Essays in Empirical Finance

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To my grandparents

Foreword

This volume is the result of a research project carried out at the Department of Finance at the Stockholm School of Economics (SSE).

This volume is submitted as a doctor's thesis at SSE. In keeping with the policies of SSE, the author has been entirely free to conduct and present his research in the manner of his choosing as an expression of his own ideas.

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Göran Lindqvist

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Athens, 20 April 2015

Kristoffer Milonas

Contents

Introduction

This thesis comprises three research papers that I wrote during the Ph.D. program. During parts of this time I was also affiliated with the Institute for Financial Research (SIFR), and parts of the first paper were written when I visited the London School of Economics and Political Science (LSE).

Each chapter is self-contained and can be read individually. While they cover rather different topics, they are all primarily empirical in nature and largely share empirical methods.

The first paper, *The Effect of Foreclosure Laws on Securitization: Evidence from U.S. States*, considers how laws affecting the cost of foreclosing on borrowers who cannot repay their loans (i.e. repossessing their property) affect the decision to securitize their mortgages (i.e. packaging them into portfolios of loans that are then sold to investors as securities). This question is based on prior literature documenting that loans that have been securitized are less likely to be renegotiated. I hypothesize that this higher exposure to foreclosure makes the value of the loan more dependent on the cost of foreclosure if it has been securitized, and that fewer mortgages will therefore be securitized when foreclosure is costlier. I document that securitization is less likely in U.S. states where foreclosure is costlier, with a cross-sectional variation in the magnitude of the effect consistent with the hypothesis. This paper thereby shows that lenders considered the costs of the higher foreclosure propensity among securitized mortgages, even during the boom years before the recent financial crisis. Hence, the results complements the literature that has considered the *ex post* effects of securitization by showing one *ex ante* determinant of the securitization decision. The results also have implications for policy makers, who are currently seeking ways to reinvigorate the securitization markets due to the potential decrease in costs of capital due to diversification, despite the problems that were uncovered during the crisis.

In the second paper, *Do daughters make family firms more sustainable?*, I study how the composition of the family owning large blocks of shares in listed companies affects the policies of the company. Using a mix of sources, I create a novel data set on the composition of all families that control at least 20% of votes in a Swedish listed company. I find that the environmental performance of the company improves when the family has more daughters, using an assessment from an external sustainability evaluator. This effect does not seem to operate through more adult daughters leading to more women in the board of directors or female CEOs, or through the appointment of family members as CEOs. Family CEOs are rare among Swedish listed companies, but family directors are rather common. It appears that when these adult daughters are chosen to the board, fewer other female candidates are chosen instead. To establish causality, I condition the analysis on the total number of children, thereby using the random variation in child gender. This study complements the literature documenting child-parent influence in other contexts, and is to my knowledge the first to do so in the context of listed companies. It thereby also presents a possible source for the unexplained blockholder heterogeneity that has been discussed in corporate finance literature.[1] Understanding such heterogeneity is important since blockholders are more widespread in the U.S. than what was previously thought, and are common around the world.[2]

The third paper, *Bank taxes, leverage and risk*, starts from an observation dating back at least to Modigliani and Miller (1958, 1963): debt is tax advantaged since payments of interests to debt holders are tax deductible while payments of dividends to equity holders are not. Based on this observation, policy makers have more recently taken an interest and some actions towards changing the tax system in ways that make leverage less attractive to banks; yet relatively few studies consider how banks react to existing taxes. To identify the effect of tax changes I rely on staggered

[1] See e.g. Cronqvist and Fahlenbrach (2009); Derrien, Kecskés, and Thesmar (2013); and McCahery, Starks, and Sautner (2014).

[2] See e.g. Faccio and Lang (2002), and La Porta, Lopez-De-Silanes, and Shleifer (1999)

changes in U.S. state-level bank taxation, and can hence compare banks with different exposure to tax changes in the same year. The results suggest an economically substantial shift towards more leverage when taxes increase, with a symmetric reduction for tax decreases. I also uncover results suggesting that banks dampen the effect of the leverage changes by adjusting their so-called Tier 2 capital, a lower-quality form of capital that is less able to absorb losses, and yet count towards legally mandated capital levels. *Ex ante*, it is not clear what effect taxes will have on bank risk taking. My results suggest that banks partly compensate for the change in balance sheet riskiness by changing their asset risk in the opposite direction when taxes change. This result is obtained using the regulatory risk-weighted to total assets as a risk measure. However, I also find that some of the reduction in the measured risk may be due to regulatory arbitrage activities. In particular, higher taxes make banks increase their holdings of so-called non-agency mortgage-backed securities, an asset which had low regulatory capital requirements in relation to its risk before the crisis.

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Chapter 1

The Effect of Foreclosure Laws on Securitization: Evidence from U.S. States*

Abstract A mortgage that runs into default is more likely to enter foreclosure rather than renegotiation if it has been securitized in the private non-agency market, according to previous research. I study whether this foreclosure-propensity affects lenders' decision to securitize ex ante. Due to the higher foreclosure probability, the value of a mortgage should be more sensitive to foreclosure costs if it is securitized. Comparing loans made in the same metropolitan area but under different foreclosure laws, I find that lenders are less likely to securitize mortgages in states with higher foreclosure costs, as proxied by laws requiring judicial foreclosure proceedings. Consistent with differences in loss given default driving the results, the effect of judicial requirements increases for loans with higher expected default rates. Borrowers in states without judicial requirements also get riskier loans, with higher average loan to income ratios and more loans lacking income documentation.

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1 Introduction

Previous research has pointed to securitization as a contributing factor to the depth of the U.S. foreclosure crisis. In particular, if a mortgage has been securitized in the private non-agency market,[1] foreclosure becomes more likely relative to different forms of renegotiation (e.g. Agarwal et al., 2011b; Kruger, 2014; Piskorski, Seru, and Vig, 2010).[2] The literature suggests that the reason behind this difference is the difficulty in designing effective contracts that give servicers of securitized mortgages the proper incentives to renegotiate, considering the unobservable effort required. Given the high private costs and the negative externalities from foreclosures, the social cost of this foreclosure-propensity is likely to be substantial.[3] Despite these problems, policy makers are eager to revive the markets for residential mortgage securitization, due to the benefits from lower cost of capital and improved risk sharing.[4] Studying how market participants respond to the renegotiation friction in their ex ante contracting therefore not only com-

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[1] The non-agency market consists of mortgage-backed securities issued without the backing of the Government Sponsored Enterprises (GSEs), e.g. Fannie Mae and Freddie Mac. Due to the government backing of the GSEs, the institutional framework surrounding mortgages securitized by them is rather different.

[2] Following Piskorski, Seru, and Vig (2010), I use the term renegotiation in its broadest meaning to include all kinds of loan resolutions that entail a change to the original contract. These include e.g. deed-in-lieu (where the borrower voluntarily returns the property to the lender), forbearance plans, short-sales (where the parties agree to sell the property to a third party for a lower price than the loan amount), refinancing borrowers into more affordable loans, and explicit modification of contractual terms. Further explanations of these and other terms can be found at e.g. http://knowyouroptions.com/find-resources/information-and-tools/glossary.

[3] See e.g. Campbell, Giglio, and Pathak (2011) for evidence on negative externalities from foreclosures on prices of surrounding properties, and Mian, Sufi, and Trebbi (2014) for evidence on negative effects on the real economy.

[4] See e.g. Bank of England and European Central Bank (2014), and U.S. Department of the Treasury (2014).

plements the literature about ex post effects of securitization, but also has implications for current policy debates.

This paper empirically investigates whether lenders respond to the higher foreclosure-propensity among securitized mortgages in their decisions on whether to securitize, using cross-state variation in foreclosure laws. By raising the probability of foreclosure given default, securitization makes the expected payoff from a mortgage more sensitive to the expected recovery rate in foreclosure. If investors are aware of this and price the differences in foreclosure costs, this would make securitization less attractive for mortgages where the expected loss in foreclosure is higher. In other words, the lower renegotiation propensity for securitized mortgages in default drives a wedge between the expected payoff if the mortgage had been retained and the price that the lender can receive in the securitization market, and this wedge increases in the cost of foreclosure.

There are however several reasons why the securitization rate may not have decreased with higher foreclosure costs. First, while the literature on these renegotiation frictions has developed rapidly after the crisis, it is less clear whether market participants were aware of these frictions before the foreclosure wave started. Second, theories based on asymmetric information or moral hazard problems suggest that the opposite pattern may hold. In the simplest case, suppose that lenders are aware of the variation in foreclosure costs while investors are not. There could then be adverse selection where lenders securitize loans with high foreclosure cost and keep the ones with lower cost. Alternatively, investors may be aware of the differences but have lower expectations on default rates than securitizers. The mispricing caused by such optimistic beliefs increases in the expected foreclosure cost; thus securitization may again be more attractive when expected foreclosure costs are higher. In contrast to these behavioral stories, the renegotiation rigidity in securitization may be a rational way for lenders to commit to an ex ante optimal policy. If this is an important motivation for securitization, there may be no difference or even higher securitization rates when foreclosure is costlier.[5]

[5] In an analogous argument in corporate finance, dispersed lenders may be a way to commit not to renegotiate, and thereby prevent strategic defaults (Bolton and Scharfstein, 1996; Diamond, 2004). In such models, the temptation to renegotiate in-

I examine this question empirically using loan-level data on U.S. mortgages from the years 2001-2012. Variation in the cost of foreclosure is obtained by some states imposing so-called judicial requirements that force the lender to go to court to foreclose. Previous literature has shown that such requirements substantially increase the time it takes to foreclose and the loss it causes the lender (e.g. Pence, 2006). Ghent (2014) documents that these laws were typically written several decades or even centuries ago and never changed since; hence, they are unlikely to be endogenous with respect to current economic conditions. To further ensure that unobserved differences between states are not driving my results I focus on metropolitan areas that cross state borders, and compare mortgages made in the same area but under different laws.

The results suggest that lenders respond to the higher expected cost of the securitization-induced renegotiation failure in judicial states. The difference is economically substantial: mortgages are approximately 3 percentage points less likely to be securitized in judicial states, which corresponds to 13% of the mean. The effect is present both before and after the financial crisis, and holds even when comparing mortgages made by the same lender in different states. Together, these results suggest that foreclosure rules can be a tool for reaching desired securitization levels, whatever the ideal levels may be.

Given that the difference in foreclosure propensity only matters in default, the magnitude of the difference also suggests that market participants expected a sizable difference in foreclosure rates between retained and securitized mortgages, and in payoffs between foreclosed and renegotiated loans. To put the figure in perspective, note that the aggregate mortgage

creases with the ex post deadweight loss from not renegotiating. Hence, the incentive to use securitization as a commitment device may be stronger when the cost of foreclosure is higher. Partly supporting this possibility, Demiroglu, Dudley, and James (2014) present evidence suggesting that borrowers are more likely to engage in strategic default in judicial states. Alternatively, if borrowers default strategically and have all bargaining power in renegotiation, they would drive down the lender's renegotiation payoff to the foreclosure amount.

delinquency rate was at most around 10% during the period,[6] and ex ante default expectations were arguably lower.

The heterogeneity in the loan-level data also lets me test additional predictions from the proposed mechanism. If differences in expected loss given default are driving the results as hypothesized, the effect of judicial requirements should be stronger for loans with higher default risks. I find support for this prediction using two measures of default risk. First, I compare loans based on the key characteristic of lack of income documentation, as such loans fared particularly poorly in the crisis. Second, I explore how judicial rules interact with soft information that banks may collect in the local market. Since banks may face an adverse selection problem when they make loans where they lack soft information, previous literature suggests that the default risk for such loans may be higher (e.g. Loutskina and Strahan, 2011). I document that banks are more sensitive to judicial rules when lending in markets where they lack soft information, as proxied by not having a bank branch. This result holds especially when comparing the same bank's behavior in different markets, while between-bank variation gives inconclusive results. The result also holds when restricting to "jumbo" loans with high notional values, for which soft information is likely to be more relevant (Loutskina and Strahan, 2011).

The argument presented in this paper focuses on the private non-agency securitization market. There is however an alternative explanation for the results which centers on the government-sponsored enterprises (GSEs). During the sample period, these enterprises did not charge different premiums for mortgages from judicial states. It could therefore be that lower rates of securitization through the private markets in judicial states are due to the profitability of offloading mortgages from these states to the GSEs. I conduct two types of tests to ensure that this is not driving the results. First, I close this channel by restricting the sample to the "jumbo market", i.e. mortgages with a notional value above the limit that GSEs are allowed to buy. If anything, the effect of judicial rules is stronger in this

[6] The record high serious delinquency rate (defined as loans 90 days past due or in foreclosure) of 9.7% was reached in the end of 2009 (source: Mortgage Bankers Association National Delinquency Survey Q4 2009).

market. Next, I control for the alternative choice of securitizing via the GSEs in a parametric fashion, using competing risks models. The effect of the judicial status on the probability of private securitization remains significant, and the marginal effects on the probability of securitization in the private market are of similar magnitude to those from the baseline linear model. In addition, a theory based on GSE activities would be inconsistent with the stronger effect of judicial rules for loans lacking income documentation, since the GSEs are highly restricted in buying these loans as well.

The identification strategy in this paper would be violated if there are other state laws that influence securitization and are systematically related to judicial requirements. By studying the correlation between judicial rules and other potentially important policies, I show that this is unlikely to be a problem. Judicial rules are not significantly correlated with laws that give recourse to other assets of defaulted borrowers, laws that affect prepayment risk, corporate tax rates, or financial market development (as proxied by the ratio of the total stock market capitalization of firms headquartered in the state to state GDP or by bank branching restrictions). Adding these variables as controls to the baseline regression also has little effect on the estimate for the coefficient of interest. Further, since the applicable foreclosure law is determined by the location of the property and not of the lender, I can add fixed effects for the lender's headquarter state. This removes state-level factors that may influence securitization such as the characteristics of bank regulators, but has no major impact on the main result. Finally, a placebo test further suggests that unobserved heterogeneity is unlikely to drive my results: judicial rules have no significant effect on the securitization of loans made to manufactured homes, which are typically not subject to state foreclosure laws.

Another threat to identification comes from potential self-selection of households into states in a manner systematically related to their potential mortgage outcomes under different regimes. I demonstrate that this is unlikely to be a major problem in the current setting by studying cross-state migration and population growth. The two types of states do not show any systematic differences either in population growth or in the average income of migrants during the sample period. Hence, although the assignment of households to states is not random, it is also unlikely to be endogenous with respect to the outcomes in this study. Moreover, I find no differential effect of judicial rules on areas with higher rates of cross-state migration.

Earlier literature has argued that securitization enabled the expansion of subprime credit (e.g. Mian and Sufi, 2009; Nadauld and Sherlund, 2013). Together with my finding that judicial rules affect the propensity to securitize, these results suggest that judicial requirements may shift the supply of risky mortgages. Consistent with this conjecture, I demonstrate that loans made in non-judicial (low foreclosure cost) states are more likely to lack income documentation and have higher loan to income ratios. Concededly, the results can however not necessarily be attributed to the ease of securitization, since rules that increase the recovery rate in default can expand the supply of risky credit even absent securitization. In line with this argument, Pence (2006) documented that lenders gave larger loans in non-judicial states using a sample of loans from 1994-1995, a period before the private securitization markets had reached substantial size.

In contrast, I find no significant effect of judicial rules on the aggregate loan supply. While judicial rules reduce the amount of securitized credit, there is a compensating change in credit from other sources rather than an aggregate supply effect. This substitution suggests that during this particular episode, states with judicial requirements were not strongly affected by potential negative effects from lower supply of credit. One potential explanation is that since credit policies were generally loose during this period, the margin of adjustment was in the risk characteristics of the mortgages that were granted rather than whether mortgages were given or not. An additional likely reason is that any effects on loan supply are clouded by the GSEs, which account for a large share of the loan supply and do not take judicial laws into account in their lending decisions.

This paper contributes to several strands of literature. First, I contribute to the literature on whether the mortgage securitization market considered risk factors appropriately before the crisis. Popular accounts and parts of the literature have stressed problems of moral hazard and asymmetric information in mortgage securitization,[7] however research has found scarce evidence for the view that insiders were systematically less optimistic than outsiders (e.g. Foote, Gerardi, and Willen, 2012; Cheng, Raina, and Xiong, 2014). Consistent with those studies, my results do not support the prediction from theories based on informed lenders fooling ignorant or overoptimistic investors. While these papers present compelling evidence from specific cases that refute the popular "inside job" view, this paper contributes by documenting aggregate contracting outcomes that also go against this view.

In addition, I contribute to the broader literature on how variation in the cost of collateral repossession affects loan contracting in other settings. Most of the literature in this area stresses the benign effect of easy collateral repossession expanding the loan supply (e.g. Jappelli, Pagano, and Bianco, 2005; Haselmann, Pistor, and Vig, 2009; Assuncao, Benmelech, and Silva, 2014, Cerqueiro, Ongena, and Roszbach, 2014). Conversely, Vig (2013) argues that easy repossession can cause a "liquidation bias" that reduces entrepreneurs' credit demand, and von Lilienfeld-Toal, Mookherjee, and Visaria (2012) argue that improvements in creditor rights may cause crowding out of low-quality borrowers when credit supply is imperfectly elastic. An important institutional difference compared to my empirical setting is that securitization was rare or non-existent in these contexts. In the current setting, the outcome is shaped by contracting not only between lenders and borrowers but also between lenders and investors in the secondary market.[8] This line of research is of increasing relevance as more lending is made through arms-length, disintermediated contracts rather than retained loans from relationship lenders.

While acknowledging that U.S. housing finance differs from other markets in important ways, the results in this paper have broader policy implications. In particular, laws that are overly debtor friendly in default

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[7] For evidence on misrepresentation of information by financial intermediaries, see e.g. Jiang, Nelson, and Vytlacil (2014); Griffin and Maturana (2015); and Piskorski, Seru, and Witkin (2015).

[8] In practice, contracting is even more complex as there are several steps in the "securitization chain" between the lender that originates the mortgage and the end investor. For closer details on these steps, see e.g. Ashcraft and Schuermann (2008).

may inhibit the recovery of the securitization market in the U.S. and other countries, which may raise the cost of credit. Consistent with this reasoning, researchers at the Bank of International Settlements have for instance concluded that "In the case of Mexico, the development of securitisation remained limited for a long time owing to (…), as well as to overly long foreclosure proceedings" (Scatigna and Tovar, 2007). Laws that make foreclosures excessively difficult may also reduce the loan supply and increase interest rates. Morse and Tsoutsoura (2013) study the extreme case of a mortgage foreclosure moratorium that was initiated in Greece, and find material effects on loan quantities and prices.

This paper proceeds as follows. Section 2 discusses the literature on how securitization may impede renegotiation and how judicial rules affect outcomes in the mortgage market. Section 3 presents the data used, and Section 4 the empirical strategy and results. Section 5 discusses policy implications and concludes.

2 Related literature

2.1 Securitization as an impediment to renegotiation

A recent literature points to securitization as an impediment to renegotiation (Agarwal et al., 2011b; Kruger, 2014; Piskorski, Seru, and Vig, 2010; and Zhang , 2013). Piskorski, Seru, and Vig (2010) argue that bank's renegotiation policies are likely closer to the optimum, although of course renegotiation is not always optimal.[9] Specifically, renegotiation entails high costs, not least from re-defaults (cf. Adelino, Gerardi, and Willen, 2013 b). In this context, it is interesting to note that renegotiated securitized loans have higher re-default rates than portfolio loans, suggesting that portfolio lenders also have better renegotiation skills than servicers of securitized mortgages (Agarwal et al., 2011b; Kruger, 2014; and Zhang, 2013). This literature argues that the differences are likely due to agency problems and institutional frictions. Such frictions include contracts between the loan servicer and the

[9] Maturana (2014) presents evidence suggesting that there would be a substantial decrease in loss rates from renegotiating more delinquent privately securitized loans.

securitization trust that explicitly prohibit securitization, the difficulty for servicers in recouping the costs involved in renegotiation processes from the securitization trust, the risk for servicers engaging in modifications for lawsuits from senior claimants in "tranche warfare", and the risk of losing preferential tax and accounting treatment (Eggert, 2007; Kruger, 2014; Levitin and Twomey, 2011). The more fundamental factor behind these frictions is largely the difficulty of writing enforceable contracts that make the servicer take the optimal action, since renegotiation relies on effort which is hard to verify (Kruger, 2014; Levitin and Twomey, 2011). Even absent institutional frictions, lenders who retain their mortgages may have stronger incentives to renegotiate if they have loans in the geographic vicinity of the defaulting borrower and hence internalize the externalities from foreclosures (Favara and Giannetti, 2014).

The emprical relevance of thes frictions is questioned by Adelino, Gerardi, and Willen (2013 a,b), who present empirical evidence suggesting that securitized mortgages are as likely to be modified as retained ones. Their conclusion is however based on an algorithm for identifying modifications rather than direct observations of them; potential problems with this algorithm are discussed by Agarwal et al. (2011b), Piskorski, Seru, and Vig (2010), and Zhang (2013). In addition, Ghent (2011) questions the role of securitization using historical evidence of low renegotiation rates during the Great Depression even though few mortgages had been securitized. However, Rose (2011) notes that at the time there was a government program that acquired troubled mortgages from banks at close to par and hence likely reduced lenders' willingness to renegotiate loans, whether or not they were securitized.

Even if a consensus is not reached, the dominant view in the literature that has emerged after the crisis is that securitization made renegotiation higher. Less is however known about whether market participants were aware of this potential problem before the crisis took hold, and adjusted their decisions to the problem. This is the question addressed in this paper.

2.2 The effect of judicial rules on mortgaged and real outcomes

Judicial foreclosure requirements impose substantial costs on lenders, due not only to the costs of the court process itself but also due to high depreciation rates during the often lenghty proceedings. Pence (2006) calculates that the total additional foreclosure costs in judicial states could be up to 10% of the loan balance, while Qi and Yang (2009) estimate that the loss given default is 2 percentage points higher in judicial states, approximately 6% of the mean in their sample. Cutts and Merrill (2008) document that the costs incurred before the actual foreclosure sale are on average 8.7% higher in judicial states.[10] The difference between the two types of states has likely increased further in the recent foreclosure wave, as courts in judicial states have been overburdened with foreclosure cases (Cordell et al., 2013).

The possibility that the higher cost of foreclosure in judicial states might lead lenders towards more renegotiation was first advanced by Clauretie (1987). More recently, Mian, Sufi, and Trebbi (2014) used judicial requirements as an instrumental variable for studying the real effects of foreclosures. They document that non-judicial states had higher foreclosure rates as well as larger downturns in house prices and real economic outcomes during the recent foreclosure wave, but also saw a sharper subsequent recovery.

Pence (2006) documented that lenders lower the loan amounts in judicial states. Harrison and Seiler (2015) study the effect of variation in judicial rules and other regulatory variables on mortgage interest rate quotes.[11] Curtis (2013) argues that judicial rules caused an expansion in the

[10] See also Clauretie (1987), Clauretie and Herzog (1990), and Pennington-Cross (2003) for evidence of higher foreclosure losses in judicial states.

[11] Their estimated effect of judicial laws on interest rates is negative, which is surprising at first sight. However, their regressions also control for the time required to foreclose, which complicates the interpretation of coefficients since the average foreclosure time is longer in judicial states. This additional delay is one of the key drivers of the higher foreclosure losses in judicial states (see Cutts and Merrill, 2008). In contrast, Mian, Sufi, and Trebbi (2014) find no significant difference in interest rates between the two types of states.

market share of subprime lenders during the boom. Finally, Dagher and Sun (2014) document that judicial rules affect the probability of loan application acceptance around the jumbo cutoff but no effect on the amount of applications, consistent with an effect on supply rather than demand of jumbo mortgages.

By studying securitization rates, this paper complements our understanding of the channel behind these outcomes. Most directly, the results suggest that the higher foreclosure rate in non-judicial states documented by Mian, Sufi, and Trebbi (2014) is partly due to the indirect effect of a higher securitization rate, which translates into a higher foreclosure rate due to the higher foreclosure propensity among securitized mortgages.[12]

3 Data

This paper combines a variety of data sources. Information about individual mortgages is obtained from the Home Mortgage Disclosure Act (HMDA). This data set is very comprehensive in that it covers both depository and non-depository institutions that are together estimated to account for at least 80% of all mortgage lending in the United States (Avery, Brevoort, and Canner, 2007). I retain only conventional loans for purchases of owner-occupied properties, following much of the literature. I also drop all mortgages that are outside a metropolitan statistical area (MSA) since

[12] To quantify this indirect effect, one can use the reduction in securitization probability for judicial states estimated in this paper to approximately 3 ppt, and combine it with the increase in probability of foreclosure given default for securitized mortgages, for which estimates range around 5 ppt in the literature. Taken at face value, these results together suggest that the lower securitization rate in judicial states causes a decrease in the probability of foreclosure given default by roughly 15 bp. This is however likely an understatement since my result suggest a stronger effect of judicial rules on riskier mortgages; the difference in securitization rates between state types is therefore likely higher among mortgages that subsequently defaulted than in the full sample. In comparison, Mian, Sufi, and Trebbi (2014) document that the probability of foreclosure given default was 16.7 ppt lower in judicial states during the years 2008-2009.

decisions may be driven by other factors for rural areas.[13] Moreover, for most of my tests I retain only MSAs that cross state borders, as these are the areas that allow me to more cleanly isolate the causal effect of laws. Following Mian and Sufi (2009), I classify a loan as privately securitized if it is coded as sold to a private securitization vehicle or to the "other" category, which they argue is likely to mostly represent private securitization.[14] Since the main variable of interest varies only at the geographical level, in most of the tests I aggregate the data to tract-year-level averages; however in later sections I explore the heterogeneity in the loan-level data.

I also gather information on local economic conditions from U.S. government agencies and the house listing service Zillow. These variables are presented in the Appendix and include tract-level[15] home ownership rate, the ratio of tract to MSA median income, and minority population share; county-level unemployment and debt to income ratio; and local house prices.[16] These are measured as averages over the years 1997-2000.[17]

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[13] For convenience, I use the term MSA to include metropolitan statistical areas (MSAs) and micropolitan statistical areas (µSAs). I use the year 2009 MSA definitions from the Census Bureau. MSAs are contiguous geographical areas with a high population (at least 50,000), a high-density core, and highly integrated adjacent areas. The definition of µSAs is similar but with a lower population threshold (10,000).

[14] The data contains information about whether a loan was sold during the year, and if so to which type of buyer. Mortgages that are sold in the calendar year after they are originated will therefore be classified as retained by the lender. While this introduces noise into my measure, it is hard to see how it would bias the results.

[15] A census tract is a geographical area formed for statistical purposes and is the finest geographical classifications for which statistical data is widely available. Tracts are designed to have a populations of around 4,000 (ranging between 1,200 and 8,000) and to be relatively homogeneous with respect to population characteristics, economic status, and living conditions. Source: U.S. Bureau of the Census, www.census.gov/acs/www/data_documentation/custom_tabulation_request_form/geo_def.php.

[16] For most census tracts I use a zip-code level house price index and convert it to the tract level (weighted by population if there are several zip codes in the same tract). In the rare cases where this index is unavailable, I use a county-level index. Similarly, I replace the county debt to income ratio by the MSA average in a few cases when it is unavailable.

The information about whether states are judicial, i.e. require court permission for foreclosure, is gathered from Cutts and Merrill (2008). From this source, I also get an estimate on the number of days required to foreclose in each state. Figure 1.1 displays this information geographically, and Figure 1.2 complements this information by showing the location of the cross-border MSAs in my sample.

Other state laws might influence the provision of risky mortgages. To this end, I gather data on availability of recourse to the other assets of borrowers, laws affecting ease of refinancing, redemption laws, and a bank branching regulation index from Rice and Strahan (2010). I also gather data on the tax rates faced by financial companies, as well as the combined stock market capitalization of firms headquartered in the state divided by state GDP. See the Appendix for the sources and construction of those variables.

Locations of bank branches are collected from the FDIC Summary of Deposits. I match banks in this database to lenders in HMDA using Federal Reserve identification numbers, FDIC certificate numbers, or OCC / OTS docket numbers depending on the regulatory agency the lender reports to.

Due to limitations in the availability of some of the controls, the sample period is set to years 2001 through 2012.

3.1 Summary statistics

Summary statistics are presented in Table 1.1, and Table 1.2 presents summary statistics grouped by the two types of foreclosure procedure. It is seen that approximately 24% of all loans are privately securitized in non-judicial states and 19% in judicial states; the difference is statistically significant at the 1% level. As expected, the foreclosure procedure also takes longer time on average in judicial states (182 vs. 71 days; significant at the

[17] To track changes in tract definitions from the year 1990 and 2000 tracts, I use census tract cross walk files from http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/13287 and www.census.gov/geo/maps-data/data/relationship.html, respectively.

