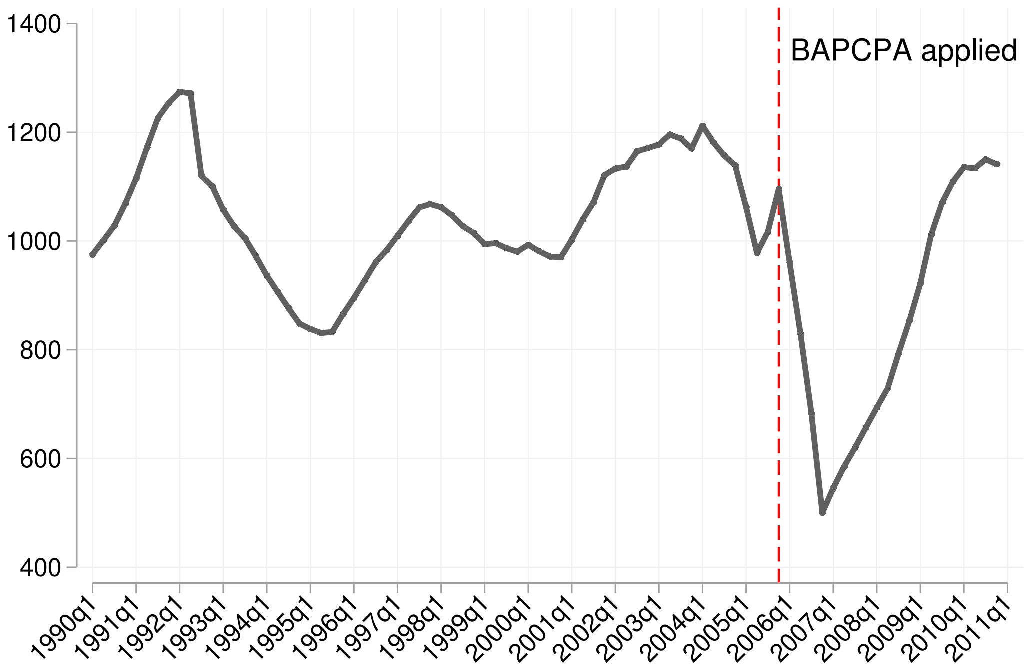
I exploit this historic drop in the caseload of US bankruptcy courts as a shock to judicial efficiency in my empirical analysis. The identifying assumption that I make is that the effects I will document later are driven by a lower workload of judicial staff, and not by a direct change in the legal framework governing business bankruptcies

[8] See the American Banker (October 17, 2005), https://www.americanbanker.com/news/in-focus-as-new-bankruptcy-era-opens-whats-now-and-whats-next.

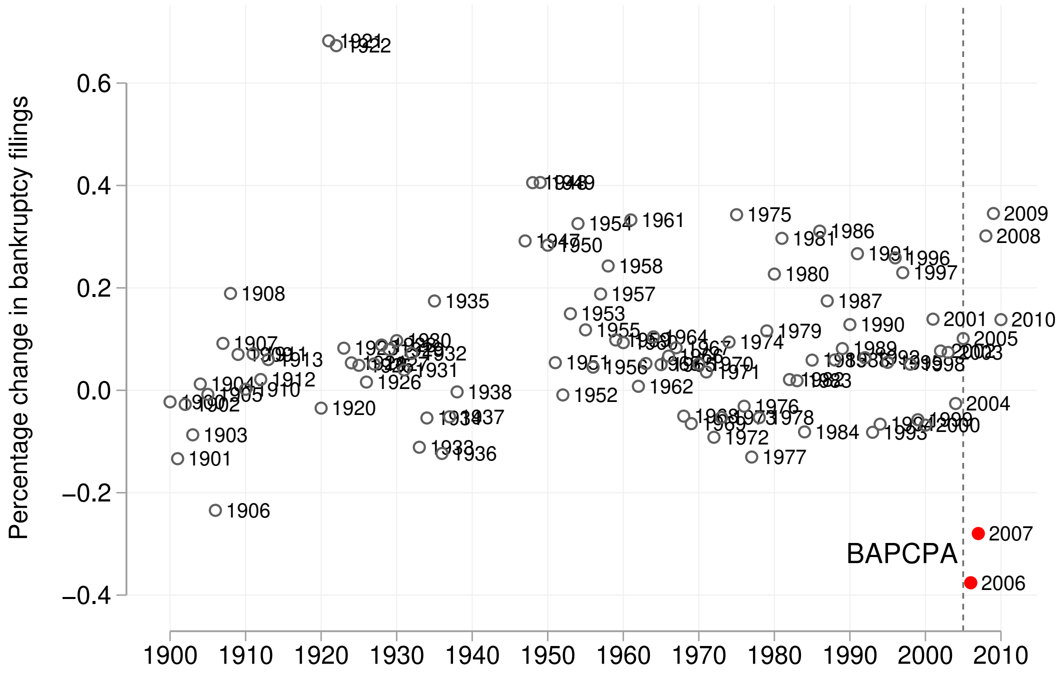
[9] See the American Banker (October 17, 2005), https://www.americanbanker.com/news/in-focus-as-new-bankruptcy-era-opens-whats-now-and-whats-next.

Figure 4.1: BAPCPA and the Caseload per Judge

Panel A: Weighted Caseload per Judge, 1990-2011



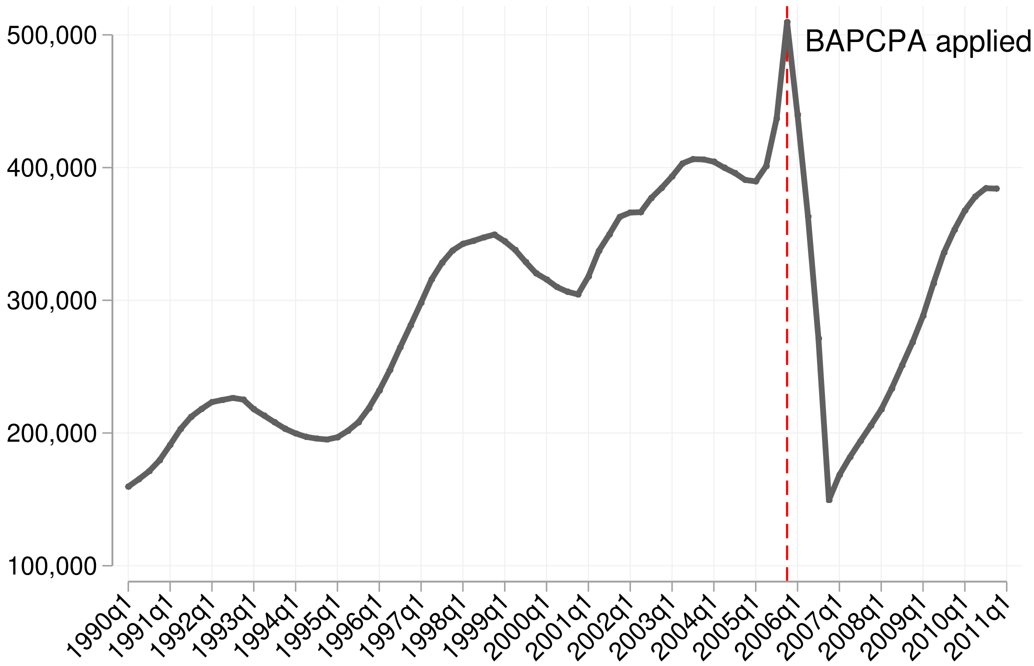
Panel B: Year-on-Year Percentage Change in Bankruptcy Filings, 1900-2010



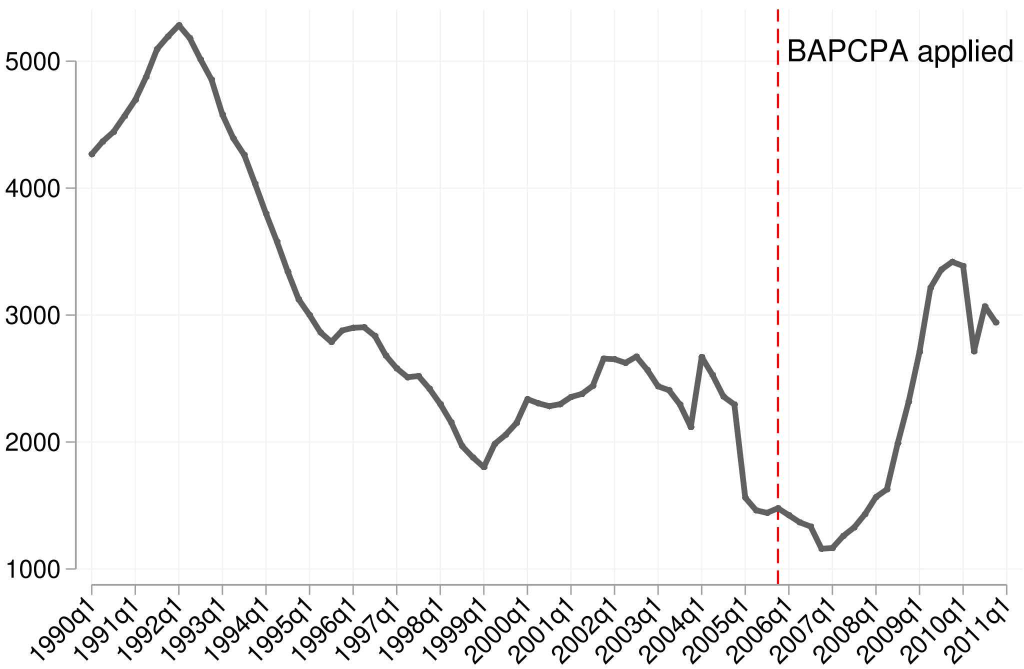
Panel A plots the total weighted caseload per judge between 1990 and 2011. Intuitively, this measure represents the hours per year that an average bankruptcy judge is expected to work on bankruptcy cases (excluding other work). Panel B plots the year-on-year percentage changes in the total number of bankruptcy filings in the United States from 1900-2010, excluding observations recorded during the two World Wars. The latter data are from the Federal Judicial Center (`https://www.fjc.gov/history/courts/caseloads-bankruptcy-cases-1899-2016`).

Figure 4.2: NON-BUSINESS AND CHAPTER 11 CASES AROUND BAPCPA

PANEL A: NON-BUSINESS CASES FILED PER YEAR



PANEL B: CHAPTER 11 BUSINESS CASES FILED PER YEAR



These figures plot the annual number of non-business and chapter 11 bankruptcy cases filed between 1990 and 2011.

or demand factors. While BAPCPA fundamentally reformed personal bankruptcy, it left firm bankruptcies largely unaffected. Nevertheless, it is important to discuss a few provisions that may be relevant for the large businesses in my sample.[10]

First, BAPCPA introduced a "drop dead" date for the exclusive right of a debtor-in-possession to file a plan of reorganization. Before BAPCPA, the initial 120-day period could be extended indefinitely; after BAPCPA, it was capped at 18 months. Since this provision only applied to cases filed on or after October 17, 2005, it would not "bite" until mid-April 2007.[11] Second, BAPCPA introduced limits to the applicability of automatic stay in bankruptcy for repeat filers to prevent abuse. The new §362(c)(3) provided that for debtors filing within one year after a dismissed earlier chapter 7, 11, or 13 bankruptcy case, the automatic stay terminates 30 days after the filing.[12] To put this into perspective, Altman (2013) finds that 15% of debtors once in Chapter 11 ultimately file for bankruptcy again. Third, BAPCPA amends provisions regarding the treatment of unexpired leases of non-residential real property, which has to be returned to the lessor within 120 days (with a single possible court extension of 90 days). While this seemingly eased the previous 60-day requirement, it permitted courts to grant only a single extension where no limit was in place prior to BAPCPA. This particularly affected borrowers who make regular use of leasing, such as retail and wholesale traders.

In my empirical analysis, I take great care to show that my findings are not driven by these provisions. To begin, the non-business share in a bankruptcy district's caseload is virtually orthogonal to firm and loan characteristics. As such, there is no reason why firms in districts with a higher exposure to the subsequent drop in caseload would be differentially affected by the minor legal changes, except for BAPCPA's effect on judicial efficiency. I also show that my findings are unchanged if I drop the firms that were likely most affected by these provisions.

[10] A contemporaneous briefing by Linklaters, available online, provides more detailed information.

[11] See, e.g. the discussion by Jones Day on their website.

[12] Of course, there are exceptions where filers or other parties can make a motion to provide evidence the bankruptcy is filed in good faith with respect to the creditor(s). §362(i) further states that subsequent cases should not be considered to be in bad faith if a prior dismissal was due to the creation of a debt repayment plan.

4.3 Data and Variable Construction

4.3.1 Loan Contract and Balance Sheet Information

The source of the loan-level information used throughout the paper is Dealscan, provided by Thomson Reuters. The main variables I construct are the size of the loan (in logs), the interest rate (as spread over a reference rate, usually LIBOR), and the natural logarithm of the loan maturity (in months). I further include a dummy for whether a loan is secured with collateral.

Balance sheet data comes from the annual Compustat Fundamentals file, which is extended with data on ratings from Mergent FISD. Following standard procedure in the literature, I exclude firms in the finance and real estate business, as well as regulated public utilities. All balance sheet variables are winsorized at the 1st and 99th percentile.[13] I construct the firm-level control variables book leverage, total assets (in natural logarithm), ROA, sales growth, a dummy for the existence of a credit rating, and the share of fixed assets (tangibility) to increase precision. Following Nini et al. (2009) and Sufi (2007), I further use the debt to cash flow ratio to proxy for credit risk, which is captured using two dummy variables indicating whether a firm is in the top quartile of the ratio or has negative cash flow.

The Compustat data is matched to Dealscan using the link provided by Roberts and Sufi (2009). The resulting sample lasts from 1987 to 2012. The exact definitions of all variables can be found in the appendix.

4.3.2 Bankruptcy Court Information

Data on the caseload and number of judges per bankruptcy district come from the Administrative Office of the U.S. Courts. The caseload statistics can be found at http://www.uscourts.gov/statistics-reports/caseload-statistics-data-tables. The time series for the number of judges in each bankruptcy district was shared with me by the Administrative Office via email. These data reach back to the start of the matched Dealscan-Compustat sample in 1987.

4.3.3 Summary Statistics

Table 4.1 presents summary statistics of the estimation sample. The median borrower is large, with more than 1 billion in assets ($exp(7.049) \approx \$1,152million$), has positive

[13] The results presented here are not driven by these winsorization choices.

sales growth of around 9%, and has a credit rating. Around 40% of firms had a loan maturing after the passing of BAPCPA in April 2005 that had been taken out before, which suggests a refinancing need. The loans taken out by the firms in my sample reflect their size: the average loan has a size of around $208 million and comes with a 175 bps spread and a maturity of around 5 years ($exp(4.094) \approx 60 months$). Most loans are backed by collateral. There is considerable variation across states in their exposure to BAPCPA, i.e. the share of nonbusiness cases in the total weighted court caseload. The median district has a nonbusiness share of around 81%.

4.4 Identification Strategy

4.4.1 The Effect of BAPCPA on Court Efficiency

To guide the empirical investigation, it is instructive to document how BAPCPA changed the outcomes of court proceedings. As a first piece of evidence, figure 4.1 plots the average caseload per judge in the United States from 1990 and 2011. We can see that the 2005 reform led to a dramatic decrease in the caseload of judges.

Importantly, there is considerable heterogeneity across districts in how much their caseload reacted. Because BAPCPA almost exclusively targeted consumer bankruptcies, there is an almost linear relationship between the share of nonbusiness cases in total caseload prior to the reform and the subsequent drop in caseload per judge. Figure 4.3 visualizes this point by plotting the non-business caseload share ($Exposure$) against the drop in judge workload, where the drop is calculated as the difference between the averages for 2006-2007 (*Post-BAPCPA*) and 2004-2005 (*Pre-BAPCPA*). I exploit this cross-sectional variation to identify the effect of BAPCPA on loan terms.

Table 4.2 confirms the relationship in a regression framework. In column (1), I start by regressing the drop in caseload per judge on the weighted non-business caseload share ($Exposure$). The exposure variable is highly statistically significant ($t = 4.77$) and implies that a standard deviation increase in the non-business share (about 12 percentage points) reduced the caseload per judge by 94 hours, or about 17% of the average caseload drop (555 hours). In column (2), I introduce the number of judges created by BAPCPA as an additional explanatory variable. The effect of $Exposure$ now *increases*, suggesting that the caseload drop is not driven by additional judges. In figure 4.3, the districts of Delaware (DE) and Southern New York (NY,S) are clear outliers. For robustness, I drop these in column (3); this makes only a marginal difference to the point estimates. At last, I restrict the sample to districts which will later be included in the loan-level analysis in column (4). This makes little difference to the implied effect

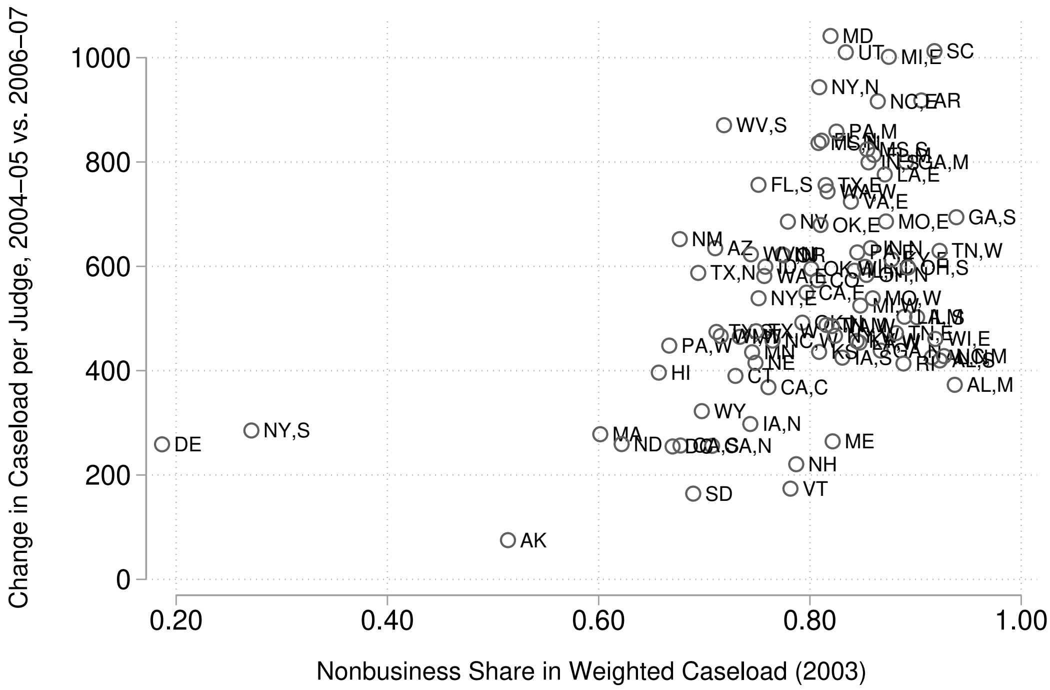
Table 4.1: Summary Statistics

This table presents descriptive statistics of all variables used in the main estimation sample of the paper. See the appendix for variable definitions.

Variable	N	Mean	SD	p10	p50	p90
Borrower characteristics						
Book leverage	3445	0.394	0.321	0.073	0.336	0.740
Log(Total assets)	3445	7.073	1.627	4.973	7.049	9.236
ROA	3445	0.130	0.097	0.049	0.119	0.229
Negative debt-to-cash flow	3445	0.019	0.136	0	0	0
High debt-to-cash flow	3445	0.522	0.500	0	1	1
Sales growth	3445	0.191	1.278	-0.060	0.093	0.418
Rating dummy	3445	0.594	0.491	0	1	1
Tangibility	3445	0.321	0.238	0.058	0.258	0.691
Refi need post-BAPCPA	3445	0.308	0.462	0	0	1
Credit rating (pre-reform)	1981	12.277	3.276	8	13.000	16
Log(Total assets) (pre-reform)	3360	7.109	1.621	5.08123	7.097	9.238662
Market leverage (pre-reform)	3023	0.316	0.236	0.054657	0.264	0.63837
Loan characteristics						
Interest rate spread	3445	204.846	147.637	50.000	175.000	355.000
Log(Maturity)	3445	3.880	0.570	3.178	4.094	4.344
Log(Loan size)	3445	5.154	1.506	3.103	5.340	6.949
Collateral dummy	3445	0.761	0.426	0	1	1
Relationship dummy	3445	0.400	0.490	0	0	1
Term loan dummy	3445	0.374	0.484	0	0	1
District characteristics						
Exposure	3445	0.770	0.132	0.694	0.807	0.875
High congestion dummy	2568	0.636	0.481	0	1	1
County characteristics						
Manufacturing emp. Share	3432	0.083	0.049	0.029	0.075	0.135
Finance emp. Share	3436	0.058	0.023	0.031	0.055	0.088
Income per capita	3439	40092.350	12507.070	29232.000	35813.000	55124.000
Population density	3396	4069.109	12224.080	247.300	1467.200	3884.500
Republican vote (2000)	3396	45.582	13.456	28.600	47.800	61.300
High school graduates/Pop.	3396	82.184	8.135	74.600	82.800	90.500
Asians/Pop.	3396	4.687	4.836	1.100	3.400	9.800
Hispanics/Pop.	3380	0.135	0.126	0.018	0.086	0.330
African-Americans/Pop.	3396	14.970	12.268	2.200	10.800	28.200
Owner-occupied housing share	3396	62.361	12.625	52.000	63.200	76.300
Δ Real estate emp. ('01-'06)	3445	0.326	0.186	0.123	0.283	0.577
Δ Construction emp. ('01-'06)	3445	0.131	0.161	-0.042	0.109	0.347
Δ House prices ('02-'06)	2863	0.292	0.200	0.067	0.270	0.564

of BAPCPA: a one standard deviation increase in the non-business share is associated with a decrease in caseload of about 91 hours ($0.083 \times 1,093 \approx 91$).

Figure 4.3: Non-Business Caseload Share and the Post-BAPCPA Caseload Drop



This figure plots the drop in the caseload per judge against the share of non-business cases in the weighted caseload of a bankruptcy district in 2003. The caseload drop is calculated as the difference of the average caseload in a district between 2006-2007 and 2004-2005. The non-business share is the weighted caseload for non-business bankruptcy cases as percentage of the total weighted caseload in 2003.

Did the large drop in non-business filings following BAPCPA also affect bankruptcy outcomes for business cases that were not materially subject to the reform? Iverson (2016) investigates this question drawing on the universe of Chapter 11 bankruptcies from 2004 through 2007. He finds that BAPCPA reduced the time in court for larger bankruptcies and made bankruptcy outcomes more efficient. In particular, BAPCPA decreased the likelihood of small cases to be liquidated in Chapter 7, compared to Chapter 11 reorganization, and the opposite for larg cases. The reform also decreased the time in bankruptcy (as a function of firm size), as well as the likelihood of repeat filings by previously bankrupt companies (recidivism). Iverson also finds that BAPCPA reduced bank charge-offs on *commercial* lending, suggesting that more efficient bankruptcy proceedings translate into higher recovery values for creditors.

A reasonable concern is that BAPCPA may have decreased the number of non-

Table 4.2: THE SHARE OF NON-BUSINESS CASES AND THE BAPCPA CASELOAD DROP

This table shows the effect of exposure to BAPCPA, as measured by the share of non-business cases in a bankruptcy district court's total weighted caseload, on the drop in caseload per judge following BAPCPA. The dependent variable is the difference between the average caseload per judge in 2006-2007 and 2004-2005. *New Judges* is the number of new judgeships per district added with BAPCPA through the Bankruptcy Judgeship Act of 2005 (28 judges in 20 districts). Robust standard errors are reported in parentheses. ***, ** and * indicate statistical significance at 1%, 5%, and 10% level, respectively.

	Dependent Variable: Drop in Caseload/Judge after BAPCPA			
	Full Sample		Drop DE/NY,S	Estimation Sample
	(1)	(2)	(3)	(4)
Mean of Dep. Var	555.11	555.11	561.62	561.00
S.D. of Exposure	0.119	0.119	0.119	0.083
Exposure	793.166***	991.631***	1,024.787***	1,092.878***
	(166.119)	(159.206)	(227.146)	(175.217)
New Judges		110.115***	118.643**	116.455***
		(36.941)	(50.242)	(38.546)
Observations	89	89	87	80
Adj. R^2	0.195	0.318	0.300	0.364

business filings but made such bankruptcy cases more difficult. Indeed, the U.S. Government Accountability Office found some evidence for higher fees and longer per-case times in consumer cases in a 2008 report (USGAO, 2008). A potential result could be that the court costs per case remained flat or even *increased* with BAPCPA, which would go against the efficiency hypothesis. I explore this possibility empirically by turning to case-level data on the liquidation proceeds and court expenses of chapter 7 cases overseen by the U.S. Trustee Program (USTP).

The USTP is in charge of overseeing private chapter 7 trustees, who are responsible for the collection and liquidation of assets and their distribution to creditors. As part of these individual cases, trustees are required to file a report with the USTP that breaks down the distribution of liquidation values across creditors and also includes information on court and administrative expenses. These data allow me to test whether court expenses increased or decreased with BAPCPA, and further directly measure the impact on recovery values, which I define as total bankruptcy receipts net of fees scaled over the total incurred fees. Between 2004 and 2007, a total of 253,540 cases were reported to the USTP. Unfortunately, the USTP offices do not correspond to judicial districts; some districts having multiple USTP offices, and some offices cover multiple districts. I thus restrict the sample to the 27 states that only have a single bankruptcy district, and then run case-level regressions of the following type:

$$Y_c = \alpha_s + \alpha_t + \beta Post - BAPCPA_t \times Exposure_s + X_c + \varepsilon_c, \tag{4.1}$$

where c, s, and t denote cases, states, and year-months; $Post - BAPCPA_t$ is a dummy equal to 1 for the years 2006 and 2007, and 0 for 2004 and 2005; $Exposure_s$ is the share of non-business bankruptcies in the total weighted district caseload in 2003; and X_s are case-level control variables.[14] I cluster standard errors by state and year-month.

The results in table 4.11 in the online appendix show that BAPCPA *reduced* total bankruptcy fees in areas with a higher share of non-business cases, controlling for the size and length of the case. More importantly, BAPCPA decreased court expenses as a fraction of total fees and increased recovery values (as measured by total net receipts as a fraction of the total fees). Quantitatively, a one standard deviation increase in $Exposure$ in the sample (0.062) is associated with a 8.7% reduction in court costs ($0.062 \times 1.389 \approx 0.087$) and a 4.4% increase in recovery values ($0.062 \times 0.699 \approx 0.044$). Figure 4.7 in the online appendix further shows that, consistent with a direct reform effect, the impact on court costs started right after the law's passing; slightly reversed during the file-to-rush in the last quarter of 2005; and then turned clearly negative in 2006 and 2007. At the same time, the effect on case fees was much smaller, albeit slightly negative; court costs thus dropped more than total bankruptcy fees in areas more exposed to the reform. Taken together with the evidence in Iverson (2016), these results suggest that BAPCPA indeed increased judicial efficiency in the exposed districts, despite provisions that made the consumer bankruptcy process more difficult.

4.4.2 BAPCPA and Ex-Ante Credit Terms

Identifying the causal effect of BAPCPA on credit contracts is challenging for a number of reasons. First, the effect was signed into law by President George W. Bush on April 20, 2005 and most provisions applied to cases filed on or after October 17, 2005. As such, the reform timing overlaps with the height of the credit and housing boom of the 2000s that culminated in the Great Financial Crisis of 2007-2008. Second, contract terms are affected by a host of observable and unobservable factors unrelated to judicial efficiency, most importantly default risk.

I attempt to mitigate these issues by running difference-in-difference regressions of

[14] Note that the results are qualitatively and quantitatively similar without control variables (unreported).

the following form:

$$Y_j = \beta Post - BAPCPA_t \times Exposure_d + \gamma X_{ijt} + \alpha_t + \alpha_i + \varepsilon_j, \qquad (4.2)$$

where Y_j is a contract term (the interest rate spread or the loan maturity) of credit facility j. i denotes borrowers, d bankruptcy districts, and t years.

$Post - BAPCPA_t$ tags the post-BAPCPA period and is set to 0 for 2004 and 2005; and 1 for 2006 and 2007. This timing is motivated by the fact that the actual drop caseload per judge around the reform occurred only in January 2006, after a dramatic "file to rush" in the run-up to the implementation date in mid-october (see figure 4.1). In section 4.5, I show that the effect is indeed driven by the contemporaneous observed drop in caseload, rather than the passing or implementation date of BAPCPA.[15] The coefficient of interest is β, which captures the effect of BAPCPA ($Post - BAPCPA_t$) conditional on the pre-reform share of non-business bankruptcies in a district in 2003 ($Exposure_d$). Note that $Post - BAPCPA_t$ and $Exposure_d$ are absorbed by the fixed effects α_t and α_i.

In the baseline set-up, I include year (α_t) and borrower (α_i) fixed effects. This means that I track changes in the terms of borrowers issuing multiple loans between 2004 and 2007, which is important in my setting to control for unobserved default risk. As I show later, the results also hold when including more stringent sets of fixed effects, e.g. to track the same borrower-lender pair over time.

Of course, loan terms have many determinants that are unrelated to judicial efficiency. The advantage of the loan-level set-up here is that I can "match" contracts using firm fundamentals and other contract characteristics by including the covariate vector X_{ijt}. In particular, I control for the interest rate or maturity (in logs), loan size (in logs), a collateral dummy, a dummy for an existing lender-borrower relationship, a dummy for term loans; as well as the lagged borrower fundamentals book leverage, total assets (in logs), ROA, sales growth, a credit rating dummy, asset tangibility, and two dummies for firms that have negative or top-quartile debt-to-cash flow ratios. See the appendix for the exact variable definitions. To address concerns that these covariates might themselves be endogenous to BAPCPA, I present robustness exercises in section 4.5.4 where I exclude all covariates; this has no bearing on the results. Throughout the paper, standard errors are allowed to be heteroskedastic and correlated within district-year clusters.[16] In the robustness section, I also show that collapsing the loan terms Y_j into the "pre"- and "post"-period on the firm-level, as recommended by Ber-

[15] In the robustness section, I consider alternative time windows and show that the estimates are not sensitive to the exact definition of the $Post - BAPCPA$ dummy.

[16] The results are robust to alternative computations of standard errors, which I show in section 4.5.4.

trand et al. (2004), leads to similar results.

4.4.3 Are Consumer-Centric Districts Different?

The main identifying assumption of the regression in equation 4.2 is that the difference in loan terms between 2006-2007 and 2004-2005 is not driven by observed and unobserved factors that are unrelated to judicial efficiency but are correlated with $Exposure_d$, i.e. the share of non-business bankruptcies in a district's total weighted caseload. In my setting, the potentially most important concern are differences across districts in their exposure to the housing bubble during that period, but also fundamental differences in the pool of firm borrowers.

To investigate whether observable differences across bankruptcy districts are a cause for concern, I plot correlations between $Exposure_d$ and a host of covariates in table 4.3. I obtain these correlations by averaging each characteristic on the district-level and regressing it on the exposure measure.[17] All dependent variables and $Exposure_d$ are standardized to have mean 0 and a standard deviation of 1; this is to make magnitudes comparable across regressions. Reassuringly, borrower and loan characteristics in the pre-reform period seem to be largely orthogonal with exposure to BAPCPA (see column (1) in panel A and B); the only coefficients slightly passing the 10% significance threshold are a dummy for the existence of a credit rating and a dummy for an existing lender-borrower relationship. This suggests that, from a borrower's perspective, exposure to the drop in court caseload following BAPCPA was as good as random.

Column (3) in panel C shows that $Exposure_d$ is significantly correlated with a few county characteristics, namely the share of manufacturing in total employment (positive), the share of finance employees (negative), income per capita (negative), and the Republican vote share in 2000 (positive). There is a also negative correlation with the share of Asians and Hispanics as fraction of the total population. Crucially, measures of exposure to the intensity of the housing boom during the period do not appear to be positively correlated with the nonbusiness caseload share. The share of owner-occupied housing as well as changes in employment in the construction or real estate sector are not statistically significant. The exposure variable is further *negatively* correlated with house price growth during the boom: if anything, this implies that there was less of a demand boom in areas with a higher share of individual bankruptcies.

While it is possible to control for the interaction of $Post - BAPCPA_t$ with these county characteristics, the few statistically significant correlations raise the question

[17] The results yield highly similar results if I do not average the characteristics.

Table 4.3: Comparing Bankruptcy Districts Before the Reform

This table presents reports coefficients from regressing each firm, loan, and county characteristic on the treatment variables *Exposure* and *High congestion dummy*. I standardize all dependent variables and the continuous *Exposure* treatment to have a mean of 0 and standard deviation of 1 to make the coefficients comparable. The sample is the main estimation sample; see column (1) and (4) in table 4.4. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. *High congestion dummy* is a dummy equal to 1 for districts in the top quartile of a state's share of non-business bankruptcy cases in 2003; and 0 for districts in the bottom quartile. Firm and loan characteristics refer to the pre-reform period (2004 and 2005). County characteristics are as of 2003, except for the education, ethnic composition, and owner-occupied share variables (which refer to 2000). All characteristics are averaged on the district-level; county characteristics are weighted by population in 2003. Standard errors reported in parentheses are clustered by district.

	Exposure (1)	High Congestion Dummy (2)
Panel A: Borrower characteristics		
Book leverage	-0.098	0.096
	(0.097)	(0.236)
Log(Total assets)	-0.004	0.176
	(0.083)	(0.246)
ROA	0.068	-0.060
	(0.065)	(0.101)
Negative debt-to-cash flow	-0.073	0.099
	(0.068)	(0.099)
High debt-to-cash flow	-0.112	0.152
	(0.121)	(0.258)
Sales growth	0.032	-0.028
	(0.034)	(0.126)
Rating dummy	-0.142*	-0.044
	(0.085)	(0.289)
Credit rating	-0.116	0.277
	(0.096)	(0.318)
Tangibility	-0.042	0.039
	(0.113)	(0.259)
Panel B: Loan characteristics		
Log(Loan size)	0.081	-0.016
	(0.068)	(0.228)
Log(Maturity)	0.122	-0.125
	(0.109)	(0.238)
Interest rate spread	-0.051	0.024
	(0.078)	(0.297)
Collateral dummy	-0.129	-0.006
	(0.110)	(0.261)
Relationship dummy	-0.192*	-0.241
	(0.113)	(0.262)
Term loan dummy	-0.122	-0.050
	(0.108)	(0.244)

	Exposure (3)	High Congestion Dummy (4)
Panel C: County characteristics		
Manufacturing employment share	0.230***	0.031
	(0.079)	(0.288)
Finance employment share	-0.423***	-0.259
	(0.136)	(0.256)
Income/capita ($\times$ 1000)	-0.486**	-0.136
	(0.222)	(0.327)
Population density	-0.487	-0.375
	(0.407)	(0.420)
Republican vote (2000)	0.428***	0.208
	(0.142)	(0.262)
High school graduates/Pop.	-0.129	-0.006
	(0.104)	(0.258)
Asians/Pop.	-0.272*	-0.041
	(0.148)	(0.214)
Hispanics/Pop.	-0.285**	-0.000
	(0.133)	(0.317)
African-Americans/Pop.	0.261	0.215
	(0.166)	(0.265)
Owner-occupied housing share	0.356	0.474
	(0.273)	(0.330)
Δ real estate employees ('01-'06)	0.001	0.118
	(0.078)	(0.260)
Δ construction employees ('01-'06)	-0.063	0.264
	(0.114)	(0.258)
Δ house prices ('02-'06)	-0.301***	-0.091
	(0.095)	(0.262)

whether unobserved district factors might drive my results. I attempt to circumvent this issue twofold. First, I create a "High congestion" dummy variable that compares the share of non-business caseload within states with multiple bankruptcy districts. In particular, the dummy is 1 for districts in the top quartile of the $Reform\,Exposure$ measure within a state; and 0 for districts in the bottom quartile. Effectively, this enables me to compare the impact of BAPCPA within the same state. Columns (2) and (4) of table 4.3 shows that the High exposure dummy is uncorrelated with *all* observable borrower, loan, and county characteristics. In fact, the coefficient signs switch signs for 11 out of the 15 borrower and loan controls. This makes it even more plausible that any effect I find is indeed due to exposure to the reform, rather than other correlates.

Second, I exploit the fact that borrowers differ markedly in their refinancing needs, default risk, and expected recovery values *prior* to the implementation of BAPCPA. As I will outline in more detail below, this cross-sectional variation is plausibly exogenous to the legal changes which were ultimately passed in April 2005. By including an interaction term of the main effect $Post - BAPCPA_t \times Reform\,Exposure_d$ with these characteristics allows me to include a full vector of $district \times year$ dummies, which absorbs all time-varying district-level correlates unrelated to judicial efficiency. I apply this methodology in section 4.5.2.

4.4.4 Filing Location and the Issue of "Forum Shopping"

This paper studies the effect of judicial efficiency on ex-ante contracting outcomes. As such, a critical question is which firms are exposed to the caseload of a given bankruptcy district. In the empirical analysis throughout the paper, I assign borrowing firms to bankruptcy districts based on their headquarter location, for which I have the exact address data from Compustat. However, according to 28 U.S.C. §1408, firms in the US can file for bankruptcy where they (1) are incorporated; (2) have their principal place of business; or (3) have an affiliate that filed for bankruptcy. In practice, this means that the largest nationwide firms can more or less file for bankruptcy in a venue of their choice, a phenomenon often referred to as "forum shopping". Delaware and Southern New York are arguably the most frequent recipient districts of such forum shopping cases. In many high-profile bankruptcy cases such as Enron, Polaroid, WorldCom or General Motors, the companies filed in bankruptcy districts where they did not have the majority of their business operations. As a result, there is an ongoing debate among legal practitioners and academics about the pervasiveness and consequences of forum shopping.

The choice of assigning firms to bankruptcy districts based on their headquarter location is likely prudent for a number of reasons. First, forum shopping is relatively rare in a quantitative sense. Iverson (2016) shows that out of *all* chapter 11 cases filed at US bankruptcy courts between 2004 and 2007, 91.3% of the filings occurred within a debtor's headquarter state. The Commercial Law League of America (CLLA), an advocate for reforming bankruptcy venue regulation, has identified a forum shopping motive for 745 cases filed in the districts of Delaware and Southern New York between 2004 and 2014.[18] Even in these prominent districts, however, this accounts for only 0.48% of overall bankruptcy filings and 2.36% of business cases.[19] Further, it is worth highlighting that bankruptcy judges are keenly aware of forum shopping and regularly overrule requests to file in "dubious" locations.[20] As a result, these filing numbers likely overstate the frequency of *successful* forum shopping, that is judges approving requests to file in a district that is arguably not the main location of a borrower's business.

Second, forum shopping is mostly limited to "mega bankruptcy cases", i.e. to the largest nationwide companies. As I show in the empirical analysis, my estimates *decrease* with firm size, implying that smaller firms are more affected. This makes it unlikely that I am picking up a spurious effect driven by firms that would likely file for bankruptcy outside of their headquarter district. Even though the borrowers in my sample are large, they are still considerably smaller than the median publicly listed forum shopper, which has $3.2 billion in assets.[21] This is considerably above the 75th percentile of borrowers in my sample. The results also hold when excluding the largest firms; states or industries who are the most frequent victims of forum shopping; or adjusting for firms headquartered or incorporated in Delaware and Southern New York. It is also unclear why exposure to BAPCPA – as measured by a bankruptcy district's pre-reform caseload share of nonbusiness filings – should be correlated with a firm's propensity to shop for a preferred venue. In fact, my exposure measure appears to be largely orthogonal to firm characteristics across districts.

Third, headquarter location is likely a sound proxy for the location of the majority

[18]See their website for data and reports at https://clla.site-ym.com/page/resources_venue_rfm.

[19]These percentages are calculated by dividing the 745 cases identified by CLLA by the number of overall business bankruptcy cases filed in Delaware and Southern New York between 2004 and 2014 (653,235 and 78,947, respectively). The total number of cases are from the US Courts statistics, see section 4.3.2.

[20]See, for example, the discussion at https://www.law360.com/articles/676417/the-bankruptcy-venue-debate-a-never-ending-story.

[21]See the UCLA-LoPucki Bankruptcy Research Database (http://lopucki.law.ucla.edu/design_a_study.asp?OutputVariable=Shop).

of a firm's assets, which is a standard assumption in the literature on geographical factors in empirical finance research (e.g. Coval and Moskowitz, 2001; Loughran and Schultz, 2004; Malloy, 2005; IvkoviÄ‡ and Weisbenner, 2005). In fact, given that borrowers are solely assigned based on their headquarter location, the estimates likely *underestimate* the true effect. This is because if creditors additionally take into account a firm's other potential bankruptcy venues, I will only capture a partial treatment effect. In other words, if headquarter location was irrelevant for a creditor's assessment of borrower recovery values, local judicial efficiency should have no effect on loan terms; as I will show below, this is strongly rejected in the data.

4.5 Results

4.5.1 BAPCPA and Ex-Ante Loan Terms: Baseline Estimates

Was the drop in caseload per judge following BAPCPA priced into the contract terms of forward-looking creditors? Column (1) of table 4.4 starts to investigate this question by running equation 4.2 with interest rate spreads as the dependent variable. The estimated coefficient is around -125 and highly statistically significant. The implied effect is large: a one standard deviation increase in the exposure to BAPCPA (around 0.13) reduced spreads by approximately 16 basis points (bps).[22] In column (2) I allow for the inclusion of county characteristics interacted with the $Post{-}BAPCPA$ dummy. The purpose is to control for local differences in employment structure and a few other significant covariates identified in section 4.4.3. Matching counties on these control variables increases the estimate to around -146: a one standard deviation increase now implies a drop of more than 19 bps.

A challenge of these regressions, however, is that unobservable differences across bankruptcy districts not captured by the control variables may bias the coefficients upwards. I thus turn to column (3), where I replace the continuous $Exposure$ measure with a dummy that tags districts in the top quartile of the non-business caseload share as "treated" (and the bottom quartile as "control"). As shown in section 4.4.3, there are no observable differences in borrower, loan, or county characteristics in these districts. The estimate of around -31 in column (3) is again highly statistically significant despite the decrease in sample size. Since both independent variables of interest are dummies, this can be interpreted as a direct elasticity of spreads (in bps) to BAPCPA: borrowers in highly exposed bankruptcy courts saw a decrease in spreads of around 31 bps compared to less exposed districts, even within the same state. In column (4), I

[22]The calculation is $-124.895 \times 0.132 \approx 16.5$.

Table 4.4: BAPCPA and Loan Contract Terms

This table presents estimated coefficients of the relation between corporate loan terms and the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). The dependent variables are the interest rate spread or maturity of a loan facility (both in natural logarithm). BAPCPA is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. *High congestion* is a dummy equal to 1 for districts in the top quartile of a state's share of non-business bankruptcy cases in 2003; and 0 for districts in the bottom quartile. See text for description of control variables. Standard errors reported in parentheses are clustered by district and year.

	Interest Rate Spread				Log(Loan Maturity)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-BAPCPA × Exposure	-124.896***	-146.425**			0.504***	1.208***		
	(26.716)	(59.292)			(0.167)	(0.252)		
Post-BAPCPA × High Congestion			-30.821***	-24.265***			0.103*	0.130**
			(9.485)	(9.731)			(0.060)	(0.065)
Loan facility controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-BAPCPA × County controls		Yes		Yes		Yes		Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,445	3,367	2,568	2,506	3,445	3,367	2,568	2,506
Adj. R^2	0.778	0.781	0.784	0.785	0.544	0.549	0.566	0.572

again "match" this estimation on the set of county-level control variables, which yields a point estimate of -24. The implied basis point reductions are large compared to the median spread of 175 bps: even the smallest estimate in column (4) implies a reduction in the cost of credit of around 14%.

In columns (5) through (8) I repeat the same exercise using the natural logarithm of loan maturities as the dependent variable. Again, all specifications are highly precisely estimated and imply a substantial impact of judicial efficiency. In the baseline regression (5), the coefficient of 0.504 implies that a one standard deviation increase in exposure extends long maturities by approximately $0.504 \times 0.132 \approx 6.7\%$.[23] Including county interactions raises this to an effect of around 16% in column (6). These magnitudes are close to the within-state comparison of districts with high vs. low exposure in columns (7) and (8), which imply an increase of around 10% or 13%, depending on whether I include county-level controls (interacted with the Post-BAPCPA dummy). Since the median loan maturity in the sample is 5 years, or 60 months ($\approx exp(4.094)$), these estimates imply that the decrease in court backlog increased loan maturities by between 4 and 10 months.

Figure 4.4 plots the dynamic impact of the BAPCPA-induced drop in caseload in the estimation sample over time. A first important observation is that there are parallel trends before the reform and the caseload drop: there is no distinguishable correlation between the non-business caseload share and loan terms prior to the reform. The second observation is that the passing and implementation of BAPCPA itself in March and October 2005 had virtually zero effect on spreads and maturities.[24] Instead, the sizeable changes in loan terms perfectly coincided with the drop in caseload in January 2006 shown in figure 4.1. This suggests that improvements in judicial efficiency, not changes to the legal framework, were behind the improvement in borrower's credit terms.

In table 4.13 in the online appendix, I find further support that the actual caseload drop is indeed the driver of these results. In particular, I run a "horse race" of my post-BAPCPA dummy (defined as 1 starting in January 2006) against dummies for the major legislative steps of the reform, namely its passing of the Senate, House, and the application of most provisions. The estimated coefficients on the legislative steps are small and statistically insignificant (except for a single estimate with the "wrong" sign); the dummy I use to tag the drop in caseload starting in January 2006 retains its

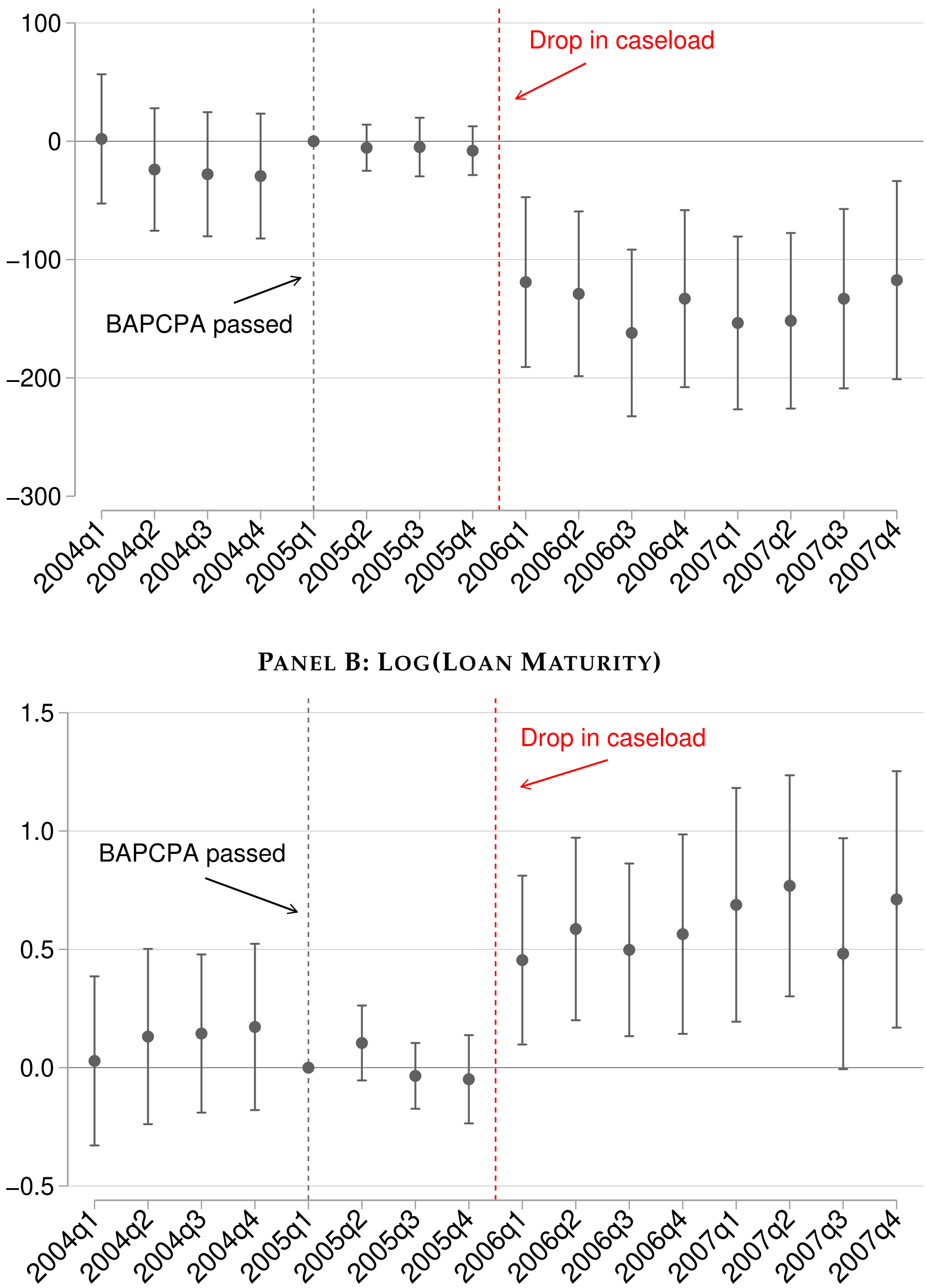
[23] I use the Taylor approximation here to translate log-points into percentage changes for convenience. Given that the numbers here are relatively small, this is going to be a relatively accurate description of the actual percentage changes.

[24] BAPCPA passed the Senate on March 10, 2005 and the House on April 14.

Figure 4.4: The Impact of BAPCPA on Loan Terms

Panel A: Interest Rate Spreads (in bps)

Panel B: Log(Loan Maturity)



These figures show the dynamic effect of BAPCPA on interest rate spreads for secured loans. They plot the estimated coefficients β of the regression $Y_j = \beta \sum_{t=2004q1}^{2007q4} D_t \times Exposure_d + \gamma X_{ijt} + \alpha_t + \alpha_i + \varepsilon_j$, where Y_j refers to the interest rate spread in Panel A and the natural logarithm of loan maturities in Panel B. "BAPCPA passed" refers to the passing of BAPCPA in the Senate on March 10, 2005.

statistical power and in fact increases in size.

Overall, the results presented here are consistent with the interpretation that a decrease in the backlog of bankruptcy judges has a sizeable effect on financial contracts. In the next section, I use plausibly exogenous exposure on the borrower-level to the caseload drop to narrow down the channel behind these findings.

4.5.2 Exploring Borrower-Level Exposure

The results above suggest that the caseload drop following BAPCPA had a large effect on the interest rate spreads and maturities of loan contracts. If the channel I am capturing is indeed a change to the efficiency of bankruptcy courts, we would expect some borrowers to be more affected by the caseload drop than others. In this section, I use firm-level variation to narrow down the channel behind my main results. Conveniently, this also allows me to address the challenge that unobserved district factors may bias my estimates by including $district \times year$ fixed effects.

To begin, I exploit that around a third of the borrowers in my sample had taken out loans prior to BAPCPA that matured after the drop in the court caseload in its aftermath. It seems unlikely that firms factored in a potential increase in judicial efficiency when they took out loans prior to BAPCPA, in many cases years before it was clear whether it would pass Congress. In fact, BAPCPA had a long legislative history. The bill was first drafted in 1997 and passed Congress in 2000, only for President Clinton to veto it. After this setback, BAPCPA was shelved numerous times over disagreements over proposed amendments by both Democrats and Republicans. Only after the Republican party increased their majority in the Senate and House in the 2004 elections was the bill re-introduced in the Senate in February 2005. As such, firms that took out loans with a fixed maturity prior to BAPCPA had an exogenously determined demand for credit at some point in the post-reform period (also see Almeida et al., 2012); this exposed them to shifts in credit supply as a result of the caseload drop that is unrelated to borrower fundamentals.

I exploit this exogenous refinancing need by constructing a dummy for borrowers who issued a loan prior to 2005 with a scheduled maturity date in 2006 or 2007. I exclude credit lines because the end-date of revolving loans, which are frequently rolled over, is less informative about refinancing needs. Around 30% of firms in the estimation sample had pre-2005 loans maturing after the caseload drop starting in 2006. This dummy variable is then interacted with the $Post - BAPCPA \times Exposure$ variable, which creates variation on the borrower-time level; this allows me to absorb unobserved local variation across bankruptcy courts using a full set of $district \times year$

dummies.[25] Note that the fixed effects in this specification absorb the main treatment effect as well as the *Refinancing need* dummy itself.

Table 4.5 plots the results. To aid comparisons, I reproduce the baseline result in column (1). In column (2), I introduce the triple interaction with the refinancing need dummy. The coefficients of -177.846 for spreads and 1.454 for maturities are highly statistically significant. Because I compare borrowers in the *same district* in the *same year* that differ in their pre-determined need to roll over debt, they are unlikely to reflect differences in firm demand. The implied magnitudes are considerably larger to the baseline coefficients in column (1), and even larger than those including county-level controls in table 4.4. Taken at face value, this suggests that the *entire* effect of BAPCPA is due to changes in credit supply. To illustrate, a one standard deviation increase in $Exposure$ decreased spreads of borrowers with a loan maturing in 2006 or 2007 by around 23 bps. The same change in the non-business share in total caseload increases loan maturities by around 19%.

Borrowers show substantial heterogeneity prior to BAPCPA not only in their refinancing needs but also in other characteristics that one would expect to matter for creditors concerned with potential losses from a borrower's default. If the reform indeed impacted credit contracts through its impact on expected recovery values, one would expect two aspects to matter in particular: (1) a borrower's probability of default and (2) a borrower's expected liquidation values in bankruptcy. In the data, I measure the probability of default using numerical credit ratings or market leverage. I proxy for liquidation values using firm size (as proxied by the log of total assets).[26] Since these firm variables might themselves have changed with BAPCPA or in reaction to the improved financing terms, I use their pre-reform values.[27]

Table 4.5 show the results of including these interactions in the main regression. As above, the borrower-level variation allows me to include a full vector of district-year dummies, which absorbs local factors unrelated to the $Exposure$ measure. Note that the pre-reform characteristics themselves are absorbed by the firm fixed effect α_i.