1% level). Further, judicial states are more likely to give borrowers further protection in the form of a statutory redemption period (40% of judicial states and 9% of nonjudicial ones; significant at the 1% level). Finally, judicial states have a higher minority population share (30% vs. 20%; marginally significant). None of the other control variables for local economic conditions used below differs significantly between the two types of states; these include tract to MSA median income, unemployment rate, homeownership rate, and house price growth, and the county level average debt to income and unemployment rate.

A graphic illustration of the key difference in summary statistics is given in Figure 1.3. The figure shows that a lower share of mortgages is securitized in judicial states each year in the sample.

4 Empirical strategy and results

The statistical differences in securitization rates seen above could be due to other economic conditions that differ systematically between judicial and non-judicial states. To mitigate this concern I include MSA-year fixed effects, thereby identifying the effect from mortgages made in the same metropolitan area but under different laws. In addition, I include a set of controls for local economic conditions, as these vary within the MSA. Since the variable of interest only varies at the geographical level, I aggregate the average securitization rate to the tract-year level. The regression specification then becomes

$$Securitized_{i,m,s,t} = \delta Judicial_s + \beta' X_{i,m,s} + \mu_{m,t} + \epsilon_{i,m,s,t}$$

In this equation, i indicates the tract; while m, s and t are respectively the MSA, state and year of origination. X is a vector of controls, $\mu_{m,t}$ is the set of MSA-year fixed effects, and ϵ is the error term. *Securitized* is the percentage of loans included in non-agency securitizations and *Judicial* is a dummy set to 1 if the state of the borrower requires judicial foreclosure. Since the information on borrower credit risk in the data is very sparse, I instead control for economic condition measured at the finest geographic level possible. These controls include the tract median income divided by that of the MSA, homeownership rate, minority share, house price growth, unemployment rate and average debt to income ratio. I measure these variables as averages over the years 1997-2000, a time before the private mortgage securitization market had reached substantial size. The philosophy behind this is to minimize the risk that these become endogenous “bad controls” (in the sense of Angrist and Pischke, 2008). The regression is estimated using OLS,[18] and standard errors are clustered at the state level. All variables are winsorized at the 1% and 99% levels.

Table 1.3 shows the result. To show the effects of my identification strategy, I start by estimating the regression without MSA fixed effects and retain all MSAs. The results suggest that tracts in states with judicial requirements have a statistically significant 2.2 percentage points lower securitization rate. I then seek to remove unobservable differences in economic conditions by restricting the sample to MSAs that cross state borders. As a first step, column 2 shows that running the same regression on this sample gives a similar result; the estimate for the judicial requirement increases in absolute magnitude to -2.6 ppt. Next, I put the identification strategy in action by including MSA-year fixed effects, thereby controlling for variation in unobservable economic conditions that are constant at the MSA-year level. As seen in column 3, the estimate of the judicial coefficient increases further in absolute magnitude to -2.9 ppt and remains significant at the 1% level. These results indicate that judicial requirements indeed inhibit securitization, and that failing to adjust for unobservable differences between the state types will understate the difference. Throughout the rest of the paper, I restrict to the MSAs that cross state borders and retain the MSA-year fixed effects.

4.1 Discontinuous shifts at state borders?

Using the fine geographic information in the data, I can more formally test the notion that the securitization probability changes discontinuously at the

[18] I use OLS even though the outcome variable is bounded between 0 and 100 since nonlinear models like Tobit can be sensitive to distributional assumptions and are not guaranteed to be consistent in the presence of fixed effects (e.g. Angrist and Pischke, 2008, p. 101-107). In unreported robustness tests, I however verify that the main results are similar (in terms of marginal effects) when using a Tobit specification.

border between judicial and non-judicial states. Following Mian, Sufi, and Trebbi (2014), I allow the securitization rate to vary flexibly with location and test for a jump at state borders by introducing the distance to the state border in the regression. Let *Dist* measure the distance from the tract to the border, with the value set to the negative of the distance for judicial state. To allow for potential nonlinearities, I also include the square and cube of this variable:

$$Securitized_{i,m,s,t} = \delta Judicial_s + \Sigma_{k=1}^{3}\beta_k Dist_i^k + \beta' X_{i,m,s,t} + \mu_{m,t} + \epsilon_{i,s,m,t}$$

The results are shown in column 4 of Table 1.3. It can be seen that the estimate for the judicial status is not substantially affected by the inclusion of the distance variables, suggesting that the effect is indeed due to a discontinuous "jump" at state borders.

4.2 How does the effect of judicial rules vary over time?

To test that the result is not driven only by the financial crisis, I split the sample period in two parts, where the first part ends in year 2006. As seen in columns 1-2 of Table 1.4, the effect of judicial rules on securitization is negative and significant at the 1% level in both the early and the late sample, while it is larger in magnitude in the early part. Next, I verify that the result is not driven by any particular year by running the baseline regression separately each year. Figure 1.4 shows the key outcome of this exercise, namely the coefficient estimates for the Judicial dummy together with their 95% confidence bands. The coefficient estimate is significantly negative for all years except 2004 and 2010, when it is only marginally significant. It is also relatively stable at around -3 ppt, although it declines in magnitude during the crisis years 2007-2008.

4.3 Testing the robustness to alternative samples and measures

To ensure the robustness of the main result, I first restrict the sample to those MSAs that contain at least one judicial and one non-judicial state. While these are the ones that identify the judicial coefficient, I retain other

MSAs in the remaining regressions since I will also be interested in other state laws. Column 3 of Table 1.4 shows the result. The estimated coefficient of -2.87 ppt is again similar to the baseline and remains significant at the 1% level.[19]

Next, I take into account the fact that the judicial dummy is not a perfect proxy for the time and cost incurred in foreclosure proceedings. Although foreclosures are on average costlier and more time consuming in judicial states, some states require judicial foreclosures but have quick and relatively creditor-friendly procedures (typically north-eastern states; see Ghent, 2014), while in other states a judicial process is optional but has benefits such as retaining the right to recourse against the borrower (Ghent and Kudlyak, 2011). Hence, I replace the judicial dummy by an estimate of the time required to foreclose from Cutts and Merrill (2008). As with the judicial variable, this variable does not vary over time. For comparability with the results using the judicial dummy, I use a dummy for the time required being above or below the median across states. Column 4 of Table 1.4 shows that the estimated effect is similar to the judicial dummy: securitization is on average 2.3 ppt less likely in states with long foreclosure times, statistically significant at the 5% level.[20]

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[19] This sample restriction reduces the number of clusters to 26, which may be too low to rely on the asymptotic theory for clustered standard errors (cf. e.g. Angrist and Pischke, 2008). I therefore test the robustness of the results to using the "wild" bootstrap procedure of Cameron, Gelbach, and Miller (2008), as well as the bias reduction modification for standard errors developed in Bell and McCaffrey (2002). The *p*-value remains below 1% for both these methods (using Judson Caskey's *cgmwildboot* command with 3,000 replications, and Joshua Angrist's *brl* command, respectively).

[20] In untabulated robustness test, I also replace the measure of judicial status with the classifications from Gerardi, Lambie-Hanson, and Willen (2013) and from Rao et al. (2009). These measures classify some states differently, but the results are qualitatively similar when using either measure. There is some discussion in the literature about the proper classification of Massachusetts (see Gerardi, Lambie-Hanson, and Willen, 2013; and Mian, Sufi, and Trebbi, 2014). All the schemes used in this paper classify Massachusetts as a non-judicial state; however changing the classification to judicial or removing Massachusetts from the sample leaves the results qualitatively unchanged (not tabulated).

4.4 Are judicial rules correlated with other policies and outcomes?

The results above suggest that securitization rates change at the borders between judicial and non-judicial states. However, there are many other state laws and policies that change discontinuously at state borders as well. If these laws are systematically related to judicial rules and affect securitization, the identification strategy would be invalidated. I therefore test how judicial laws are correlated with other policies across states and the robustness to including these laws as controls in the main regression.[21]

I include several other dimensions of state mortgage laws. First, I include a dummy for whether the state allows recourse to the unsecured assets of defaulted borrowers. Ghent and Kudlyak (2011) argue that such rules affect borrowers' propensity to default strategically; hence they may also have an impact on the willingness to renegotiate for both lenders and borrowers. Next, I include two measures of mortgage laws affecting prepayment risk. The first measure is an index of the strength of prepayment penalty regulation, gathered from White et al. (2010). The second measure is a dummy for whether the state allow the lien order to be maintained in refinancing, rather than assigning priority by seniority. This information is collected from Bond et al. (2012), who show that such laws affect the prepayment risk by making it easier to refinance first lien mortgages when there are second liens. Finally, I include a dummy measuring whether the state has a statutory redemption period that gives further potential benefits to the borrower in default, while imposing a cost on the lender.

I also consider broader state laws and conditions that might be correlated with mortgage securitization. Han, Park, and Pennacchi (2013) suggest that bank taxes influence securitization; hence, I include tax rates. Another potential concern is that non-judicial states may simply have more developed financial systems, and hence see higher securitization rates. To address this concern, I first collect the bank branching restrictiveness index

[21] For these additional controls as for the basic control variables, I use the averages over the years 1997-2000 (except for the prepayment penalty index, where it would be infeasible since only one state had such regulation in 2000).

from Rice and Strahan (2010), and take the states' averages over the same years. In addition, I include the ratio between the stock market capitalization of firms headquartered in the state to state GDP as an alternative measure of financial development.

As seen in Table 1.2, only two of the other policies considered are significantly correlated with judicial requirements, namely the complementary borrower protection measures of statutory redemption (more common in judicial states) and prepayment penalty restrictions (index on average lower in judicial states, but marginally significant difference). While it is impossible to rule out any other laws that may be driving the results, the lack of significant differences in those key laws and policies indicates that my findings are unlikely to be spurious.

Furthermore, I add the measures introduced above as controls in the baseline regression. In Table 1.5, I first add the variables one by one, and then include all of them as additional controls. The coefficient of interest for the Judicial dummy changes relatively little from the inclusion of these controls; it ranges between -2.86 and -3.28 ppt (recall that the baseline estimate is -2.90). Moreover, none of the newly introduced variables have a statistically significant effect on the outcome variable, and the coefficient of interest is not significantly different from the baseline estimate.[22] The largest absolute change in the *Judicial* coefficient comes from including the Redemption dummy; the inclusion of this variable makes the coefficients harder to interpret since judicial rules and redemption periods are both conceptually related and statistically correlated. I focus on judicial rules in the main analysis since redemption rules are generally believed to be less important (e.g. Pence, 2006).

4.5 Further evidence on the channel: loan level tests

In this section, I use the more granular information in the loan level data to further understand the channel behind the results. First, I verify that my

[22] In untabulated additional robustness checks, I find that results are qualitatively similar if I use lagged values rather than averages from years 1997-2000 for the time-varying additional controls.

results are not driven by the government sponsored enterprises (GSEs). Next, to rule out unobserved differences between states or between lenders driving the results, I include fixed effects for the state of incorporation of the lender, or for the lender. I also present a placebo test for a sample of loans where judicial rules do not apply, further supporting the notion that unobserved heterogeneity is unlikely to drive the results. Further, I use loan characteristics to understand how default risk interacts with judicial rules on securitization. Finally, I test how judicial requirements interact with access to soft information.

4.5.1 Ruling out alternative explanations based on GSEs

The results above suggest that a lower share of loans are securitized through the private market in judicial states, consistent with a market reaction to failures in renegotiating defaulted securitized mortgages. There is however an alternative explanation. The government sponsored enterprises (GSEs) did not charge differential premiums to mortgages from judicial states during the sample period.[23] It is possible that this caused adverse selection, where lenders found it more profitable to securitize loans from judicial states through these entities. In the baseline analysis, I grouped loans sold to the GSEs with retained loans, since the renegotiation failure was observed only in in the private securitization market by e.g. Agarwal et al. (2011b). The results above may then be driven by the profitability of selling to the GSEs, and not by renegotiation frictions in the private market.

I conduct two types of tests for ensuring that this is alternative explanation is not driving the results. The results are presented in Table 1.6.

In Panel A, I run the baseline model but restrict the sample to the "jumbo market", i.e. mortgages with a notional amount higher than the upper limit for what the GSEs may buy.[24] The effect of judicial rules remains

[23] Cordell et al. (2013) discuss the recent policy changes where the GSEs raised their premiums for some high-cost judicial states.

[24] Conformable loan limits are obtained from www.fhfa.gov/Default.aspx?Page=185. I use the limits that were applicable and known in the beginning of the respective year (starting in 2008, there were changes that became effective retroactively or during a calendar year).

negative and statistically significant. For reference, I re-run the baseline level on the loan-level data in column 2.[25] It appears that if anything, judicial rules have a stronger effect in the jumbo market than in the full sample. The estimate for the judicial dummy when restricting the sample to the jumbo market in column 1 is -4.4 ppt, while in the full sample in column 2 it is -3.3 ppt. Comparing these two estimates should be done with caution since the samples may differ in many ways, but an explanation centered on the activities of the GSEs could not explain the significantly negative effect of judicial rules in the jumbo market where they cannot operate.

In Panel B, I control parametrically for the alternative choice of securitization using multinomial logit and probit models. In these models, securitization through the private market and selling to the GSEs is modeled as competing risks, relative to the baseline of retention. In columns 1 and 2 I estimate a multinomial logit model. The effect of the judicial status on the probability of private market securitization remains significant in this model. Coefficients are not directly comparable to the baseline due to the nonlinearity of the model. For the private securitization outcome, the judicial dummy has a negative effect, significant at the 1% level. The mean predicted probability of securitization in the private market decreases by around 3.6 ppt in judicial states, which is of similar magnitude to the baseline linear model. In contrast, the coefficient for the GSE sale outcome is positive but insignificant. Results are relatively similar between the model with MSA-year fixed effects in column 1 and the one with MSA and year fixed effects in column 2.

Column 3 shows the results from instead using a multinomial probit model; the results are again similar (mean predicted securitization probability decreases by 3.5 ppt in judicial states). This model makes less restrictive

[25] This specification is equivalent to the baseline model run at the tract level but with different weights. Since the regressions are run with standard OLS, the baseline model gives equal weight to all tract-years, while the current model gives equal weight to all loans. Since tracts are designed not to have too much variation in the number of inhabitants, it is not surprising that the coefficient estimate does not change much. Alternatively, I can aggregate the average securitization probability at the tract level using only jumbo mortgages and run the same regression as before. Results are similar (not tabulated).

assumptions,[26] but is on the other hand computationally demanding. For this reason, it was only feasible to estimate it with MSA and year (not MSA-year) fixed effects.

4.5.2 Removing unobserved heterogeneity between states and lenders

The rich information in the loan level data allows me to address unobserved heterogeneity that might otherwise lead to spurious findings. First, judicial states might differ from others in important ways that shape lender behavior. In addition to controlling for other state laws as in Section 4.4, part of the geographic heterogeneity can be stripped out by noting that foreclosure law depends on the state where the property is located, while other potentially important factors are determined by the state where the lender is incorporated, such as the characteristics of bank regulators and of institutions more generally. To remove the influence from such factors, I include fixed effects for the lender state of incorporation×year into the regression. As seen in column 1 of Table 1.7, this does not have a major impact on the coefficient of interest; it even increases slightly in magnitude to -3.6 ppt (from -2.9 ppt) and remains statistically significant at the 1% level.

Next, I remove all heterogeneity between lenders by including fixed effects for the lender-year. Note that the baseline effect I identify could be due to a combination of two channels. First, judicial states could attract lenders with different securitization policies, e.g. fewer lenders with a business model of securitizing all mortgages. Alternatively, judicial requirements could affect the behavior of the same lender when operating in different states. While both channels are consistent with a causal interpretation, the lender-year fixed effects remove the first channel. Column 2 of Table 1.7 shows that the inclusion of these fixed effects changes the magnitude of the

[26] The multinomial logit model relies on an assumption of independence of irrelevant alternatives (IIA), which may be overly restrictive (e.g. Wooldridge, 2010, p. 648). In the current setting, this is unlikely to present a major concern as there is both institutional and econometric evidence on segmentation between loans intended for sale to the GSEs on the one hand compared to loans kept in portfolio or intended for sale to private securitization on the other (see Keys, Seru, and Vig, 2012 and references therein).

estimate for the judicial coefficient to -0.65 ppt, but again it remains significant at the 1% level.[27]

The relatively large change in the magnitude of the coefficient when including lender-year fixed effects may make us suspicious that unobserved heterogeneity between borrowers served by different lenders is driving my results, and that uncovering even more heterogeneity would weaken the results further. Including controls for observable loan risk can help us gauge whether this is likely to be a problem. To this end, I control for the loan to income ratio in column 3, and a dummy for lack of income documentation in column 4. The inclusion of these controls is potentially problematic since they are endogenous; however as seen in the table their inclusion has no major effect on the coefficient for the judicial status. Moreover, the fact that controlling for variation in observable borrower risk does not have a major impact on the coefficient of interest suggests that within-lender variation in unobservable borrower characteristics is unlikely to drive the results.

The change in the magnitude of the coefficient of interest from including lender-year fixed effects also suggests that my results are largely driven by between-lender variation. To test this conjecture more explicitly, I form two subsamples based on how focused the lender is on states with the same foreclosure procedure. Specifically, I form a Herfindahl-Hirschman index where the "market shares" are the percentages of loans in a lender-year that are from judicial and non-judicial states, respectively. I then split the sample such that there are equally many loans in the sample of high- and low-concentration banks each year.[28] Columns 5-6 of Table 1.7 show the results. For the subsample of lenders that are relatively concentrated to one type of states, the effect of judicial requirements is -6.3 ppt, which is larger in magnitude than the baseline and significant at the 1% level. Conversely,

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[27] I treat lenders belonging to the same bank holding company as one; results are however qualitatively unaffected if I don't do this (not tabulated). The holding company information in HMDA is sparse; when it is missing I complement it with information from structure reports from the Federal Reserve, located at www.ffiec.gov/nicpubweb/nicweb/nichome.aspx.

[28] The subsamples may not be of exactly equal size since all loans from lenders with the same concentration index are put into the same subsample.

for the sample of lenders that are relatively dispersed across state types, the coefficient for judicial foreclosure requirements is -1.6 ppt, which is smaller in magnitude than the baseline estimate but still significant at the 1% level. Together with the results from the specification with lender-year fixed effects, these results indicate that although the baseline result is to a large extent driven by between-lender variation, the reaction to judicial requirements is present also within lenders that operate in both state types.

4.5.3 Placebo test: manufactured homes

To further verify that the differences in securitization rates are not driven by other factors than judicial rules, I seek to run the same regression on a category of loans where these rules do not apply. One such category is loans to manufactured homes, which are factory-constructed homes that are generally low-cost and cater to low-income tenants. Such homes are usually not classified as real property, and hence collateral repossession of such loans usually does not fall under state foreclosure laws.[29] Manufactured loans can be identified in my data starting in the year 2004. Although the data does not indicate whether the underlying property is titled as personal or real property, the Consumer Financial Protection Bureau (2014) reports that most manufactured homes are titled as personal rather than real property, and that the vast majority of loans are given using only the building and not the underlying land as collateral. To the extent that many of these loans in my data are titled as real property, this will only bias the placebo test towards finding an effect where there should be none.

I implement the placebo test by re-running the baseline regression on the sample of loans made to manufactured homes. As seen in Column 7 of Table 1.7, the coefficient estimate for the *Judicial* dummy is insignificantly positive and close to 0 (0.001; around 4% of the mean for this sample). Moreover, standard errors are rather tight: at the 1% level, we can reject the effect being larger than 10% of the standard deviation in this sample.

[29] Manufactured homes can be converted to real property by affixing them permanently to land owned by the home owner. States differ in how they allow this conversion to be made; see e.g. Consumer Financial Protection Bureau (2014).

4.5.4 Are judicial rules more important for riskier loans?

Simple economic intuition suggests that if differences in expected loss given default are driving the results like I suggest, these differences should be larger when the expected default rate is higher. In the extreme when there is no default risk, there should be no impact of foreclosure rules. In this section, I test this prediction using two measures of loans riskiness: lack of income documentation, and the presence of local branches through which the bank may collect information about borrowers.

4.5.4.1 Judicial rules and risky loans lacking income documentation

One key variable in assessing the riskiness of a loan is the borrower income, but lenders are not obliged to collect such data. Previous research has documented that mortgage loans with missing documentation of income are riskier than others (Agarwal, Chang, and Yavas, 2012; Jiang, Nelson, and Vytlacil, 2014; and Kolb and Sherlund, 2010).

The higher risk of loans without income documentation was prevalent especially during the peak of the subprime boom. Mayer, Pence, and Sherlund (2009) suggest that at this time loans with no or reduced documentation might have increasingly become a tool to avoid reporting low incomes. In earlier years, such loans were predominantly offered to borrowers with volatile or hard-to-verify income, such as the self-employed. For this reason, I also conduct tests where the sample is restricted to start in 2004. This time restriction also allows me to restrict the sample to first lien loans on regular single-family houses, where measurement of the no-income status is arguably sharper.[30]

[30] These exclusions are not possible to make for earlier years since the variables identifying such loans are not available then. The reason for excluding multifamily homes is that income should not be reported for those according to the HMDA manual. Loans to manufactured homes are excluded because state foreclosure laws are typically not applicable to them, as described above. The exclusion of second and lower liens is standard in the literature and serves to make the loans more comparable.

Based on the evidence of higher riskiness, I test whether judicial rules are more important for loans lacking income documentation.[31] This investigation also helps me separate my explanation for the effect of judicial rules on securitization from an alternative story where the effect is driven by the GSEs. If the latter was true, one would expect to see a smaller effect of judicial rules on loans without income information, as the GSEs are highly restricted in purchasing such loans (e.g. Keys, Seru, and Vig, 2012). Compared to my preferred interpretation, this explanation therefore gives the opposite prediction for the differential reaction to judicial rules between loans with and without income documentation.

A simple way of testing this is to expand the original equation by the *NoIncome* dummy and its interaction with the judicial dummy. The regression is then

$$\begin{aligned} Securitized_{j,m,s,t} &= \delta_1 Judicial_s + \delta_2 NoIncome_j + \delta_3 Judicial_s \\ &\times NoIncome_j + \beta' X_{i,m,s} + \gamma_{m,t} + \epsilon_{j,m,s,t} \end{aligned}$$

… where the subscripts *i*,*j*, *m*,*s*, and *t* index tracts, loans, MSAs, states and years, respectively.

Since risky loans lacking income documentation was mainly a feature of the subprime boom, I first restrict the sample to years 2001-2007 and restrict to the sample of single-family first-lien loans, where measurement of the *NoIncome* dummy is arguably sharper. Nevertheless, below I also show that the results go through for less restrictive samples.

Table 1.8 shows the results of this regression. It is seen that the effect of judicial requirements is indeed amplified for loans lacking income information. Column 1 shows that the effect of judicial requirements is significantly higher for loans lacking income documentation – the additional 6.8 ppt reduction in the probability of securitization is also sizable compared to

[31] Lack of income documentation is not generally due to errors in the data construction; the instruction from the regulator for filling out the HMDA form states that the income field should be empty "when an institution does not ask for the applicant's income or rely on it in the credit decision" (see www.ffiec.gov/hmda/glossary.htm). Income may also be missing for other reasons in special cases, such as to protect privacy.

the -3.2 ppt effect of judicial requirements on other loans. In column 2, I replace the MSA-year fixed effects by tract-year fixed effects. This deletes much of the variation in economic conditions, as these are likely to be very similar for borrowers in the same tract. Since the judicial dummy only varies at the state level it is swept away in this specification, but the interaction coefficient of interest remains. The coefficient estimate and its standard errors are remarkably similar to the previous specification. One interpretation of this result is that the effect from lack of income documentation is due to the idiosyncratically higher risk of such borrowers, rather than local economic conditions that make borrowers and lenders agree to such loans. In column 3, I also verify that the results are not due to unobserved heterogeneity at the lender level by adding lender-year fixed effects. The interaction between judicial rules and lack of income documentation decreases in magnitude to -2.7 ppt but remains statistically significant.

Columns 4-6 show the corresponding specifications for the sample of single-family first-lien loans in the years 2004-2012, and columns 7-9 for the full sample. For the former sample results are similar to the sample that is restricted to the pre-crisis years but slightly smaller in magnitude (the interaction term is estimated to -6 ppt in the baseline specification). For the full sample, results are weaker, consistent with a higher measurement error. The interaction term is consistently negative, but only marginally significant in the full sample.

Admittedly, lenders choose whether to document income for their loans, and there is therefore a potential endogeneity problem with the results above. However, endogeneity problems apply more directly to the simple *NoIncome* term, while it is harder to see how they would bias the interaction term. For this to be a problem, it would need to be the case that unobservable factors that affect the securitization probability differently between loans with and without income documentation would work differently in judicial and non-judicial states.[32] I find it hard to come up with a story for why this would be.

[32] The consistency of an interaction term when one of the component terms is endogenous is treated more formally in econometric literature. Bun and Harrison (2014) derive the consistency of the coefficient for the interaction term under the assumption that the "degree of endogeneity" does not depend on the exogenous component of the interac-

An alternative and partly complementary explanation for the results could be that judges in judicial states are more likely to dismiss foreclosure cases involving no-documentation loans, on the grounds that they constitute predatory lending. In practice however, seeking to prevent foreclosures using predatory lending rules has proved ineffective.[33] Hence, it appears unrealistic that market participants would have expected predatory lending laws to significantly impede foreclosures, whether or not they were triggered by no-income loans.

4.5.4.2 Judicial rules and soft information – the role of bank branches

When banks operate with a geographic focus they may be able to collect soft information about borrowers (e.g. Agarwal et al., 2011a; Cortés, 2015; Ergungor 2010; Ergungor and Moulton, 2011; and Loutskina and Strahan, 2011). Banks lacking local information may then be subject to an adverse selection problem in the primary lending market. In line with this theory, Ergungor and Moulton (2011) document that banks have higher default rates when they lend in markets where they lack a branch. On the other hand, Loutskina and Strahan (2011) suggest that locally focused banks use their comparative advantage by lending to borrowers who look riskier based on public information but for which they may have positive private information. This information however gives the lenders a liquidity problem in the securitization market, since they cannot be compensated for their benign soft information. They may even face an adverse selection

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tion (i.e. the conditional correlation between the endogenous term and the error term conditioned on the exogenous term equals the unconditional correlation) together with higher-order independence assumptions that they argue are likely to be unrestrictive in typical applications. Nizalova and Murtazashvili (2014) derive similar results, but under stronger assumptions.

[33] See e.g. Lehe (2010). Moreover, the possibility to appeal to courts for preventing foreclosures on predatory loans exists in non-judicial states as well, making it ambiguous what to expect from this theory for the interaction between judicial rules and lack of income documentation.

problem there, as investors may fear that the loans they sell are those for which they possess negative private information.[34]

It is therefore not obvious what implications private information has for the securitization decisions. The cost of the adverse selection problems faced both by uninformed lenders in the primary market and by informed lenders in the secondary market are likely to be higher for loans in judicial states, as the loss given default is higher. It is therefore an empirical question which effect dominates, i.e. if locally informed lenders are more or less sensitive to judicial rules.

To test which way the relationship goes, I use a dummy for whether the bank has a branch in the county of the borrower, based on the premise that soft information is hard to communicate over long distances.[35] In these tests, banks that belong to the same bank holding company are treated as one. I then include this variable and its interaction with the judicial dummy in the same regression as before:

$$Securitized_{j,m,s,t} = \delta_1 Judicial_s + \delta_2 NoBranch_{i,j} + \delta_3 Judicial_s \times NoBranch_{i,j} + \beta' X_{i,s,t} + \gamma_{m,t} + \epsilon_{j,m,s,t}$$

The results are shown in Table 1.9. In column 1, the interaction between *NoBranch* and *Judicial* is negative but insignificant.

Since banks with and without branches in a given county may differ in many important ways, a possibly sharper test of the theory is to use within-lender variation. To this end, I add bank-year fixed effects to the model. Identification is now obtained by comparing the decisions of the same lender in judicial and non-judicial states, as well as in counties where it has or does not have a branch. Column 2 shows the result. The coefficient for the interaction term is now estimated to be -1.6 ppt, significant at the 5% level and sizable in relation to the mean securitization rate of 22%. In column 3, I add controls for observable loan risk, measured as an indicator for missing income documentation and loan to income ratio. The coefficients

[34] This trade-off is analyzed theoretically in a more formal fashion by Frankel and Jin (2014).

[35] The same measure has been used in e.g. Cortés (2015), and Ergungor (2010).

for the variables of interest are barely affected, indicating that the difference in securitization rate is not driven by differences in observable risk taken by lenders with local branches.[36] Finally, I seek to verify that the results are not driven by differences between counties that have different number of bank branches. To this end, I remove all heterogeneity between tracts (and hence between counties) by replacing the MSA-year fixed effects by tract-year fixed effects. As seen in column 4 this does not have a major impact on the interaction coefficient of interest, which goes to -1.5 ppt.

Consistent with Loutskina and Strahan (2011), the simple coefficient for *NoBranch* is significantly positive throughout the specifications, indicating that banks are more likely to securitize mortgages from remote areas.

A potential concern with these results is that the role of soft information is likely to be limited for many standard loans, where underwriting is heavily automated. Loutskina and Strahan (2011) suggest that it is more likely to play a role among jumbo mortgages, due to their heterogeneity and the inability of selling them to the GSEs. Hence, I repeat the analysis while restricting the sample further to only include jumbo loans. As seen in column 5-8 of Table 1.9, the results are qualitatively similar to the main sample, both in terms of magnitude and statistical significance.

In untabulated robustness tests, I take into account the possible residual correlation structure within banks by clustering the standard errors at the bank level; the results are qualitatively similar.[37]

Together, these results suggest that banks become more sensitive to judicial rules when they are able to gather soft information through local proximity, but that this channel operates mainly within rather than between banks.

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[36] In unreported regressions, I find that lenders make observationally riskier loans in the areas where they have branches, using the risk measures above. This result is broadly consistent with those in Loutskina and Strahan (2011), who argue that lenders use the comparative advantage from their local information by lending to customers with worse observable characteristics.

[37] The same holds if I use double clustering by state and bank (not tabulated).

4.6 How do judicial rules affect loan supply?

The results so far suggest that judicial rule influence the decision to securitize loans. Considering the evidence presented elsewhere that securitization fuelled the subprime boom (e.g. Mian and Sufi, 2009; Nadauld and Sherlund, 2013), I examine the impact of these rules on supply.

To examine whether these rules caused an expansion in total supply or just a substitution towards securitized loans from other loan sources, I vary the left hand side of the regression. In column 1 of Table 1.10, I first test the effect of these laws on the log number of securitized loans in a tract-year.[38] In addition, I control for log population to ensure that this is not driving differences. Unexpectedly given the main results, judicial laws have a significantly negative effect. The coefficient indicates that judicial states face a 0.15 log decrease in the number of loans, or in other words an approximate 14% decrease. Next, I replace the outcome variable by the log number of loans not securitized, again adding 1 before taking the log. Column 2 shows a statistically significant 0.06 log increase in judicial states. In column 3, I replace the outcome variable by the log number of all loans, securitized or not. The estimate for the judicial dummy is then positive but insignificant. Together, these results indicate that during the sample period, judicial laws primarily caused a substitution towards securitized credit from other sources, with no discernible effect on the total amount of credit. This result is consistent with the finding in Mian, Sufi, and Trebbi (2014) that judicial rules do not impact aggregate loan supply. It is on the other hand somewhat surprising given that if securitization reduces the cost of capital as commonly believed, the credit supply ought to be higher in non-judicial states. One way to interpret the lack of a significant difference is that credit was generally unconstrained during my sample period, and hence the margin of adjustment may have shifted to loan riskiness rather than denial of application (cf. Chomsisengphet and Pennington-Cross, 2006). In addition, we may lack power for studying loan supply given that the government sponsored enterprises constitute such an important share of all loans, and do not take foreclosure laws into account in their loan decisions. A poten-

[38] To ensure the value is always defined, I add 1 to the number before taking logs.

tially more powerful test is therefore to focus on loan segments where the GSEs cannot operate, such as the jumbo market.[39]

In columns 4-6, I use the log of the total loan amounts rather than the number of loans, and split in the same way. The results tell the same story: judicial laws decreased the total value of securitized loans, while increasing the value of other loans and having no clear effect on the total loan supply.[40]

4.6.1 Judicial rules and loan characteristics

While the previous section shows a lack of effect on aggregate loan supply, this section examines if there are any effect on loan risk characteristics. Column 7 of Table 1.10 shows that such rules significantly reduce the share of loans granted without income documentation. The effect is also economically substantial; the estimate of -0.44 ppt is approximately 13% of the mean.[41] Column 8 shows the effect on the average loan to income ratio of those mortgages that had income documentation.[42] Correspondingly, this ratio drops significantly in judicial states. The estimated effect of -10.5 ppt

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[39] Consistent with this conjecture, Dagher and Sun (2014) document an increase in loan rejections around the jumbo cutoff in judicial states without a corresponding decrease in the total number of loan applications, which they interpret as a decline in supply but not in demand. In contrast, in unreported tests I do not find a decrease in the number or volume of jumbo loans given for judicial states.

[40] In unreported robustness tests, I also verify that controls are qualitatively unaffected by using the log working age population rather than log total population as control.

[41] In unreported tests, I verify that the effects are similar if I restrict the sample over which I take the share of loans lacking income documentation to first lien loans on regular single-family houses; as I argue in Section 4.5.4, the measurement of the no-income status is arguably sharper for these loans.

[42] In principle, I could use the loan level data and run a Heckman-type selection regression where the first stage is the selection of loans into income documentation status and the second stage is loan to income ratio given selection. However, given lack of a strong instrument for the income documentation status, such a regression would be identified solely from the joint normality assumption (e.g. Angrist and Pischke, 2008, p. 100). Rather than relying on strong distributional assumptions, I prefer the simpler option of analyzing the decisions separately.

is significant at the 5% level and economically meaningful in relation to the sample mean of 234%.

4.7 Are effects due to self-selection?

The identification strategy would be violated if households move across state borders in a fashion that is related to differences in the variable of interest. Households that relocate may be different on dimensions that are relevant to loan outcomes, and such differences may vary systematically with the foreclosure laws in the states they move between. In that case, I could not attribute the findings to causal effects of the foreclosure laws. It could for instance be that distressed households systematically move to non-judicial states, which might have a higher supply of mortgages to risky borrowers since the recovery value is higher. If these risky mortgages are securitized in the subprime market, the securitization would not be due to a causal effect of foreclosure laws.

I test if such problems appear to be present in the data by studying migration and population growth patterns. First, I test whether states exhibit differential population growth rates based on their judicial status. I therefore aggregate the population at the MSA-state-year level, calculate the population growth, and run a regression similar to the main regression:

$$Popgrowth_{m,s,t} = \delta Judicial_s + \mu_{m,t} + \epsilon_{i,s,m,t}$$

Columns 1 and 2 of Table 1.11 show the results. It can be seen that there are no significantly different population growth patterns during my sample period, whether I study simple or log growth rates. Judicial states have slightly lower population growth rate (0.2 ppt) but the difference is insignificant and standard errors are rather tight at 0.29 ppt.

Next, I seek to understand whether the households that do move are different on a key characteristic, namely income. To this end, I use a data set on county-to-county migration data from the IRS.[43] Unfortunately, this data set ends in 2004. The data provides proxies for the number of households that moved, the number of persons, and their aggregate adjusted

[43] www.irs.gov/uac/SOI-Tax-Stats-Migration-Data

gross income. I retain migrations between counties that are in the same MSA but in different states. I then again aggregate to the MSA-state-year level, and run the same regression as before but replacing the dependent variable by the log of the average income. The results are shown in column 3 of Table 1.11. It is seen that migrants to judicial states are not systematically different on this dimension. Migrants into judicial states are slightly wealthier (log average income increases by $0.13-0.14) but the difference is insignificant.

Together, these tests show no differences in migration rates or the average income of migrants between the two types. This complements the lack of differences in migration patterns between the two types of states documented by Mian, Sufi, and Trebbi (2014) using other data sources.

Another way to test if self-selection is driving the result is to examine whether the estimates in the baseline regression differ based on the migration in the MSA. To this end, I measure the migration rate as the percentage of the county population that moved between states but within the same MSA, using the same data source as before. I then average this number over the years for which there is data, and include it in the regression together with its interaction with the Judicial dummy. If self-selection is driving the result, one would expect a larger effect in counties with high migration rates, i.e. a negative interaction term. As seen in the first column of Table 1.12, this is not the case in the data. The interaction between the judicial dummy and a dummy for higher than median migration rate is instead positive, but statistically insignificant. Next, I build on the premise that self-selection is less likely for those households that reside far from the border. To this end, I form a dummy measuring whether the distance from the tract to the state border is longer than the median. As column 2 shows, both the simple term and the interaction with the judicial dummy are insignificant.[44]

A problem in creating a clean test for whether self-selection is driving the results is that MSAs that are more integrated are on the one hand more subject to self-selection, but on the other hand the different parts of such

[44] In unreported robustness tests, I verify that results are qualitatively similar if instead of the dummies I use the nominal values.

an area are cleaner control groups for each other. The tests above can therefore also be seen as tests for whether the baseline results are driven by areas that are less likely to be integrated and hence potentially less similar. The lack of significant findings indicates that this is not the case.

5 Conclusion

This paper documents that mortgage lenders adjust to the ex ante cost of renegotiation frictions in securitization. Recent literature concludes that troubled mortgages are hard to renegotiate if they have been privately securitized, and are hence more likely to go into foreclosure. Based on this finding, I hypothesize that fewer mortgages will be securitized when foreclosure is more costly and time consuming, and the expected loss from failure to renegotiate is therefore higher. I test this prediction using cross-state variation in foreclosure laws, where so-called judicial states make foreclosure more costly by requiring a court process. Comparing mortgages made on different sides of the border in cross-border metropolitan areas, I document that judicial states have lower securitization rates. The difference of approximately 3 percentage points is both statistically significant and economically meaningful, corresponding to 13% of the mean. Consistent with the proposed mechanism, I show that the effect of judicial rules is stronger for loans with higher default risk.

These results are consistent with lenders internalizing the securitization-induced renegotiation failure; moreover, they are the opposite of what is predicted by theories of lenders fooling ignorant or overoptimistic investors. On the particular dimension considered in this paper, the data therefore speak against the popular belief that during the boom, loans were securitized indiscriminately and investors did not know what they were buying.

Earlier literature has suggested that securitization expanded the supply of risky mortgages. Together with my finding that judicial rules affect securitization decisions, this suggests that such rules may also affect mortgage supply. Consistent with this prediction, I show that mortgages in judicial states are less likely to lack income documentation and have lower average loan to income ratios. Easier collateral repossession however likely enables the provision of riskier loans even absent securitization. Disentangling these channels is an interesting avenue for furthering our understanding of how securitization affects loan supply. In contrast, I find no significant effect on aggregate loan volumes; rather, securitized credit seems to substitute credit from other sources. This finding is slightly puzzling given that securitization is widely believed to have expanded the loan supply during the boom, for better or worse. A possible explanation is that during this period, lenders responded to variation in the cost of capital and foreclosure costs by for instance raising interest rates rather than rejecting applications. Investigating this issue in more detail is left for future research.[45]

Drawing welfare conclusions from these results is challenging. Although economists would generally believe that people are better off ex ante if they get loans they have applied for, the extremely loose credit conditions during the boom enabled loans that made borrowers worse off ex post and perhaps even ex ante. In addition, the higher foreclosure propensity in securitization may have negative externalities that separate the social from the privately optimal securitization rate (cf. Campbell, Giglio, and Pathak, 2011; and Mian, Sufi, and Trebbi, 2014). The lack of a significant effect on aggregate loan supply suggests that judicial states may not have lost much from lower securitization rates during the period in question. One possibility is that during the sample period, the margin of adjustment was not in mortgages being denied but rather the riskiness of the mortgages being accepted. Hence, negative effects on total credit supply may be more pronounced in other circumstances. In particular, the aggregate effect of judicial rules is arguably diminished by the government sponsored enterprises, who currently buy a large share of loans and do not take these rules into account.

Policy makers both in the U.S. and internationally are currently seeking ways to revitalize the mortgage securitization markets, due to the perceived benefits of diversification and lower costs of capital. The results in this paper suggest that the interaction between renegotiation rigidities and foreclo-

[45] Mian, Sufi, and Trebbi (2014) find no differences in interest rates during the years 2002-2005. Note however that their regressions do not control for loan risk; hence it is possible that a riskier pool of borrowers in non-judicial states offsets the lower expected costs of foreclosing and potentially lower cost of capital, such that there is no aggregate effect on interest rates.

sure rules should be considered in this discussion. While proposals that give creditors less power in foreclosure are appealing from the short-run perspective of keeping families in their homes,[46] they might have negative consequences from the perspective of impeding the recovery of securitization. Still, policy makers are mindful to avoid a return to the excesses seen before the crisis. My results also show that these excesses were less pronounced for the judicial states where foreclosure is costlier, as evidenced by fewer loans made without income documentation. Theoretically, the optimal policy action may be to address the underlying factors that cause the renegotiation failures in securitization rather than its consequences. Such efforts are also underway in the U.S. under the Consumer Financial Protection Bureau's mortgage servicing standards. Earlier attempts at working directly with servicers' incentives have however been rather ineffective in the context of the Home Affordable Modification Program (Agarwal et al., 2013). Unless more successful ways at promoting renegotiation can be found, the results in this paper suggest that foreclosure laws can be a powerful way to influence securitization, no matter what one believes the optimal securitization rate to be.

[46] For example, several U.S. states have introduced forced mediation programs, and Mian and Sufi (2014) have proposed the more radical path of "cramdown" where a judge is given power to write down the notional value of a mortgage. The even more radical step of a foreclosure moratorium has been tried in Greece, and several U.S. state and local governments have unsuccessfully tried to implement such programs (Collins, Pearcy, and Urban, 2013).

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Tables and figures

Appendix. Variable definitions (continued on next page)

Variable	Description	Source
Bank branch regulation index	An index for restrictiveness of bank branching regulation, where 0 is least and 4 is most restrictive	Rice and Strahan (2010)
Corporate tax rates	The effective corporate income tax rate for the highest income bracket, calculated as in Han, Park, and Pennacchi (2013); note that these are rates that apply to banks, which are sometimes different than those for other companies.	Book of the States, 1996-'97, '98-'99, and 2000-'01 editions; available from the Council of State Governments
Debt / Income ratio	The county debt to income ratio, calculated as the average total debt, divided by the average personal income. In a few cases, this measure is not available at the county level; I then use the MSA average instead.	Debt balance: New York Fed Consumer Credit Panel. Personal income: Bureau of Economic Analysis.
Distance	The distance (in kilometers) from the tract to the state border.	U.S. Bureau of Census Tiger shapefiles; distance calculated using the Stata module *geodist*
Easy subrogation dummy	A dummy that takes the value 1 for states where courts have adopted the principle of equitable subrogation, i.e. that refinancing mortgages inherit the seniority of the mortgages they replace, rather than strict time seniority.	Bond et al. (2012)
Homeownership rate	The share of owner-occupied to all housing units in the census tract.	U.S. Bureau of Census/ Federal Financial Institutions Examination Center (FFIEC)
Judicial foreclosure	Dummy that takes the value 1 for states requiring judicial foreclosure.	Cutts and Merrill (2008)
Loan to income ratio	The loan amount divided by the gross annual borrower income.	HMDA
Log house price growth	Log growth in Zillow's All Homes Index. Converted from zip-code to tract level; when the zip-code level index is unavailable I use a county-level index, or in rare cases an MSA- or state-level index (in that preference order).	Zillow.com; conversion from zip code to tract using tools provided by the Missouri Census and HUD

Appendix 1.1. Variable definitions (continued)

Variable	Description	Source
Manufactured dummy	A dummy that takes the value 1 for loans made to manufactured homes; available only starting in year 2004	HMDA
Minority percent	The percent of the tract population not classified as white.	U.S. Bureau of Census / FFIEC
No Income	A dummy taking the value 1 for loans that lack income on applicant income.	HMDA
No Branch	A dummy taking the value 1 for loans made by lender without a branch in the county, where lenders under the same bank holding company are treated as one.	HMDA, FDIC Summary of Deposits
Prepayment penalty index	An index of prepayment penalty regulation, ranging from 0 (loosest) to 4 (strictest).	White et al. (2010)
Recourse dummy	A dummy measuring whether the state permits recourse ("deficiency judgements") against the unsecured assets of defaulted borrowers	Ghent and Kudlyak (2011)
Redemption dummy	A dummy measuring whether a statutory right of redemption is required in the state	Cutts and Merrill (2008)
Stock market cap to GDP	The ratio of the total stock market capitalization of all firms headquartered in the states available in the Compustat-CRSP merged database to the state GDP. To get a firm's state of incorporation, I use the historical information in CRSP's *hstate* variable when available, and otherwise the Compustat variable *state*, which is backfilled by Compustat when firms change state.	CRSP/ Compustat merged database (stock market capitalization), Bureau of Economic Analysis (state GDP)
Securitized	Dummy that takes the value 1 for loans securitized in the private non-agency market; classified as the "purchaser" column in HMDA being "private securitization" or "other"	HMDA
Tract/MSA median income	The ratio of median family income in the tract to that in the MSA.	U.S. Bureau of Census / FFIEC
Unemployment rate	The county unemployment rate.	U.S. Bureau of Labor Statistics
Long foreclosure time dummy	A dummy for the state being above the median in the estimated number of days required days from foreclosures referral to sale (excluding the post-sale redemption period).	Cutts and Merrill (2008)

Table 1.1. Summary statistics (continued on next page)

This table presents summary statistics of the variables used in the analysis. The sample is restricted to tracts within an MSA over the years 2001-2012; Panel B restricts to MSAs crossing state borders. Variables with names followed by constant do not vary over time within the respective geographic unit. The variables with the suffix 97-00 are averages over years 1997-2000. See the Appendix for variable definitions.

	Panel A. Full sample						Panel B. Cross border MSA sample					
	N	mean	sd	p10	p50	p90	N	mean	sd	p10	p50	p90
Loan-level variables												
Bank branch dummy	32,315,688	0.39	0.49	0	0	1	8,581,739	0.39	0.49	0	0	1
Jumbo dummy	32,315,688	0.09	0.28	0	0	0	8,581,739	0.1	0.31	0	0	1
Manufactured dummy	23,537,596	0.02	0.14	0	0	0	5,919,948	0.01	0.09	0	0	0
No Income dummy	32,315,688	0.04	0.19	0	0	0	8,581,739	0.04	0.19	0	0	0
Loan to income (%)	31,161,816	223.8	122.4	56.82	220.9	384.5	8,256,034	234.7	122.0	62.71	233.3	392.1
Securitized dummy	32,315,688	0.23	0.42	0	0	1	8,581,739	0.22	0.41	0	0	1
Tract level variables												
Homeownership rate$_{97\text{-}00}$	66,650	67.16	20.95	36.29	71.77	90.33	17,771	69.38	23.08		75.59	94.39
Log house price growth$_{97\text{-}00}$	66,650	0.05	0.03	0	0.05	0.09	17,771	0.05	0.03	0.01	0.05	0.09
Minority share$_{97\text{-}00}$	66,650	24.64	28.73	1.53	11.82	77.76	17,771	25.94	31.67	1.59	10.2	90.65
Tract/MSA median income$_{97\text{-}00}$ (%)	66,650	98.96	36.86	52.29	97.48	144	17,771	99.28	38.82	50.19	97.37	147.0
Population (thousand)$_{97\text{-}00}$	66,650	4.83	2.92	1.84	4.22	8.51	17,771	4.45	2.72	1.59	3.94	7.98
N. of loans	637,805	47.87	59.91	4	28	112	177,076	47.13	55.99	4	29	109
N. of securitized loans	637,805	10.8	17.7	0.0	4	29	177,076	10.02	15.86	0	4	27
Total amt. of loans (M$)	637,805	9.19	13.62	0.41	4.05	23.84	177,076	10.16	13.42	0.53	5.29	25.47
Total amt. of securitized loans (M$)	637,805	1.98	3.74	0.0	0.55	5.49	177,076	2.02	3.42	0	0.71	5.50

Table 1.1. Summary statistics (continued)

	Panel A. Full sample						Panel B. Cross border MSA sample					
	N	mean	sd	p10	p50	p90	N	mean	sd	p10	p50	p90
County level vars.												
Debt to income ratio (%)$_{97\text{-}00}$	1,788	62.36	19.22	40.14	59.28	90.39	339	65.89	20.48	41.14	62.84	98.31
Unempl. rate$_{97\text{-}00}$	1,788	4.74	2.23	2.47	4.23	7.67	339	4.39	1.96	2.4	3.93	7.03
State level variables												
Estimated days to foreclose (constant)	51	119.7	76.56	38	120	260	40	120.5	83.02	38	116	267.5
Judicial foreclosure dummy (constant)	51	0.43	0.5	0	0	1	40	0.45	0.50	0	0	1
Recourse dummy (constant)	51	0.8	0.4	0	1	1	40	0.85	0.36	0	1	1
Corporate tax rate$_{97\text{-}00}$ (%)	51	38.2	2.49	35	38.9	41.11	40	37.9	2.49	35	38.58	40.97
Branch regulation index$_{97\text{-}00}$	51	2.36	1.37	0	3	4	40	2.28	1.42	0	3	4
Stock market cap to GDP$_{97\text{-}00}$	51	0.96	0.95	0.08	0.56	2.21	40	1.02	0.91	0.13	0.72	2.32
Easy subrogation dummy (constant)	51	0.17	0.39	0	0	1	40	0.13	0.23	0	0	1
Prepayment penalty index	618	0.72	1.26	0	0	3	478	0.79	1.34	0	0	3
Redemption dummy (constant)	51	0.25	0.44	0	0	1	40	0.25	0.44	0	0	1

Table 1.2. Summary statistics, split by state foreclosure law (continued on next page)

This table compares the variables used in the analysis across judicial and non-judicial states for areas within an MSA that crosses state borders. The test of differences by the two types of states in the last two columns is adjusted for heteroskedasticity and clustering at the state level. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. See the Appendix for variable definitions.

	Non-judicial states			Judicial states			Test diff.
	N	mean	sd	N	mean	sd	in means
Loan-level variables							
Bank branch dummy	3,899,544	0.39	0.49	4,682,195	0.39	0.49	
Jumbo dummy	3,899,544	0.10	0.30	4,682,195	0.11	0.31	
Loan / income (%)	3,756,398	237.26	127.48	4,499,636	232.6	117.15	
Manufactured dummy	2,729,778	0.01	0.10	3,190,170	0.01	0.08	
NoIncome dummy	3,899,544	0.04	0.19	4,682,195	0.04	0.19	
Securitized dummy	3,899,544	0.24	0.43	4,682,195	0.19	0.40	***
Tract level variables							
Homeownership rate$_{97\text{-}00}$	6,844	69.68	21.73	10,927	69.2	23.89	
Log house price growth$_{97\text{-}00}$	6,844	0.05	0.04	10,927	0.05	0.03	
Minority share$_{97\text{-}00}$	6,844	20.14	26.67	10,927	29.58	33.93	*
Tract/MSA median income$_{97\text{-}00}$	6,844	97.8	36.11	10,927	100.2	40.4	
Population (thousand) $_{97\text{-}00}$	6,844	4.77	2.8	10,927	4.25	2.65	**
Number of loans	65,600	57.03	65.26	111,476	41.31	48.8	***
Number of securitized loans	65,600	13.45	20.0	111,476	8.01	12.41	***
Total amount of loans (M$)	65,600	11.63	14.89	111,476	9.3	12.39	*
Total amt. of securitized loans (M$)	65,600	2.60	4.26	111,476	1.68	2.75	***
County level variables							
Debt to income ratio$_{97\text{-}00}$	182	67.82	22.78	157	60.85	17.42	
Unempl. rate$_{97\text{-}00}$	182	4.34	2.08	157	4.45	1.81	

Table 1.2. Summary statistics, split by state foreclosure law (continued)

	Non-judicial states			Judicial states			Test diff.
	N	mean	sd	N	mean	sd	in means
State level variables							
Estimated days to foreclose (constant)	22	70.55	42.93	18	181.5	79.79	***
Recourse dummy (constant)	22	0.86	0.35	18	0.83	0.38	
Corporate tax rate (%)$_{97\text{-}00}$	22	38.42	2.5	18	37.27	2.4	
Branch regulation index$_{97\text{-}00}$	22	2.07	1.5	18	2.54	1.3	
Stock market cap to GDP$_{97\text{-}00}$	22	1.06	0.89	18	0.96	0.95	
Easy subrogation dummy (constant)	22	0.14	0.35	18	0.11	0.32	
Prepayment penalty index	263	1.06	1.51	215	0.46	1.00	*
Redemption dummy (constant)	22	0.09	0.29	18	0.44	0.51	**

Table 1.3. Foreclosure laws and the probability of securitization

This table shows how the percentage of loans privately securitized in a tract-year depends on the state requiring a judicial foreclosure procedure. Distance is the distance of the tract to the state border, multiplied by -1 for judicial states. All variables are explained in the Appendix. Controls are measured as averages over the years 1997-2000. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

	Dependent variable: % of loans securitized			
Sample:	All MSAs	Multistate MSAs		
	(1)	(2)	(3)	(4)
Judicial foreclosure dummy	-2.24***	-2.63***	-2.90***	-3.04***
	(0.64)	(0.80)	(0.74)	(0.95)
Debt to income ratio (%)	0.13***	0.087***	0.081**	0.076**
	(0.016)	(0.025)	(0.035)	(0.029)
Tract/MSA median income (%)	-0.0064	0.0038	-0.00042	0.0012
	(0.0054)	(0.0070)	(0.0059)	(0.0066)
County unemployment rate (%)	0.061	-0.47***	-0.35***	-0.31***
	(0.25)	(0.17)	(0.11)	(0.093)
Log house price growth	-13.7*	-30.2***	-13.9	-15.4
	(6.85)	(9.67)	(10.1)	(9.81)
Homeownership rate (%)	0.0063	-0.011	-0.0095	-0.011
	(0.0075)	(0.011)	(0.012)	(0.013)
Minority share (%)	0.055***	0.079***	0.068***	0.070***
	(0.011)	(0.0090)	(0.011)	(0.013)
Distance to state border (1000 km)				-15.3
				(9.82)
Distance to state border squared				-0.011
				(0.013)
Distance to state border cubed				0.070***
				(0.013)
Fixed effects	Year	Year	MSA×year	MSA×year
No. of states	51	40	40	40
No. of obs.	637,805	177,076	177,076	177,076
R^2	0.32	0.32	0.39	0.39

Table 1.4. Foreclosure laws and securitization: alternative samples and measures

The table shows how judicial foreclosure requirements and foreclosure timelines affect securitization. The outcome variable is the percent of loans in a tract-year privately securitized. Column 1 restricts the same to MSAs spanning at least one judicial and one non-judicial state. In column 2 the variable of interest is a dummy for the estimated time required to foreclose being longer than the median; in other columns the variable of interest is the judicial foreclosure dummy. Controls are measured as averages over the years 1997-2000. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

	Dep. var: % of loans securitized			
Sample:	Years 2001-2006	Years 2007-2012	MSAs with variation in judicial rules	Full
	(1)	(2)	(3)	(4)
Judicial foreclosure dummy	-3.72***	-2.05***	-2.87***	
	(0.82)	(0.75)	(0.64)	
Long foreclosure time dummy				-2.28**
				(1.09)
Debt to income ratio (%)	0.13**	0.031*	0.058*	0.076**
	(0.061)	(0.017)	(0.034)	(0.037)
Tract/MSA median income (%)	0.0021	(0.0034	0.0051	-0.00049
	(0.0087)	(0.0076)	(0.015)	(0.0059)
Unemployment rate(%)	-0.30**	-0.35***	0.0053	-0.31**
	(0.15)	(0.10)	(0.32)	(0.12)
Log house price growth	-16.9	-7.76	16.2	-13.3
	(15.9)	(5.98)	(9.92)	(9.92)
Homeownership rate (%)	-0.022	0.0063	-0.00081	-0.0092
	(0.02)	(0.0082)	(0.020)	(0.012)
Minority share (%)	0.12***	0.00096	0.041***	0.068***
	(0.014)	(0.012)	(0.011)	(0.011)
Fixed effects	MSA×year	MSA×year	MSA×year	MSA×year
No. of states	40	40	26	40
No. of obs.	88,681	88,395	47,837	177,076
R^2	0.21	0.16	0.32	0.39

Table 1.5. Controlling for other state laws and outcomes

This table tests the robustness of the main results to controlling for other state policies and outcomes that might be correlated with securitization. *Corporate tax rate*, *Bank branch regulation index*, and *Stock mkt cap to GDP* are measured as averages over the years 1997-2000, while the dummies for judicial, recourse, subrogation, and redemption did not vary over the sample period. All variables are defined in the Appendix. Local economic controls include county debt to income ratio and unemployment; and tract log house price growth, homeownership rate, minority share and median income divided by MSA median income. These are measured as averages over the years 1997-2000; see the Appendix for details. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

	Dependent variable: % of loans securitized							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judicial foreclosure dummy	-2.91***	-2.92***	-2.90***	-2.86***	-3.11***	-2.92***	-3.22***	-3.28***
	(0.74)	(0.71)	(0.75)	(0.80)	(0.79)	(0.73)	(0.85)	(1.02)
Recourse dummy	-0.58							-0.55
	(0.41)							(0.88)
Corporate tax rate		-0.065						-0.013
		(0.11)						(0.18)
Bank branch regulation index			-0.11					0.029
			(0.22)					(0.27)
Stock mkt cap to GDP				0.44				0.31
				(0.47)				(0.52)
Prepayment penalty index					-0.41**			0.55
					(0.19)			(0.95)
Easy subrogation dummy						0.54		-0.28
						(0.72)		(0.32)
Redemption dummy							0.57	0.42
							(0.64)	(0.76)
Local econ. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA×year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of states	40	40	40	40	40	40	40	40
No. of obs.	177,076	177,076	177,076	177,076	177,076	177,076	177,076	177,076
R^2	0.39	0.39	0.39	0.39	0.39	0.39	0.39	0.39

Table 1.6. Are the GSEs driving the effect?