[25] The estimates plotted here are almost equivalent when the regressions are run with the less stringent standard set of borrower and year fixed effects. They are also qualitatively and quantitatively similar when using the "High congestion" dummy. I omit the results for brevity but they are available upon request.

[26] In unreported results, I find that proxies for liquidation values such as the asset redeployability score of Kim and Kung (2017) or the ratio of research and development expenditures to total assets yield qualitatively similar findings.

[27] I define pre-reform values as the average of 2004 and 2005, i.e. when $Post - BAPCPA$ is equal to 0. Note that I continue to include the full vector of lagged borrower control variables, which however makes no differences to the results.

Table 4.5: BAPCPA AND LOAN CONTRACT TERMS: BORROWER HETEROGENEITY

This table presents estimated coefficients of the relation between corporate loan terms and the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). The dependent variables is the interest rate spread or natural logarithm of the loan maturity. BAPCPA is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. All firm-level variables are the simple average of the years where BAPCPA is 0, i.e. 2004 and 2005. Refinancing need is a dummy for borrowers that issued a loan that is not a credit line before 2005 that matures in 2006 or 2007. See text for description of other variables. Standard errors reported in parentheses are clustered by district and year.

		Interaction with pre-reform...			
	Baseline	Refinancing Need	Credit Rating	Market Leverage	Log(Total Assets)
	(1)	(2)	(3)	(4)	(5)
		Panel A: Interest Rate Spread			
Post-BAPCPA × Exposure	-124.896***	–	–	–	–
	(26.716)				
Post-BAPCPA × Exposure × Var.		-177.846***	-40.541***	-730.623***	54.428***
		(71.606)	(6.558)	(120.088)	(16.118)
Post-BAPCPA × Var.		100.339*	18.332***	428.791***	-42.972***
		(55.864)	(4.207)	(99.287)	(11.567)
Observations	3,445	3,142	1,756	2,743	3,057
Adj. R^2	0.778	0.839	0.854	0.845	0.833
		Panel B: Log(Loan Maturity)			
Post-BAPCPA × Exposure	0.504***	–	–	–	–
	(0.167)				
Post-BAPCPA × Exposure × Var.		1.454***	0.138***	2.244*	-0.200**
		(0.436)	(0.034)	(1.263)	(0.094)
Post-BAPCPA × Var.		-1.025***	-0.106***	-1.605*	0.172***
		(0.328)	(0.027)	(0.930)	(0.061)
Observations	3,445	3,142	1,756	2,743	2,987
Adj. R^2	0.544	0.650	0.635	0.633	0.640
Loan controls	Yes	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	–	–	–	–
District × Year FE		Yes	Yes	Yes	Yes
Bank × Year FE		Yes	Yes	Yes	Yes

Column (3) starts by considering whether firms with a higher probability of default (as measured by numerical credit ratings) were more affected by the BAPCPA-induced caseload drop. A higher number on the rating signifies higher risk. The estimate of the triple-interaction term is -40.541 for spreads and highly statistically significant. Since AAA ratings are assigned a 1 in the rating variable and BB+ a 11, this captures the difference in spreads between prime- and junk-rated borrowers. The coefficient thus implies that BAPCPA had a differential effect of $40.451 \times 0.13 \times 10 \approx 53$ bps on junk-rated borrowers for a one standard deviation increase in $Exposure$. This is a large effect. For loan maturities, the difference is around 0.18 log-points. I find similar effects in column (4) when using market leverage as a measure of bankruptcy probability. A standard deviation increase in both market leverage (0.236) and the non-business share (0.132) is associated with a differential effect of about 23 bps in spreads and 7 log-points in maturities.

In column (5), I consider whether the effect of BAPCPA varied by firm size, which I use as a proxy of liquidation values. This estimation also serves as a sanity check for the importance of forum shopping: if my findings were spuriously driven by the largest firms with potential leeway over their filing location, the effect should *increase* in firm size. The data suggests the opposite. For interest rates, the estimate of 54.428 (highly statistically significant) implies that a standard deviation increase in pre-reform size (1.621) paired with a one standard deviation increase in $Exposure$ is associated with a 12 bps lower reaction for larger firms. For maturities, the corresponding value if $-0.200 \times 1.621 \times 0.132 \approx 4.2$ log-points.

Overall, the results in this section are consistent with the interpretation that bankruptcy court backlog matters particularly for borrowers that are closest to bankruptcy or have lower expected recovery values. They also suggest that the *entire* effect of BAPCPA is driven by credit supply, rather than differences in credit demand; this is the implication of finding larger than baseline effects for borrowers with exogenous refinancing needs. It is also consistent with the fact that the share of non-business cases is largely uncorrelated with firm fundamentals prior to the reform, including borrower risk. I conclude that creditors price in local judicial efficiency contemporaneously with observed improvements.

4.5.3 Ruling Out Alternative Explanations

The sharp discontinuity in credit terms after the implementation of BAPCPA could have a number of alternative interpretations. First, as highlighted above, BAPCPA coincided with the housing boom of the 2000s. I already showed above that the exposure measure based on the non-business share of total bankruptcy caseload is largely uncorrelated with district fundamentals and my results persist even when comparing borrowers with different ex-ante exposure within the *same district* in the *same year*. Could unobserved exposure to the housing boom still play a role? To investigate such concerns, I follow Mian and Sufi (2014a) and construct measures of the nontradable and construction sector and drop these from the estimation, since these are the industries that are most directly affected by swings in house prices and local demand. Column (2) of table 4.6 shows that this has no bearing on my main results.

Second, BAPCPA included a few provisions that indeed had an impact on corporate bankruptcies. In section 4.4.1, I already showed that these had virtually no effect on the number of *corporate* bankruptcy filings; it is also unclear why these legal changes should vary with a court's ex-ante share of non-business caseload. Levin and Ranney-Marinelli (2005) conduct a detailed legal analysis of BAPCPA and conclude that, if anything, the reform changes to business cases would *reduce* recovery values for firms, which works against finding a decrease in spreads. While both points make it unlikely that the minor legal changes had a substantial direct impact on corporate credit terms, a large empirical literature has shown that bankruptcy laws matter greatly for firm financing (see e.g. Acharya et al., 2011; Vig, 2013; Aretz et al., 2016; Rodano et al., 2016). A related paper by Sautner and Vladimirov (2017) finds that riskier firms used more trade credit (and had higher sales) in 2006 compared to 2003-2005. They attribute this to the changes in corporate bankruptcy law that were part of BAPCPA.[28]

In the case of BAPCPA, the amendments for corporate bankruptcies were largely targeted at specific groups, particularly retailers.[29] In column (3) of table 4.6, I thus exclude borrowers with SIC codes starting with 50 and re-run the baseline regressions. This makes no difference to the estimates. Another line of argument is that BAPCPA changed the treatment of derivatives (Edwards and Morrison, 2005).[30] However, the sample only includes non-financial corporations, so it is unlikely that this change had a substantive impact on my estimates. A last potentially biasing feature of BAPCPA

[28]Note that the main variation for the analysis in Sautner and Vladimirov (2017) comes from a cross-country panel study of differences in debt enforcement.

[29]Before BAPCPA, debtors-in-possession had to assume or reject unexpired leases of nonresidential real property within 60 days after the initial filing, a period that could be extended by the court. After BAPCPA, bankruptcy courts were only allowed to grant a single extension. Since retailers often have large numbers of leased properties, they were particularly affected by this provision. BAPCPA also hit businesses with large inventories by giving these "administrative expense priority status", which again impacted retailers. See Levin and Ranney-Marinelli (2005) for further discussion.

[30]See, e.g., a post on the "Synthetic Assets" blog for a thorough discussion of the effects of BAPCPA on the classification of different financial instruments.

is that it likely decreased interest rates on car loans by eliminating so-called cramdown provisions (Chakrabarti and Pattison, 2016).[31] If this led to an increase in local demand, it could boost firm liquidation values by raising house prices, and thus compress corporate spreads. Chakrabarti and Pattison measure the exposure to BAPCPA using the share of non-business bankruptcies that are filed under Chapter 13. Changes to cramdown provisions are an unlikely explanation for my findings; the exposure variable in Chakrabarti and Pattison (2016) has a relatively low correlation of 19% with the share of non-business cases in total caseload I use. Including their interaction in column (4) makes little difference to the point estimate. Also recall from section 4.4.3 that BAPCPA exposure is, if anything, *negatively* correlated with the change in house prices from 2001 through 2006.

[31] Before BAPCPA, borrowers with underwater car loans could have their debts reduced to the market value of the car through a "cramdown" in Chapter 13 bankruptcy. This was quite common because cars depreciate quickly. BAPCPA eliminated these for the first 910 days of car loans.

Table 4.6: BAPCPA and Loan Contract Terms: Alternative Explanations

This table presents estimated coefficients of the relation between corporate loan terms and the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). The dependent variables is the interest rate spread or natural logarithm of the loan maturity. BAPCPA is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. Exposure is a bankruptcy district's share of non-business bankruptcy cases in 2003. *Forum shopping states* are CA, NJ, PA, IL, FL. See text for description of other variables. Standard errors reported in parentheses are clustered by district and year.

	Baseline (1)	Exclude construction and nontradables (2)	Exclude retailers (3)	Cramdown control (4)	Exclude largest 10% (5)	Exclude forum shopping victim states (6)	Exclude forum shopping victim industries (7)	Exclude potential CLO loans (8)
Panel A: Interest Rate Spread								
Post × Exposure	-124.896***	-124.368***	-123.005***	-118.382***	-150.373***	-126.994***	-157.913***	-70.829**
	(26.716)	(33.010)	(30.604)	(28.178)	(28.070)	(26.723)	(26.400)	(36.541)
Observations	3,445	3,049	2,908	3,445	3,025	2,545	2,427	1,453
Adj. R^2	0.778	0.778	0.779	0.778	0.770	0.773	0.796	0.826
Panel B: Log(Loan Maturity)								
Post × Exposure	0.504***	0.529***	0.495***	0.527***	0.517***	0.470***	0.600***	0.518***
	(0.167)	(0.175)	(0.191)	(0.174)	(0.154)	(0.184)	(0.208)	(0.179)
Observations	3,445	3,049	2,908	3,445	3,025	2,545	2,427	1,453
Adj. R^2	0.544	0.547	0.543	0.544	0.572	0.537	0.522	0.482
Loan controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

A third concern might be that the relatively large borrowers in my sample should not be subject to changes in local judicial efficiency due to "forum shopping". While I already outlined in section 4.2 why forum shopping, if anything, works against finding any results in my setting, I can run a number of validity checks to rule this out. In columns (5) through (7), I exclude firms that are most likely to be forum shoppers: very large firms (those in the top 10% of total assets); firms headquartered in five states most subject to forum shopping (CA, NJ, PA, IL, FL); and firms in industries where more than 75% of public bankruptcy cases are marked as "forum shoppers".[32] The results in table 4.6 again suggest that these exclusions make no meaningful change to the point estimates. Forum shopping is unlikely to be behind my results.

Fourth, the 2004-2007 period was characterized by an increase in the use of securitization (see e.g. Benmelech et al., 2012; Keys et al., 2010). If securitization is correlated with the $Exposure$ measure, this might be behind the decrease in spreads and increase in maturities. Data reported by the Securities Industry and Financial Markets Association (SIFMA) suggests that the volume of outstanding Collateralized Loan Obligations (CLOs), CDOs backed by corporate loans, increased from $123.4 to $189.7 between 2005 and 2006, at 54% the highest growth rate since the mid-1990s. To control for the impact of securitization, I exclude loan facilities that are possibly owned by CLOs, building on Benmelech et al. (2012). In particular, I drop all term loan B and C facilities, which are specifically structured for institutional investors such as CLOs, as well as loans of borrowers with no credit rating at issuance. Since CLOs are primarily active in the leveraged loan segment (i.e. unrated or junk-grade borrowers), this likely excludes the vast majority of securitized loans. I report the estimation results of this reduced sample in column (8). Despite the substantial drop in sample size, the coefficients are still significant at the 1% level; securitization does not drive my results.

As a fifth point, one might speculate that omitted geographical drivers affecting firm risk are behind the results presented here. Such changes are likely to work *against* me: table 4.3 suggests that house prices growth is, if anything, *negatively* correlated with higher exposure to BAPCPA (although insignificant for the high congestion dummy). Further, I already showed in section 4.5.2 that ex-ante more exposed borrowers saw a larger change in loan terms even within the *same district* in the *same year*. In an alternative story, firms in these areas could have experienced a sudden change in fundamentals that made them less likely to default, and thus improving loan terms. I test this hypothesis in a district-year-quarter panel by regressing the number of business (or Chapter 11) bankruptcies (scaled over total employees) in a district on the interaction

[32] The latter two measures are constructed using the UCLA-LoPucki Bankruptcy Research Database at `http://lopucki.law.ucla.edu/`.

$Post - BAPCPA_t \times Exposure_d$, as above.[33] The results are reported in table 4.12 in the online appendix to conserve space. The estimated coefficients on the interaction in columns (1) and (3) are far from regular levels of statistical significance and have different signs: following BAPCPA, there was no change in business bankruptcies in districts more exposed to the reform. As a next test, I consider whether the $Exposure$ measures picks up districts with higher ex-post *realized* risk, again proxied by the number of bankruptcies. I implement this by replacing the $Post - BAPCPA_t$ variable with $Crisis_t$ in columns (2) and (4), which is 0 for the sample period 2004-2007 and 1 for the crisis period 2008-2011. The results, again, suggest a zero correlation between the non-business share in total caseload and business bankruptcies during the Great Financial Crisis, which makes it unlikely that lower ex-post risk drives my results.

4.5.4 Further Robustness Checks

In the online appendix, I report a battery of additional robustness checks of the main results presented in table 4.14. Column (1) shows the baseline estimates for convenience. The first number of exercises is aimed at establishing that the exact time window chosen here does not drive my findings. Columns (2) through (4) thus show specifications where I allow the $Post - BAPCPA_t$ dummy to vary differently by excluding 2007 from the "treatment" status or extending the pre and post period. The results still hold, suggesting that the effect of BAPCPA on loan terms was neither driven by pre-trends in earlier years nor reverted back over the course of the financial crisis. What is particularly worth highlighting here is that the point estimates are hardly affected by dropping 2007 from the regression in column (2). This is reassuring because it suggests that the effect is neither driven by (1) the onset of financial market stress in summer 2007 nor (2) the changes BAPCPA made to the exclusivity period of firm bankruptcies, which in practice applied from mid-April 2007 (see section 4.2).

The regressions on the loan-level I run throughout the paper have the advantage that they allow for the inclusion of other loan characteristics as controls, which improves the precision of the estimates. There may, however, be a concern that these control variables might themselves be endogenous. In column (5), I thus drop all covariates from the regression and only retain borrower and year fixed effects. The coefficients now imply somewhat smaller magnitudes, as one may expect, but remain

[33] I acknowledge that filing for bankruptcy is a sufficiently rare form of default on debt instruments. What matters for the identification strategy in this paper, however, is that the cross-sectional $Exposure$ measure does not predict differential local business conditions going forward, which is what I test here. Also note that the results are not affected by the scaling variable; I repeated these estimations using the log of bankruptcy cases, which makes no material difference.

highly statistically significant at the 1% level.

Next, I address that some firms in the sample take out multiple loans only before *or* after BAPCPA. While this still allows for the inclusion of borrower fixed effects, it does not capture a true within-firm effect of the reform. Dropping these firms in column (6) yields almost perfectly equivalent point estimates that are highly statistically significant. In a similar spirit, one may be concerned that the loan-level analysis here suppresses standard errors due to the repeated number of observations per firm before and after "treatment"; this is an important point highlighted in Bertrand et al. (2004). To mediate such concerns, I collapse all loan observations into the pre- and post-period, which allows me to take the first difference of average borrower loan terms.[34] The result is a single data point for each borrower. Column (7) presents the result of regressing the borrower-level change in contract terms on the $Exposure$ variable. Reassuringly, both in terms of precision and magnitude, the result is closely aligned with the baseline result in column (1).

Another issue in analyzing loan outcomes is that borrowers and lenders are not randomly allocated to each other over the credit cycle (Schwert, 2017). I thus restrict the sample to borrowers issuing at least one loan to the same bank before *and* after BAPCPA between 2004 and 2007. Then, I re-run the main specification with $bank \times borrower$ fixed effects. Effectively, this means tracking loan term changes within the same borrower-bank pair over time. Even though this leads to a substantial drop in sample size, the results in column (8) suggest that the results still hold.

In section 4.4.1, I show that the districts of Delaware and Southern New York represent outliers in the first-stage relationship between the non-business caseload share and the subsequent drop in the caseload per judge. As a robustness exercise, I thus "winsorize" these to take the caseload share of Alaska, as in Iverson (2016). The coefficients in column (9) are now considerably *larger* than before, suggesting that the nonlinearity introduced by the two districts does not have an impact on my results.

As a last exercise, I conduct a placebo test. In section 4.5, I already showed that there are no detectable differential trends along $Exposure$ variable prior to BAPCPA. The results in table 4.14 further suggest that the effects I uncover are of similar magnitude when using 2000-2005 or 2006-2012 as the pre or post period, respectively. Here, I conduct a counterfactual experiment by pretending the BAPCPA-induced caseload drop happened two years earlier. In this set-up, $Post - BAPCPA_t$ is 0 in 2002-2003 and 1 in 2004-2005; $Exposure_d$ analogously is now measured in 2001. The coefficient estimate in column (10) is quantitatively small and undistinguishable from 0.

[34]In particular, I create the loan size-weighted spreads and maturities before and after BAPCPA for each firm and take the difference between these two values.

Talbe 4.15 in the online appendix includes a range of additional validity exercises and shows that my estimates are not driven by the 20 districts for which BAPCPA added additional judges; time-varying shocks on the state-, industry-, or bank-level; or the pre-reform year in which I define the $Exposure$ variable. In table 4.16, I further present 8 sets of alternative standard errors for the baseline results, all of which still imply that my estimates are highly statistically significant.

4.6 The Social Costs of Court Backlog

How large are the welfare losses associated with inefficient legal enforcement in the court system? The law and economics literature has long grappled to find plausible answers to this question because court backlog is endogenous to local factors. Most obviously, poorly designed laws beget poor enforcement: uncertainty about the law and its implementation are likely a major contributor to judicial backlog.

In this section, I use the estimated effects of quasi-random exposure to BAPCPA on interest rate spreads and maturities to conduct a back of the envelope calculation. At the heart of this exercise lies the idea that, under a set of several simplifying assumptions, a reduction in interest rates (a price term) can be translated into aggregate cost savings (a quantity). This is because, in my setting, the estimated savings of lower spreads and longer maturities reduce the annual interest burden of non-financial corporations. Together with estimates on the costs of creating new judgeships in the United States, I can conduct a cost-benefit analysis for reducing the workload of courts. As such, I attempt to provide an empirical assessment of whether the costs of an overburdened bankruptcy system are indeed "enormous" – the charge raised by the U.S. district judge in the opening quote.

From the onset, it should be noted that this exercise should be regarded as illustrative: the estimates are based on a reduced-form linear model that uses the caseload per judge as sole measure of judicial efficiency. While I find large macroeconomic savings due to the drop in court backlog following BAPCPA, extrapolating from these in-sample estimates requires additional simplifying assumptions. My approach is also entirely silent about whether one can extrapolate estimates based on bankruptcy courts I study here to other settings, e.g. the costs of congestion in family courts.

4.6.1 Estimating the Elasticity of Loan Terms to Judicial Caseload

To estimate the macroeconomic costs of court backlog, I need point estimates for how much interest rates would fall (and maturities increase) for a given exogenous drop in caseload. I use the within-firm estimates of ≈ -90.144 bps for spreads and $\approx +29.5\%$ for maturities, reported in column (7) in table 4.14. These are around a third lower than the baseline estimates, and thus conservative.

These values imply that a one standard deviation increase in $Exposure$ (0.132) decreases spreads by 11.9 bps and increases maturities by 3.9%. In the estimation sample, table 4.2 shows that a one standard deviation increase in exposure to BAPCPA is associated with a drop of around 91 hours in annual caseload per judge ($0.083 \times 1092.878 \approx 91$), around two full work-weeks. Assuming a linear relationship, we can interpret these estimates as an elasticity of 11.8 basis points for spreads, and 3.9% for maturities, to a drop in caseload per judge of around 91 hours. This does, however, require the additional assumption that the changes in spreads and maturities are permanent and react linearly to changes in the caseload.

The point estimates I use here are likely conservative for another reason, because the median borrower in my sample is much larger than the typical corporation in the United States. Because smaller firms were considerably more affected by the BAPCPA-induced drop in caseload (see table 4.5), this means I will underestimate the effect on the population of US firms.

4.6.2 Estimating Changes to the Interest Burden of US Corporations

To translate these numbers into an aggregate cost, I draw on the approach of Drehmann et al. (2015) to calculate the US-wide interest payments of non-financial corporations. They calculate the macroeconomic debt service burden as follows, where I omit subscripts for brevity:

$$Interest\ Burden = \frac{Interest\ Rate}{(1-(1+Interest\ Rate)^{-Maturity})} \times Debt, \tag{4.3}$$

where $Debt$ refers to the total stock of non-financial corporate debt; $Interest\ Rate$ to the average interest rate on the existing stock of debt; and $Maturity$ to its average remaining maturity. For the baseline estimates, I ignore the effect of an exogenous drop in caseload on maturities. This is highly conservative, because incorporating the increase in maturities substantially increases the macroeconomic savings associated with more efficient bankruptcy courts. In the online appendix, I provide back-of-the-envelope calculations with a change in maturities that suggest the findings presented here are an extreme lower bound of the social costs of overburdened judicial systems.

Because I am interested in how much the cost of debt service *change* with judicial efficiency, and ignore the change to maturities, this yields the following equation:

$$\Delta Interest\ Burden = \Delta Interest\ Rate \times Debt. \tag{4.4}$$

Note that $Interest\ Rate$ consists of an interest rate *spread* and an underlying reference rate. Because I am only interested in the change to spreads, I directly translate it into changes in interest rates. Keeping reference rates constant, a decrease in spreads and increase in maturities can be directly translated into a reduction in aggregate corporate debt servicing costs.

Because the BAPCPA setting allows me to calculate the elasticity of a change in spreads to a change in caseload, I could directly calculate its aggregate effects by replacing $\Delta Interest\ Rate_t$ in equation 4.4 with the estimated elasticity:

$$\Delta Interest\ Burden = \frac{-90.144\ /\ 10,000}{1092.878} \times Caseload\ Drop \times Debt, \tag{4.5}$$

where $-90.144\ /\ 10,000$ is the estimated coefficient for the change in spreads (in basis points) and 1092.878 the estimated coefficient for the caseload drop. $Caseload\ Drop$ specifies the size of the drop in the weighted caseload per judge one is interested in. However, the aggregate effect may depend crucially on how firms differ in their exposure to such changes and how much debt they have. For example, firms that would be highly exposed to an increase in judicial efficiency may be large and highly levered, or small and hold no debt at all. I thus allow for firm-level variation in the caseload drop and outstanding debt, which yields:

$$\Delta Interest\ Burden_i = \sum_i \left(\frac{-90.144\ /\ 10,000}{1092.878} \times Caseload\ Drop_i \times Debt_i \right). \tag{4.6}$$

I will refer to equation 4.6 as "direct estimation", compared to the formula-based estimation in equation 4.3. The advantage of calculating effects directly is that I can use firm-level variation in outstanding debt and exposure without having to assume homogeneity in a firm's debt structure (bonds versus loans) or average financing terms. A disadvantage is that I cannot easily gauge the effect of a change in maturities, because calculating the outstanding maturity of a firm's outstanding debt is difficult (see also Drehmann et al., 2015). Another disadvantage is that the large, publicly listed borrowers in my estimation sample are not representative; this introduces a strong downward bias, because smaller firms are more likely to file for bankruptcy, and thus more affected by changes to the efficiency of bankruptcy courts (see the results section 4.5.2). The firms in my sample also hold only a fraction of total non-financial corporate debt in the US, and may pay different spreads on bonds and loans.

I address these challenges two-fold. First, I will scale up the outcomes of equation 4.6 using the share of debt of firms in my sample as a fraction of total debt to arrive at more representative values. Note that to maximize the external validity of my calculation, I use data on all firms in the matched Dealscan-Compustat data set for which I can identify their bankruptcy district.[35] Second, I compare the firm-level estimates to changing the inputs of the aggregate debt service ratio in equation 4.3, which I refer to as "formula-based". This has the advantage that it allows for different interest rates and maturities for loans and bonds (which are assumed to be equal across firms), and also makes calculating changes to maturities manageable. According to data from the US Financial Accounts, non-financial corporates had a total of \$5,070.1 billion in debt at the end of 2004 (around 58% in bonds), and \$7,680 by the end of 2016 (around 66% in bonds).

4.6.3 BAPCPA and the Interest Burden of Corporate Borrowers

Equipped with point estimates for the effect of a drop in caseload on the spreads of corporate borrowers, and the methodology outlined above, we can make a back-of-the-envelope calculation for the macroeconomic savings of BAPCPA. To quantify the effect, I use the total drop in the caseload per judge between 2006-2007 and 2004-2005, equal to around 493 hours (see section 4.2). Because firms were more exposed to the caseload drop than others, I weight the firm estimates by the district's exposure, i.e. the non-business share in the weighted caseload of the bankruptcy district. This yields the following equation:

$$\Delta Interest\ Burden = \sum_i \left(\frac{-90.144\ /\ 10,000}{1092.878} \times 493 \times Exposure_i \times Debt_i \right), \qquad (4.7)$$

where $Debt_i$ is the sum of long-term debt and debt in current liabilities from Compustat, averaged over 2004 and 2005. As outlined above, I assume that the district differences in $Exposure$ map linearly into improvements in caseload (1092.878 from table 4.2) and spreads (-90.144 from column (7) in table 4.14). The assumption is thus that lower caseload allows firms to roll over pre-existing debt at better financing conditions – which is consistent with my findings on borrowers with exogenous financing needs in section 4.5.2.

[35]Note that the results presented here yield highly similar results if I instead use only the firms in the estimation sample.

Table 4.7: THE SOCIAL COSTS OF CONGESTED BANKRUPTCY COURTS

This table presents the inputs required to calculate the social costs of excessive bankruptcy court caseload. The *Implied elasticity* is calculated as the ratio of $Exposure$ on spreads, divided by $Exposure$ on the caseload drop. The *Estimated drop in spreads* is calculated as the *Drop in caseload per judge (average)* times the implied elasticity. The *Judge multiplier* is the ratio of savings in interest burden to required judgeships. *Net gains* are the difference between the savings in interest butden and required judgeships.