This table shows how the probability of loan securitization depends on the foreclosure laws of the state while seeking to remove or control for the actions of the Government Sponsored Enterprises (GSEs).

Panel A shows the result from running the baseline OLS regression only on jumbo mortgages, which the GSEs are precluded from buying. The number of states decreases since there were no such loans in one state.

Panel B shows the results from running multinomial models where the choice of securitizing via the private and the GSE markets are competing outcomes relative to the baseline of retention.

Local economic controls include county debt to income ratio and unemployment; and tract log house price growth, homeownership rate, minority share and median income divided by MSA median income, all measured as averages over the years 1997-2000; see the Appendix for details. Heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Panel A. Restrict to jumbo market

Dependent variable:	Securitization dummy	
Sample:	Jumbo market (1)	Full (2)
Judicial foreclosure dummy	-0.044**	-0.033***
	(0.018)	(0.0092)
Local economic controls	Yes	Yes
Fixed effects	MSA×year	MSA×year
No. of states	39	40
No. of obs.	898,101	8,581,739
R^2	0.065	0.047

Panel B. Parametrically controlling for the alternative outcome of GSE securitization

Model:	Multinomial logit		Multinom. probit
Outcome: private securitization	(1)	(2)	(3)
Judicial foreclosure dummy	-0.22***	-0.22***	-0.16***
	(0.068)	(0.067)	(0.051)
Mean marginal effect (%)	-3.60	-3.58	-3.54
Outcome: GSE sale			
Judicial foreclosure dummy	0.001	0.012	-0.002
	(0.032)	(0.032)	(0.027)
Mean marginal effect (%)	1.37	1.55	1.39
Local economic controls	Yes	Yes	Yes
Fixed effects	MSA×year	MSA,year	MSA,year
No. of states	40	40	40
No. of obs	8,581,739	8,581,739	8,581,739
Pseudo R^2	0.044	0.044	N/A

Table 1.7. Unobserved heterogeneity and placebo

This table removes possible sources of heterogeneity that might drive the results, and conducts a placebo test. Column 1 adds fixed effects for the state of the lender's headquarter×year, while col. 2 adds lender×year FE (where lenders under the same holding company are treated as one). Cols. 3 & 4 add controls for observable measures of borrower risk: respectively, loan to income ratio and a dummy for missing income documentation. Col. 5 (6), restricts the sample to lenders with a relatively high (low) concentration to state types. State type is here defined as judicial or non-judicial, concentration is defined as a Herfindahl-Hirschman index, and the split is made such that the number of loans in the two samples is (approximately) equal every year. Col. 7 restricts to loans to manufactured homes, for which judicial laws typically don't apply. The outcome variable is a dummy that takes the value 1 if the loan is privately securitized. Local econ. controls include county debt to income ratio and unemployment; and tract log house price growth, homeownership rate, minority share and median income divided by MSA median income. Controls are measured as averages over years 1997-2000; see the Appendix for closer details. OLS estimation; cols. 1-4 use Guimares' and Portugal's *reg2hdfe* command. Heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

	Dependent variable: securitization dummy						
Sample	Full				Concentrated lenders (to one state type)	Unconcentrated lenders	Manufactured home loans; years 2004-'12 (placebo)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Judicial dummy	-0.036***	-0.0065***	-0.0066***	-0.0065***	-0.063***	-0.016***	0.0011
	(0.0106)	(0.0022)	(0.0021)	(0.0022)	(0.022)	(0.0045)	(0.0053)
Loan/income ratio			0.0008				
			(.0009)				
NoIncome dummy				0.010*			
				(0.005)			
Local econ. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA×year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional FE	Lender HQ state×year	Lender ×year	Lender ×year	Lender ×year			
N. of states	40	40	40	40	40	40	40
N. of lenders	7,424	7,424	7,383	7,424	7,091	1,835	1,829
N. of obs	8,581,739	8,581,739	8,256,034	8,581,739	4,150,427	4,431,312	55,210
R^2	0.12	0.59	0.59	0.59	0.064	0.059	0.065

Table 1.8. The interaction between lack of income info and judicial requirements

In this table, I test if the effect of judicial status on loan securitization is different between loans lacking income documentation and other loans. The outcome variable is a dummy that takes the value 1 if the loan is privately securitized. Local economic controls include county debt to income ratio and unemployment; and tract log house price growth, homeownership rate, minority share and median income divided by MSA median income. These controls are measured as averages over the years 1997-2000; see the Appendix for closer details. In columns 2, 5 and 8, these controls are swept away by the tract-year fixed effects. OLS estimation; columns 3, 6 and 9 are estimated using Guimares' and Portugal's Stata command *reg2hdfe* for two high-dimensional fixed effects. Heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

	Dependent variable: securitization dummy								
Sample:	2004-'07, first lien single family			2004-'12, first lien single family			Full		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Judicial foreclosure dummy	-0.032***		-0.012**	-0.025**		-0.0045	-0.032***		-0.0060***
	(0.0093)		(0.0046)	(0.0096)		(0.0034)	(0.0093)		(0.0023)
Judicial foreclosure ×NoIncome dummy	-0.068***	-0.066***	-0.027***	-0.060***	-0.059***	-0.024***	-0.032*	-0.033**	-0.017**
	(0.022)	(0.019)	(0.0095)	(0.019)	(0.015)	(0.0075)	(0.018)	(0.014)	(0.0077)
NoIncome dummy	0.076***	0.068***	0.037***	0.060***	0.054***	0.030***	0.073***	0.067***	0.020***
	(0.0093)	(0.0082)	(0.0046)	(0.010)	(0.0077)	(0.0036)	(0.010)	(0.0087)	(0.0045)
Local econ. controls	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Fixed effects	MSA×year	Tract×year	MSA×year, lender×year	MSA×year	Tract×year	MSA×year, lender×year	MSA×year	Tract×year	MSA×year, lender×year
No. of states	40	40	40	40	40	40	40	40	40
N. of lenders	4,643	4,643	4,643	6,466	6,466	6,466	7,424	7,424	7,424
No. of obs.	3,817,397	3,817,397	3,817,397	5,308,557	5,308,557	5,308,557	8,581,739	8,581,739	8,581,739
R^2	0.024	0.064	0.57	0.052	0.098	0.62	0.048	0.090	0.59

Table 1.9. Judicial rules and local information – do bank branches matter?

This table tests if the effect of judicial rules differs when the bank has a branch in the county of the loan. *NoBranch* is a dummy measuring the absence of a branch (where banks under the same holding company are treated as one). The outcome variable is a dummy that takes the value 1 if the loan is securitized in the private non-agency market. Local economic controls include county debt to income ratio, tract to MSA median income, unemployment, log house price growth, homeownership rate, and minority share are measured as averages over the years 1997-2000; see the Appendix for further details. These controls are swept away in the regressions that include tract×year fixed effects. Loan risk controls include an indicator for missing income documentation and the loan to income ratio (set to 0 for loans lacking income documentation). OLS estimation; specifications with lender-year fixed effects are estimated using Guimares' and Portugal's Stata command *reg2hdfe* for two high-dimensional fixed effects. Heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Sample:	Full				Jumbo loans			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judicial foreclosure dummy	-0.012	0.0055	0.0056		-0.019	0.0097	0.0096	
	(0.010)	(0.0041)	(0.0040)		(0.015)	(0.0067)	(0.0068)	
Judicial ×NoBranch	-0.026	-0.016**	-0.016**	-0.015**	-0.035	-0.021***	-0.021***	-0.022***
	(0.016)	(0.0061)	(0.0061)	(0.0065)	(0.027)	(0.0065)	(0.0066)	(0.0073)
NoBranch	0.20***	0.019**	0.019**	0.019**	0.26***	0.025***	0.025***	0.026***
	(0.014)	(0.0075)	(0.0075)	(0.0078)	(0.013)	(0.0074)	(0.0074)	(0.0084)
Local econ. controls	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Loan risk controls	No	No	Yes	No	No	No	No	No
Lender-year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Local fixed effects	MSA×year	MSA× year	MSA×year	Tract×year	MSA×year	MSA×year	MSA×year	Tract×year
N. of states	40	40	40	40	39	39	39	39
N. of lenders	7,424	7,424	7,424	7,424	4,269	4,269	4,269	4,269
N. of obs	8,581,739	8,581,739	8,581,739	8,581,739	898,101	898,101	898,101	898,101
R^2	0.092	0.59	0.59	0.60	0.13	0.57	0.57	0.60

Table 1.10. The effect of judicial rules on loan supply

This table shows how laws requiring a judicial foreclosure procedure affect the loan supply. The dependent variables in columns 1-3 are the logs of the number of loans securitized in the private non-agency market, other loans, and all loans, respectively. In columns 4-6, the dependent variables are the logs of the total loan amounts securitized in the private non-agency market, of other loans, and of all loans, respectively. Columns 7 and 8 test the effect on of judicial rules on the share of loans lacking income documentation and average loan to income ratios, respectively. The unit of observation is a tract-year. The sample consists of loans originated in MSAs (i.e. cities) that cross state borders. All variables are explained in the Appendix. Local economic controls include county debt to income ratio and unemployment; and tract log house price growth, homeownership rate, minority share and median income divided by MSA median income, all measured as averages over the years 1997-2000. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Dependent var.:	ln (1+ #securitized loans)	ln (1+ #un-securitized loans)	ln (1+ #loans)	ln (1+ amount securitized loans)	ln (1+ amount un-securitized loans)	ln (1+ amount loans)	% loans w/o income doc-ument-tation	Loan to income ratio (%)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judicial foreclosure dummy	-0.15***	0.063**	0.030	-0.31***	0.057	0.017	-0.44***	-10.5**
	(0.054)	(0.030)	(0.031)	(0.11)	(0.046)	(0.047)	(0.11)	(4.01)
Log population	0.54***	0.66***	0.68***	1.07***	0.73***	0.72***		
	(0.064)	(0.049)	(0.052)	(0.075)	(0.047)	(0.048)		
Local econ. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	MSA×year	MSA×year	MSA×year	MSA×year	MSA×year	MSA×year	MSA×year	MSA×year
No. of obs.	177,076	177,076	177,076	177,076	177,076	177,076	177,076	177,076
No. of states	40	40	40	40	40	40	40	40
R^2	0.68	0.64	0.66	0.54	0.61	0.64	0.24	0.43

Table 1.11. Testing for self-selecting state migration

This table tests whether judicial and non-judicial states exhibit differential patterns in their population growth and the average income of migrants. Columns (1)-(4) display results for population growth. Columns (5)-(6) measure the logarithm of the average adjusted gross income for individuals that moved to another state in the same MSA during the year. The sample is restricted to years 2001-2004 in these regressions due to data availability. In both sets of regressions, the observations have been aggregated to the state-MSA-year level. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Dependent variable:	Population growth (%)	Log population growth×100	Log average income of in-migrants
Sample years	2001-2012	2001-2012	2001-2004
	(1)	(2)	(3)
Judicial foreclosure dummy	-0.20	-0.20	0.013
	(0.29)	(0.29)	(0.042)
Fixed effects	MSA×year	MSA×year	MSA×year
No. of obs.	977	977	527
No. of states	40	40	40
R^2	0.77	0.77	0.87

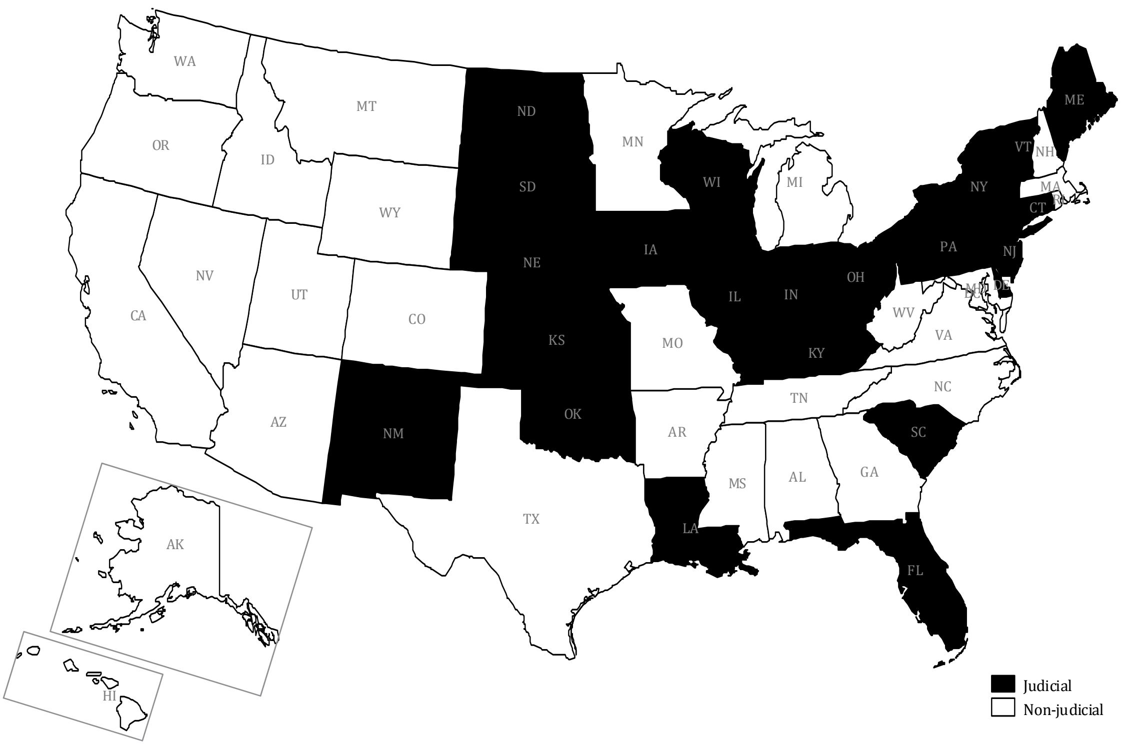
Table 1.12. Does the effect of judicial rules vary with migration rates?

This table shows how the percentage of loans in a tract being privately securitized depends on the state requiring a judicial foreclosure procedure. The sample consists of loans originated in MSAs (i.e. cities) that cross state borders. *High migration dummy* takes the value 1 for counties that are above the median in terms of the percentage of the county population that moved between states but within the city, measured as the average during years 2001-2004. *Long distance* is a dummy that measures whether the distance from the tract to the state border is longer than the median. For comparability, these variables are standardized. Local economic controls include county debt to income ratio and unemployment; and tract log house price growth, homeownership rate, minority share and median income divided by MSA median income, all measured as averages over the years 1997-2000; see the Appendix for details. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

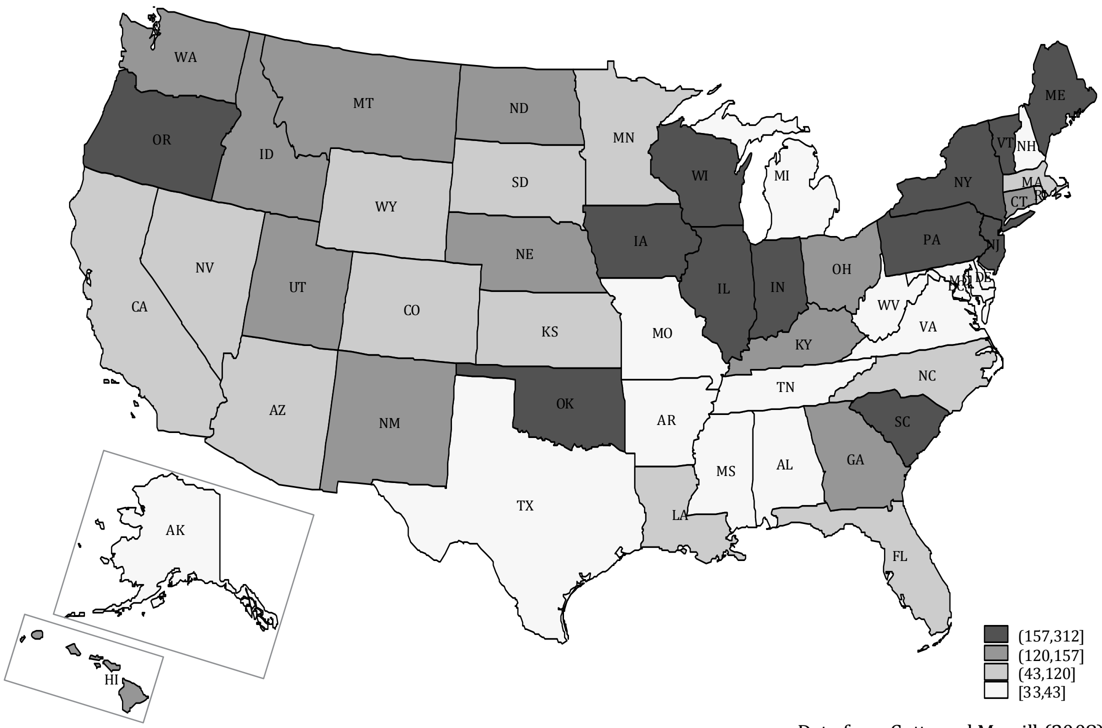
Dependent variable:	% of loans securitized	
	(1)	(2)
Judicial foreclosure dummy	-2.96***	-2.91***
	(1.06)	(0.75)
Judicial dummy× High migration dummy	0.020	
	(1.14)	
Judicial dummy × Long distance dummy		0.41
		(0.44)
High migration dummy	-0.29	
	(0.73)	
Long distance dummy		0.032
		(0.15)
Local economic controls	Yes	Yes
Fixed effects	MSA×year	MS× year
No. of obs.	177,076	177,076
No. of states	40	40
R^2	0.39	0.39

Figure 1.1. Geography of state foreclosure laws

Panel A. Judicial and non-judicial states

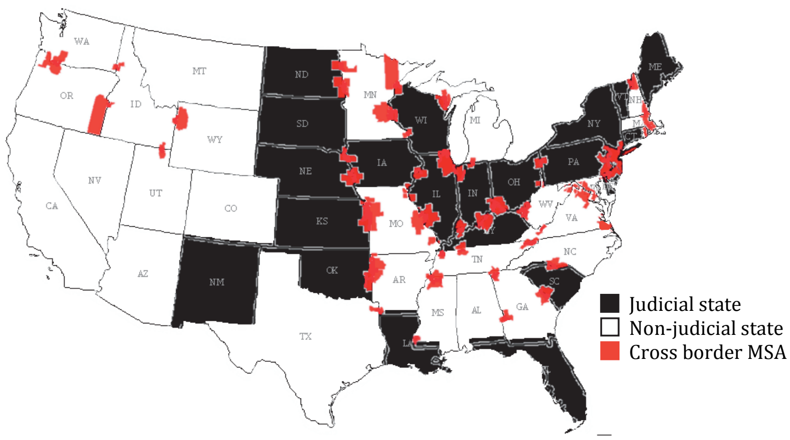


Panel B. Days required to foreclose



Data from Cutts and Merrill (2008)

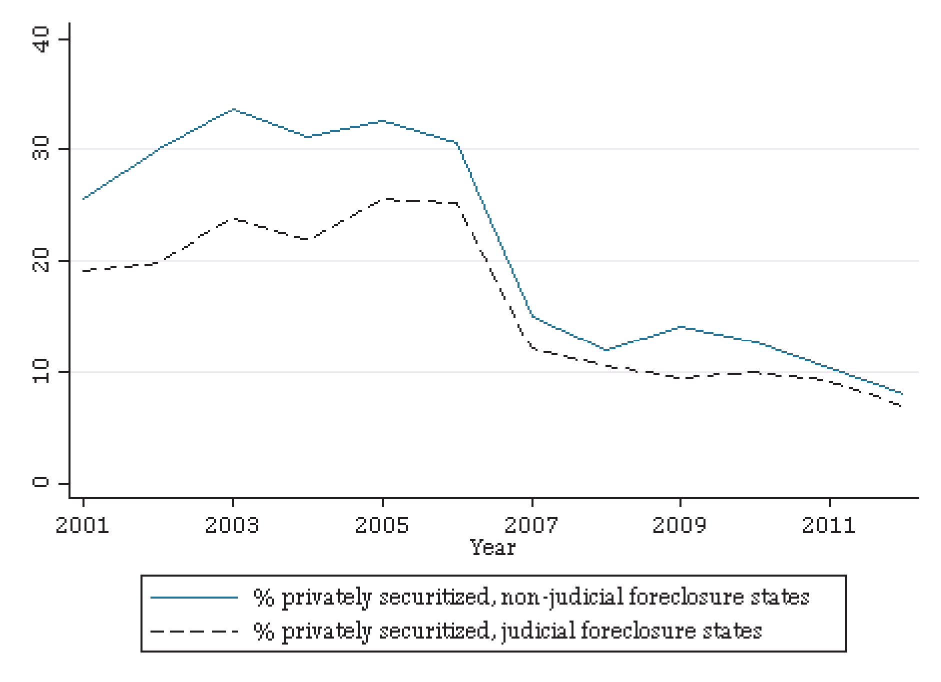
Figure 1.2. List and map of cross-border MSAs



Allentown-Bethlehem-Easton, PA-NJ
Augusta-Richmond County, GA-SC
Berlin, NH-VT
Bluefield, WV-VA
Boston-Cambridge-Quincy, MA-NH
Burlington, IA-IL
Cape Girardeau-Jackson, MO-IL
Charlotte-Gastonia-Rock Hill, NC-SC
Chattanooga, TN-GA
Chicago-Joliet-Naperville, IL-IN-WI
Cincinnati-Middletown, OH-KY-IN
Clarksville, TN-KY
Columbus, GA-AL
Cumberland, MD-WV
Davenport-Moline-Rock Island, IA-IL
Duluth, MN-WI
Eufaula, AL-GA
Evansville, IN-KY
Fargo, ND-MN
Fayetteville-Springdale-Rogers,AR-MO
Fort Madison-Keokuk, IA-MO
Fort Smith, AR-OK
Grand Forks, ND-MN
Hagerstown-Martinsburg, MD-WV
Huntington-Ashland, WV-KY-OH
Iron Mountain, MI-WI
Jackson, WY-ID
Kansas City, MO-KS
Kingsport-Bristol-Bristol, TN-VA
La Crosse, WI-MN
Lebanon, NH-VT
Lewiston, ID-WA
Logan, UT-ID
Louisville/Jefferson County, KY-IN
Marinette, WI-MI
Memphis, TN-MS-AR
Minneapolis-St. Paul-Blooming ton, MN-WI
Natchez, MS-LA
New York-Northern New Jersey-Long Island, NY-NJ-PA
Omaha-Council Bluffs, NE-IA
Ontario, OR-ID
Paducah, KY-IL
Parkersburg-Marietta-Vienna, WV-OH
Philadelphia-Camden-Wilmington,PA-NJ-DE-MD
Point Pleasant, WV-OH
Portland-Vancouver-Hillsboro, OR-WA
Providence-New Bedford-Fall River, RI-MA
Quincy, IL-MO
Sioux City, IA-NE-SD
South Bend-Mishawaka, IN-MI
St. Joseph, MO-KS
St. Louis, MO-IL
Steubenville-Weirton, OH-WV
Texarkana, TX-Texarkana, AR
Union City, TN-KY
Virginia Beach-Norfolk-Newport News, VA-NC
Wahpeton, ND-MN
Washington-Arlington-Alexandria, DC-VA-MD WV
Wheeling, WV-OH
Winchester, VA-WV
Youngstown-Warren-Boardman, OH-PA

Figure 1.3. Average securitization rates by state foreclosure laws

Panel A. Simple averages



Panel B. Demeaned by MSA-year

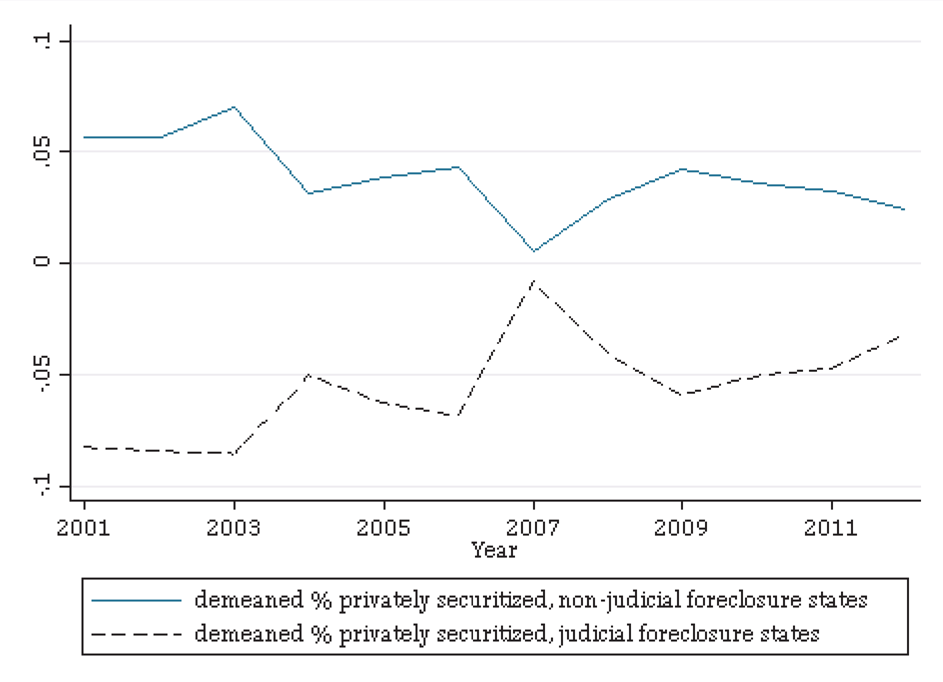
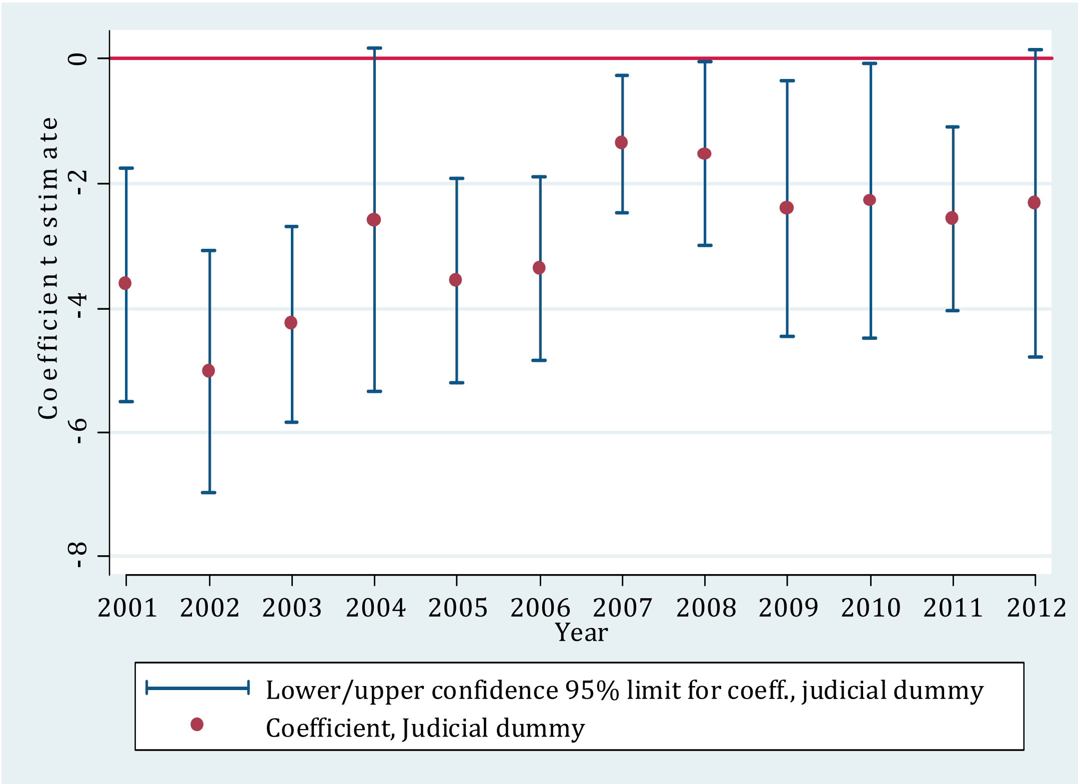


Figure 1.4. Regression estimate, by year

The figure shows 95% confidence bands for the coefficient for the Judicial dummy when running a regression of the percent of loans privately securitized in the tract-year on a dummy for judicial requirements and controls for local economic conditions. The regressions include MSA fixed effects, and standard errors are clustered at the state level.



Chapter 2

Do daughters make family firms more sustainable?*

Abstract: I construct a novel data set on the children to individual blockholders in publicly listed Swedish companies and study how they influence the firm. Conditioning on the total number of children to rely on random variation, I show that having more daughters makes the firm adopt more environment-friendly policies. This indicates that child-to-parent influence documented elsewhere matters also in large companies. To further understand the mechanisms behind this, I show that offspring gender has little if any effect on the appointments of women as directors or CEO, and that the adjustment in the gender composition of the board that I observe is due to adult daughters becoming directors.

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1 Introduction

Companies increasingly face calls to take responsibility towards its stakeholders and the natural environment in a trend often labeled Corporate Social Responsibility (CSR).[1] This paper seeks to understand one of the determinants of how much environmental responsibility companies take. In particular, I consider the influence of offspring gender in listed Swedish family firms.

It is often argued that women have more pro-social preferences than men, and that increasing their representation in companies would therefore make companies more socially responsible. There are also studies indicating that increased female representation among the company's directors and executives improves companies' social behavior (e.g. Galbreath, 2011; Kimball et al., 2012; Krüger, 2010; Marquis and Lee, 2013; Post et al., 2011; and Tam, Fu, and Chang, 2012). Due to the potentially endogenous nature of these institutions, establishing causality is however challenging. For instance, when companies are doing better for unrelated reasons, they may be more likely both to appoint female directors (Adams et al., 2009)[2] and to take social responsibility (Hong et al., 2013). I propose a new channel through which to investigate whether gender differences in preferences affects corporate decisions. Since offspring gender is plausibly random, the effects I document can be given a causal interpretation.

I study firms listed on the Stockholm Stock Exchange with a block holder who is a family or an individual. Sweden offers an ideal setting to study these issues. Family control of listed firms is common, high-quality

[1] While there is no generally accepted definition of this term, proponents of CSR generally demand that companies should take responsibility beyond their contractual obligations towards stakeholders other than the company's shareholders. A complicating aspect is that such initiatives may benefit the company's shareholders as well ("doing well by doing good"). For an overview of conceptual issues and empirical evidence on this trend, see e.g. Benabou and Tirole (2010), Kitzmueller and Shimshack (2012), and Lougee and Wallace (2008).

[2] Ryan and Haslam (2005) draw the opposite conclusion; it is therefore not obvious in which direction a possible reverse causality goes.

data is available, and the institutional framework is such that the findings likely extend to other advanced economies. I show that companies take greater environmental responsibility when the blockholder has more daughters, controlling for the number of children, where "children" will henceforth be taken to mean all direct descendants of the blockholder, irrespective of their age. This effect does not appear to work through the decision to appoint female directors or CEOs, since I find that offspring gender has little if any effect on these decisions.

These patterns are consistent with gender-based differences in preferences. Experimental studies have documented gender-based differences in risk-taking and altruistic behavior (see Croson and Gneezy, 2009). More specifically, women are generally more concerned about the environment than men (e.g. Funk and Gathmann, 2015; and Djerf-Pierre and Wängnerud, 2011, for Switzerland and Sweden, respectively). There is an ongoing debate about the extent to which these differences persist in the population of professional managers and entrepreneurs (e.g. Atkinson et al., 2003; Birley, 1987; Johnson and Powell, 1994; and Master and Meier, 1988). Evidence that women in top corporate positions are more oriented towards stakeholders and society in general is given in Adams and Funk (2012), who survey directors in listed Swedish companies. Experimental studies have also documented gender-based differences in long-term orientation with women generally being more patient (Frederick, 2005; and Silverman , 2003). This might matter in my setting since investments that reduce the firm's environmental impact often have long payback-time (Bloom et al., 2010). Consistent with more altruism and long-term orientation, there is evidence that companies undertake less workforce reductions when they are affected by quotas for female directors (Matsa and Miller, 2013) or women-owned (Matsa and Miller, 2014).

The economic literature has established that offspring gender may influence parents' preferences, even among sophisticated decision-makers. Having more daughters has been shown to lead to more liberal voting among US legislators by Washington (2008) and US judges by Glynn and Sen (2015). Dahl et al. (2012) also argue that having daughters make CEOs pay their employees more equally in the gender dimension, using Danish data. Bennedsen et al. (2007) use data from privately owned Danish companies, and use the gender of the first born child as an instrument for appointing a family CEO. Tsoutsoura (2014) uses the same variable as an instrument for the decision of Greek entrepreneurs to pass on their firm to their children rather than selling it to an external party. Finally, Bertrand et al. (2008) shows that business groups in Thailand do worse when the controlling family has more sons, which they interpret as a "race to the bottom" in tunneling resources from the firm among the sons who are typically more active in its management. Like the current paper, these papers use exogenous variation in offspring gender to study company policies. To the best of my knowledge, this is however the first paper to develop comprehensive data about the families controlling listed firms in a developed country. The benefit of this is that more data is available for such firms, for instance the environmental data that I focus on.

A complication in the interpretation of this paper is that the effects I document are likely to reflect the joint effect of several channels. First, children may influence the preferences of their parents as noted above. Second, offspring gender may influence the gender composition of the firm's board of directors and CEO, either directly through the adult children becoming directors, or indirectly by for instance daughters making their fathers more prone to promoting women. My results show that there is little effect on the overall gender composition of the directors and CEO. There is an increase in the share of female directors when the blockholder has more daughters, which is however insignificant for most specification. Moreover, it appears to be driven by daughters becoming directors, while partly crowding out other female potential directors. It might still be the case that daughters are more powerful on the board than other female directors, or that they have roles in decision-making bodies that the family sets up internally for managing its companies. Since little is known about the inner workings of boards (Adams et al., 2010) and families who govern family firms (Bennedsen et al., 2010), these possibilities are hard to investigate. Speaking partly against the hypothesis of more power on the board, I find that offspring gender composition does not significantly affect the decision to appoint a female chairman. Third, I cannot fully disentangle between a selection effect, where children affect which companies the parent enters or exits, and an influence effect where children affect their parents' actions in the companies they already control. This is an issue since Tsoutsoura (2014) shows that offspring gender has an impact on the exit decisions of Greek entrepreneurs. Many of the companies in my sample have however been held by the same family for several generations, implying that selection is unlikely to fully explain my results. Moreover, the environmental measure I use is industry adjusted, meaning that selection would have to work through selection of the more sustainable companies within an industry rather than through selection of companies in sectors with lower environmental impact.[3]

An additional contribution of this paper is to shed light on the sources of blockholder heterogeneity. It has been documented that blockholders display consistent preferences for corporate policies across the companies they invest in (Cella, 2014; Cronqvist and Fahlenbrach, 2009; Derrien et al., 2013; Kisin, 2011; McCahery et al., 2014), and that these differences have performance implications.[4] How these differences emerge is less well understood. I document that offspring gender is a determinant of revealed preferences for one category of blockholders, namely individuals and families. While it has an effect on policies that shape environmental performance, I find no impact on financial performance. This is perhaps to be expected since there is no clear prediction on financial performance from the gender-based differences in preferences documented before. Moreover, the literature has failed to find a consistent relation between financial performance and social or environmental performance (see e.g. the surveys in Kitzmueller and Shimshack, 2012; and Margolis et al., 2009). The lack of a clear effect also provides cautionary evidence against the claim often made in popular and practitioner publications that increasing female representation in the corporate world would improve perfor-

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[3] While in principle it would be possible to separate between some of these channels with panel data, getting meaningful time-series variation is difficult in my setting since the environmental score that is the variable of interest has only been publicly available since 2006.

[4] These papers all use US data. Ghachem (2008) argues that effects similar to those documented by Cronqvist and Fahlenbrach (2009) are present in Sweden as well.

mance.[5] Such moderating conclusions are also reached in recent academic literature (e.g. Adams and Ferreira, 2009; Ahern and Dittmar, 2012; and Faccio et al., 2014).

The impact from female influence on company financial and environmental performance is also of interest since there is an ongoing policy debate about how to increase women's participation in the management of large companies. Towards that end, several European countries have installed or are considering installing quotas for female directors, and there have been discussions about an EU-wide quota.[6] Extrapolating from my results to speculate on the possible impact of such increased representation is difficult. First, the effect I establish may work through several channels as noted before. Second, daughters of blockholders may not be representative of the wider population of women who might enter company decision making. The same can however be said about women who get board seats following quotas or manage to succeed in the current male-dominated environment (e.g. Hillman et al., 2002; and Singh et al., 2008), i.e. the individuals who have been studied elsewhere in the literature. With these caveats in mind, this paper can therefore be seen as complementary to other studies of increased female influence in the corporate world. The environment-related effects I document in this paper are also policy relevant since many business leaders report that sustainability is a prioritized target for them, but institutional barriers within their own

[5] See e.g. Catalyst, 2007, The Bottom Line: Corporate Performance and Women's Representation on Boards www.catalyst.org/system/files/The_Bottom_Line_Corporate_Performance_and_Womens_Representation_on_Boards.pdf , Credit Suisse, 2012, Gender diversity and corporate performance https://infocus.credit-suisse.com/data/_product_documents/_shop/360145/csri_gender_diversity_and_corporate_performance.pdf , and McKinsey, 2007, Women matter: gender diversity, a corporate performance driver, www.mckinsey.de/downloads/publikation/women_matter/Women_Matter_1_brochure.pdf.

[6] Quota legislation has been introduced in several European countries including France, Germany, and Italy, and have been discussed at the EU level by the European Commission (see e.g. Smith, 2014).

companies impede their efforts.[7] The results in this paper indicate that increasing the influence of women may help companies overcome such obstacles.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents data sources and defines the variables. Section 4 describes the empirical strategy and results, and Section 5 concludes.

2 Institutional setting

Sweden offers a promising environment for studying the issues I raise. Family control of listed companies is widespread, and owners make extensive use of super-voting shares and pyramids to leverage their power (e.g. Faccio, 2002; La Porta et al., 1999). However, insiders are unlikely to expropriate resources from the firm as investor protection is high and measured private benefits of control are relatively low in Sweden (Dyck and Zingales, 2004; La Porta et al., 1998; and Nenova, 2003).

The controlling ownership of listed Swedish companies has been relatively stable since the 1930's, and increasingly concentrated to a small group of families and bank-affiliated entities (Högfeldt, 2005). In my setting, this implies that blockholders have often inherited control, and are often blockholders in several of the companies in the sample.[8]

Like in other parts of the world, the issues of CSR and SRI (Socially Responsible Investment) have caught increasing attention in Sweden in recent years. At the end of 2006 about 12% of assets under management

[7] For instance, in The Economist Intelligence Unit's survey of executives, 57% believed that the benefits of sustainable practices outweigh the costs, but "more than one in four businesses report that a lack of clear responsibility for sustainability at the board level is a major impediment to progress." Source: The Economist Intelligence Unit, 2008, Doing good: business and the sustainability challenge, http://graphics.eiu.com/upload/Sustainability_allsponsors.pdf. Bloom et al. (2010) also describe frictions in companies' internal governance that may hinder them from making profitable environment-friendly investments.

[8] Högfeldt (2005) argues that stability of corporate ownership has been a favored political goal in Sweden for most of the 20th century, and achieved through different means.

in the Swedish mutual fund sector were using some form of social responsibility criteria; this can be compared to 11% in the US asset management sector in 2012.[9] Swedish companies are also considered relatively sophisticated in their CSR practices by e.g. Steurer et al. (2008). Also mirroring other countries, women are underrepresented in the top layers of business in Sweden. For instance, the average share of female directors in my sample is approximately 20 percent.[10] Together, these facts indicate that the findings in this paper are likely to extend to other countries as well.

3 Data and definitions

I start with all companies that had a primary or parallel listing at the Stockholm Stock Exchange in the beginning of 2009, and had a blockholder who is a family or an individual, as defined below. I then use the family structure of the blockholder in the beginning of 2009 to study board and CEO composition in May 2009 as well as performance in 2009 and subsequent years. Due to data availability, companies that delisted during 2009 are excluded. If the company delisted in 2010 or 2011, I use the average of the longest time period available.

[9] The figure from Sweden is from Folksam, 2006, Folksams Etikfondindex, available at www.folksam.se/resurser/pdf/Etikfondindex2006.pdf; the one for the US is from The Social Investment Forum, 2013, Sustainable and Responsible Investing Facts, http://ussif.org/resources/sriguide/srifacts.cfm.

[10] For the same time period, the EU average was 11% and the corresponding US figure was 12.1%. Sources: European Commission, 2012, Largest quoted companies, http://ec.europa.eu/justice/gender-equality/files/database/037_en.xls (EU) and GMI Ratings, 2012, GMI Ratings' 2012 Women on Boards Survey, http://library.constantcontact.com/download/get/file/1102561686275-86/GMIRatings_WOB_032012.pdf (US). Note that these figures are not directly comparable to mine since they are restricted to the largest companies in the respective countries, while I include all companies on the main market in Sweden. The figure for Sweden in the EU data is 27% and covers the 30 largest companies.

3.1 Ownership and board membership

I obtain ownership data from Sundqvist and Fristedt (2009).[11] The data covers directly held stocks as well as stocks held through brokerages and custodians or by closely related parties. It is based on a list of all large shareholders that the Swedish central securities depository (Euroclear Sweden) is required to publish twice a year in combination with other sources (for detailed information on the methodology, see Sundqvist and Fristedt, 2009). This source only covers Sweden-registered companies; for companies registered elsewhere I get ownership data from company web sites and annual reports. When there is no information about the end owner of a company, I conduct news and web searches to try to complement the information.

When stocks are held through a pyramid I set the control right as the weakest link in the chain, following Claessens et al. (2000) and Faccio and Lang (2002). A shareholder is then considered the controlling shareholder if he controls at least 20% of the votes of the company. If there are several such owners, I only consider the largest one. In a few cases there are two blockholders with the same vote shares; I then aggregate their respective number of children.

The data also includes information about consortium agreements between major shareholders (although it does not specify the exact nature of those) and trading restriction on high-voting shares; however these are rare and I do not take them into account.

I exclude companies with no controlling shareholder or when the controlling shareholder is not an individual or a family from the Nordic countries.[12]

The composition of the board is obtained from Sundqvist and Sundin (2009). This source reflects the composition of the boards as of the end of May 2009, i.e. chosen by the annual general meeting of 2009 for most

[11] The data and method for identifying controlling owners are similar to Giannetti and Simonov (2006).

[12] There are three controlling shareholders from Norway, of which I find information about children for two.

companies. I include only shareholder-elected ordinary directors.[13,14] Again, I use annual reports for foreign-registered companies.

3.2 Financial and environmental performance

Accounting and stock market data is obtained from Thomson Datastream. Industry classifications are obtained from the Nasdaq OMX web site,[15] with some modifications.[16] Environmental performance indicators are obtained from GES Investment Services through insurance company Folksam.[17] The ratings are based on an evaluation of the company's ability to manage environment-related risks. Specifically, the score is based on whether the company has policies, management systems, plans or programs, accounting, and external verification regarding its environmental impact, as well as on its industry-relative performance in terms of emissions, energy consumption etc. These criteria are based on guidelines from Global Compact and the

[13] By Swedish company law, employees have the right to appoint representatives in the board of directors of large companies. For details, see e.g. Swedish Corporate Governance Board, 2006, Special features of Swedish corporate governance, www.corporategovernanceboard.se/media/8980/special_features_or_swedish_corporate_governance_av_sven_unger.pdf.

[14] I exclude shareholder-elected deputy directors. These are rare, and the Swedish Corporate Governance Code stipulates that such directors should not be elected.

[15] Nasdaq OMX, 2009, Monthly report - equity trading by company and instrument December 2008,https://newsclient.omxgroup.com/cdsPublic/viewDisclosure.action?disclosureId=357434&lang=en.

[16] Due to the low number of companies in some industries, I merge the following industries pairwise: Energy and Materials; Telecommunication Services and Information Technology; and Consumer Staples and Consumer Discretionary.

[17] This paragraph builds on Folksam, 2009, Index för ansvarsfullt företagande 2009, http://feed.ne.cision.com/wpyfs/00/00/00/00/00/10/36/DB/wkr0013.pdf (in Swedish). Folksam is one of the largest Swedish pension and insurance companies. Each year (except some gaps), it publishes a review of the "sustainability performance" of all companies listed in Sweden, which is based on the scores provided by GES Investment Services. GES' ratings are also used by *inter alia* Hassel et al. (2005). They are also used by Nasdaq OMX in the creation of the tradeable OMX GES Sustainability Indexes.

OECD. The criteria are then broken down into sub-criteria. For each sub-criterion fulfilled, the company is awarded one point, and the scores are averaged and then normalized. The maximum score of seven points is awarded if the company fulfills all criteria, or otherwise clearly demonstrates that it has the required environmental management. The ratings are based on the company's own reporting complemented with other public information; those not reporting are automatically assigned a score of 0.

3.3 Number and gender of children

3.3.1 Data sources

Data on the age and gender of children are obtained from several sources:

- News searches using the database *Mediearkivet*[18] as well as Google.[19]

- Biographies of the controlling owners or their families (usually I have been made aware of these sources through the sources above).

- Genealogical literature (*Sveriges Adelskalender 2010* for families that are part of the nobility, and *Svenska släktkalendern* for other families that have been considered notable by the editors of these works).

- The books by Sundqvist and Fristedt (2009) and Sundqvist and Sundin (2010) which cover owners and directors, respectively, have sections that sometimes clarify family relationships.
- The databases *Sveriges befolkning* (the population of Sweden) editions

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[18] This database covers among other sources the largest Swedish daily newspapers *Dagens Nyheter*, *Svenska Dagbladet*, and *Göteborgs-Posten*, as well as the business daily *Dagens Industri* and the business weeklies *Affärsvärlden* and *Veckans Affärer*. It covers print as well as web sources.

[19] The keywords used in the search are the name of the controlling owner together with the terms "barn" (child or children), "son" (son), and "dotter" (daughter). In cases where the controlling owner has a common name, I also include the name of the company he is associated with.

1970, 1980, and 1990. These sources are based on administrative registers and contain records on almost everyone who was living in Sweden at the end of the respective year.[20] When using this source, I classify as children persons at least 18 years younger than the owner who live on the same address and have the same surname as the owner (or his spouse). Searches based on the name and year of birth of the controlling owner in these sources sometimes give multiple hits. In those cases, I seek to find the right individual by using complementary sources.[21]

I cross-check the above sources when the information from the first hit appears dubious. One limitation is that they will be more likely not to cover children born after 1990 (as the *Sveriges befolkning* data ends in that year, and the media is presumably less likely to cover young individuals). I believe this will only bias my findings towards 0, since the children who are omitted from the sample are equally likely to be of either gender (i.e. a classic errors-in-variables-problem).

The gender of the child is given explicitly in the sources *Sveriges befolkning* and *Sveriges adelskalender*; in the other sources I can determine it either from the name of the child or the text. Finally, I drop a handful of companies where I could not find information on only the number but not the gender of children.[22]

[20] The help files included on the CDs on which the databases are distributed explain that e.g. persons with protected identity are not included.

[21] Specifically, I obtain the person's full name and date of birth from the databases *Affärsdata* and *PAR* (the annual report typically omits middle names and sometimes just gives nicknames). These sources contain history of directors and CEOs starting from 1993. In some cases, the persons are directors of foreign-registered companies and I use *Affärsdata* to find the Swedish subsidiaries of these companies, in which they will sometimes also be directors. In other cases, the person cannot be found this way but in connection to other companies which I have found him to be affiliated with through news searches.

[22] There were 3 such cases, one of them being a UK-based family.

3.3.2 Definition of parents and children

A complication in the classification of individuals as "children" is that there are often individuals of several generations who have all inherited their power in the same company. In those cases, I classify as children the oldest living generation, subject to the individual being no more than 80 years old. Hence, I use children to mean any direct descendant of the blockholder even if that person is an adult. To determine whether the individual inherited his stake, I use ownership histories from Sundqvist (1985-1993), Sundqvist and Fristedt (2003-2008), and Sundqvist and Sundin (1994-2002), complemented with news searches. A more subtle complication is when an individual inherited a company but changed its industry; in these rare cases I classify the individual as entrepreneur rather than heir.[23]

The identification of children is complicated by the fact that the children may change surnames (and first names, although that is presumably rare). In this case, they can be identified by their first names and birth years, and the fact that the corporate governance report (a compulsory part of the annual report) identifies them as not being independent of major shareholders, in many cases explicitly stating the family relationship. Further, news searches and the table in Sundqvist and Sundin (2010) that identifies family directors are used to identify these individuals.

Since the issues of multiple generations described above make it somewhat arbitrary whom to classify as blockholder or child, I run all regressions both on the full sample and restricting to companies where these issues are not involved, i.e. where the current blockholder is the founder or the one who first acquired the stake. I will refer to such companies as "simple companies".

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[23] The "industry history" is obtained from annual reports as well as the news searches and biographies referred to above. Admittedly, the classification is somewhat subjective at this stage (it is not based on histories of formal industry codes or the like).

3.4 Descriptive statistics

Table 2.1 shows summary statistics at the company and family levels. It is seen that approximately 19% of the companies give no environmental information and hence get the lowest score, and that the average family has control of 1.5 companies in the sample.

Table 2.2 compares companies where the blockholder has at least one daughter to those where he has none. The environmental score is on average significantly higher in companies where the blockholder has at least one daughter; the difference corresponds to approximately two thirds of the standard deviation of the variable. Such companies also have larger boards with a higher share of female directors (the latter difference is however only marginally significant).

4 Empirical strategy and results

I use two empirical strategies that both rely on random variation in the gender of the blockholder's children. First, I adapt the specification from Washington (2008) and run the regression

$$Y_i = \alpha + \beta No.Daughters + \gamma_i + \epsilon_i \tag{2.1}$$

, where γ_i is a set of fixed effects for the total number of children of the blockholder associated with firm *i*. Since one individual may be a blockholder in several companies, I cluster the standard errors at the blockholder level.

As noted by Washington (2008), the coefficient β in this regression can be interpreted as the effect of either one daughter more or one son less, since the number of daughters and sons are linearly dependent conditional on the number of children; I will follow Washington in speaking of the effect in terms of the number of daughters.

Next, I consider the possibility that the firstborn child may take a larger role in the management of the family's business (e.g. Bennedsen et al., 2007). I therefore run regressions of the same form as in Bennedsen et al.:

$$Y_i = \alpha + \beta Female\ firstborn_i + \epsilon_i \tag{2.2}$$

I also run the same regressions with and without industry fixed effects. These fixed effects may be effective in absorbing noise since some of the outcome variables I study have significant industry variation. They are however problematic in the sense that the children's gender may influence which industries the blockholder enters. Therefore they may be endogenous "bad controls" in the sense of Angrist and Pischke (2009). Note that the outcome variable of main interest, *Environmental score* is also constructed so as to be industry-neutral.

4.1 Child gender and environmental performance

Table 2.3 shows the results from running regressions with the environmental score as the outcome variable. Columns 1–4 show that environmental scores improve when the blockholder has more daughters, controlling for the number of children. The baseline specification in column 1 shows that an additional daughter improves environmental score by 0.36 units, which corresponds to approximately 19% of the mean and 24% of the standard deviation of this variable. For the other specifications, the effect ranges between 15–28% of the mean and 10–18% of the standard deviation. The effect is statistically significant, but only marginally so for the full sample test with industry fixed effects.

Columns 5–8 show that the gender ordering of children is less relevant in this context. When the firstborn is female environmental scores improve, but the effect is statistically insignificant for most specifications.

Comparing the full set of companies (columns 1–2, 5–6) to the "simple" ones, it is seen that the coefficients are higher and have higher statistical significance when restricting to simple companies. It would be interesting to gain further understanding of the reasons for this difference, for instance with a view to disentangling selection of companies to control from influence in companies the blockholder controls. However, the small sample size prevents me from doing so in a reliable manner.

In untabulated robustness tests, I replace the child dummies by linear controls for the number of children. In addition, I control for the size of the company. The results are qualitatively similar and statistical significance is always at least as high. Note however that the inclusion of size is somewhat problematic due to its possible endogeneity. Additionally, I verify that the results are qualitatively unaffected when running ordered logit or probit models instead of the baseline linear model, indicating that the results are not sensitive to the particular scaling system used by the environmental score provider. Finally, I verify the robustness of the results to different clustering methods.[24]

4.1.1 Effect on reporting or actions?

The interpretation of the environmental performance is complicated by the fact that almost 20% of the sample companies have been assigned a score of 0 due to lack of reporting. The effect is therefore likely to cover the joint effect of reporting and performance conditional on reporting. One might try to separate these effects, as is done in Jo and Harjoto (2011). I do not do this for several reasons. First, most of the criteria on which the score is based measure whether the company has described different aspects of an environmental management system, i.e. the reporting decision is not binary. Second, the decision to report may be highly correlated with the decision to implement environment-friendly policies. It is hard to imagine why a large organization undertaking systematic work for managing its environmental impact would not report it. Finally, even if we accept the decision to report as binary, it is also hard to think of an instrument that affects only the decision to report and not the environmental score. Hence, it would be difficult to implement a two-stage regression credibly (e.g. Angrist and Pischke, 2008, p. 100).

[24] In particular, the results that are significant in the baseline remain so when using either the "wild" bootstrap procedure of Cameron, Gelbach, and Miller (2008), or the bias reduction modification for standard errors of Bell and McCaffrey (2002) including with the degrees of freedom adjustments of Imbens and Kolesár (2015); using Judson Caskey's *cgmwildboot* command with 3,000 replications, and Joshua Angrist's *brl* command, respectively.

4.2 Child gender and financial performance

Table 2.4 shows the results from running regressions where the dependent variable is financial performance. I measure financial performance both as return on assets (ROA) and as Tobin's *Q* (proxied by the ratio between market and book value of equity). The sign of the coefficient varies depending on specification, but is never significant. This is hardly surprising, since it is hard to see *ex ante* in which direction a possible effect on financial performance should run.[25] The lack of a result may also be due to noise in the data, especially since it comes from the rather special period of 2009–2011, when the world experienced a severe financial crisis.

4.3 Channels: board composition and CEO appointment

To understand how the effect of daughters on environmental friendliness works, I study how it affects the gender composition of the firm's directors and CEO. An effect on these outcome variables may come about either because the adult children themselves enter the company's board or management, or through an indirect effect where e.g. daughters make their fathers more likely to promote women more generally. Table 2.5 shows that the share of female directors increases when the blockholder has more daughters (columns 1–4) and when the firstborn is female (columns 5–8). The effect is however statistically insignificant for most specifications and of relatively small economic magnitude; for instance column 1 indicates that one more daughter is associated with an increase in the share of female directors corresponding to 8% of the mean. Closer inspection reveals that most of the adjustment is due to daughters of the

[25] Since Bennedsen et al. (2007) use the gender of the first-born child as an instrument for family CEOs and show that family CEOs are detrimental to financial performance, one might initially expect a positive effect in a reduced-form regression like the one I run. There are however important differences. First, Bennedsen et al. focus on differences-in-differences around the CEO transition; indeed they show that offspring gender does not affect financial performance prior to the CEO transition in their sample. Second, as I show below the decision to appoint a family member as CEO is not visibly affected by offspring gender in my sample.

blockholder becoming directors. Panel A of Table 2.6 shows that when the blockholder has more daughters or the firstborn is female, there is an increase in the share of directors who are daughters of the blockholder. Panel B meanwhile shows that there is little effect on the share of other female directors (point estimates are of varying sign but insignificant).

A remaining possibility is that daughters are more powerful on the board than other female directors. Speaking partly against this possibility, Panel A of Table 2.7 show that offspring gender composition does not have a significant effect on the probability of appointing a female chairman.

Finally, I test whether the gender composition of the blockholder's children affects the probability of appointing a female CEO. Panel B shows that this is not the case. While the estimated coefficients are positive, they are statistically insignificant.

In sum, the tests in this section indicate that the effect of daughters on firm policies do not work through the gender of the firm's directors or CEO. To verify that the low significance of the results are not due to the blockholder's children being too young to feasibly become directors or executives, I test the robustness of the results to restricting only to children who are at least 25 years old. The results are qualitatively very similar and are not tabulated.

4.4 Are the results explained by family directors and CEOs?

Bennedsen et al. (2007) use a regression similar to my Equation (2.2) in an IV strategy for studying the effect of appointing a CEO from within the family.[26] To verify that this does not drive my results, I run regressions that are close in spirit to their first stage. Unlike their results, I find no significant effect from offspring gender structure on the decision to appoint blockholders' adult children as CEOs (Panel A of Table 2.8). There is also no clear effect on the decision to appoint a child as chairman of the board as seen in Panel B, as the sign of the estimated coefficient varies but

[26] The only difference is that Bennedsen et al. (2007) have a different set of control variables; the variable of interest is the same except that they formulate it differently.

is insignificant for most specifications. The effects on the decision to appoint blockholder's children as directors more generally are displayed in Panel C and are somewhat hard to interpret. Columns 1–4 show the results from the specification in Equation (2.1), which measures the effect of daughters conditional on the number of children. The estimated coefficient is insignificant for this specification. Columns 5–8 show results from the specification in Equation (2.2), where the independent variable is a dummy for the firstborn being female. The estimated coefficient is now significantly negative. While this indicates that offspring gender-age composition does have an effect on the presence of the family on the board, the interpretation is not obvious since I am not aware of any evidence of differences in CSR-preferences between family directors and other directors.