	Direct Estimation		Formula-Based
	In-sample debt	Total corporate debt	Total corporate debt
	(1)	(2)	(3)
Panel A: Point Estimates			
$Exposure$ on spreads		-90	
$Exposure$ on caseload drop		1,092.878	
Implied elasticity (bps/caseload hour)		-0.082	
Panel B: Estimated Effect of BAPCPA			
Total debt (2004, in \$ billion)	2,686	5,070.1	5,070.1
Drop in caseload per judge (average)	493	493	493
Estimated drop in spreads	-40.599	-40.599	-40.599
Savings in interest burden (in \$ billion)	8.109	15.308	9.536
Panel C: Estimated Effect of Bankruptcy Judgeship Act of 2017			
Total debt (latest, in \$ billion)	5,303	7,680	7,680
Drop in caseload per judge (average)	154	154	154
Savings in interest burden (in \$ billion)	0.476	0.689	0.414
Required judgeships (in \$ billion)	0.004	0.004	0.004
Judge multiplier	119	172	104
Net gains	0.472	0.685	0.41
Panel D: Estimated Effect of New Judges for Highly Congested Courts			
Total debt (latest, in \$ billion)	5,303	7,680	7,680
Drop in caseload per judge (average)	310	310	310
Savings in interest burden (in \$ billion)	0.681	0.986	0.592
Required judgeships (in \$ billion)	0.008	0.008	0.008
Judge multiplier	85	123	74
Net gains	0.677	0.982	0.588

I start with the in-sample calculation for the publicly listed firms in the matched Dealscan-Compustat sample in column (1) of table 4.7. The borrowers in the sample had a total of $2,686 billion in outstanding debt prior to BAPCPA in 2004. This means they account for a substantial fraction of 53% of the total outstanding debt of non-financial corporates of around $5,070 in that year. Equation 4.6 implies that the drop in caseload per judge following BAPCPA saved the sample firms $8.109 billion in interest payments *per year* - a substantial magnitude, even if one abstracts from changes to maturities.

Next, I turn to the aggregate debt service burden of *all* non-financial corporations in column (2). Because the borrowers in my sample make up a significant part of total borrowing, I their geographical distribution is likely a solid approximation for the geographical distribution of total debt. With that assumption in mind, I can calculate the macroeconomic savings due to BAPCPA by scaling up the in-sample estimate of $8.109 billion by the factor of total to in-sample debt ($\frac{\$5{,}070}{\$2{,}686}$ billion). This implies total interest burden savings of $15.308 billion as a result of the caseload drop following BAPCPA.

How do these firm-level estimates compare to those from changing the inputs in the formula in equation 4.3? Answering this question requires a number of additional inputs, are assumed to be equal across firms: the fraction of total debt accounted for by bonds and loans, as well as the average maturity and interest rates of the outstanding debt in each category. I describe how to calculate these in more detail in the online appendix and compare them to data reported by the BIS. Accounting for the drop in spreads due to BAPCPA in the aggregate implies total savings of $9.536 billion. This is slightly lower than the estimate of $15.307 billion when taking heterogeneity in firm-level exposure into account. It does, however, suggest that changes to the aggregate interest burden yield similar results to those based on firm-level data.

How does these savings in interest burden compare to the costs of BAPCPA? The United States Government Accountability Office estimated implementation costs of around $72.4 million – but these were almost exclusively related to the legal changes for consumer cases, and in fact do not incorporate the 28 new temporary judgeships BAPCPA created (USGAO, 2008). As I will discuss in more detail below, bankruptcy judgeships cost approximately $1 million a year, adding $28 million in annual expenses. These numbers pale in comparison with the estimated savings from the exogenous drop in caseload for my sample firms alone, which clocked in at around $8.1 *billion*.

4.6.4 The Costs and Benefits of Resolving Excessive Court Caseload

BAPCPA was a watershed event in the history of bankruptcy in the United States. This makes it attractive to isolate the causal effect of judicial efficiency, but less attractive as an indication of what constitutes congestion in US bankruptcy courts. After all, the caseload drop following BAPCPA was a reflection of a substantial legal change, and specifying the excessive or undesirable component of the caseload per judge is clearly challenging. In this section, I propose two simple approaches and calculate the costs of making courts less busy.

First, I will use the Bankruptcy Judgeship Act of 2017 as a template for a minimum desirable reduction in caseload. The Act passed the Senate on September 5, 2017 and added four new permanent judgeships for the districts Delaware (2), Florida Middle (1), and Michigan East (1), following the recommendations of the Judicial Conference.[36] Data from the Federal Judicial Caseload Statistics for Q1/2018 and Q1/2017 allow me to calculate how the new judgeship affected the annual change in the weighted caseload per judge. For Delaware, the judge workload changed from around 626 to 476 hours (a drop of 150); for Florida Middle from 906 to 766 (a drop of 140); and for Michigan East from 948 to 777 (a drop of 171). The average drop was 154 hours per judge per year.

Second, I will estimate the effect of reducing the caseload per judge of districts above the 90th percentile in Q1/2017 by hiring an additional judge in each of these districts.[37] This would reduce the annual workload per judge in these courts by an average of 310 hours, ranging from a minimum of 102 caseload hours in the northern district of Illinois to a maximum of 731 hours in the northern district of Mississippi. Note that these courts do not overlap with the districts receiving new judgeships as part of the 2017 bill: the average weighted caseload per judge of Delaware, Florida Middle, and Michigan East in Q1/2018 was 827 hours per year, which lies between the 75th and 90th percentile of the caseload distribution.

In both scenarios, the majority of courts would be unaffected by hiring additional judges. I thus begin by using the firm-level calculation as in equation 4.6 to calculate the savings in interest rate burden, and then scale up these values using the frac-

[36] See `https://www.congress.gov/bill/115th-congress/senate-bill/1107/text` for the full text of the bill and `http://www.uscourts.gov/news/2017/04/07/judiciary-seeks-bankruptcy-judgeships-warns-crisis` for the recommendations of the Judicial Conference. The Act also made permanent many previously temporary judgeships that were extended on a regular basis. Since these judges were already in place, this does not have an immediate effect on the court workload.

[37] In Q1/2017, these were the districts AL,M; GA,N; IL,N; LA,W; MO,E; MS,N; TN,W; and TX,E.

tion of total debt in Compustat in my sample to total non-financial corporate debt in Q4/2016.[38] The in-sample firms accounted for approximately 69% of total debt in 2016 ($\frac{\$5{,}303}{\$7{,}680}\ billion \approx 0.69$).

For the Bankruptcy Judgeship Act of 2017, this procedure yields estimated savings in the interest burden of approximately $689 million for the entire stock of non-financial corporate debt ($476 million in-sample). If one were to hire an additional judge in each of districts with a caseload above the 90th percentile, the savings would amount to $986 million ($681 million in-sample). How do these numbers compare to those one would arrive at using the aggregate formula 4.3, ignoring the firm heterogeneity? My estimates suggest a somewhat lower drop of $414 million for the Bankruptcy Judgeship Act and $592 million in the high-congestion scenario. Again, these differences arise because the aggregate debt service formula relies on assumptions about the maturity of outstanding debt on one hand, but also allows for different financing terms for loans and bonds. Taking the simple average of the four estimates on the total non-financial corporate debt suggest that the costs of overburdened courts are indeed "enormous": at least $670 million per year.

How much would it cost to resolve excessive court inefficiency? For the Bankruptcy Judgeship Act of 2017, the Congressional Budget Office estimates annual salaries and benefits for bankruptcy judges of about $232,000.[39] The Congressional Budget Office also provides an estimate for judicial administrative costs for personnel, security, and court operations of about $700,000 per judge per year. Hiring an additional judge would thus cost approximately $932,000; I will round this estimate up to $1,000,000 for simplicity. Four judges in the Bankruptcy Judgeship Act scenario thus would cost $4,000,000, and the 8 new judgeships required by the 90th percentile scenario $8,000,000. Clearly, these costs are miniscule compared to the estimated benefits: from a fiscal policy perspective, they imply a "judge multiplier" of above 100 in almost all estimations. The average multiplier for the four total debt estimates is 118.

Taken together, I conclude that the social costs of court backlog are large, and that addressing these costs could be a potentially profitable avenue for government expenditures. Again, these findings rely on a number of simplifying assumptions. Despite this caveat, the range of values I present is likely an extreme lower bound for

[38]More precisely, I use the sum of debt in current liabilities and total long-term debt of firms in the Dealscan-Compustat sample, and average these values over 2015 through 2017 (as available).

[39]Bankruptcy judges are entitled to compensation equal to 92% of that of a district judge, which puts their listed annual salary at approximately $191,000. See https://www.law.cornell.edu/uscode/text/28/153 for the background covering bankruptcy judge compensation and http://www.uscourts.gov/judges-judgeships/judicial-compensation for the time series of judicial pay. District judges in the United States were entitled to $208,100 in compensation in 2018.

multiple reasons. First, they are solely based on the effect of judicial caseload on publicly listed firms with low bankruptcy risk. Second, they are based on some of the smallest point estimates I find. Third, they ignore knock-on effects of financing terms on foregone firm investments and employment, as well as the effect of judicial efficiency on the interest burden of households. Fourth, I do not consider costs arising from an inefficient resolution of bankruptcy cases due to congested courts (see e.g. Iverson, 2016). Fifth, up to this point, I have not considered the effect of caseload on loan maturities. All of these factors bias my estimates downwards.

4.6.5 Incorporating the Effect on Loan Maturities

Up until this point, I have ignored the effect of an exogenous drop in caseload on loan maturities. This is to be conservative: Drehmann et al. (2015) show that even small changes to the average maturity of outstanding debt can drastically shift the interest burden, because repayment is spread out over a longer time period. The details of this exercise are reported in the online appendix in table 4.17.

Allowing for court caseload to affect debt maturities increases my estimates of its social costs. For the BAPCPA evaluation, the formula-based calculation implies total savings in the corporate interest burden of \$47.274 billion when incorporating maturities compared to \$15.307 billion when considering the change in spreads only. For the Bankruptcy Judgeship Act of 2017, the effect is \$2.111 billion, up from \$685 million. And for the 90th percentile scenario, incorporating maturities increases the formula-based estimate from \$982 million to \$3.014 billion. I take these values as suggesting, again, that my assessment of the costs of court congestion above are likely to be highly conservative, and that resolving excessive caseload may yield even larger dividends.

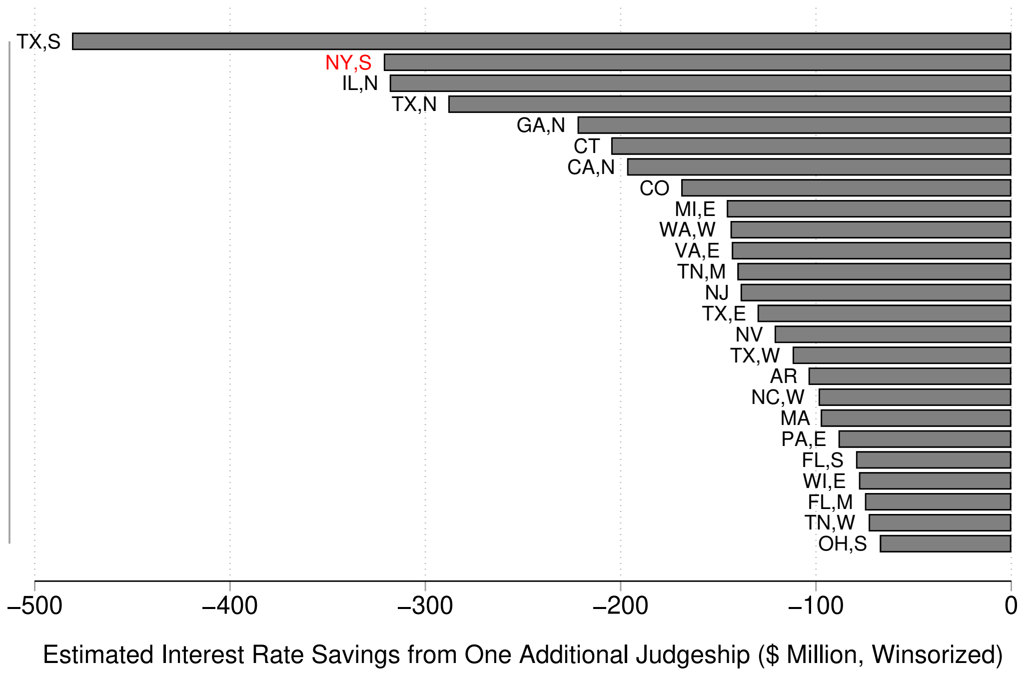
4.6.6 The Return to Judges in 71 Bankruptcy Districts

The calculation above suggests that we can uncover the benefits associated with hiring additional judgeships in each bankruptcy district. In other words, I can answer the question which districts – according to my estimates – would yield the highest social returns if they were given an additional judgeship. To arrive at the estimated savings per district, I use equation 4.6, plug in how much the caseload per judge would drop with one additional judgeship (assuming a constant caseload), sum the firm-level estimates by district, and scale up the district-level estimate using the share of total outstanding corporate debt to that in the sample.

Compared to a calculation of macroeconomic costs, this requires much fewer assumptions, because we can directly infer the returns to judges using the geographical distribution of corporate debt. Note, however, that the estimation still ignores household debt, which in many areas might be a considerably more important factor. I will also restrict the exercise to the case of *one* additional judgeship to maximize the likelihood that the linear reduced-form model I estimated for the BAPCPA caseload drop yields a valid approximation. I also drop districts with less than three headquartered firms.

Table 4.18 in the online appendix plots the results for each of the 71 remaining bankruptcy districts in the sample. I also plot information on the key inputs, namely the caseload per judge and its projected drop for hiring an additional judge (as of Q1/2017); the number of firms and their total debt per district; and the average and total savings in interest burden. To reduce the influence of individual firms, I also present the total and average savings after winsorizing the firm-level estimates at the 1% level; however, 23 out of the 25 top districts remain unchanged.

Figure 4.5: 25 Bankruptcy Districts with the Highest Return to Judges



This figure plots the estimated savings in corporate interest burden from creating one additional judgeship by district, as described in the text. The southern district of New York (NY,S) is marked red, because it represents an outlier in the relationship of the non-business caseload share and the drop in caseload per judge following BAPCPA (see figure 4.3).

Figure 4.5 plots the 25 districts with the highest total savings in corporate interest burden for hiring one additional judge (based on the winsorized values). The southern district of Texas has the highest savings, followed by the southern district of New York, and the northern districts of Illinois, Texas, and Georgia. However, the inclusion of the southern district of New York should be taken with a grain of salt, given that it is an outlier in the relationship between the non-business caseload share and caseload drop following BAPCPA.

Is the order of districts I find mechanically driven by the estimated drop in caseload per judge? In figure 4.8 in the online appendix, I show that this is not the case. Instead, the district-level savings are highly correlated with the distribution of outstanding debt (-0.74) and the number of firms in a district (-0.39, unreported).

Because the savings are measured in $ million per year, and the costs of an additional judgeship are approximately $1 million per year, we can directly interpret these values as returns to hiring a bankruptcy judge. Clearly, many factors go into the decision where new judges should be hired. The Bankruptcy Judgeship Act of 2017 is a prime example, which only partially reflected the corporate returns to judgeships: it added a new judgeship to the eastern district of Michigan (ranked #1 in the basic and #9 in the winsorized return measure), but also Delaware and the middle district of Florida (ranked #23 and #53 in the winsorized measure). Nevertheless, assessing the potential benefits from reducing the workload of bankruptcy judges using my approach could serve as a transparent quantitative input for policy makers.

4.7 Conclusion

A large literature has identified legal frameworks as important predictors of financial contracting. Yet, there is little evidence on how much the implementation of laws into practice matters quantitatively. I draw on the US bankruptcy system to shed some light on this important issue.

Exploiting exposure to the Bankruptcy Abuse and Consumer Protection Act of 2005 as an exogenous shock to court caseload, I show that judicial efficiency has quantitatively large effects on the ex-ante terms of corporate credit. The implied effects are large: estimations imply that BAPCPA decreased interest rate spreads by 40 basis points and increased maturities by 10%, equivalent about half a year.

By exploiting pre-determined borrower characteristics, I can further disentangle how much of this effect is driven by credit supply, rather than unobserved demand across bankruptcy districts. I find that the *entire* impact of the BAPCPA-related drop in court backlog is due to changes in credit supply. This is also consistent with the time series pattern: loan terms remained unchanged during the passing and effective implementation of BAPCPA in April and October 2005, but improved drastically once the caseload of bankruptcy judges saw a dramatic drop in early 2006.

The relatively mild identifying assumptions that are required to interpret the BAPCPA shock as a causal effect of court caseload on ex-ante contracting enable me to calculate the social costs of inadequate court staffing. Given that interest rate spreads are a price term, I can back out the social costs of inefficient enforcement (as measured by court backlog) under a set of simplifying assumptions. This exercise implies that the halving of caseload per judge in the wake of BAPCPA saved US non-financial corporates between $9.5 and $15.3 billion in interest payments, even in the most conservative calculation.

I also use two approaches to quantify the costs of "excessive" court backlog today and the benefits of resolving it. The results suggest that busy bankruptcy courts have macroeconomic costs of at least $670 million per year, and hiring additional judges to ease these costs would have implied fiscal multipliers of above 100. My approach also uncovers the districts with the highest social returns to judges for the corporate interest burden, which can serve as a transparent quantitative input for policy makers.

An important open question coming out of these findings is the extent to which they apply to judicial efficiency in other settings. While the corporate loan market provides me with a relatively clean set-up, financial contracts may be particularly exposed to the reliability of court outcomes; this is also reflected in findings that liquidation values and default risk explain the bulk of variation in credit spreads (e.g. Benmelech et al., 2005; Gilchrist and ZakrajÅ¡ek, 2012). Future work should provide more evidence from other areas of legal enforcement.

Appendix

Table 4.8: BANKRUPTCY CASE WEIGHTS

This table shows the average number of hours spent on bankruptcy cases of a particular type, building on the findings of Bermant et al. (1991).

Type of case	Case weight in hours
Chapter 7 Business	0.397
Chapter 7 Consumer	0.101
Chapter 11	7.559
Chapter 12	4.040
Chapter 13	0.381
Other cases	0.194

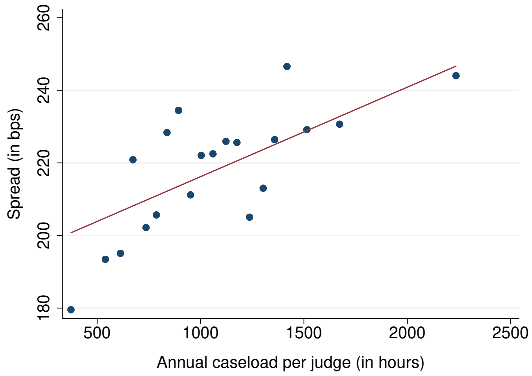
Online Appendix

A Cross-Sectional Correlations

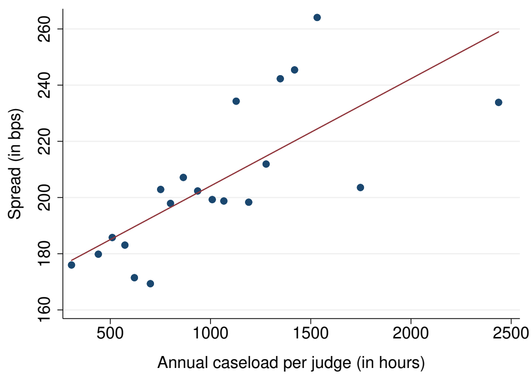
Figure 4.6: Caseload per Judge and Loan Terms – Cross-Sectional Correlations

All-in-drawn Interest Rate Spreads

(a) Dealscan-Compustat Sample

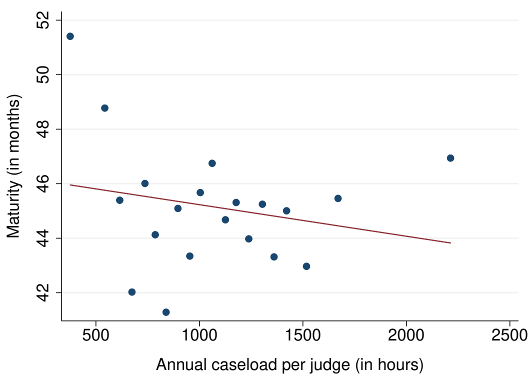


(b) Estimation Sample

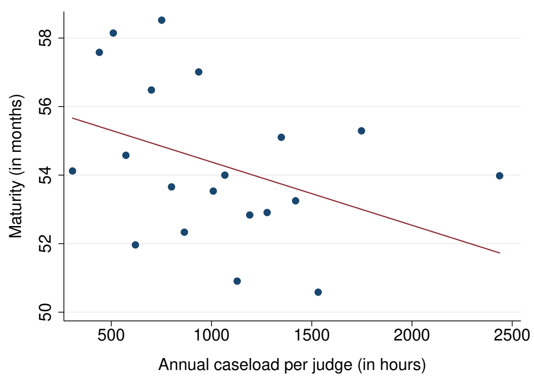


Maturity at Issuance in Months

(c) Dealscan-Compustat Sample



(d) Estimation Sample



Notes: These figures show binned scatter plots, where the interest rate spread and loan maturity are grouped into 20 quantiles and plotted against the caseload per judge in each bankruptcy district. Panels (A) and (C) use the entire matched Dealscan-Compustat sample from 1987 to 2012. Panels (B) and (D) use the estimation sample.

Table 4.9: VARIABLE DEFINITIONS

Variable	Description
Borrower characteristics (Compustat)	
Book leverage	[Long-term debt (dltt) + debt in current liabilities (dlc)] / Total assets (at).
Log(Total assets)	Natural logarithm of total assets (at).
ROA	Operating income before depreciation (oibdp) / Total assets (at).
Negative debt-to-cash flow	[Long-term debt (dltt) + debt in current liabilities (dlc)] / [Operating income before depreciation (oibdp) + Depreciation and amortization (dp). Equal to 1 for negative values.
High debt-to-cash flow	[Long-term debt (dltt) + debt in current liabilities (dlc)] / [Operating income before depreciation (oibdp) + Depreciation and amortization (dp). Equal to 1 for the top quartile.
Sales growth	Growth in sales/turnover (net) [(sale - sale (t - 1)) / sale (t - 1)].
Rating dummy	Equal to 1 if a firm has any rating from Standard & Poors, Fitch, Moody's, or Duffs & Phelps.
Tangibility	[Property, plant and equipment (ppent) / Total assets (at).
Pre-reform borrower characteristics (Compustat/Dealscan, average for 2004 and 2005)	
Refinancing need	Dummy variable equal to 1 for borrowers with a loan in Dealscan with an issuance date prior to 2005 and a maturity date in 2006 or 2007.
Firm rating	Numerical credit rating, ranging from AAA to D.
Log(Total assets)	Natural logarithm of total assets (at).
Market leverage	[Long-term debt (dltt) + debt in current liabilities (dlc)] / [Market value of capital (csho $\times$ prcc_c + dlc + dltt)].
Loan characteristics (Dealscan)	
Interest rate spread	Interest rate spread, usually over LIBOR, in basis points.
Log(Maturity)	Natural logarithm of loan maturity in months.
Log(Loan size)	Natural logarithm of facility amount in million USD.
Collateral dummy	Equal to 1 if loan is backed by collateral.
Relationship dummy	Equal to 1 if a firm received a bank from the same lead bank before.
Term loan dummy	Equal to 1 if loan is a term loan.
Bankruptcy district characteristics (US Courts)	
Exposure	The share of non-business cases in the total weighted caseload.
High congestion dummy	Equal to 1 for districts in the top quartile of a state's $Exposure$; 0 for the bottom quartile. Missing for states with a single district.
County characteristics (BEA/US Census)	
Manufacturing emp. share	Share of employees working in manufacturing in 2003 (BEA).
Finance emp. share	Share of employees working in finance in 2003 (BEA).
Income per capita	Income per capita in 2003 (BEA).
Population density	Population scaled over county area (Census).
Republican vote (2000)	Republican share in total votes (Census).
High school graduates/Pop.	Share of population with a high school degree or higher (Census).
Asians/Pop.	Share of population classified as Asian (Census).
Hispanics/Pop.	Share of population classified as Hispanic (Census).
African-Americans/Pop.	Share of population classified as African American (Census).
Owner-occ. housing share	Share of housing occupied by owners (Census)
Δ Real estate emp. ('01-'06)	Percentage change in the number of employees in real estate (BEA).
Δ Construction emp. ('01-'06)	Percentage change of construction employees (BEA).
House price growth ('02-'06)	Percentage change in the Federal Housing Finance Agency's All-Transactions house price index between 2002 and 2006. I fill in house prices in the following order: five-digit ZIP code, three-digit ZIP code, county, state.

B BAPCPA and Court Efficiency – Evidence from Chapter 7 Costs

Table 4.11: Effect of BAPCPA on Chapter 7 Case Outcomes

This table shows the estimated coefficients of case-level regressions on the effect of BAPCPA on chapter 7 case outcomes reported to the US Trustee Program. The regression specification is

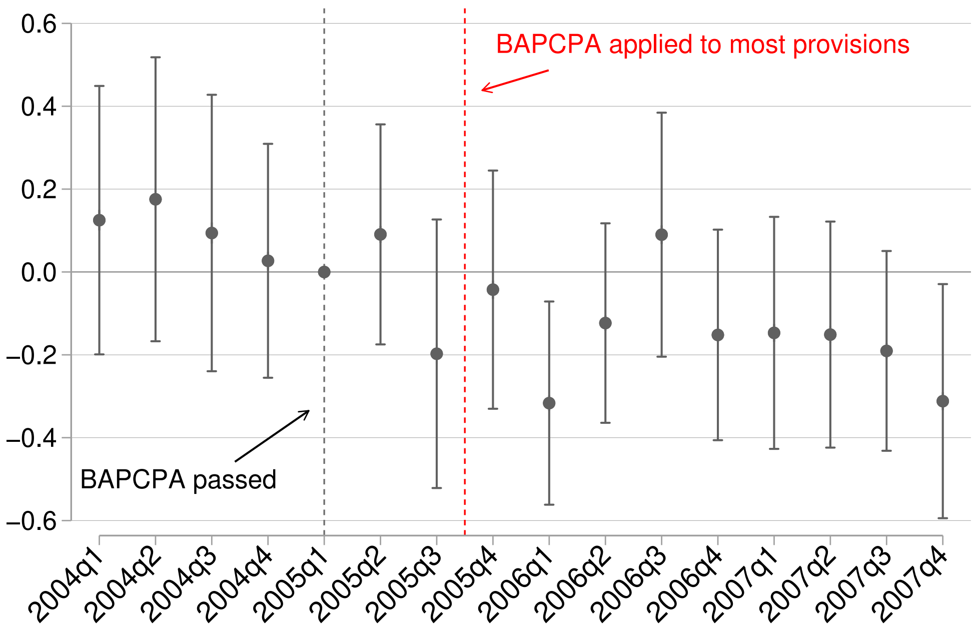
$$Y_c = \alpha_s + \alpha_t + \beta Post-BAPCPA_t \times Exposure_s + \gamma X_c + \varepsilon_c,$$

where c, s, and t index bankruptcy cases, states, and year-months, respectively . $Post_t$ is a dummy equal to 1 for the years 2006 and 2007, and 0 for 2004 and 2005. $Exposure_s$ is the share of the weighted non-business bankruptcy caseload in the total district caseload in 2003. Because the UST office regions and bankruptcy districts cannot be clearly assigned to each other, the sample is restricted to the 27 states with a single bankruptcy district. The case-level control variables X_c are the log number of days a case took and the logarithm of the total amount of gross receipts. Standard errors reported in parentheses are clustered by state and year-month.