Again, the results are qualitatively very similar when I restrict to only children who are at least 25 years old (not tabulated).

4.5 Testing the exogeneity assumption - gender stopping rules

The identification strategy in Equation (2.1) may be violated if parents follow gender-biased stopping rules, e.g. keep having more children until they get one son (Washington, 2008). I repeat the test that Washington proposes to check whether parents follow such rules: if parents have children until they have at least one son, they will on average have more children if the firstborn child is female. Table 2.9 shows that this is not the case in my sample. As seen, when the first-born child is female the number of daughters increase, but not the number of children. The identification strategy may also be violated if parents employ techniques for identifying the gender of children and make gender-based abortion decisions; I consider this unlikely however.[27]

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[27] Such techniques became available around 1980 (UNFPA Guidance Note on Prenatal Sex Selection, www.unfpa.org/webdav/site/global/shared/documents/publications/2010/guidenote_prenatal_sexselection.pdf), i.e. few children in the sample are likely to have been born after that time. Furthermore, I am not aware of any evidence that these techniques are

5 Concluding remarks

I show that companies take more environmental responsibility when the blockholder has more daughters, conditional on the total number of children. In the baseline specification, one daughter more (or equivalently, one son less) is associated with an increase in the environmental score that corresponds to 19% of the sample mean and 24% of the standard deviation. The mechanisms behind this effect are likely to be manifold, and my data is not rich enough to fully separate between them. For instance, they are likely to reflect gender-based differences in preferences and influence of children on their parents, phenomena that have previously been documented elsewhere in the literature. They may also reflect increased female participation among the company's directors and executives, although my results do not strongly support this possibility. Further, child gender may impact the parent's decisions on both which companies to enter and how to run the company conditional on entering.

While the policy implications of this study differ somewhat depending on which channels are active, the results are broadly informative about the possible implications of increasing female representation in corporate decision-making. They are therefore complementary to the growing literature that seeks to answer that question with plausibly exogenous variation (e.g. Adams and Ferreira, 2009; Ahern and Dittmar, 2012; Faccio et al., 2014; and Matsa and Miller, 2013). Such studies are important in the light of policy debates on how to increase female representation using e.g. female board quotas. Somewhat discouraging compared to arguments based on correlational studies that have often been aired in that debate, I do not find an impact on financial performance.

The findings of this paper also point towards a previously underexplored role of the family in family firms. Since females are roughly equally represented among children but traditionally underrepresented in the corporate world, they can help in the transition towards a more gender-equal business world. The family, and in particular its female members, may also

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widely practiced in Sweden. Consistent with the absence of such techiniques, approx. 44% of the children in my sample are daughters.

help the firm meet sustainability-related challenges. Detailed examination of these possibilities is an avenue I leave for future research.

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Tables

Table 2.1. Summary statistics

This table shows summary statistics for the companies used in the analysis. At the date market capitalization is measured, US$1=7.91 SEK. *ROA* is defined as Operating income/Total assets$_{t-1}$. *Tobin's* Q is (Total assets-Total shareholders' equity+Market value of equity)/Total assets. *Environmental score* is the score reported by GES Investment Services (scale 1-7); companies are automatically given a score of 0 if GES could not find any environmental information from the company. "Children" are any direct descendants of the blockholder, irrespective of age. The number of companies in which family is blockholder only includes other companies in the sample, i.e. holdings in private and foreign-listed companies are excluded.

				Percentiles		
Company level variables	*N*	*Mean*	*SD*	*10*	*50*	*90*
Market capitalization (mSEK)	124	7,421	25,585	160.8	790.5	16,533
Vote share of blockholder (%)	124	44.5	19.6	23.4	40.5	73.1
Share of female directors (%)	124	19.8	12.5	0.0	18.3	33.3
Female CEO dummy	124	0.02	0.15	0.0	0.0	0.0
Blockholder child CEO dummy	124	0.05	0.22	0.00	0.0	0.0
Blockholder child chair dummy	124	0.09	0.29	0.00	0.0	0.0
Share of directors who are children of blockholder (%)	124	7.30	9.78	0.0	0.0	20.0
ROA (%)	120	7.19	14.06	-3.44	7.11	20.6
Tobin's *Q*	120	2.14	3.06	0.88	1.38	3.16
Environmental score	124	1.93	1.52	0.0	1.84	4.11
Environment info. not disclosed	23					

				Percentiles		
Family level vars.	*N*	*Mean*	*SD*	*10*	*50*	*90*
Number of children	84	2.61	1.52	1	2	5
Number of daughters	84	1.15	1.01	0	1	3
Number of companies in which family is blockholder	84	1.45	1.24	1	1	2

Table 2.2. Summary statistics by blockholder having at least one daughter

This table compares the variables used in the analysis based on whether the blockholder family has at least one daughter. "Children" are any direct descendants of the blockholder, irrespective of age. At the date market capitalization is measured, US$1=7.91 SEK. *ROA* is Operating income$_t$/Total assets$_{t-1}$. *Tobin's* Q is (Total assets-Total shareholders' equity+Market value of equity)/Total assets. *Environmental score* is the score reported by GES Investment Services (scale 1-7); companies are automatically given a score of 0 if GES could not find any environmental information from the company. Standard errors are in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *,**, and ***.

	No daughters	At least one daughter	Difference
Market cap. (million SEK)	2,725	8,986	6,261
	(1,258)	(3,021)	(5,298)
Share of female directors (%)	16.1	21.1	4.93*
	(2.0)	(1.31)	(2.56)
Share with female CEO (%)	0.0	3.22	3.23
	(0.0)	(1.84)	(3.20)
Share with female chairman (%)	3.22	4.30	1.08
	(3.22)	(2.11)	(4.11)
Share w/ blockholder child CEO (%)	0.0	6.46	6.46
	(0.0)	(2.56)	(4.44)
Share with blockholder child chairman (%)	6.45	9.68	3.23
	(4.49)	(3.08)	(5.94)
Share of directors who are children of blockholder (%)	6.56	7.55	0.98
	(1.97)	(0.98)	(2.04)
ROA (%)	5.62	7.67	2.05
	(2.33)	(1.52)	(3.04)
Tobin's *Q*	1.55	2.32	0.77
	(0.13)	(0.36)	(0.66)
Environmental score	1.17	2.18	1.01***
	(0.22)	(0.16)	(0.30)
N	31	93	

Table 2.3. Blockholder daughters and environmental score

This table shows how the company's environmental score is affected by the gender of the children of the blockholder. ”Children” are any direct descendants of the blockholder, irrespective of age. *Female firstborn* is a dummy indicating that the firstborn child of the blockholder is female. *Environmental score* is the score reported by GES Investment Services (scale 1-7); companies are automatically given a score of 0 if GES could not find any environmental information from the company. ”Simple” companies are those where the blockholder did not inherit the stake. The sample size varies since in some cases I cannot infer the birth order of children, and since the specifications with the Female firstborn dummy require that the blockholder have at least one child. OLS estimation; standard errors (in parentheses) are heteroskedasticity-robust and clustered by blockholder. Statistical significance at the 10%, 5%, and 1% level is indicated by *,**, and ***.

	Dependent variable: environmental score							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of daughters	0.36**	0.28*	0.52***	0.39**				
	(0.14)	(0.15)	(0.17)	(0.18)				
Female firstborn					0.20	0.044	0.68**	0.46
					(0.39)	(0.40)	(0.32)	(0.32)
Number of children FE	Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes
Restrict to ”simple” companies			Yes	Yes			Yes	Yes
N	124	124	87	87	112	112	79	79
R^2 (%)	26.5	34.6	17.7	29.5	0.4	9.8	6.1	19.8

Table 2.4. Blockholder daughters and financial performance

This table shows how the company's financial performance is affected by the gender of the children of the blockholder. ”Children” are any direct descendants of the blockholder, irrespective of age. *Female firstborn* is a dummy indicating that the firstborn child of the blockholder is female. *ROA* is Operating income$_t$/Total assets$_{t-1}$. *Tobin's* Q is (Total assets-Total shareholders' equity+Market value of equity)/Total assets. ”Simple” companies are those where the blockholder did not inherit the stake. The sample size varies since in some cases I cannot infer the birth order of children, and since the specifications with the Female firstborn dummy require that the blockholder have at least one child. OLS estimation; standard errors (in parentheses) are heteroskedasticity-robust and clustered by blockholder.

	Dependent variable: ROA (%)								Dependent variable: Tobin's *Q*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Number of daughters	0.2	-0.1	0.9	0.1					-0.2	-0.3	0.4	0.4				
	(1.3)	(1.3)	(1.8)	(1.9)					(0.4)	(0.5)	(0.3)	(0.3)				
Female firstborn					-1.8	-1.9	0.4	0.8					-0.5	-0.6	-0.7	-1.0
					(8.0)	(3.0)	(4.3)	(4.4)					(0.6)	(0.6)	(0.9)	(0.9)
N. of children FE	Yes	Yes	Yes	Yes					Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes
Restricted to "simple" companies			Yes	Yes			Yes	Yes			Yes	Yes			Yes	Yes
N	120	120	84	84	110	110	78	78	120	120	84	84	110	110	78	78
R^2 (%)	3.9	9.7	4.5	9.1	0.4	7.1	0.0	7.0	7.9	24	68	77	0.6	15	0.9	16

Table 2.5. Blockholder daughters and board gender composition

This table shows how the board gender composition is affected by the gender of the children of the blockholder. ”Children” are any direct descendants of the blockholder, irrespective of age. *Female firstborn* is a dummy indicating that the firstborn child of the blockholder is female. ”Simple” companies are those where the blockholder did not inherit the stake. The sample size varies since in some cases I cannot infer the birth order of children, and since the specifications with the Female firstborn dummy require that the blockholder have at least one child. OLS estimation; standard errors (in parentheses) are heteroskedasticity-robust and clustered by blockholder. Statistical significance at the 10% level is indicated by *.

	Dependent variable: share of female directors (%)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of daughters	1.59	1.46	1.85	1.48				
	(1.60)	(1.52)	(2.16)	(2.02)				
Female firstborn					3.66	4.09	4.78	6.18*
					(2.69)	(2.56)	(3.00)	(3.23)
Number of children FE	Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes
Restrict to ”simple” companies			Yes	Yes			Yes	Yes
N	124	124	87	87	112	112	79	79
R^2 (%)	10.0	18.8	9.53	20.8	2.26	15.0	3.84	16.6

Table 2.6. Blockholders daughters and other female directors

This table shows how the composition of female board members is affected by the gender of the children of the blockholder. Panel A shows the effect on the number of directors who are daughters of the blockholders, while Panel B shows the effect on other female directors. "Children" are any direct descendants of the blockholder, irrespective of age. *Female firstborn* is a dummy indicating that the firstborn child of the blockholder is female. "Simple" companies are those where the blockholder did not inherit the stake. The sample size varies since in some cases I cannot infer the birth order of children, and since the specifications with the Female firstborn dummy require that the blockholder have at least one child. OLS estimation; standard errors (in parentheses) are heteroskedasticity-robust and clustered by blockholder. Statistical significance at the 5% and 1% level is indicated ** and ***.

Panel A. Blockholder daughters on the board

	Dependent var.: % of directors who are daughters of blockholder							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of daughters	2.73***	2.74***	1.61**	1.69**				
	(0.78)	(0.79)	(0.70)	(0.64)				
Female firstborn					4.21***	4.77***	1.81	2.15
					(1.39)	(1.47)	(1.10)	(1.45)
Number of children FE	Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes
Restrict to "simple" companies			Yes	Yes			Yes	Yes
N	124	124	87	87	112	112	79	79
R^2 (%)	22.1	23.6	9.23	12.2	11.3	16.1	2.93	6.64

Panel B. Other female directors

	Dependent var.: % of directors non-blockholder-daughter females							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of daughters	-1.14	-1.29	0.24	-0.20				
	(1.61)	(1.54)	(2.17)	(2.07)				
Female firstborn					-0.56	-0.69	2.97	4.03
					(2.74)	(2.73)	(3.20)	(3.50)
Number of children FE	Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes
Restrict to "simple" companies			Yes	Yes			Yes	Yes
N	124	124	87	87	112	112	79	79
R^2 (%)	8.1	14.5	7.10	17.4	0.05	9.94	1.38	13.3

Table 2.7. Blockholder daughters and female chairmen & CEOs

This table shows how gender of the children of the blockholder affects the decision to appoint a female chairman (Panel A) or CEO (Panel B). "Children" are any direct descendants of the blockholder, irrespective of age. *Female firstborn* is a dummy indicating that the firstborn child of the blockholder is female. "Simple" companies are those where the blockholder did not inherit the stake. The sample size varies since in some cases I cannot infer the birth order of children, and since the specifications with the Female firstborn dummy require that the blockholder have at least one child. OLS estimation; standard errors (in parentheses) are heteroskedasticity-robust and clustered by blockholder.

Panel A. Blockholder daughters and female chairmen

	Dependent var.: Chairman is female (dummy)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of daughters	-0.01 (0.02)	-0.007 (0.02)	-0.02 (0.02)	-0.07 (0.02)				
Female firstborn					0.04 (0.04)	0.06 (0.04)	-0.00 (0.04)	0.02 (0.03)
Number of children FE	Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes
Restrict to "simple" companies			Yes	Yes			Yes	Yes
N	124	124	87	87	112	112	79	79
R^2 (%)	7.41	11.4	3.37	12.1	1.06	6.1	0.01	6.3

Panel B. Blockholder daughters and female CEOs

	Dependent var.: CEO is female (dummy)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of daughters	0.02 (0.02)	0.02 (0.02)	0.03 (0.03)	0.03 (0.03)				
Female firstborn					0.02 (0.03)	0.02 (0.03)	0.02 (0.02)	0.02 (0.02)
Number of children FE	Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes
Restrict to "simple" companies			Yes	Yes			Yes	Yes
N	124	124	87	87	112	112	79	79
R^2 (%)	8.92	14.6	17.2	26.4	0.38	7.0	1.13	11.8

Table 2.8. Blockholder daughters and family CEOs & directors (cont'd on next)

This table shows how gender of the children of the blockholder affects the decision to appoint a family CEO (Panel A) or chairman (Panel B) or to elect family directors (Panel C). "Children" are any direct descendants of the blockholder, irrespective of age. *Female firstborn* is a dummy indicating that the firstborn child of the blockholder is female. "Simple" companies are those where the blockholder did not inherit the stake. The sample size varies since in some cases I cannot infer the birth order of children, and since the specifications with the Female firstborn dummy require that the blockholder have at least one child. OLS estimation; standard errors (in parentheses) are heteroskedasticity-robust and clustered by blockholder. Statistical significance at the 5% level is indicated by **.

Panel A. Blockholder daughters and family CEO

	Dependent variable: CEO is child of blockholder (dummy)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of daughters	0.02	0.02	0.00	0.00				
	(0.02)	(0.02)	(0.02)	(0.02)				
Female firstborn					-0.05	-0.05	-0.05	-0.06
					(0.04)	(0.04)	(0.04)	(0.05)
Number of children FE	Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes
Restrict to "simple" companies			Yes	Yes			Yes	Yes
N	124	124	87	87	112	112	79	79
R^2 (%)	6.43	12.3	8.9	14.7	1.49	6.51	2.95	9.06

Panel B. Blockholder daughters and family chairman

	Dependent variable: chairman is child of blockholder (dummy)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of daughters	-0.03	-0.03	-0.01	-0.01				
	(0.03)	(0.03)	(0.01)	(0.01)				
Female firstborn					-0.15**	-0.18**	0.02	0.03
					(0.08)	(0.08)	(0.02)	(0.03)
Number of children FE	Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes
Restrict to "simple" companies			Yes	Yes			Yes	Yes
N	124	124	87	87	112	112	79	79
R^2 (%)	18.0	21.6	6.9	9.83	6.68	13	1.13	3.24

Table 2.8. Blockholder daughters and family CEOs & directors (continued)

Panel C. Blockholder daughters and family directors

	Dependent var.: % of directors who are children of blockholder							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of daughters	0.10	0.20	-0.68	-0.09				
	(1.25)	(1.28)	(1.28)	(1.21)				
Female firstborn					-4.78**	-4.63**	-4.21**	-4.17*
					(1.98)	(2.13)	(2.04)	(2.33)
Number of children FE	Yes	Yes	Yes	Yes				
Industry FE		Yes		Yes		Yes		Yes
Restrict to "simple" companies			Yes	Yes			Yes	Yes
N	124	124	87	87	112	112	79	79
R^2 (%)	10.4	14.6	9.24	14.8	5.77	11.2	5.74	11.9

Table 2.9. Validity of assumed exogeneity of child-having outcomes

This table tests the validity of the assumption of exogeneity of child gender, by showing how the gender of the firstborn child of the blockholder affects the number of daughters (columns 1 and 2) and the total number of children (columns 3 and 4). ”Children” are any direct descendants of the blockholder, irrespective of age. *Female firstborn* is a dummy indicating that the firstborn child of the blockholder is female. OLS estimation where standard errors (in parentheses) are heteroskedasticity-robust. OLS estimation; standard errors (in parentheses) are heteroskedasticity-robust and clustered by blockholder. Statistical significance at the 1% level is indicated by ***.

Dependent variable:	Number of daughters		Number of children	
	(1)	(2)	(3)	(4)
Female firstborn	1.02***	1.00***	-0.45	-0.31
	(0.21)	(0.20)	(0.33)	(0.32)
Constant	0.80***	0.69***	2.95***	2.58***
	(0.13)	(0.13)	(0.24)	(0.26)
Restrict to families owning ”simple” companies		Yes		Yes
N	74	52	74	52
R^2 (%)	26.0	32.0	2.5	1.8

Chapter 3

Bank taxes, leverage and risk*

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Abstract: I use staggered changes in the taxation of banks by U.S. states to show how banks adjust their capital structure in response to taxes. A one percentage point increase in the income tax rate leads to a decrease in the ratio of equity to total assets of 15 basis points. The effect is symmetric for tax increases and decreases, but heterogeneous in that small and strongly capitalized banks react more. Banks appear to engage in regulatory arbitrage activities for keeping down their regulatory risk measures in response to taxes, consistent with a motive of keeping regulatory ratios at acceptable levels despite increasing their leverage. Finally, higher taxes may decrease banks' ability to survive crises.

1 Introduction

Excessive leverage in the financial sector is often blamed as one of the main causes of the financial crisis,[1] and policymakers increasingly recognize the potentially distortive effects from the tax benefit of debt. For instance, in the United States, The President's Framework for Business Tax Reform recently stated that "large bias towards debt financing in the corporate tax code may lead to greater aggregate leverage and the associated firm-level and macroeconomic costs of debt financing".[2] In response, several countries have recently taken interest in changing their taxation of the financial sector.[3] Yet there is surprisingly little systematic evidence for how im-

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[1] See e.g. FSB (2009) and French et al. (2010).

[2] The President's Framework for Business Tax Reform, 2012, available at www.treasury.gov/resource-center/tax-policy/Documents/The-Presidents-Framework-for-Business-Tax-Reform-02-22-2012.pdf

[3] See Claessens, Keen, and Pazarbasioglu (2010) and Shackelford, Shaviro, and Slemrod (2010) for an overview of taxation of the financial sector as well as a discussion of recently proposed reforms, and Devereux, Johannesen, and Vella (2013) for early evidence on the effects of new bank levies introduced by countries in the European Union. In the U.S., discussions about new taxes on bank balance sheets are currently taking place at the top political level (see e.g. "Biggest banks said to face asset tax in republican plan", Bloomberg News, February 25, 2014). More general academic discussions about taxation as an alternative to prudential regulation is provided in Chaudhry, Mullineux, and Agarwal (2014) and Cochrane (2014).

portant the tax benefit of debt is in shaping the behavior of financial institutions.

This lack of evidence is particularly surprising given that there is a large literature on the influence of taxes on funding policies for non-financial companies. *Ex ante* it is not clear if banks will react similarly to other corporations, since there is little theoretical research on the topic as well. For instance, it might be the case that banks are less influenced by taxes since their capital structure is subject to regulation, while on the other hand they may have more room for changing their capital structure due to their ability to quickly raise short-term liabilities.[4]

This paper seeks to fill this gap by providing evidence from variation in the taxes levied on banks by U.S. states. Using changes in these tax rates over the years 1994-2012, I document that banks respond to tax changes in the way predicted by theory. The effect is also economically meaningful; in particular, a one percentage point increase in the corporate tax rate is estimated to lead to a reduction in the ratio of equity capital to total assets of 15.3 basis points, approximately 3% of the standard deviation and 6% of the within-bank standard deviation. Interestingly, the effect comes mainly from an adjustment of equity rather than other liabilities. Banks also compensate some of the decrease in equity capital with an increase in Tier 2 capital, a lower-quality form of capital that includes tax-advantaged instruments such as subordinated debt. I find a heterogeneous reaction where small and better capitalized institutions react more to tax changes. This may imply that tax policy is a less useful tool in preventing excessive risk taking than what is explicitly or implicitly assumed by the recent proposals for new bank taxes, as large and risky banks may be least responsive.

Using the ratio of regulatory risk weighted assets to total assets as a measure of credit risk taking, I find that banks reduce their assets risks to compensate for the increased risk on the liabilities side. However, in additional tests I show that some of the reduction in the measured risk may be due to regulatory arbitrage activities. In particular, higher taxes make banks increase their holdings of non-agency mortgage-backed securities, an asset that had low regulatory capital requirements in relation to their risk. The effect of taxes on holdings of these securities and on regulatory measures of risk was also particularly strong in the period leading up to the recent

[4] The latter possibility is discussed by Admati et al. (2013), which I review below.

financial crisis, when the potential for such regulatory arbitrage was arguably particularly strong. Finally, I present suggestive evidence that the net effect of taxes is to make banks more risky in the sense that higher taxes reduce banks' ability to withstand financial crises.

The staggered nature of these changes allows me to use a difference-in-difference approach that can control for factors influencing banks that were not affected by the tax change. Still, it is possible that that the tax changes were undertaken for reasons related to macroeconomic factors that would have affected banks even absent the reforms. However, econometric tests and an exploration of the stated reasons for the changes suggest that they are not explained by macroeconomic conditions. My results are also robust to adding controls for the macroeconomic conditions faced by the bank, and changes in these variables are not significantly correlated to changes in bank capital structure.

I find no important asymmetries between tax increases and decreases, which stands in contrast to what is predicted in recent theoretical work and also found in recent empirical literature. While differences in research design may be the explanation, the result suggests that the focus solely on tax increases in recent work (e.g. Schandlbauer, 2015) may be premature.

The tax changes I study are often discussed and decided well in advance by state legislatures. I find that some of the reaction comes in the year before the tax change. While it is quite likely that bankers knew about the impending changes, it is interesting that they start implementing changes before the reform takes place. One possible reason for this is that it is costly for banks to adjust their capital structure too abruptly, and that they prefer to start the change in advance rather than not being to implement the full capital structure change in the year of the tax change.

States tax banks in ways that are more varied than what is commonly appreciated. Several states have substantial taxes on bank capital or deposits. In contrast to the income taxes, I fail to find a significant effect of changes in these taxes. This might be because there are relatively few such changes. Nevertheless, the prevalence of these taxes documented in this paper can be useful for future work, not least since these taxes are largely the opposite of the taxes on non-core liabilities (generally defined as liabilities net of deposits or equity) that have recently been proposed and introduced in some counties.

The current paper proceeds as follows: Section 2 discusses related literature. Section 3 provides an overview and historical background to how U.S. states tax banks. Section 4 presents the data and discusses the potential endogeneity of the tax changes. Section 5 presents the empirical strategy and results. Section 6 concludes.

2 Related literature

While the issue of how corporate income taxes affects capital structure has been studied several decades in corporate finance and accounting (see e.g. the surveys in Graham 2008 and Hanlon and Heitzman 2010), the issue has received renewed interest in recent empirical literature.

The paper most closely related to mine is a concurrent working paper by Schandlbauer (2015). This paper also uses changes in U.S. state tax rates to study the effects of bank's capital structure. An important difference from this paper is that Schandlbauer uses only tax increases. He find that these increases make banks increase their non-depository debt in the period prior to the increase, but not in the year of the increase. This paper is complementary to mine since there are important differences in sample selection and methodology,[5] and since the outcome variables we study are different; in particular, he does not study bank survival.

Also using U.S. data, Ashcraft (2008) documents an effect of taxes on banks' decision to include subordinated debt in their regulatory capital. Berger and Bouwman (2009) use state bank taxes as an instrument for capital structure for studying bank liquidity creation. However, they only find an effect of these taxes on large banks.[6] Finally, Han, Park, and Pennacchi

[5] Schandlbauer restricts to listed bank holding companies, while I study all banks meeting some size and data availability restrictions. This makes his sample substantially smaller in the cross-section, and he also covers a shorter time period. He also uses other and seemingly less complete sources for tax changes than I do; in particular he notes one of the changes in taxes on other tax bases than income which I have uncovered, while there are several more.

[6] Berger and Bouwman (2013) consider the possibility of using taxes as an instrument for capital structure of large banks for studying default risk, but do not report IV regression results since Hausman tests do not indicate endogeneity problems in their OLS regressions

(2013) provide theoretical and empirical evidence suggesting that banks are more likely to securitize loans when taxes are higher, especially if they operate in markets with better lending opportunities.

Variation in U.S. state taxes has also been used in the corporate finance literature; the paper most similar to mine is Heider and Ljungqvist (2015). Using difference-in-difference specifications, they document an economically and statistically significant reaction to tax increases, but not decreases. Arguing that this apparent asymmetry is unlikely to be due to statistical chance or endogeneity problems, they also discuss theoretical explanations for it. In particular, Admati et al. (2013) present a theory arguing that by changing tax rates or other policies, it is harder to make firms decrease their leverage rather than increase it. The reason for this asymmetry is that once a firm has more debt in place, a version of the debt overhang problem makes shareholders unwilling to delever. In another empirical study, Farre-Mensa and Ljungqvist (2014) use variation in taxes on banks in U.S. states as an exogenous shock in the supply of bank credit to non-financial firms, as a means to studying those firms' financial constraints.

Recent research has also used natural experiments from other countries. Doidge and Dyck (2015) provide evidence that a group of Canadian firms that had previously enjoyed tax advantages reduced their leverage relative to other firms when this tax advantage was removed. Panier, Perez-Gonzalez, and Villanueva (2013) document an effect on capital structure from a reform largely removing the tax benefit of debt in Belgium. Wu and Yue (2009) show that Chinese companies increased their leverage in response to a cancellation of locally granted tax rebates.

Another stream of literature uses international panel data on corporate taxes to study bank choices of leverage and other policies. In this vein, Keen and de Mooij (2015) documents that banks increase their leverage when the tax rates increase. Heckemeyer and de Mooij (2013) use the same source of variation and compare the tax responsiveness between banks and non-financials, as well as between banks with different characteristics. In contrast to Berger and Bouwman (2009, 2013), they find that larger banks are less responsive. Using quantile regressions, they also find that banks with stronger capitalization are more responsive. Further, de Mooij, Keen, and Orihara (2013) argue that the higher leverage resulting from higher taxes has an impact on the probability of financial crises at the country level. Horváth (2013) studies the effect of taxes on bank risk taking in an international panel. He finds that higher taxes make banks reduce the risk measured as average risk weight of their assets, which leads him to question the suggested positive effects of lower bank taxes on financial stability. While he recognizes that some of the apparent reduction may be due to regulatory arbitrage activities, he argues that this is unlikely to be the full explanation since there is also a reduction in lending activity and non-performing loans. Finally, Schepens (2014) utilizes the same Belgian tax reform discussed above, where the tax benefit of debt was largely removed. Schepens shows that this made Belgian banks decrease their leverage and reduce their risk-taking compared to their European peers.

3 Taxation of banks by U.S. states

Most U.S. states tax corporations, usually based on income. Some states additionally impose taxes based on equity, while some states do not tax corporations at all. Most states tax banks the same way they tax other corporations.[7] Still, some states have substantial differences between the taxes on banks and other corporation. For example, Ohio has phased out its general corporate income tax but retains a tax of 1.3% of the book value of equity for banks, giving a large tax disadvantage of equity.[8]

There is substantial variation in bank taxes between the states. Income tax rates range from 0% (e.g. NV, WA, WY) to 10.84% in California. Taxes in excess of 1% of bank equity (KY, OH, PA,VA) give a substantial tax benefit of debt, while taxes on deposits (e.g. 0.96 basis points in VT) discourage deposit taking, which is the main source of liabilities for most

[7] Historically, differences were more common due to restrictions in how states could tax national banks (McCray, 1986), and due to the protected nature of banking and related attempts of states to maximize revenues by bank ownership or heavy taxation (Kroszner and Strahan, 1999; Sylla, Legler, and Wallis, 2009). Further, special rules have been needed to prevent banks from using tax-exempt interest on treasury securities to pay tax-deductible interest on deposits (Gravelle, 1994). Today, some states call the bank income tax a franchise tax due to technical rules that then allow them to tax income on otherwise tax-exempt securities, but use the same base and rate as the tax on other corporations.