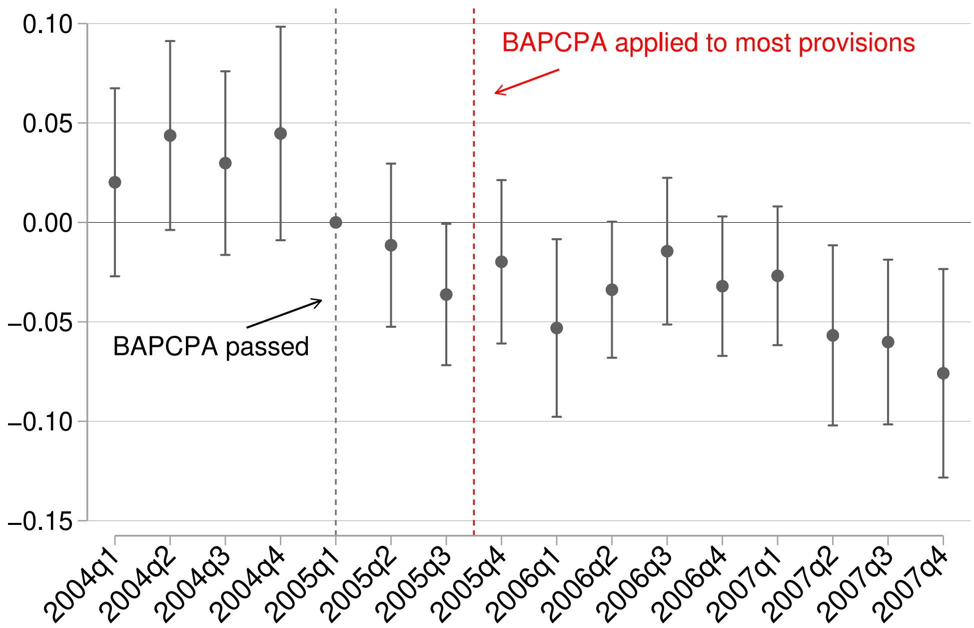
	Log(Total Fees) (1)	Log(Court Costs) (2)	Court Costs/ Total Fees (3)	Net Receipts/ Fees (4)
Post-BAPCPA × Exposure	-0.204***	-1.389***	-0.060***	0.699***
	(0.069)	(0.274)	(0.010)	(0.192)
Log(Days)	0.300***	0.785***	0.016***	-0.764***
	(0.006)	(0.024)	(0.001)	(0.018)
Log(Gross Receipts)	0.879***	0.090***	-0.007***	0.403***
	(0.003)	(0.008)	(0.000)	(0.009)
State FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Observations	86,951	86,951	86,879	86,879
Adj. R^2	0.896	0.085	0.087	0.211

Figure 4.7: The Impact of BAPCPA on Consumer Chapter 7 Outcomes

Panel A: Log(Total Bankruptcy Fees)



Panel B: Court Costs / Total Bankruptcy Fees



These figures show the dynamic effect of BAPCPA on chapter 7 outcomes in cases supervised by the US Trustee Program. They plot the estimated coefficients β of the regression $Y_c = \beta \sum_{t=2004q1}^{2007q4} D_t \times Exposure_s + \gamma X_c + \alpha_t + \alpha_s + \varepsilon_c$, where Y_c refers to the natural logarithm of total bankruptcy fees in panel A and the ratio of court costs to total bankruptcy fees in Panel B. The sample are the 27 states with a single bankruptcy district. X_c includes the log number of court days and log value of gross proceeds of each case as control variables. I also include state (s) and year-month (t) fixed effects. Standard errors are clustered by state and year-month.

C Does Exposure to BAPCPA Capture Lower Ex-Post Borrower Risk?

Table 4.12: FIRM BANKRUPTCIES AROUND BAPCPA AND THE FINANCIAL CRISIS

This table presents the estimated coefficients of panel regressions on the district-year level. The dependent variable is the change in the number of bankruptcy cases/employees in a district, where bankruptcies refer to all firm cases in column (1) and (2) and chapter 11 cases in column (3) and (4). All regressions include district and year fixed effects. Standard errors reported in parentheses are clustered by district and year.

	Δ Firm Cases/Employees		Δ Ch. 11 Cases/Employees	
	(1)	(2)	(3)	(4)
Post-BAPCPA $\times$ Exposure	0.064		-0.042	
	(0.147)		(0.055)	
Crisis $\times$ Exposure		-0.033		-0.050
		(0.047)		(0.034)
Observations	1,280	2,560	1,280	2,560
Adj. R^2	0.136	0.111	0.078	0.045
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

D Additional Robustness Checks

Table 4.13: BAPCPA AND LOAN CONTRACT TERMS: LEGISLATIVE STEPS

This table presents estimated coefficients of the relation between corporate loan terms and the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). The dependent variables is the interest rate spread or natural logarithm of the loan maturity. $Post - BAPCPA$ is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Senate Intro.* is 1 for the period after February 1, 2005, and 0 otherwise. *Senate Pass* is 1 for the period after March 10, 2005, and 0 otherwise. *Applied* is 1 for the period after October 17, 2005, and 0 otherwise. See text for description of other variables. Standard errors reported in parentheses are clustered by district and year.

	Interest rate spread	Log(Maturity)
	(1)	(2)
Post-BAPCPA × Exposure	-117.098***	0.693***
	(31.399)	(0.197)
Senate Intro. × Exposure	-3.717	-0.286*
	(21.912)	(0.164)
Senate Pass × Exposure	-4.737	0.163
	(15.327)	(0.099)
Applied × Exposure	-0.237	-0.126
	(11.861)	(0.084)
Observations	3,445	3,445
Adj. R^2	0.778	0.546
Loan facility controls	Yes	Yes
Borrower controls	Yes	Yes
Borrower FE	Yes	Yes
Year FE	Yes	Yes

Table 4.14: BAPCPA and Loan Contract Terms: Robustness Checks

This table presents estimated coefficients of the relation between corporate loan terms and the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). The dependent variables are loan maturity or interest rate spread of a loan facility (both in natural logarithm). BAPCPA is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. See text for description of control variables. In column (7), I collapse the time series by taking the difference between a firm's average interest rate or maturity in the before and after period, and then regressing it on the *Exposure* variable as well as the pre-post difference in all control variables. Standard errors reported in parentheses are clustered by district-year, except in column (7) where they are clustered by district.

	Baseline (1)	Exclude 2007 (2)	Pre from 2000 (3)	Post until 2012 (4)	No controls (5)	Only same firm (6)	Collapsed first difference (7)	Bank $\times$ borrower FE (8)	Winsorized DE NY,S (9)	Placebo reform (10)
Panel A: Interest Rate Spread										
Post $\times$ Exposure	-124.896***	-118.944***	-83.280***	-72.333***	-55.804***	-122.923***	-90.144***	-64.571**	-154.920***	-11.883
	(26.716)	(37.648)	(28.141)	(21.657)	(21.965)	(26.533)	(22.456)	(33.284)	(47.732)	(23.633)
Observations	3,445	2,619	7,586	6,081	4,678	2,344	559	1,082	3,445	3,866
Adj. R^2	0.778	0.795	0.757	0.755	0.753	0.751	0.150	0.847	0.778	0.792
Panel B: Log(Loan Maturity)										
Post $\times$ Exposure	0.504***	0.474***	0.320**	0.234**	0.354***	0.491***	0.285**	0.574*	0.595**	-0.030
	(0.167)	(0.130)	(0.145)	(0.113)	(0.135)	(0.160)	(0.122)	(0.297)	(0.246)	(0.199)
Observations	3,445	2,619	7,586	6,081	5,133	2,344	559	1,082	3,445	3,866
Adj. R^2	0.544	0.580	0.560	0.517	0.451	0.490	0.044	0.652	0.544	0.613
Loan controls	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4.15: BAPCPA and Loan Contract Terms: Additional Robustness Checks

This table presents estimated coefficients of the relation between corporate loan terms and the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). The dependent variables are loan maturity or interest rate spread of a loan facility (both in natural logarithm). BAPCPA is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. See text for description of control variables. In column (2), I control for the interaction of the number of judges BAPCPA added to 14 districts with the $Post-BAPCPA$ dummy. Column (3) limits the sample to states with multiple districts and adds state × year FE. In columns (4) and (5), I add industry × year and $bank \times year$, respectively. Column (6) weights the baseline estimates by the number of firms in each district. Column (7) uses the weighted caseload share of non-business cases in 2004 (instead of 2003) as exposure variable. Standard errors reported in parentheses are clustered by district and year.

	Baseline (1)	BAPCPA judges control (2)	State × year FE (3)	Industry × year FE (4)	Bank × year FE (5)	Weighted least squares (6)	Caseload in 2004 (7)
Panel A: Interest Rate Spread							
Post-BAPCPA × Exposure	-124.896***	-126.104***	-232.969***	-121.969***	-113.361***	-157.206***	-94.098***
	(26.716)	(27.408)	(29.780)	(28.079)	(27.428)	(23.798)	(19.804)
Observations	3,445	3,445	2,594	3,431	3,185	3,445	3,445
Adj. R^2	0.778	0.778	0.798	0.797	0.826	0.774	0.778
Panel B: Log(Loan Maturity)							
Post-BAPCPA × Exposure	0.504***	0.492***	0.387*	0.420**	0.604**	0.647***	0.432***
	(0.167)	(0.167)	(0.233)	(0.170)	(0.259)	(0.176)	(0.140)
Observations	3,445	3,445	2,594	3,431	3,185	3,445	3,445
Adj. R^2	0.544	0.544	0.585	0.584	0.604	0.564	0.545
Loan controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4.16: BAPCPA and Loan Contract Terms: Alternative Standard Errors

This table presents estimated coefficients of the relation between corporate loan terms and the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). The dependent variables are loan maturity or interest rate spread of a loan facility (both in natural logarithm). BAPCPA is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. See text for description of control variables. Standard errors reported in parentheses are clustered as indicated, with clustering by district and year being the baseline.

	Interest Rate Spread (1)	Log(Loan Maturity) (2)
Post-BAPCPA × Exposure	-146.425	1.208
S.E. Cluster		
Robust	(59.812)**	(0.324)***
District × Year	(59.292)**	(0.252)***
District	(73.788)**	(0.381)***
State × Year	(58.709)***	(0.258)***
State	(60.774)**	(0.328)***
Borrower × Year	(64.186)**	(0.325)***
Borrower	(69.565)**	(0.359)***
Bank × Year	(61.662)**	(0.444)***
Bank	(71.285)**	(0.471)**
Loan controls	Yes	Yes
Borrower controls	Yes	Yes
Post-BAPCPA × County controls	Yes	Yes
Borrower FE	Yes	Yes
Year FE	Yes	Yes

E Social Cost Estimation – Additional Details

This section provides additional details on the inputs required to conduct a back-of-the-envelope calculation for the macroeconomic costs of excessive bankruptcy court caseload. The approach taken by Drehmann et al. (2015) requires estimates of the outstanding debt, average interest rates, and average maturity of non-financial corporations.

As outlined above, I use data from the US Financial Accounts to measure total outstanding debt. At the end of 2004, US non-financial corporates had \$5,070.1 billion in debt, out of which \$2,123.8 were loans and \$2,946.3 bonds. At the end of 2016, total debt was \$7,680, out of which \$2,598.8 were loans and \$5,081.2 bonds.

For the interest rates on the outstanding stock of loans, data from the Federal Reserve's Survey of Terms of Business Lending (Table E.2) suggests that the weighted-average effective loan rate on commercial and industrial loans was 3.39 percent in 2004, and 2.53 percent in 2016. For bonds, I construct the weighted-average coupon for bonds that were outstanding in 2016 from the Mergent Fixed Income Securities Database (FISD); this yields a bond interest rate of 5.39 percent for 2004 and 3.39 percent for 2016.

Calculating the interst burden also requires an estimate of the maturity of outstanding (rather than newly issued) loans and bonds. This is difficult in practice, because average remaining maturities may differ widely from contractual maturities due to refinancing arrangements such as rollovers. For loans, I thus use the generic 13 years average remaining maturity as in Drehmann et al. (2015) and the average corporate bond maturity reported in the World Bank Global Financial Database (10.8 years for 2004 and 12 years for 2016). Note that constructing a measure of the maturity of outstanding bonds from Mergent FISD data yields almost equivalent maturities.

With these ingredients, one can calculate the total non-financial interest burden as $\frac{Interest\ Rate}{(1-(1+Interest\ Rate)^{-Maturity})} \times Debt$. This yields a total interet rate burden of \$571.6 billion for 2004 and \$759.6 billion for 2016.

How do these values compare to what is reported by the BIS? For Q4/2016, for example, they report a debt service ratio for the US non-financial corporate sector of 40.1% (relative to corporate income). To arrive at the interest rate burden, we thus need a measure of corporate income. According to data from the Bureau of Economic Analysis, the sum of after-tax profits of non-financial corporate businesses and net interest and miscellaneous payments – which is what the BIS measure is based on – totaled \$1,258.9 in the same quarter. Put together, this suggests a corporate interest rate burden of \$504.8 billion, slightly lower than my estimate. The total profits of *all* non-financial businesses (including those not incorporated), minus taxes on corporate income, were $2,582.4 billion – implying an interest burden of about $1,035.5 billion.[40] As such, my baseline estimate of $759.6 billion seems reasonable.

In the formula-based estimation, I change the inputs in the interest burden calculation to reflect permanent changes in financing terms due to a drop in the bankruptcy caseload per judge. Because the average maturities and interest rates differ for bonds and loans, the results are also weighted by the share of these in aggregate. More formally, I use a firm-level version of equation 4.3 in the main text:

$$\begin{aligned} Interest\ Burden_i = & \frac{Interest\ Rate^b}{(1-(1+Interest\ Rate^b)^{-Maturity^b})} \times Debt_i \times \frac{Bonds}{TotalDebt} \\ & + \frac{Interest\ Rate^l}{(1-(1+Interest\ Rate^l)^{-Maturity^l})} \times Debt_i \times \frac{Loans}{TotalDebt}, \end{aligned} \tag{4.8}$$

where the subscripts b and l refer to bonds and loans, respectively. Equation 4.8 is calculated separately with the original inputs specified above and the new inputs assuming a change in caseload on spreads (and maturities). The difference between the original and new inputs, then, is the change in interest burden.

For the BAPCPA estimation, for example, I replace the loan interest rate ($Interest\ Rate^l$) of 3.39 percent pre-BAPCPA with $0.0339 - (0.00406 \times Exposure_i)$, where 0.00406 is the estimated effect of the caseload drop of 493 hours per judge around BAPCPA relative to the estimated coefficient of $Exposure$ on the caseload drop of $1,092.878$. $Exposure_i$ is a firm's non-business caseload share. For the "excessive caseload" estimation, I use a district's actual drop in the caseload per judge instead of $Exposure_i$.

As outlined above, incorporating maturities has a substantial effect on the debt burden of US corporates. This is intuitive, because even relatively small changes in the average time to maturity of the stock of debt reduce the annual payments. Drehmann et al. (2015), for example, show that changing the assumption of an average 13-year maturity on the debt of non-financial corporate debt in their sample to 10 years increases the ratio of debt service to corporate income by around 20%.

[40] This number is calculated as the difference of "Nonfinancial business; income before taxes" ($2,904.7 billion) and "Nonfinancial corporate business; taxes on corporate income" ($322.3 billion), both from the Federal Reserve's Financial Accounts.

Table 4.17: THE SOCIAL COSTS OF CONGESTED COURTS – INCORPORATING MATURITIES

	No Change to Maturities (Direct Estimation)		Change to Maturities (Formula-Based)
	In-sample debt (1)	Total corporate debt (2)	Total corporate debt (3)
Panel A: Point Estimates			
$Exposure$ on maturity	–	–	0.285
Panel B: Aggregate Debt Service Burden (2004)			
Total debt	2,686	5,070.1	5,070.1
Loans			
Outstanding		2,123.8	2,123.8
Weighted-average rate (in %)		3.39	3.39
Maturity of outstanding debt (in years)		13	13
Bonds			
Outstanding		2,946.3	2,946.3
Weighted-average rate (in %)		5.39	5.39
Maturity of outstanding debt (in years)		10.8	10.8
Corporate interest burden (in $ billion)		571.646	571.646
Panel C: Estimated Effect of BAPCPA			
Drop in caseload per judge (average)	493	493	493
Estimated drop in spreads	-40.599	-40.599	-40.599
Estimated increase in maturities	–	–	0.129
Savings in interest burden (in $ billion)	8.109	15.307	47.274
Panel D: Aggregate Debt Service Burden (2016)			
Total debt	5,303	7,680	7,680
Loans			
Outstanding		2,598.8	2,598.8
Weighted-average rate (in %)		2.53	2.53
Maturity of outstanding debt (in years)		13	13
Bonds			
Outstanding		5,081.2	5,081.2
Weighted-average rate (in %)		3.39	3.39
Maturity of outstanding debt (in years)		12	12
Corporate interest burden (in $ billion)		759.65	759.65
Panel E: Estimated Effect of Bankruptcy Judgeship Act of 2017			
Drop in caseload per judge (average)	154	154	154
Savings in interest burden (in $ billion)	0.476	0.689	2.115
Required judgeships (in $ billion)	0.004	0.004	0.004
Judge multiplier	119	172	529
Net gains	0.472	0.685	2.111
Panel F: Estimated Effect of New Judges for Highly Congested Courts			
Drop in caseload per judge (average)	310	310	310
Savings in interest burden (in $ billion)	0.681	0.986	3.018
Required judgeships (in $ billion)	0.008	0.008	0.008
Judge multiplier	85	123	377
Net gains	0.677	1.176	3.014

Table 4.18: Return on Judges, by District

Notes: This table plots the estimated savings in corporate interest burden from creating one additional judgeship by district, as described in the text. The values are sorted by the total interest savings based on winsorized firm-level estimates (column 9). I only plot the values for districts for which I can identify at least three firms.

						Scaled		Scaled, Winsorized (1%)	
District	Caseload per Judge (1)	Number of Judges (2)	Hypothetical Caseload Drop (3)	Number of Firms (4)	Total Firm Debt (5)	Average Interest Savings (6)	Total Interest Savings (7)	Average Interest Savings (8)	Total Interest Savings (9)
TX,S	786.3	6	131.0	184	315418.9	-2.7	-493.8	-2.6	-480.9
NY,S	*581.1*	*10*	*58.1*	*181*	*528945.3*	*-2.0*	*-367.2*	*-1.8*	*-321.3*
IL,N	1017.7	10	101.8	132	271481.3	-2.5	-330.1	-2.4	-318.3
TX,N	720.4	6	120.1	106	337281.2	-4.6	-483.8	-2.7	-288.2
GA,N	964.0	8	120.5	75	180108.0	-3.5	-259.3	-3.0	-222.1
CT	334.2	3	111.4	62	186128.8	-4.0	-247.7	-3.3	-204.8
CA,N	376.0	9	41.8	331	432079.7	-0.7	-215.6	-0.6	-196.7
CO	447.7	5	89.5	132	157969.8	-1.3	-169.0	-1.3	-169.0
MI,E	790.2	5	158.0	38	275042.2	-13.7	-519.3	-3.8	-145.7
WA,W	482.5	5	96.5	58	171405.5	-3.4	-197.6	-2.5	-143.8
VA,E	639.9	6	106.6	74	112331.3	-1.9	-143.1	-1.9	-143.1
TN,M	706.8	3	235.6	26	74349.2	-8.0	-209.3	-5.4	-140.2
NJ	732.2	9	81.4	136	142614.7	-1.0	-138.6	-1.0	-138.6
TX,E	676.1	2	338.0	31	45643.2	-5.9	-184.3	-4.2	-129.9
NV	626.5	4	156.6	48	64768.8	-2.5	-121.2	-2.5	-121.2
TX,W	504.5	5	100.9	69	117234.4	-2.0	-141.3	-1.6	-112.0
AR	720.2	3	240.1	15	72641.9	-13.9	-208.3	-6.9	-103.8
NC,W	430.0	2	215.0	25	41193.5	-4.2	-105.8	-3.9	-98.7
MA	449.1	5	89.8	203	90930.0	-0.5	-97.6	-0.5	-97.6

Table 4.18: Return on Judges, by District (continued)

District	Caseload per Judge (1)	Number of Judges (2)	Hypothetical Caseload Drop (3)	Number of Firms (4)	Total Firm Debt (5)	Scaled: Average Interest Savings (6)	Scaled: Total Interest Savings (7)	Scaled, Winsorized (1%): Average Interest Savings (8)	Scaled, Winsorized (1%): Total Interest Savings (9)
PA,E	398.1	6	66.4	78	131056.4	-1.3	-103.9	-1.1	-88.5
FL,S	738.2	7	105.5	75	63115.2	-1.1	-79.5	-1.1	-79.5
WI,E	532.0	4	133.0	40	49109.5	-2.0	-78.0	-2.0	-78.0
FL,M	805.7	8	100.7	76	62301.7	-1.0	-75.0	-1.0	-75.0
TN,W	817.6	5	163.5	9	37381.3	-8.1	-73.0	-8.1	-73.0
OH,S	431.7	7	61.7	44	91352.4	-1.5	-67.3	-1.5	-67.3
IL,C	221.7	3	73.9	4	76641.3	-16.9	-67.7	-16.3	-65.3
PA,W	452.1	4	113.0	31	54136.6	-2.4	-73.1	-2.1	-64.7
LA,W	842.0	3	280.7	9	35642.4	-13.3	-119.5	-7.2	-64.4
NE	357.6	2	178.8	18	30108.6	-3.6	-64.3	-3.5	-63.1
IN,S	637.3	4	159.3	22	30078.9	-2.6	-57.2	-2.6	-57.2
DC	161.8	1	161.8	8	29012.0	-7.0	-56.1	-7.0	-56.1
RI	244.0	1	244.0	10	35015.3	-10.2	-102.1	-5.4	-54.1
MN	360.1	4	90.0	74	47020.5	-0.7	-50.6	-0.7	-50.6
KS	489.2	4	122.3	18	50848.1	-4.1	-74.3	-2.8	-49.6
NC,M	316.9	3	105.6	27	36543.1	-1.7	-46.1	-1.7	-46.1
OH,N	380.8	8	47.6	51	79202.4	-0.9	-45.0	-0.9	-45.0
MD	542.8	7	77.5	51	48599.4	-0.9	-45.0	-0.9	-45.0
OK,W	264.6	3	88.2	16	34790.4	-2.3	-36.7	-2.3	-36.7
AZ	503.4	7	71.9	53	42386.8	-0.7	-36.4	-0.7	-36.4
NY,E	533.4	7	76.2	57	35826.9	-0.6	-32.6	-0.6	-32.6
TN,E	599.7	4	149.9	13	16243.4	-2.2	-29.1	-2.2	-29.1
IN,N	493.1	3	164.4	13	14694.1	-2.2	-28.9	-2.2	-28.9

Table 4.18: Return on Judges, by District (continued)

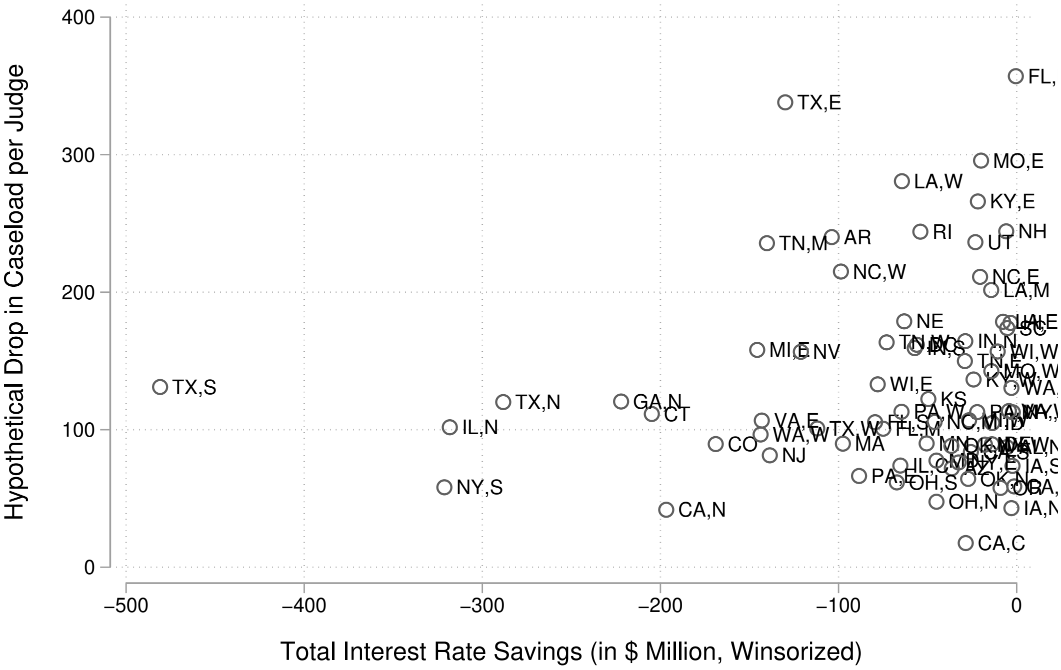
District	Caseload per Judge (1)	Number of Judges (2)	Hypothetical Caseload Drop (3)	Number of Firms (4)	Total Firm Debt (5)	Scaled: Average Interest Savings (6)	Scaled: Total Interest Savings (7)	Scaled, Winsorized (1%): Average Interest Savings (8)	Scaled, Winsorized (1%): Total Interest Savings (9)
CA,C	421.9	24	17.6	215	136211.4	-0.1	-28.6	-0.1	-28.6
OK,N	128.5	2	64.2	23	35512.5	-1.2	-27.3	-1.2	-27.3
MI,W	320.7	3	106.9	12	20934.0	-2.2	-26.7	-2.2	-26.7
CA,S	335.1	4	83.8	91	25726.6	-0.3	-25.7	-0.3	-25.7
KY,W	409.5	3	136.5	12	14855.8	-2.0	-24.2	-2.0	-24.2
UT	709.1	3	236.4	42	8212.8	-0.6	-23.2	-0.6	-23.2
PA,M	338.4	3	112.8	12	16441.4	-1.8	-22.2	-1.8	-22.2
KY,E	531.9	2	266.0	6	6856.4	-3.6	-21.8	-3.6	-21.8
NC,E	633.4	3	211.1	18	8149.0	-1.1	-20.6	-1.1	-20.6
MO,E	886.9	3	295.6	8	5662.5	-2.5	-20.0	-2.5	-20.0
NY,W	267.5	3	89.2	27	16738.0	-0.7	-17.8	-0.7	-17.8
LA,M	201.5	1	201.5	5	5944.8	-2.9	-14.3	-2.9	-14.3
MO,W	427.7	3	142.6	8	8354.8	-1.8	-14.2	-1.8	-14.2
DE	*536.5*	*6*	*89.4*	*6*	*13006.1*	*-2.3*	*-13.9*	*-2.3*	*-13.9*
ID	209.7	2	104.8	5	10936.8	-2.7	-13.7	-2.7	-13.7
WI,W	313.7	2	156.9	9	5691.2	-1.2	-10.7	-1.2	-10.7
OR	288.3	5	57.7	25	13138.5	-0.4	-9.1	-0.4	-9.1
LA,E	357.0	2	178.5	4	3595.4	-1.9	-7.7	-1.9	-7.7
NH	244.2	1	244.2	9	2015.7	-0.7	-5.9	-0.7	-5.9
AL,N	523.6	6	87.3	7	5327.4	-0.8	-5.6	-0.8	-5.6
SC	521.5	3	173.8	12	2547.5	-0.4	-5.3	-0.4	-5.3
VA,W	340.6	3	113.5	11	3228.9	-0.4	-4.4	-0.4	-4.4
HI	177.7	1	177.7	6	1630.7	-0.6	-3.5	-0.6	-3.5

Table 4.18: Return on Judges, by District (continued)

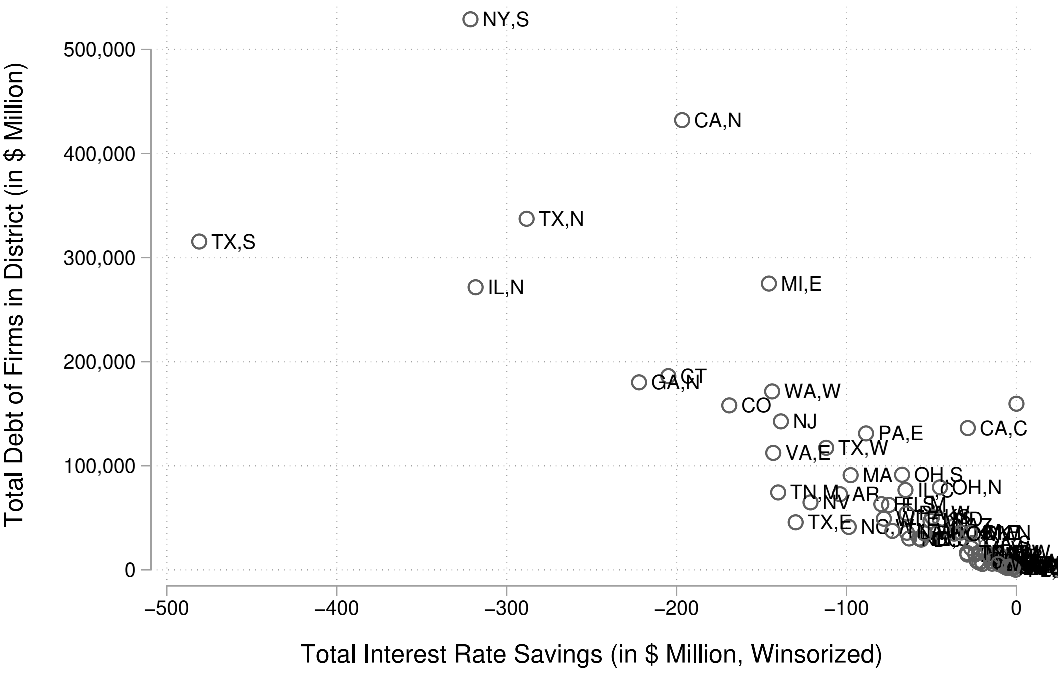
						Scaled		Scaled, Winsorized (1%)	
District	Caseload per Judge (1)	Number of Judges (2)	Hypothetical Caseload Drop (3)	Number of Firms (4)	Total Firm Debt (5)	Average Interest Savings (6)	Total Interest Savings (7)	Average Interest Savings (8)	Total Interest Savings (9)
IA,N	85.9	2	43.0	5	5774.0	-0.6	-3.0	-0.6	-3.0
WA,E	260.8	2	130.4	12	1902.3	-0.2	-3.0	-0.2	-3.0
IA,S	147.4	2	73.7	7	2674.1	-0.3	-2.4	-0.3	-2.4
NY,N	337.9	3	112.6	11	1612.1	-0.2	-2.2	-0.2	-2.2
CA,E	411.4	7	58.8	9	1951.2	-0.2	-1.4	-0.2	-1.4
FL,N	357.1	1	357.1	6	113.0	-0.1	-0.5	-0.1	-0.5

Figure 4.8: The Return on Judges – Correlations with Caseload Drop and Firm Location

Panel A: Returns to Judges and Hypothetical Drop in Caseload per Judge



Panel B: Returns to Judges and Firm Debt per District



These figures plot the correlation of the estimated total savings in corporate interest burden (firm values winsorized at 1% of the distribution) with the hypothetical drop in caseload per judge (Panel A) and the total outstanding debt of firms in the matched Dealscan-Compustat sample by district (Panel B). Districts where less than three firms in the sample are headquartered are omitted.

Chapter 5

Implications for Policy and Future Research

Up to this point, I have documented new stylized facts on credit markets around the world and attempted to provide a few answers about what might explain them. My results on electoral cycles and bankruptcy courts also raise the question how legal frameworks should be designed to regulate credit markets.

These new findings, however, raise many more questions that will take more than a single PhD thesis to answer. In this chapter, I discuss the implications of my thesis for future research and for policy makers. I begin by showing some preliminary results on the predictive ability of sectoral credit on banking crises, discuss implications for international credit cycles and factors shaping financial development, and propose a test on how to identify the effect of financial development on productivity through credit allocation. I then consider the policy implications of my work.

5.1 Implications for Future Research

A Credit Booms under the Microscope

In chapter 2, I document how the lending of banking systems around the globe has developed over the past 70 years. In this section, I use these data to provide some preliminary evidence on what drives recurring "credit booms gone bust" in the period after 1940, which have been subject of much attention in the wake of the Great Financial Crisis 2007-09. In particular, the granular data allow me to assess whether such lending booms are broad-based phenomena that differ substantial on a case-by-case basis, or rather a recurring theme concentrated in a few sectors. This has important policy implications: targeted instruments, such as macroprudential tools aimed at the housing market, are based on the assumption that rapid credit growth in specific sectors of the economy are particularly detrimental for medium-term macroeconomic performance. However, an often-voiced criticism is that such policy instruments may not get into "all the cracks" and further be subject to Goodhart's law; once in use, say the critics, the indicators used to activate them will lose their reliability.

I argue that if credit booms gone wrong are "sui generis" events, there should be little or no heterogeneity across sectors in their ability to predict banking crises.[1] This is another way of saying that such crises may be preceded by an unsustainable growth of debt in any sector of the economy. To gain intuition, I briefly review the relevant classes of theories on financial crises below, and then turn to an empirical test.

Theories of credit booms and financial crises: a brief review

Theories of the nature of financial crises following credit booms can be broadly grouped into two categories: Those based on aggregate frictions or overoptimism and those with a clear role for misallocation during the boom.[2] I briefly review different classes of theories to, which I will later Not all elements of these groups are mutually exclusive, but they do make distinct predictions about why crises are costly, and whether and how policy makers should try to prevent them.

Theories of general booms. The first group is made up of behavioral theories and theories on financial frictions. An important idea that has gained considerable attention is that crises are fundamentally the result of periods of excessive overoptimism or "sentiment", which leads to flawed decision making during a boom that is ultimately not justified by fundamentals. At the high point of overconfidence, which usually only becomes apparent ex-post, a key event triggers the reversal of expectations, leading to a contraction in economic activity. This idea is rooted in Austrian theories, such as Von Mises (1912) and Hayek (1929), and is probably most prominently featured in Kindleberger (1978) and Minsky (1986); important recent formulations include Greenwood et al. (2016) and Bordalo et al. (2017). It also meshes well with narrative accounts of many crisis episodes where investors were convinced that "this time is different" (Reinhart and Rogoff, 2009b).

Importantly, flawed belief formation in these theories is very general: It is not necessarily limited to specific sectors in the economy, such as finance, but may also hold for households (as in Case et al., 2012), or managers more generally (e.g. Greenwood and Hanson, 2015; Gennaioli et al., 2015). Mian and Sufi (2014a) stress how both borrowers and creditors play a role in boom phases. Because broad-based sentiment is the fundamental driver in this class of theories, many put emphasis on the unique

[1] I use the term "prediction" to refer to the in-sample predicted values by an independent variable of interest. This contrasts with the term "forecast", which refers to the out-of-sample predicted values.

[2] I focus here on explanations with financial frictions or agents without perfectly rational expectations. A large body of research shows that such frictions matter, and that return predictability with proxies of "sentiment" or "mispricing" is pervasive. See, among others, Shiller (2000) and López-Salido et al. (2017) for helpful pointers to the relevant literature.

nature of "bubble" episodes. For example, Kindleberger (1978) revisits crisis episodes arising from overinvestment in asset classes as diverse as Tulips, railroads, or government lending. Many authors have highlighted the role of utility and technology stocks in the stock market booms of the late 1920s and 1990s, respectively (see e.g. Barberis et al., 2016). Rajan and Ramcharan (2015) study the boom-bust pattern in agricultural land during the Great Depression.

The unifying feature of behavioral and frictional theories is that (leveraged) bubbles can arise in very different sectors and asset classes, independent of what sets them in motion. As such, a departure from rational expectations is not a necessary component: Models with perfectly rational agents can generate large externalities if individuals do not fully internalize the impact of their decisions on the macroeconomy. Such ex-post overborrowing can be induced by mechanisms such as nominal ridigities or the zero lower bound (e.g. Korinek and Simsek, 2016; Farhi and Werning, 2016; Eggertsson and Krugman, 2012).

Theories of misallocation. The second category of theories has a particular role for misallocation of resources during the boom phase preceding crises. There is now substantial evidence that capital misallocation played a critical role in the the financial crisis in the Eurozone countries (see e.g. Gopinath et al., 2017; Martin and Philippon, 2017).[3] However, the probably most striking empirical evidence in this area comes from the strong predictive ability of household credit, as compared to corporate credit, for particularly costly economic contractions (Büyükkarabacak and Valev, 2010; Jordà et al., 2014, 2015; Mian et al., 2017b).

More generally, Gorton and Ordoñez (2016) show that credit booms that are associated with a decline in productivity growth are more likely to end badly. They rationalize this finding in a model where a decrease in collateral screening can trigger credit booms, which eventually burst when the productivity slowdown leads to a wake-up call for producing information about borrowers. Similarly, Borio et al. (2016) show that credit booms are associated with labor reallocation towards low-productivity sectors (see also Cecchetti and Kharroubi, 2015). Mian et al. (2017a) show that the 1980s banking deregulation in the US led to a boom-bust pattern in the nontradable sector, which is usually much less productive than tradables; a similar pattern holds for the Great Recession (Mian and Sufi, 2014a,b). In Schneider and Tornell (2004), the nontradable sector is endogenously borrowing constrained, which gives rise to a boom-bust pattern. These findings are also in line with the model of "investment hangover" in Rognlie et al. (2014), where durable capital is overbuilt during boom times, which

[3] See Schivardi et al. (2017) for an assessment suggesting a more modest role for capital misallocation in Italy.

diverts resources away from other sectors.

Credit booms may also feature excessive risk-taking. Greenwood and Hanson (2013) show that the share of risky firms in total debt issuance increases during credit booms and that this increase in risk is not priced by investors: Higher lending to risky firms is associated with lower, not higher bond returns. To the extent that the predictable reversal of this mispricing leads to economic downturns, as shown by López-Salido et al. (2017), it could be interpreted as evidence for credit misallocation during the boom.[4]

Empirical predictions. There is one fundamentally distinct prediction these two classes of models make about the nature of credit booms. In the first group, there is no reason to expect that the costs of such episodes such vary across sectors: crises are the outcome of overconfidence or frictions, which are not limited to particular areas of the economy. To the contrary, an important contribution has been to show the diversity of "bubbly" assets. In the data, we should thus expect that credit booms *generally* should predict crises; we would not expect systematic heterogeneity across sectors over the broad sweep of modern banking history. In the second group, however, such heterogeneity is expected to the extent that sectors might differ in their propensity of being subject to booms and subsequent crises. The reasons for such differences are manifold: government subsidies or regulations, organizational incentives for lenders, differences in industry cyclicality or the costs of screening borrowers are just some candidates that may generate lending booms that differ in their outcomes. For the purpose of sorting through existing theories, the source of the heterogeneity is irrelevant. What matters is whether *specific* credit booms are more likely to end in crises than others.

Empirical results

In this section, I present some initial evidence on the empirical validity of the theories reviewed above based on a simple prediction framework for financial crises. I test whether financial crises over the period 1940 through 2014 are more likely to be associated with credit booms in particular sectors of the economy. To do so, I use a simple log-odds prediction framework of the type:

$$log \frac{P[B_{it} = 1 | \Delta C_{i,t-h}]}{P[B_{it} = 0 | \Delta C_{it}]} = \alpha + \beta \Delta C_{it} + \varepsilon_{it}, \tag{5.1}$$

where i and t denote countries and year, respectively. B_{it} is a dummy indicating whether a country experienced a financial crisis in a given year (as defined below). C_{it}

[4]This is also related to the finding that subprime mortgages were insufficiently screened by lenders during the housing boom (see e.g. Keys et al., 2010).

is a vector of five lags of changes in credit to GDP (following Schularick and Taylor, 2012), where I vary the type of credit by sector to explore heterogeneity in their predictive ability.[5] Since my sample includes many countries, some of which never experienced a crisis, I present here the results without country fixed effects. In untabulated results, I find that the findings are not affected by the inclusion of a full set of country and/or year dummies.

To compare the ability of different types of credit growth to correctly classify future periods into crisis and non-crisis times, I use the by now often-used Area Under the Curve (AUC) statistic (see e.g. Schularick and Taylor, 2012; Jordà and Taylor, 2011; Jordà et al., 2013b,a, 2015). The AUC captures the intuition that what matters for prediction is the ratio of true positives to false positives; it measures whether given a signal given by C_{it} is informative over and above what a coin toss would suggest. For interpretation, an AUC with a value of 1 denotes perfect classification ability and 0.5 is perfectly uninformative. The origin of AUC use is the area of biostatistics, where researchers have long used it to gauge, for example, the likelihood of rare illnesses conditional on certain predictors. In the case of financial crises, it can provide a useful benchmark to test how informative different credit measures are.

I already introduced the credit data set used for the analysis above. In addition, we need data on financial crises. Identifying such crises across countries and time is a non-trivial task, which has led to some disagreement between existing sources (Bordo and Meissner, 2016). To keep the classification tractable, I follow a simple approach. First, I identify systemic banking crises based on the data of Laeven and Valencia (2013), which is arguably the most frequently used and reliable source that is available for a broad cross-section of countries from 1960. I assign the crisis dummy a value for 1 in the first year of a systematic banking crisis that was not preceded by a crisis in the previous five years. Second, I add the crisis dates from Reinhart and Rogoff (2009b), Jordà et al. (2016), and Bordo et al. (2001). Again, I require that countries did not experience a crisis in the previous five years. In unreported regressions, I experimented with a great deal of alternative crises classifications; it turns out, the exact definitions do not make a difference to the results presented here.

Table 5.1 presents the regression results from estimating equation 5.1. For clarity, I only report the sum of the five lags of credit growth. P-values are based on a joint two-sided F-test under the null hypothesis that the sum of the coefficients is zero. Recall that the coefficient sizes per se are not meaningful, because equation 5.1 is estimated as a logit regression; the AUC, however, is comparable across models. As in the work

[5] I use five lags to allow for heterogeneity in the predictive horizon of different credit types for crises. The results, however, are qualitatively unchanged when I use three lags instead.

Table 5.1: CREDIT BOOMS AND CRISES: BROAD SECTORAL HETEROGENEITY

	Dependent Variable: Banking Crisis Dummy				
	Total (1)	Household (2)	Corporate (3)	NFC (4)	Non-Bank Finance (5)
$\Delta C_{i,t-h}$	0.114***	0.356***	0.132***	0.220***	0.190
(Sum of 5 Lags)	(0.270)	(0.868)	(0.043)	(0.071)	(0.122)
$PseudoR^2$	0.051	0.063	0.049	0.046	0.021
Observations	4,040	3,093	3,091	2,118	2,287
AUC	0.665	0.678	0.700	0.648	0.613

Note: Table 5.1 shows the results from predictive regressions as in equation 5.1. Standard errors are based on a two-sided F-test that the sum of five lags of $\Delta C_{i,t-h}$ is significantly different from zero. ***, **, and * denotes statistical significance at the 1, 5, and 10% level, respectively. See text for details.

of Schularick and Taylor (2012), Gourinchas and Obstfeld (2012), and others, credit growth is a powerful predictor of banking crises. In column (1), I start by estimating the baseline model with the growth of *total* credit to the private sector as predictor, which enters highly significantly. This finding is reassuring, given that I am estimating the regression using what, to my knowledge, is the largest available macroeconomic lending dataset.

Next, I allow for heterogeneity across broad institutional sectors. In particular, I start by dividing total credit into household and firm lending in columns (2) and (3), respectively. This yields two findings. First, both household and corporate credit growth predicts banking crises. This confirms earlier findings by Büyükkarabacak and Valev (2010) in a much larger sample and also chimes well with the historical analysis of Jordà et al. (2014) on mortgage vs. non-mortgage debt.[6] Importantly, both corporate and household credit are more informative indicators (as indicated by the AUC values) than total credit: The values here are 0.700 and 0.678, respectively, compared to the baseline for total credit of 0.665. This suggests that there is merit to using disaggregated measures for predicting crises, but also that it is difficult to rule out particular corporate lending booms as harmful for financial stability.

I explore this issue further by dissecting total corporate credit into its non-financial and (non-bank) financial components in columns (4) and (5). This is an attempt to extend previous findings, where corporate credit usually includes both types; the down-

[6]As a reference point, the total observation count here is almost ten times that of Büyükkarabacak and Valev (2010) and double the historical data used in Jordà et al. (2015); yet, the basic finding holds.

side is that the observation count decreases, which is driven by the availability of lending to the non-bank financial sector. Perhaps surprisingly, I find that growth in the non-financial corporate credit segment retains a significant classification ability with an AUC of 0.648, while non-bank finance does not seem to matter. The sum of the coefficients on the latter are statistically indistinguishable from zero and the AUC only 0.613.

Table 5.3: CREDIT BOOMS AND CRISES: DETAILED SECTORAL HETEROGENEITY

	Dependent Variable: Banking Crisis Dummy						
		Corporate credit				Household credit	
	Total (1)	Agriculture (2)	Industry (3)	CRE (4)	Wholesale/Retail (5)	Mortgage (6)	Consumer (7)
$\Delta C_{i,t-h}$	0.114***	-0.160	0.298	0.447***	0.684***	0.344***	55.268**
(Sum of 5 Lags)	(0.270)	(0.259)	(0.246)	(0.121)	(0.179)	(0.129)	(26.509)
$Pseudo R^2$	0.051	0.009	0.010	0.047	0.034	0.049	0.042
Observations	4,040	3,027	2,909	2,842	2,878	2,064	1,927
AUC	0.665	0.512	0.556	0.650	0.661	0.653	0.715

Note: Table 5.3 shows the results from predictive regressions as in equation 5.1. Standard errors are based on a two-sided F-test that the sum of five lags of $\Delta C_{i,t-h}$ is significantly different from zero. ***, **, and * denotes statistical significance at the 1, 5, and 10% level, respectively. See text for details.

To understand the source of this heterogeneity, I next differentiate between more narrowly defined corporate sectors in table 5.3: agriculture, industry (referring to manufacturing and mining), construction and real estate, and wholesale/retail trade (including hotels and restaurants). As outlined above, there is a large literature to motivate each of these sectors as potential candidates for breeding instability in the financial system. For reference, I include the regression using total credit growth as predictor in column (1). The results in columns (2) through (5) show that there are substantial differences across credit booms. Consistent with previous evidence on mortgage lending in 17 advanced economies in Jordà et al. (2014, 2015), I find that *corporate* credit growth in construction and real estate is a powerful indicator for future banking crises. The same holds true for the wholesale/retail trade sector, which is closely tied to household consumption and overlaps strongly with the definition of non-tradable industries as defined in Mian and Sufi (2014a). This evidence is also consistent with the boom-bust pattern in the non-tradable sector often accompanying credit booms, as documented in Mian et al. (2017a) and Mendoza and Terrones (2012). Importantly, the ability of lending to agriculture and industry to predict financial crises is basically zero: the estimated coefficients for these sectors are statistically insignificant and the

AUC values close to 0.5.

I also investigate differences in the types of lending to households in columns (6) and (7). Both mortgage and consumer credit (defined as the residual of total household lending and mortgages) predict banking crises. While the somewhat lower observation count for the disaggregated data marginally reduces the AUC for mortgages (0.653) compared to total household credit (0.678), it is considerably larger for non-mortgage credit (0.715). I believe to be the first to document that heterogeneity in household lending matters for systemic crises.

The results presented here suggest that there is considerable underlying heterogeneity in which types of credit growth precedes financial crises. These differences are inconsistent with at least simple versions of models where *general* euphoria or sentiment drive booms (and subsequent busts), which would not suggest differences across sectors. I find a role for firm and household lending, where booms in the former hurt when they are concentrated in construction, real estate, and non-tradables. Growth in both mortgage and non-mortgage credit to households regularly precedes crises. These findings are consistent with misallocation during booms, where credit is systematically allocated towards a few concentrated sectors. However, it remains fundamentally unclear which factors are behind the occurrence of such concentrated booms in the first place and why these sectors are particularly affected. I consider my work as a laying the groundwork to address these questions in future research.

B International Credit Cycles

A related but distinct question to the cyclical properties of different sectors in the loan market is why some studies find considerable co-movement of financial variables across countries. In influential work, Miranda-Agrippino and Rey (2015) and Rey (2015) show that global factors predict a considerable component of the cross-country variation in credit aggregates, house prices, among other variables. For policy makers, the influence of foreign central banks' interest rates on domestic conditions is a crucial concern, because it implies a lack of power over the national economy. As such, Rey (2015) suggests that one of the central tenets of international economics, the Mundell-Fleming trilemma, may not hold in modern economies.

The idea of a "global financial cycle", however, has recently been called into question by an analysis of actual capital flows in Cerutti et al. (2017c). The authors show that global factors only explain a small fraction of international capital flows, the most plausible candidate for cross-country transmission. However, it is possible that global factors matter for domestic credit – and I hope the sectoral database I have constructed will be useful in assessing this empirically.

In particular, a pressing question is what *types* of credit co-move across countries, and to which extent this may be driven by global industry-level shocks or country-specific trends. The long-run nature of my data also allows for an agnostic approach in studying breaks in such co-movements across countries. As such, one can let the data dictate a classification of different periods of international credit cycles, which can then be re-aligned with the historical narrative and policy variables such as capital account openness.

C Credit Markets in the Long-Run

While the main contribution of the new database I have constructed is setoral data, it also includes new long-run time series on total credit to the private sector for many countries. I relied extensively on historical secondary sources that have not been tapped into before to study banking – such as statistical publications of the League of Nations and United Nations. In other cases, I was able to use collections of historical statistics compiled by other researchers or archival publications of national central banks. The result is a dataset on total credit that spans back to 1910 for many dozens of countries, many of which are still classified as "developing" today.

These data are interesting for two reasons. First, they add to the seminal work of Schularick and Taylor (2012) by providing researchers studying credit dynamics with long-run data for a broader cross-section of countries. Second, they allow for a much richer analysis of what shapes the size of financial sectors around the world. What is particularly interesting here is the long-standing debate whether "deep parameters" (such as the legal origin of a country's judicial system imposed by colonial powers) or time-varying factors (such as political decisions) play a more important role (La Porta et al., 1997, 1998; Rajan and Zingales, 2003). Equipped with cross-country variation in credit market size for more than 100 years, it would be possible to much more credibly investigate these questions.

D Financial Development and Credit Allocation

The Great Financial Crisis of 2008-2009 was a stark reminder that the financial sector can be a source of substantial macroeconomic turmoil. It also marks an important caesura in economic thought: while bubbles and overlending were known to play a role in financial crises (e.g. Minsky, 1977; Kindleberger, 1978), the overwhelming consensus was that the growth of the financial sector would beget growth of the macroeconomy, and thus benefit society (Zingales, 2015).

Research after the global financial crisis has yielded a new, much more nuanced consensus. Many studies have examined the costs imposed by overleveraged households and firms drawing on detailed micro data. On the macro level, the predictive ability of credit growth for financial crises is now well-documented, which I have discussed in detail above. An influential line of empirical research goes a step further than highlighting these risks, arguing that "too much finance" may indeed be detrimental for economic growth beyond a certain threshold (Arcand et al., 2015; Cecchetti et al., 2011; Cecchetti and Kharroubi, 2012). In essence, these authors argue that the positive cross-country correlation of financial development with economic growth found in earlier – but notably, not more recent studies – masks a non-linear relationship. Out of the many potential channels why this might be the case, an excessive allocation of highly-skilled workers into finance has received particular attention (Cecchetti and Kharroubi, 2015); however, some recent evidence suggests that this channel may not have aggregate effects (D'Acunto and Frésard, 2018).

Despite these qualifications to the idea that finance benefits growth, the most methodologically convincing studies appear to have withstood the test of time: the seminal contributions by Rajan and Zingales (1998) and Wurgler (2000). In brief, Rajan and Zingales argue that more developed financial systems allocate capital more to industries that are financially constrained for technological reasons, which they dub "dependence on external financing". In the data, they show that – after conditioning on country and industry fixed effects – sectors with a higher dependence on external financing show a higher growth in value added in countries with larger financial sectors (relative to GDP). Wurgler's approach is similar in spirit: he shows that industries in countries with more developed financial sectors show a higher sensitivity of investment to value added, which he takes as finance increasing the efficiency of resource allocation. It seems fair to say that, despite the post-crisis soul searching in the economics profession, the conclusions from these two studies are regarded as nearly too obvious to mention by many financial economists.

Of course, every study is a product of its time, and the use of within-country variation to identify causal effects rests on econometric assumptions that are considered much less plausible today than they were in the late 1990s. For example, we know that measures of financial development – such as the ratio of private credit to GDP – are highly correlated with a sheer endless number of observable country characteristics, such as GDP per capita, productivity growth, education attainment, the rule of law, or even cultural factors. As such, a country's credit-to-GDP ratio can hardly be interpreted as being "as good as random" across countries, the benchmark for credibly exogenous variation in observational data. Similarly, there is no reason to believe that an industry's technologically-determined demand for external financing should not change over time; be fixed across countries (assuming equal production functions); or be distributed "as good as randomly" across sectors. It is also not clear why an industry's sensitivity of investment to value added solely measures of "efficiency". This opens the door to alternative explanations, for example that industries with a higher sensitivity to aggregate productivity shocks grow faster in more developed countries – and that the causal impact of financial development is zero.