[8] The taxable capital base excludes some items such as retained earnings and ownership interest of depositors.

banks. Some states also impose taxes based on total bank assets; I ignore those since they do not affect the relative tax costs of different funding sources.

Banks are subject to taxation if they have "nexus" within a state, which by most states is interpreted broadly as having clients in the state without necessarily having branches there. States then use different apportionment formulas for determining which part they of the tax base they are entitled to when a bank has nexus in several states.[9]

4 Sample and data

4.1 Bank and macroeconomic data

I obtain financial data on commercial banks using Call Reports from the Federal Reserve.[10] The sample is set to the December reports of years 1994-2012.[11] From the sample, I drop banks with negative or missing total assets or equity, or total assets less than $25 million. I also drop banks that are organized as S-corporations, which are not subject to corporate taxes in most states. I combine this data with the Summary of Deposits data from the FDIC[12] to get a proxy for the geographic location of the bank's activity. Further, I require all control variables to be non-missing.

Following Berger and Bouwman (2009) and Ashcraft (2008), I calculate the bank's tax rate as the average rate in the states where the bank is active, weighted by the bank's deposits in each state. To remove the effect from possibly endogenous changes in the bank's geographic focus, I use the weights from 1994 as an instrument for the weights in subsequent years.

[9] Most states currently use the formula recommended by the Multistate Tax Commission, which puts equal weight on receipts, property, and payroll. For details on nexus and apportionment rules, see e.g. Pielsnik (1999a, b).

[10] www.chicagofed.org/webpages/banking/financial_institution_reports/commercial_bank_data.cfm

[11] I retain only fourth quarter observations since the fiscal year coincides with the calendar year for almost all banks in the sample.

[12] www2.fdic.gov/SOD/dynaDownload.asp

I measure the bank's capital structure choice by the equity capital ratio, defined as total equity capital over total assets. Similar measures are used by e.g. Berger and Bouwman (2009) and by U.S. bank regulators.

The construction of variables is described in more detail in Appendix B. Panel A of Table 3.1 shows summary statistics of the variables used in the analysis.

4.2 State taxation of banks

I use several sources to find the taxes levied on banks by U.S. states and the District of Columbia. To get basic information of which taxes apply to banks in different states, I rely on Fox and Black (1994), and Pielsnik (1999a, b). The procedure for getting a panel of tax rates is then similar to the one in Heider and Ljungqvist (2015) and Farre-Mensa and Ljungqvist (2014). The starting point is the series *The book of the states*.[13] This data is however incomplete and sometimes wrong. I therefore complement it with the article series "Current corporate income tax developments" in the *Journal of State Taxation*; state tax laws, tax forms and other information on state websites; and information from various sources obtained through searches on Google, LexisNexis, and HeinOnline when the above sources do not suffice. In a few cases I also got clarifications from representatives of tax agencies and bankers' associations in the respective states.

4.2.1 Measures of tax rates

State taxes on banks differ on several dimensions (both cross-sectionally and over time) such as tax rates on income, equity and deposits, as well as deductibility of federal taxes. I therefore need ways to make tax systems comparable.

For income taxes, I follow e.g. Ashcraft (2008), Berger and Bouwman (2009), and Han, Park, and Pennacchi (2013) and use the effective tax rate on 1$ million of income.[14] Using the formula in Han, Park, and Pennacchi (2013), the effective income tax rate is

[13] Available in print and at http://knowledgecenter.csg.org/drupal/view-content-type/1219.

[14] This is the highest bracket in all states except DE and SD, which have regressive taxes on bank income, where $1 million falls in the lowest-income bracket.

$$\tau_{income} = c + (1 - pt_f)t_s - t_f t_s[1 - p(t_f + t_s)]$$

, where t_f and t_s are respectively the federal and state corporate income tax rates, and p is the deductible portion of federal taxes in the state tax calculation. Following Han, Park, and Pennacchi (2013), I set $t_f = 35\%$. Similarly, taking the federal deductibility of state taxes into account, the effective tax rates on equity and deposits are $\tau_{equity} = (1 - t_f)t_{equity}$ and $\tau_{deposits} = (1 - t_f)t_{deposits}$.

To get a comprehensive measure of the tax benefit of debt, also I form the measure *debtbias* which is a proxy for the tax saving of $1 of debt (assumed to be deposits) compared to equity, normalized by the cost of debt:

$$debtbias = (r \times \tau_{income} + \tau_{capital} - \tau_{deposits})/r$$

where r is an interest rate. Absent equity and deposit taxes, this measure obviously simplifies to the effective income tax rate. To remove the influence of time series variation in interest rates, I set r to be the average 1-year USD LIBOR rate over the period.[15]

4.3 Description of the tax changes

I identify 80 changes in the tax rates on income, equity or deposits. Of those, 63 were changes in rates that were common to all industries, and are hence unlikely to have been instituted with a particular regard to the banking industry.[16] Most of these changes have been explored in Heider and Ljungqvist (2015), who argue that they are unlikely to be endogenous with respect to firms' leverage choices. The remaining ones are specific to the banking industry, which might raise endogeneity concerns. However, 4 of those changes were undertaken concurrently with changes in the tax rate

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[15] Note that the first-order influence of variation in interests rates on capital structure will be removed via year fixed effects in the regressions. LIBOR rates are obtained from The Federal Reserve Bank of St. Louis at http://research.stlouisfed.org/fred2/release?rid=253.

[16] 49 of these 55 changes in turn affected corporations and banks the same way; the other were changes in taxes with special provisions for banks (e.g. interest deductibility only for banks).

for other corporations.[17] Another 7 were changes that eliminated or reduced tax differences between banks and other corporations.[18] The remaining changes are of different and somewhat idiosyncratic characters. To understand the factors driving these changes, I conduct internet searches for finding the motivation given by legislators at the time. There was one increase in the surcharge that California banks pay on the regular corporate income tax in 1995, which was decided in order to compensate for increases in local taxes and levies that banks were exempt from. One change was a modernization and simplification of a very complex tax based on equity to a simpler one with a higher base rate (KY 1997). Similarly, one change was a reduction in the tax rate that was meant to compensate for the elimination of a loophole that allowed banks to decrease their tax base (KS 1999). Two (OH 2000 and MI 2009) were reductions in equity-based bank taxes that were not timed to changes in the taxation of other companies; however the former was part of a scheduled gradual decrease decided upon at the same time as the corporate tax rate was reduced. Finally, one state (MI 2008) went from taxing banks based on income to taxing their equity. At the same time, it changed the taxation of other companies from an income-based tax to a gross receipts tax that was deemed inappropriate for bank due to their low profit margins.[19] While this enquiry does not constitute a full investigation of the political economy behind those changes, it does indicate that the changes were unlikely to have been undertaken for reasons that were endogenous with respect to macroeconomic variables likely to affect bank leverage.

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[17] These are MA 2010, OH 1999, VT 1998, and TX 2008. In the last change, an income-based tax was changed to a gross receipts tax, with the important difference that interest payments remained deductible only for banks.

[18] These are MA 1995-1999 (gradual decrease in bank income tax to reduce difference to corporate tax rate), and RI 1997 & 1998 (increase in bank income tax rate to corporate tax rate, and cut in bank deposit tax rate followed by repeal of deposit tax).

[19] It should however be acknowledged that bankers openly state that they lobbied for the option of a tax on equity capital (see "Single Business Tax Replacement: Everybody had a Plan", mba Banking Magazine, available at www.mibankers.com/downloads/doc_download/60-winter2007). An e-mail discussion with a representative of the Michigan Bankers' Association indicated that one reason why banks opted to be taxed on capital rather than income as before was that the old system had been administratively cumbersome and subject to repeated litigation.

The tax changes are listed in the Appendix. As seen, 66 of 80 changes are on income taxes, while there are 13 changes in taxes on equity capital. There are only four changes in deposit tax rates (note that the numbers do not add up to the total number of changes since several tax rates are changed at the same time on some occasions). Summary statistics of taxes and other state characteristics are provided in Panel B of Table 3.1.

A visualization of the changes is provided in Figure 3.1, which places the years when any tax change took place on a map. It is seen that some states change their taxes several times while many keep them constant. Except for the northeastern states, the geographic clustering of tax changes is limited in the sense that most changes do not coincide with changes in neighboring states.

4.3.1 Determinants of tax changes – regression evidence

Tax changes are obviously not randomly assigned, and may be the outcome of local political and economic conditions. For instance, states may raise tax rates to help balance the budgets in local recessions, or increase them in times when they are more left-leaning. To get a sense of how important this potential endogeneity problem is, I run regressions of the determinants of tax rate changes. These regressions are very similar to the ones in Farre-Mensa and Ljungqvist (2014). The results are shown in Table 3.2. Column 1 indicates that states tend to decrease tax rates in times of positive state budget balance, and as seen in column 4 this also leads to a reduction in the tax benefit of debt. Column 2 shows that higher unemployment is associated with a marginally significant increase in the equity tax rate. The other coefficients are insignificant. A remaining possibility is that positive and negative effects cancel out – for instance some states might raise tax rates in bad times to keep the budget balanced, while other states might cut them as a pro-cyclical tool in such times. Columns 5 through 12 therefore estimate the effects separately on tax rate increases and decreases. Column 6 shows that income tax decreases are particularly well explained by GSP growth rate and state budget balance, with states reducing taxes more in times when these variables are higher. Column 12 shows that a negative budget balances are associated with negative changes in the *debtbias* measures, and that rating downgrades have the same effect although marginally significant.

5 Empirical strategy and results

Relying on the tax changes documented above, I use a difference-in-difference strategy similar to the one in Heider and Ljungqvist (2015):

$$\Delta CapitalRatio_{it} = \delta' \Delta Tax_{it} + \beta' \Delta \mathrm{X}_{i,t-1} + \alpha_t + \epsilon_{it} \qquad (3.1)$$

, where i and t, index banks and years, respectively, and Δ is the first difference operator. Tax_{it} is a measure of the tax rules applicable in the bank-year; in some specifications it is a vector consisting of tax rates on different tax bases. $X_{i,t-1}$ is a vector of control variables, α_t is a year fixed effect and ε_{it} is the usual error term. While the first difference structure removes bank characteristics that are fixed over time, including controls can reduce the noise in the estimation and account for the possibility that changes in other bank characteristics could explain the change in capital structure. Hence, I include some controls that are common in the literature (e.g. Gropp and Heider, 2010), namely the lags of log total assets, return on assets (ROA; measured as net income over total assets), and asset liquidity ratio (ratio of liquid assets to total assets).

Since banks headquartered in the same states are likely to face similar changes in tax rates (recall that the state tax rates are weighted by the bank's deposits), I cluster standard errors at the state level.[20]

I use different measures of the Tax_{it} variable. The first one is τ_{income} as used in previous studies. I then add in turn τ_{equity} and $\tau_{deposits}$. To reduce the number of coefficients to be estimated, I also run regressions with the *debtbias* measure.

The use of effective tax rates differs from the approach in Heider and Ljungqvist (2015), whose main specification uses dummies for increases and decreases in tax rates. The main benefit of my approach is that the coefficients can more directly be quantified and interpreted in terms of tax

[20] To account for the possible interdepence of financial policies within banking groups, I also tried with clustering at the bank holding company level, which however produced smaller standard errors for the coefficients of interest. I also tried double clustering at these two levels. This produced standard errors that were similar to the state-clustered ones for the coefficients of interest, but could not always be calculated for all coefficients.

sensitivities. There are however a few changes in Heider and Ljungqvist (2015) that cannot be quantified in terms of effective tax rates, such as changes in the amount of taxable losses that can be carried forward. To the extent that such changes affect banks' marginal tax rates, they will cause measurement error in my setting.

5.1 The effect of taxes on capital structure

Table 3.3 presents the effect of changes in different tax rates on the equity capital ratio. Columns 1 through 3 show that banks react to higher income tax rates by lowering their capital ratios as expected. The effect is significant in all specifications, but becomes larger in magnitude when changes in taxes on equity and deposits are controlled for. In the specification that only includes income tax changes (column 1), a 1 percentage point change in taxes is estimated to lead to a 14.9 basis point decrease in the equity capital ratio, approximately 3.4% of the standard deviation in the equity capital ratio, and 6.3% of the standard deviation of the change in the variable. The measured effect increases to 15.1 basis points when changes in taxes on equity are controlled for, and further to 15.6 basis points when deposit tax changes are also included (columns 2 and 3). Taxes on equity also have the expected effect, but with weak significance.[21] Deposit taxes have an insignificant effect with the opposite sign of the expected (column 3), likely due to power problems since there are so few of them. The coefficient for the *debtbias* measure has the expected sign but is only insignificant.

Comparing the magnitudes of the estimates to previous work is not entirely straightforward since different measures have been used in the literature. Nevertheless, the baseline estimate of a 14.9 basis point decrease in the equity capital ratio for a one percentage point increase in income tax rates is roughly comparable to the estimate in Berger and Bouwman (2009) of an approximate 30 basis point decrease in capital to gross total assets[22] per percentage increase in income taxes. The estimate can be compared to the international evidence in Keen and de Mooij (2015), where each per-

[21] The effect is marginally significant when also including for deposit tax changes (column 3), and insignificant when not doing so (column 2).

[22] As explained in their paper, *gross total assets* is total assets plus the allowance for loan and lease losses, and the allocated transfer risk reserve.

centage increase in tax rates is associated with an approximate 25 basis points decrease increase in the equity capital ratio. Finally, the estimate can be compared to the estimate for non-financials in Heider and Ljungqvist (2015), with the caveat that their outcome variable is long term debt over total assets, while mine is equity capital over assets, since the classification into long-term and short-term debt is likely to have different meaning for financial institutions. They estimate that firms raise long term debt to total assets by 30.9 basis points for every percentage point increase in income taxes, while they find an insignificant reaction to tax rate decreases.

5.1.1 Means of adjustment

Given that taxes affect the ratio of capital to total assets, it would be interesting to understand which balance sheet item is driving the change. To this end, I replace the dependent variable by the logs of equity and liabilities, respectively, where liabilities are measures as total assets minus equity. Table 3.4 shows the results. Columns 1-3 show that companies reduce their log equity in response to income tax increases, and column 4 shows that this also holds for the *debtbias* measure. For the log of other liabilities there is no significant effect, as seen in columns 5-8. While this finding may be surprising at first sight, it might be explained by a counteracting effect discussed by Heider and Ljungqvist (2015): when taxes decrease not only does the tax benefit of debt change, but the after-tax return on investments also changes. In my setting, lower tax rates may make banks extend loans that had not been profitable at higher tax rates. If the marginal source of funding for these new loans is liabilities (e.g. deposits), it can be expected that this counter-effect decreases the effect of taxes on capital structure.

Next, I seek to understand if equity capital is replaced by Tier 2 capital, which is considered to be of lower quality and includes *inter alia* subordinated debt. I therefore replace the dependent variable by Tier 2 capital to total assets. Columns 9-12 indicate that banks increase this capital level somewhat in response to tax income changes, though the effect is only marginally significant when controlling for changes in the other tax bases. It is also interesting to compare the magnitudes. While a 1 percentage point increase in the income tax rate was seen to lead to an increase of the equity capital ratio of approximately 15 basis points, the Tier 2 to assets ratio only increases by between 1.3 and 1.4 basis points (depending on specification). This suggests that the increase in other forms of capital is inadequate for compensating the decrease in equity capital, especially when the lower quality of these forms of capital is taken into account.[23]

5.1.2 Asymmetric effects of taxes

There are reasons to believe that negative and positive tax changes do not have symmetric effects. Admati et al. (2013) presents a theory for such asymmetries, and, Heider and Ljungqvist (2015) find empirical support for it in the data for U.S. non-financial companies.

To see if the asymmetry is present in my setting as well, I split the tax changes into positive and negative ones:

$$\begin{aligned}\Delta CapitalRatio_{it} \\ &= \delta_1{}'\Delta Tax_{it} + \delta_2{}'Positive\ \Delta Tax_{it} \\ &+ \beta'\Delta \mathrm{X}_{i,t-1} + \alpha_t + \epsilon_{it}\end{aligned} \tag{3.2}$$

, where $Positive\ \Delta Tax_{it} = \begin{cases}\Delta Tax_{it} & \text{if } \Delta Tax_{it} > 0 \\ 0 & \text{otherwise}\end{cases}$

Table 3.5 shows the results. For income taxes, increases are seen to lead to larger changes in absolute terms than tax cuts (columns 1-3). The difference between increases and decreases (measured by the coefficient for *Positive*ΔTax_{it}) is however never significant. The same holds for equity taxes (columns 2-3). Again, the results for deposit taxes are somewhat confusing: the coefficients show a significantly negative estimate for the change itself (implying that banks increase their capital ratios when deposit taxes are reduced) while the coefficient for *Positive*$\Delta\tau_{deposits}$ shows a significantly different reaction for deposit tax increases (for the increases, adding the interaction and the simple term we see that the total estimated effect is positive, which is the expected sign since deposit taxes makes equity funding relatively more attractive).

[23] See e.g. Detragiache, Demirgüç-Kunt, and Merrouche (2013) for evidence that Tier 2 capital was less useful as a buffer for preventing failure in the recent crisis. Acharya et al. (2011) discuss why lower-quality forms of capital are problematic from the perspective of giving managers the right incentives, and documents a shift towards such capital before the crisis.

One reason for the lack of significant differences between tax increases and decreases could be the interaction between banks and their clients. In particular, suppose the income tax increases for banks and non-financial firms at the same time, which is likely as most of the tax changes I document apply to banks and corporations sin the same way. If the firms lever up as documented by Heider and Ljungqvist (2015), banks' clients effectively become riskier. This may mitigate the incentives for increased leverage for banks, since their risk level has already increased.

Admittedly, the theory of Admati et al. (2013) may be correct while I lack the power to reject the null of a symmetric effect. In particular, since I weight the tax rates by the bank's deposits in the respective states, many banks will be subject to both tax increases and decreases at the same time, which would likely make it hard to show asymmetric reactions.

5.1.3 Heterogeneous treatment effects

I consider potential differences in the reaction between banks of different size, and banks with different levels of the equity capital ratio. For both these variables, there are potentially counteracting forces, which implies that the tax sensitivity may differ non-linearly on them. For size in particular, large banks may have more opportunities for changing their capital structure due to their better access to capital markets. On the other hand, they may be less sensitive to measured changes in marginal tax rates since they likely have more scope for tax planning through e.g. interstate transactions (c.f. Dyreng, Lindsey, and Thornock, 2013).[24] Such nonlinearities may explain the conflicting results from earlier studies (where Berger and Bouwman (2009) find an effect only for large banks, while Heckemeyer and de Mooij (2013) find weaker effects for large banks).

To address such possible non-linearities, I split the sample into three groups based on total assets and the equity capital ratio, respectively. The classification is made in the first year a bank enters the sample and kept constant thereafter. I then add dummies for being in the top/bottom tercile and their respective interactions with the income tax change to the baseline regression:

[24] In addition, the measurement error will likely be larger for larger banks in my setting since larger banks are on average more geographically dispersed, as I only have an imperfect proxy for assigning their tax base to different states.

$$\begin{aligned} \Delta CapitalRatio_{i,t} & \\ &= \delta_0 \Delta\tau_{income,it} + \delta_1 \Delta\tau_{income,it} \times Bottom \\ &+ \delta_2 \Delta\tau_{income,it} \times Top \\ &+ \alpha_1 Bottom + \alpha_2 Top + \beta' \Delta X_{i,t-1} + \alpha_t + \epsilon_{it} \end{aligned} \tag{3.3}$$

The results are presented in Table 3.6.[25] Column 1 shows that the magnitude of the tax sensitivity is significantly higher for the bottom tercile of banks, while the difference between the middle and top terciles is insignificant. The difference between terciles is also economically significant; the total effect of a one-percentage point increase in income taxes is a 28.3 percentage point decrease in equity capital ratio for small banks, nearly double the baseline estimate.

Column 2 shows that better-capitalized banks are more tax-sensitive. Again the difference is also statistically significant. The total effect of a one-percentage point increase in the income tax rate is a 26.3 basis point decrease in the equity capital ratio for the best-capitalized tercile, again nearly double the baseline estimate. This result echoes the finding in Heckemeyer and de Mooij (2013) that better capitalized banks are more responsive to tax changes in the international data. It is also to some extent in line with the results in Schandlbauer (2015) where better capitalized banks raise their leverage when taxes are increased, while worse capitalized ones decrease their assets (which effectively lowers their leverage since he finds no significant effects on the amount of liabilities).[26]

The higher tax sensitivity of better capitalized banks can be explained by them having more scope for changing their capital structure without being subjected to regulatory restrictions. For instance, well capitalized banks can take on brokered deposits without restrictions, while undercapitalized banks cannot do it at all.[27] One could also see this in a simple tradeoff

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[25] For brevity, these regressions do not control for changes in the other tax rates, however doing so makes no qualitative difference to the results (not tabulated).

[26] The comparison is however somewhat misleading since he finds most of the reaction in the year before the increase, while I focus on the year of the change.

[27] U.S. bank regulators classify banks into well capitalized, adequately capitalized, undercapitalized, and critically undercapitalized, based on mechanical rules. Adequately capi-

model where a bank trades of linear tax benefits of debt to convex non-tax costs of debt, which would make a highly levered bank relatively unresponsive to a tax increase (Heckemeyer and de Mooij, 2013). Some of these explanations also predict an asymmetric effect: better capitalized banks should exhibit a higher sensitivity especially to tax increases. In untabulated regressions conditioning on both capitalization level and the sign of the tax change, I could not find such differences; possibly my data lacks power to allow for such a quadruple difference-in-difference specification. It could also be that weak banks in practice have more difficulties decreasing their leverage in response to tax decreases, since raising equity may also be harder for them than for better capitalized banks. For instance, higher cost of raising capital due to more asymmetric information may be the reason why they are poorly capitalized in the first place.

5.1.4 Timing of response

This section tests if banks also adjust their capital structure in anticipation of tax changes, and if adjustment is sluggish. To that end, columns 1 through 4 of Table 3.7 add leads and lags of the tax changes. It is seen that most of the changes are on impact, but there are also some significant effects of leads and lags; in particular there is an effect on capital structure in the year before income tax rate changes. This is consistent with banks foreseeing changes (which is likely in many cases when tax changes follow a planned schedule) and starting their adjustment ahead of the change, perhaps as a response to a cost of changing capital structure too abruptly. The inclusion of leads and lags decreases the magnitude of the estimates for the immediate impact, for instance the coefficient for τ_{income} decreases to -12.2 from the baseline of -14.9 in the specification that only includes income taxes. The lags have the same signs as the coefficients for the effect on impact but are generally insignificant (the coefficient of lagged income tax changes is marginally significant in one specification, and that of lagged deposit tax changes is significant). These findings indicate that the baseline estimate understates the total effect of taxes on capital structure. For instance, adding the coefficients for the year of an income tax change and the previous year from column 1, the total effect on the equity capital ratio is -

talized banks can accept brokered deposits subject to regulatory approval. For closer institutional details, see e.g. Peek and Rosengren (1997).

20.7 basis points. The lags also indicate that the effect is rather attenuated than reversed.

5.1.5 Are results driven by variation in macroeconomic conditions?

To verify that the results found so far are not driven by variation in economic conditions that are correlated to tax changes, I add controls for the macro conditions that were most important in explaining tax changes. These are GSP growth rate, budget balance, and unemployment. As seen in columns 5 through 8 of Table 3.7, the coefficients are statistically insignificant, and moreover the inclusion of these variables does not substantially change the estimates of the tax change coefficients. For instance, the coefficient for τ_{income} is now -14.6 while the baseline was -14.9, again in the specification that only includes taxes on income. However, the coefficient for the *debtbias* measure now becomes insignificant.

5.2 The effects of taxes on bank risk taking

Since the previous section shows that banks increase the riskiness on the liabilities side of their balance sheets in response to higher taxes, it is important to understand how this affects their total risk taking. In this section, I try to offer some clues on this issue.

Ex ante, it is not obvious whether banks will reduce asset side risk to mitigate the increase in riskiness, or if they will exacerbate the change by granting riskier loans, since equity owners now have even more convex payoffs. The choice may also be influenced by regulation. If a bank seeks to maintain a target ratio of capital to risk weighted assets (a key ratio in bank regulation), it will need to compensate a lower ratio of equity to total assets with a lower ratio of risk weighted assets to total assets.

5.2.1 Regulatory risk weights

As a measure of credit risk taking, I use the ratio of risk-weighted assets to total assets, as in e.g. Berger and Bouwman (2009, 2013) and Keen and de Mooij (2015). Since assets perceived as riskier by the regulator receive higher risk weights, a higher ratio indicates more risk taking. While this is an imperfect measure of actual risk which may have been subject to manipulation as detailed below, a bank that would seek to maintain its regulatory risk ratios would have an incentive to induce changes in exactly this measure.

Table 3.8 shows the results. The effects of taxes are insignificant. A remaining possibility is that there are effects working in opposite directions that cancel out on average. For instance, banks may increase true risks but take actions that keep their regulatory risk measures down. I explore this in the next section.

5.2.2 Regulatory arbitrage and risk weights

Banks may take actions that can be seen as regulatory arbitrage, in the sense that they increase their holdings of assets whose regulatory risk weights are lower than warranted by their true risk profile. To investigate this possibility, I study the holdings of non-agency MBS (i.e. mortgage-backed securities not backed by the GSEs Fannie Mae, Freddie Mac and Ginnie Mae). Before the recent financial crisis, such securities carried a relatively low risk weight but had higher yields than other securities with similar ratings, indicating that the market understood that they were riskier (Coval, Jurek, and Stafford, 2009; Ianotta and Pennachi, 2011). They also resulted in lower risk weights than if the underlying loans would have been held directly (e.g. Acharya and Richardson, 2009). In contrast to agency MBSs, these securities did not have the implicit backing of the U.S. government, and turned out to be highly risky ex post. On these grounds, Acharya and Richardson (2009) suggest that they may have been use as an instrument for regulatory arbitrage before the crisis. I therefore run the same regression as before but with the ratio of non-agency MBS to total assets on the left hand side.[28] Since the potential of regulatory arbitrage through these securities was especially severe in the period leading up to the crisis,[29] I also interact the tax

[28] This measure is similar to the "bottom-up" measure in Erel, Nadauld, and Stulz (2013) but I use amortized cost of the securities rather than fair value estimates when available, to remove the variation from valuation changes.

[29] This is due to a 2001 rule change by the Federal Reserve that increased the benefit of MBSs (at least highly rated ones) compared to the underlying loans and corporate bonds in terms of lower risk weights. See Erel, Nadauld, and Stulz (2013) for closer institutional details. Since the rule change allowed banks to use credit ratings in their calculation of risk weights, the benefit of holding such securities would arguably have decreased following the rating downgrades that started in 2007 (e.g. Brunnermeier, 2009).

variables with a dummy that takes the value 1 in the years 2001 through 2006.[30]

Table 3.9 shows the results. Columns 1–4 show that banks raised their holdings of these securities in response to equity taxes, especially in the crisis build-up period. The response is also economically meaningful in this period; the total effect of a one-percentage point increase in equity tax rates is a 1.9 percentage point increase in the ratio of non-agency MBS to assets ratio, more than twice its standard deviation.

To understand if the reduction in risk weights was also lower in those years, I run the same regression with the ratio of risk weighted to total assets on the left hand side. Columns 5–8 show that the negative effect of equity taxes on the risk weighted assets ratio uncovered above was also particularly strong in this period. Taken together, these findings indicate that at least some of the decrease in the measured risk may have been due to holdings of the mortgage backed securities that were inappropriately classified as relatively safe before the recent financial crisis. It can also be seen that changes in income taxes had a positive total effect on risk weighted to total assets during the boom; note however that the total effect (the sum of the coeffiecents for the income tax change and its interaction with the boom dymmy) is only marginally significant.

5.2.3 Taxes and bank survival

A bank's survival may be the ultimate measure of its risk taking. To investigate how taxes affect survival probabilities, I follow a simplified version of the approach in Berger and Bouwman (2013). In particular, I split the sample years into three categories: crisis, normal and pre-crisis.[31] The classification is presented in Panel A of Table 3.10. Every set of consecutive years classified as crisis or normal is then preceded by one or more pre-crisis years. The independent variables are measured as the average in the pre-

[30] The interaction cannot be formed for the deposit taxes, since they never changed in that period.