The main difficulty in assessing the conclusions of the work by Rajan and Zingales, and Wurgler, is that they are using macroeconomic outcomes (value added and investment) to *indirectly* infer a role for the financial sector. While credit and "real" economic outcomes often move hand-in-hand, the over-leveraging of construction and real estate companies in the run-up to the 2008 Eurozone crisis is only one particularly striking reminder that this may not always be the case.

In ongoing work, I propose a simple, *direct* test for whether a larger financial sector benefits the allocation of credit to efficient but financially constrained sectors using the sectoral credit database I have assembled. In contrast to Rajan and Zingales (1998) and Wurgler (2000), I can directly investigate whether a higher ratio of credit-to-GDP, for example, is associated with a reallocation of credit towards industries that are highly dependent on external financing. Apart from the data on the amount of sectoral credit, I have also collected a considerable amount of data on sectoral interest rate spreads. By combining data on prices and quantities, I could infer whether credit supply factors (as stressed by Rajan & Zingales) or demand are the driving force.

To be clear, the idea of this test is *not* to interpret the estimates as causal effects, although the staggered implementation of banking reforms throughout the world starting in the 1970s may provide some opportunity to construct an identification strategy. Rather, I want to assess whether the correlations found with value added data indeed coincide with changes in credit markets or reflect other factors. Of course, a lack of a correlation does not imply that larger financial sectors cannot have a causal effect, because we do not have a credible counterfactual without exogenous variation. Nevertheless, a zero or even negative correlation would make it much less plausible that it is indeed financial development that causes a more efficient resource allocation, which is the preferred interpretation of Rajan and Zingales (1998) and Wurgler (2000).

5.2 Implications for Policy

Since the Great Financial Crisis of 2008-2009, policy makers have grappled with the question of how to re-design financial regulation in the wake of massive bank failures, skyrocketing stocks of non-performing loans, and deep macroeconomic downturns. One of the major policy innovations has been to establish frameworks for the real-time identification of systemic risk – usually associated with strong increases in the growth of asset prices, credit, and a compression of risk premia. If these could be identified with reasonable accuracy, the story goes, the most costly financial crises may be prevented – or at least their damage contained – by "leaning against the wind" using macroprudential and/or monetary policy.

At the heart of the dilemma that policy makers face is, of course, that such policies are likely costly ex-ante in states of favorable economic prospects. In particular, they have to be squared and constantly justified in light of the large body of evidence suggesting that access to finance is, in fact, an important determinant of growth on the firm and macroeconomic level (Fraser et al., 2015, e.g.). This is especially true for emerging economies: if a country's firms and households face severe constraints in accessing credit (Ayyagari et al., 2017), a potential worsening of such constraints with the stated goal of preventing potentially costly crises is a hard sell politically.

I believe that the work presented in this thesis has implications for financial regulation that might be helpful in addressing these challenges. A first insight is as important as it is trivial, once one has seen the data: most countries around the globe have not experienced sustained growth in firm lending relative to GDP over the past three decades. It is disconcerting that this is particularly true for emerging economies, which do not only have much less developed financial sectors, but are usually also far from estimated thresholds for potential effects of finance and growth (Arcand et al., 2015). What is even more disconcerting is that the stalling of corporate credit growth has not been offset by bond markets, cross-border lending, or trade credit. If one takes models seriously that consider heterogeneity in credit allocation (Matsuyama, 2007, 2013), this suggests that many countries may be caught in "bubbly growth traps" (Tripathy, 2017), where credit growth does not contribute to growth but still increases the risk of costly bubbles.

What does this mean from a policy perspective? It is obviously tempting to conclude that the results of the finance-growth literature do not imply that financial deregulation benefits growth, and that credit policy (such as that used by France in the post-WWII period (Monnet, 2014)) should be used to steer the allocation of financing towards high-productivity sectors. Some support for such conclusions even comes from the deregulation episodes in the United States – often cited as the best evidence for the positive impact of lifting regulatory restrictions – which may have discouraged innovators that depend on relationship lending (Hombert and Matray, 2017). In my data, I also find that financial deregulation is associated with higher share of household credit, which has been associated with more crises, but not higher growth (Jordà et al., 2014; Beck et al., 2012).

Nevertheless, I would argue that such conclusions would be ill-advised given the current state of research. The main reason is that the institutional requirements for implementing an efficient system of credit controls are likely steep: even in the case of France, there is evidence that the lifting of these controls in the 1980s led to a more efficient allocation of credit (Bertrand et al., 2007). Indeed, the widespread abuse of the powers that come with directing credit has been one of the primary motivations for international organizations and academics to call for their liberalization in the first place. It remains for future research to determine efficient policies to influence the allocation of credit without brute force. The German system of local savings banks and cooperative banks, for example, might be an interesting case study, because it is often seen as an important backbone of the country's industry despite being de-facto subject to geographical and sectoral lending restrictions.

A related question is whether countries should use their macroprudential policy arsenal to limit the flow of credit to particular crisis-prone sectors. At first glance, the preliminary results I presented in the previous section are clearly supportive: in the modern history of banking crises since 1940, only lending to a few sectors was systematically associated with future crises. If one is willing to accept these initial findings at face value, policy tools that prevent excess growth of credit to construction and non-tradable sectors may help to prevent the worst financial meltdowns.

But again, great powers confided to regulators may come with great temptations. Because access to credit is a salient aspect of the daily lives of most individuals, it may not be entirely surprising that I find a lower likelihood of tightening for targeted prudential policy in the run-up to elections. However, the policy implications of this finding are potentially enormous, particularly because this electoral cycle does not seem to depend on the degree of central bank independence or the quality of other institutions. Indeed, the existence of an electoral cycle makes it difficult to judge whether the already hard-to-gauge welfare effects of sectoral lending restrictions are positive.

I believe that some help in this dilemma may come from my work on judicial efficiency in chapter 4 and other studies in the law and finance literature. Fundamentally, credit institutions have to be able to make a profit if they are to provide loans at reasonable terms. This requires creditors to compensate for the fact that some fraction of borrowers will default by recovering some of the value of their debt claims. Recovery values, in turn, are to a large extent a function of bankruptcy regimes and the efficiency of the court system.

It does not seem to be widely appreciated that recovery values may be a direct link between legal frameworks and their enforcement on the allocation of credit and financial stability. This is because a firm's ability to pledge collateral (and thus its expected recovery values) are negatively correlated with firm productivity (Buera et al., 2011). During credit booms that end in crises, in turn, credit appears to be primarily allocated to sectors with low productivity (Gorton and Ordoñez, 2016; Borio et al., 2016). This also meshes with my findings that financial crises tend to be preceded by booms in lending to the non-tradable sector, which tends to be less productive than manufacturing.

Taken together, this suggests that structuring legal frameworks to make it more profitable for creditors to lend to borrowers with higher productivity may be a hitherto underappreciated way to address financial stability issues. Campello and Larrain (2016), for example, show that allowing firms to pledge more specific types of collateral leads to substantial reallocation effects. Because specific assets have lower liquidation values (see e.g. Benmelech, 2009) and are thus more productive, such reforms might be a way to sidestep political economy limitations.

Bibliography

Abiad, A., Detragiache, E., and Tressel, T. (2010). A New Database of Financial Reforms. *IMF Staff Papers*, 57(2):281–302.

Acharya, V. V. and Subramanian, K. V. (2009). Bankruptcy codes and innovation. *The Review of Financial Studies*, 22(12):4949–4988.

Acharya, V. V., Sundaram, R. K., and John, K. (2011). Cross-country variations in capital structures: The role of bankruptcy codes. *Journal of Financial Intermediation*, 20(1):25–54.

Aikman, D., Haldane, A. G., and Nelson, B. D. (2015). Curbing the credit cycle. *The Economic Journal*, 125(585):1072–1109.

Aiyar, S., Calomiris, C., and Wieladek, T. (2014). Does macro-prudential regulation leak? evidence from a uk policy experiment. *Journal of Money, Credit and Banking*, 46(s1):181–214.

Aiyar, S., Calomiris, C. W., and Wieladek, T. (2016). How does credit supply respond to monetary policy and bank minimum capital requirements? *European Economic Review*, 82(C):142–165.

Akhmedov, A. and Zhuravskaya, E. (2004). Opportunistic political cycles: Test in a young democracy setting. *The Quarterly Journal of Economics*, 119(4):1301–1338.

Akinci, O. and Olmstead-Rumsey, J. (2015). How Effective are Macroprudential Policies? An Empirical Investigation. International Finance Discussion Papers 1136, Board of Governors of the Federal Reserve System (U.S.).

Alesina, A., Cohen, G. D., and Roubini, N. (1992). Macroeconomic policy and elections in oecd democracies. *Economics & Politics*, 4(1):1–30.

Almeida, H., Campello, M., Laranjeira, B., and Weisbenner, S. (2012). Corporate Debt Maturity and the Real Effects of the 2007 Credit Crisis. *Critical Finance Review*, 1(1):3–58.

Altman, E. I. (2013). Revisiting the recidivism-chapter 22 phenomenon in the us bankruptcy system. *Brook. J. Corp. Fin. & Com. L.*, 8:253.

Altunbas, Y., Binici, M., and Gambacorta, L. (2018). Macroprudential policy and bank risk. *Journal of International Money and Finance*, 81(C):203–220.

Amore, M. D., Schneider, C., and Žaldokas, A. (2013). Credit supply and corporate innovation. *Journal of Financial Economics*, 109(3):835 – 855.

Antoniades, A. and Calomiris, C. W. (2018). Mortgage market credit conditions and u.s. presidential elections. Working Paper 24459, National Bureau of Economic Research.

Arcand, J., Berkes, E., and Panizza, U. (2015). Too much finance? *Journal of Economic Growth*, 20(2):105–148.

Arestis, P. and Demetriades, P. O. (1997). Financial Development and Economic Growth: Assessing the Evidence. *Economic Journal*, 107(442):783–99.

Aretz, K., Campello, M., and Marchica, M. T. (2016). Conflicting security laws and the democratization of credit: France's reform of the napoleonic code. Technical report, Available at SSRN: http://ssrn.com/abstract=2731043 or http://dx.doi.org/10.2139/ssrn.2731043.

Armingeon, K., Wenger, V., Wiedemeier, F., Isler, C., Knöpfel, L., Weisstanner, D., and Engler, S. (2017). Comparative political data set 1960-2015. *Bern: Institute of Political Science, University of Berne*.

Ayyagari, M., Beck, T., and Peria, M. S. M. (2017). Credit growth and macroprudential policies: preliminary evidence on the firm level. In for International Settlements, B., editor, *Financial systems and the real economy*, volume 91 of *BIS Papers chapters*, pages 15–34. Bank for International Settlements.

Badev, A., Beck, T., Vado, L., and Walley, S. (2014). Housing finance across countries : new data and analysis. Policy Research Working Paper Series 6756, The World Bank.

Bae, K.-H. and Goyal, V. K. (2009). Creditor rights, enforcement, and bank loans. *The Journal of Finance*, 64(2):823–860.

Bagehot, W. (1873). *Lombard Street: A Description of the Money Market*. London: Henry S. King and Co.

Bahadir, B. and Gumus, I. (2016). Credit decomposition and business cycles in emerging market economies. *Journal of International Economics*, 103(C):250–262.

Baker, S. R., Bloom, N., and Davis, S. J. (2016). Measuring economic policy uncertainty*. *The Quarterly Journal of Economics*, 131(4):1593–1636.

Banerjee, A. V. and Duflo, E. (2014). Do firms want to borrow more? testing credit constraints using a directed lending program. *Review of Economic Studies*, 81(2):572–607.

Barberis, N., Greenwood, R., Jin, L., and Shleifer, A. (2016). Extrapolation and bubbles. Working Paper 21944, National Bureau of Economic Research.

Barro, R. J. and Ursua, J. F. (2008). Macroeconomic Crises since 1870. *Brookings Papers on Economic Activity*, 39(1 (Spring):255–350.

Beck, T. (2008). The econometrics of finance and growth. Policy Research Working Paper Series 4608, The World Bank.

Beck, T., Büyükkarabacak, B., Rioja, F. K., and Valev, N. T. (2012). Who gets the credit? and does it matter? household vs. firm lending across countries. *The B.E. Journal of Macroeconomics*, 12(1):1–46.

Beck, T., Clarke, G., Groff, A., Keefer, P., and Walsh, P. (2001). New tools in comparative political economy: The database of political institutions. *The World Bank Economic Review*, 15(1):165–176.

Beck, T., Demirgüç-Kunt, A., and Levine, R. (2007). Finance, inequality and the poor. *Journal of Economic Growth*, 12(1):27–49.

Beck, T., Demirguc-Kunt, A., and Maksimovic, V. (2005). Financial and legal constraints to growth: Does firm size matter? *Journal of Finance*, 60(1):137–177.

Beck, T., Levine, R., and Levkov, A. (2010). Big bad banks? the winners and losers from bank deregulation in the united states. *The Journal of Finance*, 65(5):1637–1667.

Beck, T., Levine, R., and Loayza, N. (2000). Finance and the sources of growth. *Journal of Financial Economics*, 58(1-2):261–300.

Behn, M., Haselmann, R., Kick, T., and Vig, V. (2015). The political economy of bank bailouts. IMFS Working Paper Series 86, Goethe University Frankfurt, Institute for Monetary and Financial Stability (IMFS).

Bekaert, G., Harvey, C., and Lundblad, C. (2001). Emerging equity markets and economic development. *Journal of Development Economics*, 66(2):465–504.

Bekaert, G., Harvey, C. R., and Lundblad, C. (2005). Does financial liberalization spur growth? *Journal of Financial Economics*, 77(1):3–55.

Bekaert, G., Harvey, C. R., Lundblad, C., and Siegel, S. (2007). Global Growth Opportunities and Market Integration. *Journal of Finance*, 62(3):1081–1137.

Benfratello, L., Schiantarelli, F., and Sembenelli, A. (2008). Banks and innovation: Microeconometric evidence on italian firms. *Journal of Financial Economics*, 90(2):197 – 217.

Benmelech, E. (2009). Asset salability and debt maturity: Evidence from nineteenth-century american railroads. *Review of Financial Studies*, 22(4):1545–1584.

Benmelech, E., Dlugosz, J., and Ivashina, V. (2012). Securitization without adverse selection: The case of CLOs. *Journal of Financial Economics*, 106(1):91–113.

Benmelech, E., Garmaise, M. J., and Moskowitz, T. J. (2005). Do Liquidation Values Affect Financial Contracts? Evidence from Commercial Loan Contracts and Zoning Regulation. *The Quarterly Journal of Economics*, 120(3):1121–1154.

Benmelech, E. and Moskowitz, T. J. (2010). The political economy of financial regulation: Evidence from u.s. state usury laws in the 19th century. *The Journal of Finance*, 65(3):1029–1073.

Bermant, G., Lombard, P. A., and Wiggins, E. C. (1991). A day in the life: The federal judicial center's 1988-1989 bankruptcy court time study. *American Bankruptcy Law Journal*, (65):491–524.

Bernanke, B. S., Gertler, M., and Gilchrist, S. (1999). The financial accelerator in a quantitative business cycle framework. In Taylor, J. B. and Woodford, M., editors, *Handbook of Macroeconomics*, volume 1 of *Handbook of Macroeconomics*, chapter 21, pages 1341–1393. Elsevier.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates?*. *The Quarterly Journal of Economics*, 119(1):249–275.

Bertrand, M., Schoar, A., and Thesmar, D. (2007). Banking deregulation and industry structure: Evidence from the french banking reforms of 1985. *The Journal of Finance*, 62(2):597–628.

BIS (2017). *Macroprudential frameworks, implementation and relationship with other policies*. Number 94 in BIS Papers. Bank for International Settlements.

Block, S. A. (2002). Political business cycles, democratization, and economic reform: the case of africa. *Journal of Development Economics*, 67(1):205–228.

Bordalo, P., Gennaioli, N., and Shleifer, A. (2017). Diagnostic expectations and credit cycles. *The Journal of Finance*, pages n/a–n/a.

Bordo, M., Eichengreen, B., Klingebiel, D., Martinez-Peria, M. S., and Rose, A. K. (2001). Is the crisis problem growing more severe? *Economic Policy*, 16(32):53–82.

Bordo, M. D. and Meissner, C. M. (2016). Fiscal and financial crises. Working Paper 22059, National Bureau of Economic Research.

Borio, C., Kharroubi, E., Upper, C., and Zampolli, F. (2016). Labour reallocation and productivity dynamics: financial causes, real consequences. BIS Working Papers 534, Bank for International Settlements.

Brancati, D. (2017). Global elections database. *New York: New York: Global Elections Database, Date Accessed 08/01/2017. Website: http://www.globalelectionsdatabase.com*.

Braun, M. and Larrain, B. (2005). Finance and the business cycle: International, inter-industry evidence. *Journal of Finance*, 60(3):1097–1128.

Braun, M. and Raddatz, C. (2008). The politics of financial development: Evidence from trade liberalization. *The Journal of Finance*, 63(3):1469–1508.

Bris, A., Welch, I., and Zhu, N. (2006). The costs of bankruptcy: Chapter 7 liquidation versus chapter 11 reorganization. *The Journal of Finance*, 61(3):1253–1303.

Brown, C. and Dinc, S. (2005). The politics of bank failures: Evidence from emerging markets. *Quarterly Journal of Economics*, 120:1413–1444.

Brown, D. J. and Earle, J. S. (2017). Finance and growth at the firm level: Evidence from sba loans. *The Journal of Finance*, 72(3):1039–1080.

Brown, J. R., Martinsson, G., and Petersen, B. C. (2013). Law, Stock Markets, and Innovation. *Journal of Finance*, 68(4):1517–1549.

Brown, M., Jappelli, T., and Pagano, M. (2009). Information sharing and credit: Firm-level evidence from transition countries. *Journal of Financial Intermediation*, 18(2):151–172.

Brunnermeier, M. K. and Sannikov, Y. (2014). A macroeconomic model with a financial sector. *American Economic Review*, 104(2):379–421.

Buera, F. J., Kaboski, J. P., and Shin, Y. (2011). Finance and development: A tale of two sectors. *American Economic Review*, 101(5):1964–2002.

Büyükkarabacak, B. and Valev, N. T. (2010). The role of household and business credit in banking crises. *Journal of Banking & Finance*, 34(6):1247–1256.

Caballero, R. J. and Krishnamurthy, A. (2003). Excessive dollar debt: Financial development and underinsurance. *The Journal of Finance*, 58(2):867–893.

Calomiris, C. W. and Haber, S. H. (2014). *Fragile by Design: The Political Origins of Banking Crises and Scarce Credit*, volume 1 of *Economics Books*. Princeton University Press.

Campello, M. and Larrain, M. (2016). Enlarging the contracting space: Collateral menus, access to credit, and economic activity. *Review of Financial Studies*, 29(2):349–383.

Canes-Wrone, B. and Park, J.-K. (2012). Electoral business cycles in oecd countries. *The American Political Science Review*, 106(1):103–122.

Canes-Wrone, B. and Ponce de Leon, C. (2018). Electoral cycles and democratic development. Technical report.

Carvalho, D. (2014). The real effects of government-owned banks: Evidence from an emerging market. *The Journal of Finance*, 69(2):577–609.

Case, K. E., Shiller, R. J., and Thompson, A. K. (2012). What Have They Been Thinking? Homebuyer Behavior in Hot and Cold Markets. *Brookings Papers on Economic Activity*, 43(2 (Fall)):265–315.

Cecchetti, S. and Kharroubi, E. (2012). Reassessing the impact of finance on growth. BIS Working Papers 381, Bank for International Settlements.

Cecchetti, S., Mohanty, M., and Zampolli, F. (2011). The real effects of debt. BIS Working Papers 352, Bank for International Settlements.

Cecchetti, S. G. and Kharroubi, E. (2015). Why does financial sector growth crowd out real economic growth? BIS Working Papers 490, Bank for International Settlements.

Cerqueiro, G., Hegde, D., a Fabiana Penas, M., and Seamans, R. C. (2017). Debtor rights, credit supply, and innovation. *Management Science*, 63(10):3311–3327.

Cerutti, E., Correa, R., Fiorentino, E., and Segalla, E. (2017a). Changes in Prudential Policy Instruments - A New Cross-Country Database. *International Journal of Central Banking*, 13(2):477–503.

Cerutti, E., Dagher, J., and Dell'ariccia, G. (2017b). Housing finance and real-estate booms: A cross-country perspective. *Journal of Housing Economics*, 38(C):1–13.

Cerutti, E. M., Claessens, S., and Laeven, L. (2015). The Use and Effectiveness of Macroprudential Policies: New Evidence. IMF Working Papers 15/61, International Monetary Fund.

Cerutti, E. M., Claessens, S., and Rose, A. K. (2017c). How Important is the Global Financial Cycle? Evidence from Capital Flows. IMF Working Papers 17/193, International Monetary Fund.

Chakrabarti, R. and Pattison, N. (2016). Auto credit and the 2005 bankruptcy reform: the impact of eliminating cramdowns. Staff Reports 797, Federal Reserve Bank of New York.

Chakraborty, I., Goldstein, I., and MacKinlay, A. (2018). Housing price booms and crowding-out effects in bank lending. *The Review of Financial Studies*, 31(7):2806–2853.

Chaney, T., Sraer, D., and Thesmar, D. (2012). The collateral channel: How real estate shocks affect corporate investment. *American Economic Review*, 102(6):2381–2409.

Chava, S., Oettl, A., Subramanian, A., and Subramanian, K. V. (2013). Banking deregulation and innovation. *Journal of Financial Economics*, 109(3):759–774.

Chavaz, M. and Rose, A. K. (2016). Political Borders and Bank Lending in Post-Crisis America. NBER Working Papers 22806, National Bureau of Economic Research, Inc.

Cihák, M., Demirgüç-Kunt, A., Feyen, E., and Levine, R. (2013). Financial Development in 205 Economies, 1960 to 2010. *Journal of Financial Perspectives*, 1(2):17–36.

Claessens, S., Kose, A., and Terrones, M. E. (2009). What happens during recessions, crunches and busts? *Economic Policy*, 24:653–700.

Clark, W. R. and Hallerberg, M. (2000). Mobile capital, domestic institutions, and electorally induced monetary and fiscal policy. *The American Political Science Review*, 94(2):323–346.

Cole, S. (2009). Fixing market failures or fixing elections? agricultural credit in india. *American Economic Journal: Applied Economics*, 1(1):219–50.

Cornaggia, J., Mao, Y., Tian, X., and Wolfe, B. (2015). Does banking competition affect innovation? *Journal of Financial Economics*, 115(1):189–209.

Coval, J. D. and Moskowitz, T. J. (2001). The geography of investment: Informed trading and asset prices. *Journal of Political Economy*, 109(4):811–841.

Crowe, C. and Meade, E. E. (2007). The evolution of central bank governance around the world. *Journal of Economic Perspectives*, 21(4):69–90.

Cukierman, A. (1992). *Central Bank Strategy, Credibility, and Independence: Theory and Evidence*, volume 1. The MIT Press, 1 edition.

D'Acunto, F. and Frésard, L. (2018). Finance, Talent Allocation, and Growth. CESifo Working Paper Series 6883, CESifo Group Munich.

Dagher, J. C. (2017). Regulatory Cycles: Revisiting the Political Economy of Financial Crises (October 18, 2017). *Available at SSRN: https://ssrn.com/abstract=2772373*.

Dam, L. and Koetter, M. (2012). Bank bailouts and moral hazard: Evidence from germany. *The Review of Financial Studies*, 25(8):2343–2380.

Danielsson, J., Valenzuela, M., and Zer, I. (2016). Learning from History : Volatility and Financial Crises. Finance and Economics Discussion Series 2016-093, Board of Governors of the Federal Reserve System (U.S.).

Davydenko, S. A. and Franks, J. R. (2008). Do bankruptcy codes matter? a study of defaults in france, germany, and the u.k. *The Journal of Finance*, 63(2):565–608.

Dembiermont, C., Drehmann, M., and Muksakunratana, S. (2013). How much does the private sector really borrow - a new database for total credit to the private non-financial sector. *BIS Quarterly Review*.

Demirgüç-Kunt, A. and Detragiache, E. (1998). Financial liberalization and financial fragility. Policy Research Working Paper Series 1917, The World Bank.

Di Maggio, M. and Kermani, A. (2017). Credit-induced boom and bust. *The Review of Financial Studies*, 30(11):3711–3758.

Dinc, S. (2005). Politicians and banks: Political influences on government-owned banks in emerging markets. *Journal of Financial Economics*, pages 453–479.

Dincecco, M. and Prado, M. (2013). Nominal GDP Series, 1870-2000. Technical report.

Djankov, S., Hart, O., McLiesh, C., and Shleifer, A. (2008). Debt Enforcement around the World. *Journal of Political Economy*, 116(6):1105–1149.

Djankov, S., McLiesh, C., and Shleifer, A. (2007). Private credit in 129 countries. *Journal of Financial Economics*, 84(2):299–329.

Drehmann, M., Borio, C., Gambacorta, L., Jiminez, G., and Trucharte, C. (2010). Countercyclical capital buffers: exploring options. BIS Working Papers 317, Bank for International Settlements.

Drehmann, M., Borio, C., and Tsatsaronis, K. (2011). Anchoring countercyclical capital buffers: The role of credit aggregates. *International Journal of Central Banking*, 7(4):189–240.

Drehmann, M., Illes, A., Juselius, M., and Santos, M. (2015). How much income is used for debt payments? A new database for debt service ratios. *BIS Quarterly Review*.

Drehmann, M. and Juselius, M. (2014). Evaluating early warning indicators of banking crises: Satisfying policy requirements. *International Journal of Forecasting*, 30(3):759–780.

Duffie, D. (2017). Financial regulatory reform after the crisis: An assessment. *Management Science*, 0(0):null.

Easterly, W., Islam, R., and Stiglitz, J. E. (2001). Shaken and stirred: Explaining growth volatility. In Pleskovic, B. and Stern, N., editors, *Annual World Bank Conference On Development Economics 2000*, pages 191–211.

Edge, R. M. and Liang, N. (2017). Who is in charge of financial stability, why, and what they can do. *Brooking Institute Working Paper*.

Edwards, F. R. and Morrison, E. R. (2005). Derivatives and the bankruptcy code: Why the special treatment? *Yale Journal on Regulation*, 22.

Eggertsson, G. B. and Krugman, P. (2012). Debt, Deleveraging, and the Liquidity Trap: A Fisher-Minsky-Koo Approach. *The Quarterly Journal of Economics*, 127(3):1469–1513.

Eichengreen, B. and Mitchener, K. (2003). The great depression as a credit boom gone wrong. BIS Working Papers 137, Bank for International Settlements.

Eijffinger, S. and de Haan, J. (1996). The political economy of central-bank independence. Princeton studies in international economics, International Economics Section, Departement of Economics Princeton University,.

Englmaier, F. and Stowasser, T. (2017). Electoral Cycles in Savings Bank Lending. *Journal of the European Economic Association*, 15(2):296–354.

Epure, M., Mihai, I., Minoiu, C., and ´, J.-L. P. (2017). Household Credit, Global Financial Cycle, and Macroprudential Policies: Credit Register Evidence from an Emerging Country. Working Papers 1006, Barcelona Graduate School of Economics.

Faccio, M. (2006). Politically connected firms. *American Economic Review*, 96(1):369–386.

Farhi, E. and Werning, I. (2016). A theory of macroprudential policies in the presence of nominal rigidities. *Econometrica*, 84(5):1645–1704.

Favara, G. and Imbs, J. (2015). Credit supply and the price of housing. *American Economic Review*, 105(3):958–92.

Favara, G., Morellec, E., Schroth, E., and Valta, P. (2017). Debt enforcement, investment, and risk taking across countries. *Journal of Financial Economics*, 123(1):22–41.

Fernández-Val, I. and Weidner, M. (2016). Individual and time effects in nonlinear panel models with large n, t. *Journal of Econometrics*, 192(1):291–312.

Fisher, I. (1933). The debt-deflation theory of great depressions. *Econometrica: Journal of the Econometric Society*, pages 337–357.

Fisman, R. and Love, I. (2007). Financial Dependence and Growth Revisited. *Journal of the European Economic Association*, 5(2-3):470–479.

Foremny, D. and Riedel, N. (2014). Business taxes and the electoral cycle. *Journal of Public Economics*, 115(Supplement C):48–61.

Foroohar, R. (2016). *Makers and Takers: The Rise of Finance and the Fall of American Business*. Crown Books. Crown Publishing Group.

Frame, W. S., Srinivasan, A., and Woosley, L. (2001). The effect of credit scoring on small-business lending. *Journal of Money, Credit and Banking*, 33(3):813–825.

Fraser, S., Bhaumik, S. K., and Wright, M. (2015). What do we know about entrepreneurial finance and its relationship with growth? *International Small Business Journal*, 33(1):70–88.

Gambacorta, L. and Murcia, A. (2017). The impact of macroprudential policies and their interaction with monetary policy: an empirical analysis using credit registry data. CEPR Discussion Papers 12027, C.E.P.R. Discussion Papers.

Gambacorta, L., Yang, J., and Tsatsaronis, K. (2014). Financial structure and growth. *BIS Quarterly Review*.

Garriga, A. C. (2016). Central bank independence in the world: A new data set. *International Interactions*, 42(5):849–868.

Gennaioli, N., Ma, Y., and Shleifer, A. (2015). Expectations and investment. Working Paper 21260, National Bureau of Economic Research.

Gilchrist, S. and Mojon, B. (2014). Credit Risk in the Euro Area. NBER Working Papers 20041, National Bureau of Economic Research, Inc.

Gilchrist, S. and ZakrajÅ¡ek, E. (2012). Credit spreads and business cycle fluctuations. *American Economic Review*, 102(4):1692–1720.

Giovanni, F., Enrique, S., and Philip, V. (2012). Strategic default and equity risk across countries. *The Journal of Finance*, 67(6):2051–2095.

Goldsmith, R. W. (1969). *Financial structure and development*. New Haven, [Conn.] : Yale University Press. Includes index.

Gopinath, G., Kalemli-Özcan, e., Karabarbounis, L., and Villegas-Sanchez, C. (2017). Capital allocation and productivity in south europe*. *The Quarterly Journal of Economics*, 132(4):1915–1967.

Gorton, G. and Ordoñez, G. (2016). Good booms, bad booms. Working Paper 22008, National Bureau of Economic Research.

Gourinchas, P.-O. and Obstfeld, M. (2012). Stories of the Twentieth Century for the Twenty-First. *American Economic Journal: Macroeconomics*, 4(1):226–65.

Greenwald, D. L. (2016). The mortgage credit channel of macroeconomic transmission. Technical report, MIT Sloan Research Paper No. 5184-16. Available at SSRN: https://ssrn.com/abstract=2735491 or http://dx.doi.org/10.2139/ssrn.2735491.

Greenwood, R. and Hanson, S. G. (2013). Issuer quality and corporate bond returns. *The Review of Financial Studies*, 26(6):1483–1525.

Greenwood, R. and Hanson, S. G. (2015). Waves in ship prices and investment. *The Quarterly Journal of Economics*, 130(1):55–109.

Greenwood, R., Hanson, S. G., and Jin, L. J. (2016). A model of credit market sentiment. Technical report, https://dash.harvard.edu/handle/1/27864354.

Greenwood, R. and Scharfstein, D. (2013). The growth of finance. *Journal of Economic Perspectives*, 27(2):3–28.

Gropp, R., Scholz, J. K., and White, M. J. (1997). Personal Bankruptcy and Credit Supply and Demand. *The Quarterly Journal of Economics*, 112(1):217–251.

Guiso, L., Jappelli, T., Padula, M., and Pagano, M. (2004a). Financial market integration and economic growth in the EU. *Economic Policy*, 19(40):523–577.

Guiso, L., Sapienza, P., and Zingales, L. (2004b). Does local financial development matter?*. *The Quarterly Journal of Economics*, 119(3):929–969.

Hackbarth, D., Haselmann, R., and Schoenherr, D. (2015). Financial distress, stock returns, and the 1978 bankruptcy reform act. *Review of Financial Studies*, 28(6):1810–1847.

Haldane, A. (2017). Rethinking financial stability. *Speech given at the Rethinking Macroeconomic Policy IV Conference, Washington, D.C., Peterson Institute for International Economics.*

Hall, B. H. and Lerner, J. (2010). *The Financing of R&D and Innovation*, volume 1 of *Handbook of the Economics of Innovation*, chapter 0, pages 609–639. Elsevier.

Halling, M., Pichler, P., and Stomper, A. (2016). The politics of related lending. *Journal of Financial and Quantitative Analysis*, 51(1):333–358.

Haselmann, R., Pistor, K., and Vig, V. (2010). How Law Affects Lending. *Review of Financial Studies*, 23(2):549–580.

Hayek, F. (1929). *Geldtheorie und Konjunkturtheorie*. Beiträge zur Konjunkturforschung. Hölder-Pichler-Tempsky a. g.

Henry, P. B. (2000a). Do stock market liberalizations cause investment booms? *Journal of Financial Economics*, 58(1-2):301–334.

Henry, P. B. (2000b). Stock Market Liberalization, Economic Reform, and Emerging Market Equity Prices. *Journal of Finance*, 55(2):529–564.

Herrera, H., OrdoÃ±ez, G., and Trebesch, C. (2014). Political booms, financial crises. Working Paper 20346, National Bureau of Economic Research.

Hombert, J. and Matray, A. (2017). The Real Effects of Lending Relationships on Innovative Firms and Inventor Mobility. *Review of Financial Studies*, 30(7):2413–2445.

Horvath, B. and Wagner, W. (2016). Macroprudential policies and the lucas critique. In Settlements, B. f. I., editor, *Macroprudential policy*, volume 86, pages 39–44. Bank for International Settlements.

Hsu, P.-H., Tian, X., and Xu, Y. (2014). Financial development and innovation: Cross-country evidence. *Journal of Financial Economics*, 112(1):116–135.

Hume, M. and Sentance, A. (2009). The global credit boom: Challenges for macroeconomics and policy. *Journal of International Money and Finance*, 28(8):1426–1461.

IMF (2011). Macroprudential Policy; What Instruments and How to Use them? Lessons From Country Experiences. IMF Working Papers 11/238, International Monetary Fund.

IMF (2017). Global Financial Stability Report October 2017: Is Growth at Risk? Technical report, International Monetary Fund.

Inklaar, R., de Jong, H., Bolt, J., and van Zanden, J. (2018). Rebasing 'Maddison': new income comparisons and the shape of long-run economic development. GGDC Research Memorandum GD-174, Groningen Growth and Development Centre, University of Groningen.

Ito, T. (1990). The timing of elections and political business cycles in japan. *Journal of Asian Economics*, 1(1):135–156.

Iverson, B. C. (2016). Get in line: Chapter 11 restructuring in crowded bankruptcy courts. Technical report, Available at SSRN: https://ssrn.com/abstract=2156045 or http://dx.doi.org/10.2139/ssrn.2156045.

IvkoviÄ‡, Z. and Weisbenner, S. (2005). Local does as local is: Information content of the geography of individual investors' common stock investments. *The Journal of Finance*, 60(1):267–306.

Jappelli, T. and Pagano, M. (1994). Saving, Growth, and Liquidity Constraints. *The Quarterly Journal of Economics*, 109(1):83–109.

Jappelli, T. and Pagano, M. (2002). Information sharing, lending and defaults: Cross-country evidence. *Journal of Banking & Finance*, 26(10):2017–2045.

Jappelli, T., Pagano, M., and Bianco, M. (2005). Courts and Banks: Effects of Judicial Enforcement on Credit Markets. *Journal of Money, Credit and Banking*, 37(2):223–44.

Jappelli, T., Pagano, M., and di Maggio, M. (2008). Households' Indebtedness and Financial Fragility. CSEF Working Papers 208, Centre for Studies in Economics and Finance (CSEF), University of Naples, Italy.

Jens, C. E. (2017). Political uncertainty and investment: Causal evidence from u.s. gubernatorial elections. *Journal of Financial Economics*, 124(3):563–579.

Jiménez, G., Ongena, S., Peydró, J.-L., and Saurina, J. (2012). Credit Supply and Monetary Policy: Identifying the Bank Balance-Sheet Channel with Loan Applications. *American Economic Review*, 102(5):2301–26.

Jiménez, G., Ongena, S., Peydró, J.-L., and Saurina, J. (2017). Macroprudential policy, countercyclical bank capital buffers, and credit supply: Evidence from the spanish dynamic provisioning experiments. *Journal of Political Economy*, 125(6):2126–2177.

Jordà, Ò., Schularick, M., and Taylor, A. M. (2013a). Sovereigns versus banks: credit, crises, and consequences. Working Paper Series 2013-37, Federal Reserve Bank of San Francisco.

Jordà, Ò., Schularick, M., and Taylor, A. M. (2013b). When credit bites back. *Journal of Money, Credit and Banking*, 45(s2):3–28.

Jordà, Ò., Schularick, M., and Taylor, A. M. (2014). The great mortgaging: Housing finance, crises, and business cycles. NBER Working Papers 20501, National Bureau of Economic Research, Inc.

Jordà, Ò., Schularick, M., and Taylor, A. M. (2015). Betting the house. *Journal of International Economics*, 96(S1):S2–S18.

Jordà, Ò., Schularick, M., and Taylor, A. M. (2016). Macrofinancial History and the New Business Cycle Facts. In *NBER Macroeconomics Annual 2016, Volume 31*, NBER Chapters. National Bureau of Economic Research, Inc.

Jordà, Ò. and Taylor, A. M. (2011). Performance evaluation of zero net-investment strategies. NBER Working Papers 17150, National Bureau of Economic Research, Inc.

Jului, B. and Yook, Y. (2012). Political uncertainty and corporate investment cycles. *The Journal of Finance*, 67(1):45–83.

Justiniano, A., Primiceri, G. E., and Tambalotti, A. (2015). Credit supply and the housing boom. Working Paper 20874, National Bureau of Economic Research.

Kay, J. (2015). *Other People's Money: The Real Business of Finance*. PublicAffairs.

Keil, J. and Müller, K. (2018). Bank branching deregulation and the syndicated loan market. *Available at SSRN: https://ssrn.com/abstract=2959534 or http://dx.doi.org/10.2139/ssrn.2959534*.

Kerr, W. and Nanda, R. (2009a). Financing Constraints and Entrepreneurship. NBER Working Papers 15498, National Bureau of Economic Research, Inc.

Kerr, W. R. and Nanda, R. (2009b). Democratizing entry: Banking deregulations, financing constraints, and entrepreneurship. *Journal of Financial Economics*, 94(1):124–149.

Keys, B. J., Mukherjee, T., Seru, A., and Vig, V. (2010). Did securitization lead to lax screening? evidence from subprime loans*. *The Quarterly Journal of Economics*, 125(1):307–362.

Kim, H. and Kung, H. (2017). The asset redeployability channel: How uncertainty affects corporate investment. *Review of Financial Studies*, 30(1):245–280.

Kindleberger, C. (1978). *Manias, panics, and crashes: a history of financial crises*. Basic Books.

Kindleberger, C. and Aliber, R. (2005). *Manias, Panics and Crashes: A History of Financial Crises*. Palgrave Macmillan UK, 5 edition.

King, R. G. and Levine, R. (1993). Finance and growth: Schumpeter might be right. *The Quarterly Journal of Economics*, 108(3):717–37.

Kiyotaki, N. and Moore, J. (1997). Credit cycles. *Journal of Political Economy*, 105(2):211–48.

Korinek, A. and Simsek, A. (2016). Liquidity Trap and Excessive Leverage. *American Economic Review*, 106(3):699–738.

Krishnamurthy, A. and Muir, T. (2017). How Credit Cycles across a Financial Crisis. NBER Working Papers 23850, National Bureau of Economic Research, Inc.

Kroszner, R. S. and Strahan, P. E. (1999). What drives deregulation? economics and politics of the relaxation of bank branching restrictions. *The Quarterly Journal of Economics*, 114(4):1437–1467.

Kroszner, R. S. and Strahan, P. E. (2014). *Regulation and Deregulation of the U.S. Banking Industry: Causes, Consequences and Implications for the Future*, pages 485–543. University of Chicago Press.

Kumhof, M., Rancière, R., and Winant, P. (2015). Inequality, Leverage, and Crises. *American Economic Review*, 105(3):1217–45.

Kuttner, K. and Shim, I. (2016). Can non-interest rate policies stabilize housing markets? evidence from a panel of 57 economies. *Journal of Financial Stability*, 26(C):31–44.

La Porta, R., de Silanes, F. L., Shleifer, A., and Vishny, R. W. (1997). Legal Determinants of External Finance. *Journal of Finance*, 52(3):1131–50.

La Porta, R., de Silanes, F. L., Shleifer, A., and Vishny, R. W. (1998). Law and Finance. *Journal of Political Economy*, 106(6):1113–1155.

Laeven, L. and Majnoni, G. (2005). Does judicial efficiency lower the cost of credit? *Journal of Banking & Finance*, 29(7):1791–1812.

Laeven, L. and Valencia, F. (2013). Systemic banking crises database. *IMF Economic Review*, 61(2):225–270.

Lelarge, C., Sraer, D., and Thesmar, D. (2010). Entrepreneurship and Credit Constraints: Evidence from a French Loan Guarantee Program. In *International Differences in Entrepreneurship*, NBER Chapters, pages 243–273. National Bureau of Economic Research, Inc.

Levchenko, A. A., Rancière, R., and Thoenig, M. (2009). Growth and risk at the industry level: The real effects of financial liberalization. *Journal of Development Economics*, 89(2):210–222.

Levin, R. and Ranney-Marinelli, A. (2005). The creeping repeal of chapter 11: The significant business provisions of the bankruptcy abuse prevention and consumer protection act of 2005. *American Bankruptcy Law Journal*, 79(603):641–42.

Levine, R. (1997). Financial Development and Economic Growth: Views and Agenda. *Journal of Economic Literature*, 35(2):688–726.

Levine, R. (1999). Law, Finance, and Economic Growth. *Journal of Financial Intermediation*, 8(1-2):8–35.

Levine, R. (2005). Finance and growth: Theory and evidence. In Aghion, P. and Durlauf, S., editors, *Handbook of Economic Growth*, volume 1 of *Handbook of Economic Growth*, chapter 12, pages 865–934. Elsevier.

Levine, R., Loayza, N., and Beck, T. (2000). Financial intermediation and growth: Causality and causes. *Journal of Monetary Economics*, 46(1):31–77.

Liberti, J. M. and Mian, A. R. (2010). Collateral Spread and Financial Development. *Journal of Finance*, 65(1):147–177.

Liu, W.-M. and Ngo, P. T. (2014). Elections, political competition and bank failure. *Journal of Financial Economics*, 112(2):251–268.

Liu, Z., Wang, P., and Zha, T. (2013). Land-price dynamics and macroeconomic fluctuations. *Econometrica*, 81(3):1147–1184.

Loayza, N. V. and Ranciere, R. (2006). Financial development, financial fragility, and growth. *Journal of Money, Credit and Banking*, 38(4):1051–1076.

López-Salido, D., Stein, J. C., and Zakrajšek, E. (2017). Credit-market sentiment and the business cycle*. *The Quarterly Journal of Economics*, 132(3):1373–1426.

Loughran, T. and Schultz, P. (2004). Weather, stock returns, and the impact of localized trading behavior. *Journal of Financial and Quantitative Analysis*, 39(2):343–364.

Lowe, P. and Borio, C. (2002). Asset prices, financial and monetary stability: exploring the nexus. BIS Working Papers 114, Bank for International Settlements.

MacRae, C. (1977). A political model of the business cycle. *Journal of Political Economy*, 85(2):239–63.

Malloy, C. (2005). The geography of equity analysis. *Journal of Finance*, 60(2):719–755.

Malmendier, U. and Nagel, S. (2011). Depression babies: Do macroeconomic experiences affect risk taking?*. *The Quarterly Journal of Economics*, 126(1):373–416.

Mankiw, N. and Weil, D. N. (1989). The baby boom, the baby bust, and the housing market. *Regional Science and Urban Economics*, 19(2):235–258.

Martin, P. and Philippon, T. (2017). Inspecting the mechanism: Leverage and the great recession in the eurozone. *American Economic Review*, 107(7):1904–37.

Matsuyama, K. (2007). Credit Traps and Credit Cycles. *American Economic Review*, 97(1):503–516.

Matsuyama, K. (2013). The good, the bad, and the ugly: An inquiry into the causes and nature of credit cycles. *Theoretical Economics*, 8(3).

Mbaye, S., Moreno Badia, M., and Chae, K. (2018). Global debt database: Methodology and sources. IMF Working Papers 18/111, International Monetary Fund.

Mendoza, E. G. and Terrones, M. E. (2012). An anatomy of credit booms and their demise. Working Paper 18379, National Bureau of Economic Research.

Mian, A. and Sufi, A. (2011). House prices, home equity-based borrowing, and the us household leverage crisis. *American Economic Review*, 101(5):2132–56.

Mian, A. and Sufi, A. (2014a). *House of Debt*. Number 9780226081946 in University of Chicago Press Economics Books. University of Chicago Press.

Mian, A. and Sufi, A. (2014b). What explains the 2007–2009 drop in employment? *Econometrica*, 82(6):2197–2223.

Mian, A., Sufi, A., and Trebbi, F. (2010). The political economy of the us mortgage default crisis. *American Economic Review*, 100(5):1967–98.

Mian, A., Sufi, A., and Trebbi, F. (2013). The political economy of the subprime mortgage credit expansion. *Quarterly Journal of Political Science*, 8(4):373–408.

Mian, A., Sufi, A., and Verner, E. (2017a). How do credit supply shocks affect the real economy? evidence from the united states in the 1980s. Working Paper 23802, National Bureau of Economic Research.

Mian, A. R. and Sufi, A. (2018). Finance and Business Cycles: The Credit-Driven Household Demand Channel. NBER Working Papers 24322, National Bureau of Economic Research, Inc.

Mian, A. R., Sufi, A., and Verner, E. (2017b). Household Debt and Business Cycles Worldwide. *Quarterly Journal of Economics*.

Minsky, H. (1986). *Stabilizing an Unstable Economy*. McGraw Hill professional. McGraw-Hill Education.

Minsky, H. P. (1977). The financial instability hypothesis: An interpretation of keynes and an alternative to "standard" theory. *Challenge*, 20(1):20–27.

Miranda-Agrippino, S. and Rey, H. (2015). US Monetary Policy and the Global Financial Cycle. NBER Working Papers 21722, National Bureau of Economic Research, Inc.

Monnet, E. (2014). Monetary Policy without Interest Rates: Evidence from France's Golden Age (1948 to 1973) Using a Narrative Approach. *American Economic Journal: Macroeconomics*, 6(4):137–169.

NBB (2017). Financial Stability Report 2017. Technical report, National Bank of Belgium.

Nini, G., Smith, D. C., and Sufi, A. (2009). Creditor control rights and firm investment policy. *Journal of Financial Economics*, 92(3):400–420.

Nordhaus, W. D. (1975). The political business cycle. *Review of Economic Studies*, 42(2):169–190.

Pagano, M. and Jappelli, T. (1993). Information sharing in credit markets. *The Journal of Finance*, 48(5):1693–1718.

Pesaran, M. H. and Smith, R. (1995). Estimating long-run relationships from dynamic heterogeneous panels. *Journal of Econometrics*, 68(1):79–113.

Petersen, M. A. and Rajan, R. G. (2002). Does distance still matter? the information revolution in small business lending. *The Journal of Finance*, 57(6):2533–2570.

Ponticelli, J. and Alencar, L. S. (2016). Court Enforcement, Bank Loans, and Firm Investment: Evidence from a Bankruptcy Reform in Brazil. *The Quarterly Journal of Economics*, 131(3):1365–1413.

Popov, A. (2017). Evidence on finance and economic growth. Working Paper Series 2115, European Central Bank.

Qian, J. and Strahan, P. E. (2007). How laws and institutions shape financial contracts: The case of bank loans. *The Journal of Finance*, 62(6):2803–2834.

Rajan, R. and Ramcharan, R. (2015). The anatomy of a credit crisis: The boom and bust in farm land prices in the united states in the 1920s. *American Economic Review*, 105(4):1439–77.

Rajan, R. G. (2010). *Fault Lines: How Hidden Fractures Still Threaten the World Economy*. Princeton.

Rajan, R. G. and Zingales, L. (1998). Financial dependence and growth. *American Economic Review*, 88(3):559–86.

Rajan, R. G. and Zingales, L. (2003). The great reversals: the politics of financial development in the twentieth century. *Journal of Financial Economics*, 69(1):5–50.

Ramey, V. (2016). *Macroeconomic Shocks and Their Propagation*, volume 2 of *Handbook of Macroeconomics*, chapter 0, pages 71–162. Elsevier.

Rancière, R. and Tornell, A. (2016). Financial liberalization, debt mismatch, allocative efficiency, and growth. *American Economic Journal: Macroeconomics*, 8(2):1–44.

Reinhart, C. M. and Kaminsky, G. L. (1999). The twin crises: The causes of banking and balance-of-payments problems. *American Economic Review*, 89(3):473–500.

Reinhart, C. M. and Rogoff, K. S. (2009a). The aftermath of financial crises. *American Economic Review*, 99(2):466–72.

Reinhart, C. M. and Rogoff, K. S. (2009b). *This Time Is Different: Eight Centuries of Financial Folly*, volume 1 of *Economics Books*. Princeton University Press.

Rey, H. (2015). Dilemma not Trilemma: The global Financial Cycle and Monetary Policy Independence. NBER Working Papers 21162, National Bureau of Economic Research, Inc.

Roberts, M. R. and Sufi, A. (2009). Control Rights and Capital Structure: An Empirical Investigation. *Journal of Finance*, 64(4):1657–1695.

Rodano, G., Serrano-Velarde, N., and Tarantino, E. (2016). Bankruptcy law and bank financing. *Journal of Financial Economics*, 120(2):363–382.

Rognlie, M., Shleifer, A., and Simsek, A. (2014). Investment hangover and the great recession. Working Paper 20569, National Bureau of Economic Research.

Rogoff, K. and Sibert, A. (1988). Elections and macroeconomic policy cycles. *Review of Economic Studies*, 55(1):1–16.

Rousseau, P. L. and Wachtel, P. (1998). Financial intermediation and economic performance: Historical evidence from five industrialized countries. *Journal of Money, Credit and Banking*, 30(4):657–78.

Sapienza, P. (2004). The effects of government ownership on bank lending. *Journal of Financial Economics*, 72(2):357–384.

Sautner, Z. and Vladimirov, V. (2017). Indirect costs of financial distress and bankruptcy law: Evidence from trade credit and sales*. *Review of Finance*, page rfx032.

Schiantarelli, F., Stacchini, M., and Strahan, P. E. (2016). Bank Quality, Judicial Efficiency and Borrower Runs: Loan Repayment Delays in Italy. NBER Working Papers 22034, National Bureau of Economic Research, Inc.

Schivardi, F., Sette, E., and Tabellini, G. (2017). Credit Misallocation During the European Financial Crisis. CEPR Discussion Papers 11901, C.E.P.R. Discussion Papers.

Schmitt-Grohé, S. and Uribe, M. (2016). Downward nominal wage rigidity, currency pegs, and involuntary unemployment. *Journal of Political Economy*, 124(5):1466–1514.

Schneider, M. and Tornell, A. (2004). Balance sheet effects, bailout guarantees and financial crises. *The Review of Economic Studies*, 71(3):883–913.

Schularick, M. and Taylor, A. M. (2012). Credit booms gone bust: Monetary policy, leverage cycles, and financial crises, 1870-2008. *American Economic Review*, 102(2):1029–61.

Schultz, K. A. (1995). The politics of the political business cycle. *British Journal of Political Science*, 25(1):79–99.

Schumpeter, J. (1912). *Theorie der wirtschaftlichen Entwicklung*. Duncker & Humblot.

Schwert, M. (2017). Bank capital and lending relationships. *Journal of Finance*.

Shiller, R. J. (2000). Measuring bubble expectations and investor confidence. *Journal of Psychology and Financial Markets*, 1(1):49–60.

Stiglitz, J. E. and Weiss, A. (1981). Credit rationing in markets with imperfect information. *The American Economic Review*, 71(3):393–410.

Sufi, A. (2007). Information asymmetry and financing arrangements: Evidence from syndicated loans. *The Journal of Finance*, 62(2):629–668.

Takats, E. (2012). Aging and house prices. *Journal of Housing Economics*, 21(2):131–141.

Tripathy, J. (2017). Bubbly equilibria with credit misallocation. Bank of England working papers 649, Bank of England.

Tufte, E. R. (1980). *Political Control of the Conomy*. Princeton University Press, 1 edition.

Turner, A. (2015). *Between Debt and the Devil: Money, Credit, and Fixing Global Finance*. Princeton University Press.

USGAO (2008). Bankruptcy reform: Dollar costs associated with the bankruptcy abuse prevention and consumer protection act of 2005. Technical report, United States Government Accountability Office, Washington D.C.

Vig, V. (2013). Access to collateral and corporate debt structure: Evidence from a natural experiment. *The Journal of Finance*, 68(3):881–928.

von Lilienfeld-Toal, U., Mookherjee, D., and Visaria, S. (2012). The distributive impact of reforms in credit enforcement: Evidence from indian debt recovery tribunals. *Econometrica*, 80(2):497–558.

Von Mises, L. (1912). *Theorie des Geldes und der Umlaufsmittel. Von Ludwig v. Mises*. Duncker & Humblot.

Warnock, V. C. and Warnock, F. E. (2008). Markets and housing finance. *Journal of Housing Economics*, 17(3):239–251.

Wicksell, K. (1898). *Geldzins und Güterpreise: Eine Studie über die den Tauschwert des Geldes bestimmenden Ursachen*. G. Fischer.

Wurgler, J. (2000). Financial markets and the allocation of capital. *Journal of Financial Economics*, 58(1-2):187–214.

Yellen, J. L. (2017). Financial stability a decade after the onset of the crisis. speech at fostering a dynamic global recovery, a symposium sponsored by the federal reserve bank of kansas city, jackson hole, wyoming, 25 august.

Zingales, L. (2015). Does Finance Benefit Society? NBER Working Papers 20894, National Bureau of Economic Research, Inc.