[31] I base the classification on Berger and Bouwman (2013), but need to make it coarser in the time dimension since my variables of primary interest are measured at the yearly level, while they are using quarterly data. In addition, they separate between market and banking crisis, which is not feasible in my setting since I have only one banking crisis episode.

crisis period, and the outcome variable is survival in the crisis/normal period. Survival is measured as not having left the sample the next year, but make an exception for banks merging with another bank in the same bank holding company and classify both as surviving, following Berger and Bouwman (2013). I study the direct effect of taxes; while using taxes as an instrument for capital might also be possible, the reduced form is of independent policy interest. I therefore estimate the logit regression

$$\begin{aligned} Survival_{it} &= \ln\left(\frac{Pr(Surv_{it})}{1 - Pr(Surv_{it})}\right) \\ &= \alpha_1 + \delta' Tax_{i,pre-t} + \beta' \mathrm{X}_{i,pre-t} + \epsilon_{it} \end{aligned} \tag{3.4}$$

, where $Surv_{it}$ is a dummy for bank i surviving crisis/normal period t. Note that t in this equation denotes a period consisting of one or more crisis/normal years together with one or more pre-crisis years, not a year as in previous regressions. Taxes are measured with the same set of variables as before, and standard errors are again clustered at the state level. Since bank fixed effects cannot be used in this regression, the source of variation in tax rates is different from the difference-in-difference specification in Equation 3.1. There, I effectively only used variation over time in the tax rates in the states where a bank is active, while the current model also uses cross-sectional differences. Keeping these caveats in mind, I present the results in Panel B of Table 3.10. Columns 1 through 4 present the estimates from a regression of survival on tax rates and control variables for bank characteristics and macro variables. The table shows that tax higher tax rates are generally associated with lower survival probabilities, but the results are insignificant (the exceptions are taxes on deposit and the *debtbias* measure, which have the opposite sign but are also insignificant).

Berger and Bouwman (2013) argue that capital is particularly important for survival in crisis times, especially so for large and medium sized banks. I therefore want to see if the effect of taxes on survival differs between crisis and normal times, and add interaction terms between the tax variables and the *Crisis* dummy:

$$Survival_{i,t} = \ln\left(\frac{Pr(Surv_{it})}{1 - Pr(Surv_{it})}\right) = \alpha_1 + Crisis_t + \delta_1' Tax_{i,pre-t} + \delta_2' Tax_{i,pre-t} \times Crisis_t + \beta' \mathrm{X}_{i,pre-t} + \epsilon_{it} \tag{3.5}$$

The results are presented in columns 5-8. These estimates are however hard to interpret since interaction estimates from a nonlinear regression cannot be interpreted in the same way as in linear models (Norton, Wang, and Ai, 2004). I therefore show marginal effects of the tax variables in crisis and normal states in Panel C. The results indicate that higher taxes on bank income negatively affect the probabilities of bank survival in crises, although the effect is only marginally significant.

In conclusion, the evidence presented above provides suggestive evidence that banks do not decrease the risks on their asset side sufficiently to compensate for their higher leverage following tax increases, as higher taxes decrease their ability to survive crises. However, more rigorous work is needed to substantiate this finding.

6 Concluding remarks

This paper presents evidence that banks decrease their capital ratio in response to tax increases, with a symmetric effect for tax decreases. Banks partly compensate the change in equity capital ratios by changing the amount of lower quality Tier 2 capital in the opposite direction, though in smaller magnitude. I also present some evidence that banks reduce the risks on their asset side when taxes increase. The total risk of the banks however appears to increase, as banks facing higher tax rates show lower ability to withstand financial crises.

The symmetry of tax increases and decreases is interesting since the possibility of an asymmetry was proposed in Admati et al. (2013). While Heider and Ljungqvist (2015) find empirical support for such an asymmetry among non-financial companies, Admati et al. argue that their theory is particularly relevant to banks. The reason for this is that banks may have more opportunity than other corporations to issue debt that is effectively senior to existing debt, since banks may have implicit government guarantees that reduce debt holders' incentives to prevent it e.g. through covenants, and since they can quickly raise short-term liabilities in the form of deposits. My results indicate that further research is needed to investigate the possibility of asymmetric effects. Such asymmetries are not only of academic interest, but are also potentially important to policy-makers who may need to consider the dynamic effects of tax reforms.

Since the variation in my paper comes from staggered changes in tax rates of U.S. states, I can use a difference-in-difference specification that allows me to remove variation unrelated to taxes that might influence banks' capital structure choices. The sample period runs from 1994 to 2012, a time when banks in different states faced very similar institutional arrangements.

While the tests suggest that banks reduce their risk measured as risk weighted assets in response to tax increases, there is also a more cynical interpretation. If banks decrease their equity capital ratios but want to keep their regulatory risk measures such as risk-weighted capital ratios constant, they may have an incentive to move into assets that have higher risk than their risk weights indicate. Consistent with this possibility, I document that higher taxes make banks increase their holdings of non-agency mortgage backed securities, an asset whose combination of low risk weights and high actual risks may have made them an instrument for regulatory arbitrage. A complementary explanation to the increase in these assets is however that they arise as a by-product of increased securitization (cf. Erel, Nadauld, and Stulz, 2013). Separating these explanations, and more generally understanding the interaction between taxes, risk taking, and risk measure manipulation, are potentially interesting areas for future research.

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Tables and figures

Appendix A. Description of tax changes

CA	'95	Cut in income tax rate from 11.47% to 11.3%
CT	'95	Cut in income tax rate from 11.5% to 11.25%
DC	'95	Cut in income tax rate from 10% to 9.5% (+2 tax surcharges at 2.5% each)
MA	'95	Cut in income tax rate from 12.54% to 12.13%
MI	'95	Cut in income-based tax rate from 2.35% to 2.3%; interest was deductible only for banks under this tax
NY	'95	Cut of surcharge on tax liability from 15% to 12.5%
CT	'96	Cut in income tax rate from 11.25% to 10.75%
MA	'96	Cut in income tax rate from 12.13% to 11.72%
NC	'96	Repeal of 1% tax surcharge
NY	'96	Cut of surcharge on tax liability from 12.5% to7.5%
CA	'97	Income tax rate reduced from 11.3% to 10.84% (through cut in underlying tax rate)
CT	'97	Cut in income tax rate from 10.75% to 10.5%
KY	'97	Increase in tax on equity from .95% to 1.1% (with some modernization of the calculation); decrease in deposit taxes from .0025% to .001%.
MA	'97	Cut in income tax rate from 11.72% to 11.32%
NC	'97	Cut in income tax rate from 7.75% to 7.5%
NY	'97	Cut of surcharge on tax liability from 7.5% to 2.5%
RI	'97	Increase in income tax rate from 8% to 9%; decrease in deposit tax from .0695% to .0438%
AZ	'98	Cut in income tax rate from 9% to 8%
CT	'98	Cut in income tax rate from 10.5% to 9.5%
MA	'98	Cut in income tax rate from 11.32% to 10.91%
NC	'98	Cut in income tax rate from 7.5% to 7.25%
NY	'98	Repeal of 2.5% surcharge on tax liability
RI	'98	Repeal of deposit tax
VT	'98	Increase in deposit rate from .004% to .0096%
WV	'98	Cut in equity tax from .75% to .7%
CO	'99	Cut in income tax rate from 5% to 4.75%
CT	'99	Cut in income tax rate from 9.5% to 8.5%
KS	'99	Cut in income tax rate from 4.25% to 2.25% (with 2.125% surtax as before)
MA	'99	Cut in income tax rate from 10.91% to 10.5%
MI	'99	Cut in income-based tax rate from 2.3% to 2.29% with further decreases during the year; interest was deductible only for banks under this tax
NC	'99	Cut in income tax rate from 7.25% to 7%
NH	'99	Increase in income tax rate from 7% to 8%
OH	'99	Cut in equity tax rate from 1.5% to 1.4%

Appendix A. Description of tax changes (continued)

AZ	'00	Cut in income tax rate from 8% to 7.968%
CO	'00	Cut in income tax rate from 4.75% to 4.63%
CT	'00	Cut in income tax rate from 8.5% to 7.5%
MI	'00	Cut in income-based tax rate from 2.29% to 2.19% with further decreases during the year; interest was deductible only for banks under this tax
NC	'00	Cut in income tax rate from 7% to 6.9%
OH	'00	Cut in equity tax rate from 1.4% to 1.3%
AL	'01	Increase in income tax rate from 6% to 6.5%
AZ	'01	Cut in income tax rate from 7.968% to 6.968%
ID	'01	Cut in income tax rate from 8% to 7.6%
MI	'01	Cut in income-based tax rate from 2.19% to 2.09% with further decreases during the year; interest was deductible only for banks under this tax
NH	'01	Increase in income tax rate from 8% to 8.5%
NY	'01	Cut in income tax rate from 9% to 8.5%
MI	'02	Cut in income-based tax rate from 2.09% to 1.99% with further decreases during the year; interest was deductible only for banks under this tax
NY	'02	Cut in income tax rate from 8.5% to 8%
AR	'03	Introduction of 3% tax surcharge on tax liability
CT	'03	Introduction of 20% tax surcharge on tax liability
MI	'03	Cut in income-based tax rate from 1.99% to 1.9%; interest was deductible only for banks under this tax
NY	'03	Cut in income tax rate from 8% to 7.5%
TN	'03	Increase in income tax rate from 6% to 6.5%
AR	'04	Cut of equity tax from .3 to .27%
CT	'04	Increase in tax surcharge on tax liability to 25%
DC	'04	Increase in income tax rate from 9.5 to 9.975 percent; repeal of surcharges (leaving total rate virtually unchanged)
AR	'05	Repeal of 3% tax surcharge
CT	'06	Cut in tax surcharge from 25% to 20%
NJ	'07	Introduction of 4% tax surcharge on tax liability
NY	'07	Cut in income tax rate from 7.5% to 7.1%
WV	'07	Cut in income tax rate from 9% to 8.75%; Cut in equity tax from .7% to .55%
CT	'08	Repeal of 20% tax surcharge
MD	'08	Increase in income tax rate from 7% to 8.25%
MI	'08	Change in bank taxation from 1.9% income-based tax to .235% tax on equity; 27.7% surcharge on latter tax
TX	'08	Abolition of income tax, replaced with gross receipts tax (with interest deductibility for banks but not other companies)

Appendix A. Description of tax changes (continued)

MI	'09	Cut in surcharge on equity tax from 27.7% to 23.4%.
NC	'09	Introduction of 3% tax surcharge on tax liability
OR	'09	Increase in income tax rate from 6.6% to 7.9%
WV	'09	Cut in rate from 8.75% to 8.5%; Cut in equity tax from .55% to .48%
MA	'10	Cut in income tax rate from 10.5% to 10.0%
NJ	'10	Repeal of 4% tax surcharge
WV	'10	Cut in equity tax from .48% to .41%
IL	'11	Increase in top corporate income tax rate from 4.8% to 7%
NC	'11	Repeal of 3% tax surcharge
OK	'11	Moratorium on franchise tax, effectively reducing marginal tax on bank capital from .125% to 0%
OR	'11	Reduction in top corporate income tax rate from 7.9% to 7.6%
WV	'11	Reduction in equity tax rate from .41% to .34%
CT	'12	Unscheduled two-year extension of tax surcharge on tax liability and increase to 20%
ID	'12	Reduction in top corporate income tax rate from 7.6% to 7.4%
WV	'12	Reduction in equity tax rate from .34% to .27%

Appendix B. Variable definitions

Variable	Description & comment	Data source
Budget balance	(State's general revenues and general expenditures scaled by its general expenditures.	U.S. Census Bureau State & Local Finances database[a]
Democratic governor	A dummy indicating that the state's governor is affiliated with the Democratic Party.	*The Book of the States*[b]
Equity capital ratio	The ratio between total equity capital and total assets	Call reports
GSP real growth rate	Real growth in gross state product	U.S. Bureau of Economic Analysis
Liabilities	Total assets minus equity	Call reports
Liquidity ratio	The ratio of liquid to total assets, calculated as in Acharya and Mora (2015).	Call reports
Non-agency MBS ratio	The ratio of mortgage backed securities not backed by the GSEs (measured at amortized cost when available) to total assets.	Call reports[c]
Rating downgrade	A dummy indicating that the state's bonds were downgraded by Standard & Poor's or Moody's	S & P and Moody's web sites
Return on assets (ROA)	Net income divided by total assets.	Call reports
Risk-weighted to total assets	The ratio between risk-weighted assets[d] and total assets	Call reports
Tier 2 capital to total assets	The ratio of Tier 2 capital[d] to total assets	Call reports
Unemployment rate	The state unemployment rate	U.S. Bureau of Labor Statistics
Union membership	The fraction of private-sector employees in a state who belong to a labor union.	Hirsch and Macpherson (2003)[e]

[a] Available at www.census.gov/govs/local.

[b] Available at http://knowledgecenter.csg.org.

[c] I follow the method described by the Federal Reserve at www.chicagofed.org/digital_assets/others/banking/financial_institution_reports/fr_y9c_time_series_security_items.pdf to map the data items into new items created in year 2009.

[d] Since Tier 2 capital and risk weighted assets are not available before 1996, for earlier years I use the approximation suggested by Ken Kuttner at www.chicagofed.org/~/media/others/banking/financial-institution-reports/regulatory-capital-pdf.pdf?la=en

[e] As updated on their website, www.unionstats.com.

Table 3.1. Summary statistics

See Appendix B for variable definitions. Effective taxes take into account the deductibility of state taxes at the federal level, and the deductibility of federal taxes in some states.

Panel A. Bank characteristics						
			Percentiles			
	Mean	St. dev.	10th	50th	90th	N
Equity capital ratio (%)	10.42	4.51	7.19	9.5	14.39	93,006
Tier 2 capital to assets ratio (%)	0.83	0.49	0.48	0.78	1.05	93,006
Total assets ($M)	520.2	1696.7	38.95	121.49	736.3	93,006
Return on assets (%)	0.01	0.02	0	0.01	0.02	93,006
Liquidity ratio (%)	0.27	0.15	0.1	0.25	0.46	93,006
Risk weighted to total assets ratio (%)	63.6	21.4	42.97	67.14	83.5	93,006
Non-agency MBS ratio (%)	0.18	0.76	0	0	0.18	93,006
Number of banks						10,823
Number of bank holding companies						7,141

Panel B. State characteristics						
			Percentiles			
	Mean	St. dev.	10th	50th	90th	N
Effective bank income tax (%)	38.63	2.11	35	38.9	41.11	969
Effective bank equity tax (basis points)	8.52	21.27	0.0	0.0	19.5	969
Effective bank deposit tax (b.p.)	0.03	0.29	0.0	0.0	0.0	969
Debt bias (%)	40.54	4.29	35.65	39.94	43.61	969
Budget balance (%)	12.15	14.77	-1.89	8.57	31.67	918
Union membership rate (%)	7.67	3.93	3.1	7	13.6	969
Unemployment rate (%)	5.54	1.95	3.4	5.2	8.3	969
GSP real growth rate (%)	1.6	2.55	-1.45	1.69	4.59	969
Democratic governor	0.44					969
Rating downgrade	0.05					969

Table 3.2. Determinants of tax changes (continued on next page)

Panel A. Regression evidence

The dependent variables are changes in different measures of state tax rates, expressed in basis points. The prefix positive (negative) indicates that the variable takes the value of the change if it is positive (negative) and zero otherwise. See Appendix B for variable definitions. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Dep. var:	$\Delta\tau_{income}$ (1)	$\Delta\tau_{equity}$ (2)	$\Delta\tau_{deposits}$ (3)	Δ*debt-bias* (4)	Positive $\Delta\tau_{income}$ (5)	Negative $\Delta\tau_{income}$ (6)	Positive $\Delta\tau_{equity}$ (7)	Negative $\Delta\tau_{equity}$ (8)	Positive $\Delta\tau_{deposits}$ (9)	Neg. $\Delta\tau_{deposits}$ (10)	Pos. Δ*debt-bias* (11)	Neg. Δ*debt-bias* (12)
GSP growth rate$_{t-1}$	-41.2	1.26	-0.050	-11.8	3.56	-44.8**	0.90	0.35	0.0052	-0.055	20.2	-32.0
	(31.6)	(1.24)	(0.063)	(40.5)	(23.7)	(17.1)	(0.82)	(0.93)	(0.0059)	(0.062)	(26.2)	(27.4)
Unemployment rate$_{t-1}$	-0.32	0.044*	-0.0014	0.71	-0.025	-0.30	0.023	0.022	-0.00009	-0.0013	0.39	0.32
	(0.29)	(0.025)	(0.0013)	(0.55)	(0.18)	(0.26)	(0.020)	(0.016)	(0.0001)	(0.0013)	(0.37)	(0.46)
Democrat governor$_{t-1}$	0.85	0.0052	0.0043	0.87	-0.29	1.14	0.070	-0.065	0.0012	0.0032	0.97	-0.10
	(0.80)	(0.091)	(0.0087)	(2.27)	(0.72)	(0.87)	(0.044)	(0.077)	(0.0011)	(0.0086)	(1.15)	(2.03)
Budget balance$_{t-1}$	-15.2**	-0.21	-0.025	-19.4**	-2.09	-13.1**	-0.15	-0.057	-0.0030	-0.022	-4.53	-14.8**
	(6.02)	(0.17)	(0.036)	(7.44)	(1.68)	(5.99)	(0.11)	(0.15)	(0.0031)	(0.036)	(2.83)	(7.09)
Rating downgr.$_{t-1}$	0.77	0.47	0.0038	11.3	2.45	-1.69	0.41	0.063	0.00018	0.0036	8.87	2.38*
	(4.12)	(0.41)	(0.0030)	(7.25)	(3.09)	(2.60)	(0.41)	(0.049)	(0.00031)	(0.0030)	(7.08)	(1.28)
Union membership$_{t-1}$	-3.72	-0.72	-0.089	-17.8	4.95	-8.68	0.67	-1.38	-0.013	-0.075	17.9	-35.7
	(13.1)	(1.26)	(0.069)	(30.3)	(5.46)	(12.5)	(0.57)	(1.11)	(0.013)	(0.067)	(11.6)	(28.0)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	918	918	918	918	918	918	918	918	918	918	918	918
R^2 (%)	4.59	3.79	2.74	3.61	2.18	5.93	4.14	2.69	2.37	2.91	4.24	3.51

Table 3.2. Determinants of tax changes (continued)

Panel B. Coincidence of changes in taxes on banks and other firms

This table shows the number of years when the respective taxes were decreased, increased or unchanged. The changes in corporate tax rates are from Heider and Ljungqvist (2015).

		Bank income tax rate change			*Bank equity tax rate change*			*Bank deposit tax rate change*			*Bank debtbias change*			
		+	-	±0	+	-	±0	+	-	±0	+	-	±0	*Total*
General corporate tax rate change	Increase	30	15	0	3	42	0	0	45	0	31	14	0	*45*
	Decr.	20	778	3	7	792	2	3	797	1	27	768	6	*801*
	±0	1	8	12	0	20	1	0	21	0	0	8	13	*21*
	Total	*51*	*801*	*15*	*10*	*854*	*3*	*3*	*863*	*1*	*58*	*790*	*19*	*867*

Table 3.3. The effect of taxes on capital structure

This table shows the effect of taxes on the equity capital ratio, which is the ratio between total equity capital and total assets expressed as a percentage. τ measures the effective tax rate on the respective tax base, weighted by the bank's deposits in the respective state. *debtbias* is the tax benefit of debt (specifically: deposit) funding compared to equity, scaled by the interest rate. *AT* is total assets, *ROA* is net income over total assets, and *liqratio* is the ratio of liquid to total assets; these control variables are winsorized at the 1% and 99% level. See Appendix B for closer definitions. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Dependent variable: change in equity capital ratio (ppt)				
	(1)	(2)	(3)	(4)
$\Delta\tau_{income}$	-14.9**	-15.3***	-15.6***	
	(5.65)	(5.48)	(5.39)	
$\Delta\tau_{equity}$		-54.8	-77.9*	
		(48.2)	(46.1)	
$\Delta\tau_{deposits}$			-642.6	
			(402.5)	
$\Delta debtbias$				-6.39
				(4.05)
$\Delta \ln AT_{t-1}$	-1.13***	-1.13***	-1.13***	-1.13***
	(0.23)	(0.23)	(0.23)	(0.23)
ΔROA_{t-1}	-9.72***	-9.72***	-9.73***	-9.71***
	(3.31)	(3.31)	(3.31)	(3.31)
$\Delta liqratio_{t-1}$	4.61***	4.61***	4.61***	4.61***
	(0.55)	(0.55)	(0.55)	(0.55)
Year FE	Yes	Yes	Yes	Yes
N	93,006	93,006	93,006	93,006
R^2 (%)	5.38	5.38	5.38	5.37

Table 3.4. The effects of taxes on different funding sources

Columns 1-4 and 5-8 show the effects of taxes on the logs of equity and liabilities, respectively. Columns 9-12 show the effect of taxes on Tier 2 capital to total assets (expressed as a percentage). τ measures the effective tax rate on the respective tax base, weighted by the bank's deposits in the respective state. *debtbias* is the tax benefit of debt (specifically: deposit) funding compared to equity, scaled by the interest rate. Bank level controls include the log of total assets, ROA, and liquidity ratio, winsorized at the 1% and 99% level. See Appendix B for closer definitions. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Dependent var.:	Δlog equity				Δlog liabilities				ΔTier 2 capital to total assets (ppt)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\Delta\tau_{income}$	-2.17**	-2.33***	-2.38***	-2.17**	-0.42	-0.51	-0.51		1.39**	1.32*	1.33*	
	(0.94)	(0.83)	(0.81)	(0.94)	(0.64)	(0.60)	(0.61)		(0.66)	(0.67)	(0.67)	
$\Delta\tau_{equity}$		-20.5	-25.2			-11.3	-11.4			-9.36	-8.56	
		(13.1)	(15.1)			(8.23)	(10.3)			(7.96)	(9.36)	
$\Delta\tau_{deposits}$			-128.9				-3.09				22.4	
			(95.6)				(65.6)				(49.9)	
$\Delta debtbias$								-0.49				0.15
								(0.32)				(0.40)
Bank level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	93,006	93,006	93,006	93,006	93,006	93,006	93,006	93,006	93,006	93,006	93,006	93,006
R^2 (%)	11.6	11.6	11.6	11.6	6.06	6.07	6.07	6.06	4.43	4.43	4.43	4.42

Table 3.5. Asymmetric treatment effects

This table tests if banks react differently to negative and positive tax changes. Variables with the prefix "positive" measure the change, conditional on it being positive. Equity capital ratio is the ratio between total equity capital and total assets, expressed as a percentage. τ measures the effective tax rate on the respective tax base, weighted by the bank's deposits in the respective state. *debtbias* is the tax benefit of debt (specifically: deposit) funding compared to equity, scaled by the interest rate. Bank level controls include the log of total assets, ROA, and liquidity ratio, winsorized at the 1% and 99% level. See Appendix B for closer definitions. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Dependent variable: change in equity capital ratio (ppt)				
	(1)	(2)	(3)	(4)
$\Delta\tau_{income}$	-13.8*	-14.5*	-15.0**	
	(7.69)	(7.46)	(7.32)	
$\Delta\tau_{equity}$		-28.9	-28.8	
		(64.2)	(64.2)	
$\Delta\tau_{deposits}$			-939.8**	
			(429.4)	
Δ*debtbias*				-9.30
				(6.15)
Positive $\Delta\tau_{income}$	-4.74	-4.01	-3.12	
	(15.3)	(15.2)	(14.8)	
Positive $\Delta\tau_{equity}$		-34.3	-88.9	
		(106.5)	(103.9)	
Positive $\Delta\tau_{deposits}$			16719.1***	
			(960.9)	
Positive Δ*debtbias*				6.05
				(7.36)
Bank level controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	93,006	93,006	93,006	93,006
R^2 (%)	5.38	5.38	5.38	5.38

Table 3.6. Heterogeneous treatment effects

This table shows the effect of taxes depending on bank size (column 1; measured as total assets) and capital strength (column 2; measured as equity capital ratio). The sample is split into three terciles depending on the level of the respective conditioning variable in the first year the bank enters the sample. The variables *Top*, *Middle*, and *Bottom* are dummies for being in the respective tercile. $\Delta\tau_{income}$ is the effective income tax rate, weighted by the bank's deposits in the respective state. *AT* is total assets, *ROA* is net income over total assets, and *liqratio* is the ratio of liquid to total assets; these control variables are winsorized at the 1% and 99% level. See Appendix B for closer definitions. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Dependent variable: ΔEquity capital ratio (ppt)		
Conditioning variable to form terciles:	Total assets (1)	Equity capital ratio (2)
$\Delta\tau_{income}$×Bottom tercile	-17.1***	-14.5
	(6.35)	(13.2)
$\Delta\tau_{income}$×Top tercile	0.86	-12.9**
	(9.54)	(5.24)
$\Delta\tau_{income}$	-11.2*	-13.4**
	(5.73)	(5.87)
Bottom tercile	-0.17***	0.25***
	(0.032)	(0.029)
Top tercile	0.19***	0.12***
	(0.025)	(0.023)
$\Delta\ln AT_{t-1}$	-0.12***	0.18***
	(0.027)	(0.021)
ΔROA_{t-1}	-1.12***	-1.14***
	(0.23)	(0.23)
$\Delta liqratio_{t-1}$	-9.63***	-9.53***
	(3.31)	(3.32)
Year FE	Yes	Yes
N	93,006	93,006
R^2 (%)	5.46	5.50

Table 3.7. Timing of adjustment, and robustness tests

This table tests the robustness to including leads and lags of tax changes (columns 1-4), and to including control variables for macro conditions (cols. 5-8). Unemployment, GSP growth, budget balance, and tax rates are weighted by the bank's deposits in the respective state. *Bank level controls* include the log of total assets, ROA, and liquidity ratio, winsorized at the 1% and 99% level. See Appendix B for closer definitions. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

	Dependent variable: change in equity capital ratio (ppt)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta\tau_{income;t+1}$	-8.49**	-8.60**	-8.48**					
	(3.80)	(3.79)	(3.84)					
$\Delta\tau_{income}$	-12.2***	-12.6***	-12.9***		-14.6**	-14.9***	-15.1***	
	(3.97)	(3.75)	(3.68)		(5.59)	(5.49)	(5.41)	
$\Delta\tau_{income;t-1}$	-8.79	-9.17	-10.0					
	(7.16)	(6.84)	(6.38)					
$\Delta\tau_{equity;t+1}$		-23.6	-13.4					
		(27.9)	(37.8)					
$\Delta\tau_{equity}$		-55.1	-75.0			-44.8	-67.7	
		(47.7)	(47.7)			(48.3)	(46.8)	
$\Delta\tau_{equity;t-1}$		-42.8	-117.3					
		(107.7)	(94.7)					
$\Delta\tau_{deposits;t+1}$			150.6					
			(359.8)					
$\Delta\tau_{deposits}$			-670.1				-636.2	
			(435.2)				(384.9)	
$\Delta\tau_{deposits;t-1}$			-1907***					
			(651.9)					
$\Delta debtbias_{t+1}$				-3.26				
				(2.08)				
$\Delta debtbias$				-5.46*				-5.92
				(2.84)				(4.06)
$\Delta debtbias_{t-1}$				-3.45				
				(4.42)				
Unemploy-ment$_{t-1}$					-0.012	-0.012	-0.011	-0.010
					(0.009)	(0.009)	(0.009)	(0.009)
GSP growth$_{t-1}$					0.80	0.80	0.81	0.89
					(1.00)	(1.00)	(1.00)	(1.00)
Budget bal-ance$_{t-1}$					-0.027	-0.029	-0.029	-0.015
					(0.10)	(0.10)	(0.10)	(0.10)
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	77,006	77,006	77,006	77,006	93,006	93,006	93,006	93,006
R^2 (%)	2.9	2.9	2.9	2.9	5.4	5.4	5.4	5.4

Table 3.8. The effect of taxes on asset risk taking

This table shows the effect of taxes on risk-taking, measured using the regulatory measure of risk-weighted to total asset ratio. τ measures the effective tax rate on the respective tax base, weighted by the bank's deposits in the respective state. *debtbias* is the tax benefit of debt (specifically: deposit) funding compared to equity, scaled by the interest rate. *AT* is total assets, *ROA* is net income over total assets, and *liqratio* is the ratio of liquid to total assets; these control variables are winsorized at the 1% and 99% level. See Appendix B for closer definitions. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Dep. var.: Δrisk-weighted to total assets ratio (ppt)				
	(1)	(2)	(3)	(4)
$\Delta\tau_{income}$	22.1	21.1	20.7	
	(39.0)	(39.5)	(39.5)	
$\Delta\tau_{equity}$		-125.3	-166.8	
		(152.9)	(181.2)	
$\Delta\tau_{deposits}$			-1153.2	
			(1301.0)	
Δ*debtbias*				3.29
				(14.6)
$\Delta\ln AT_{t-1}$	1.67***	1.67***	1.67***	1.67***
	(0.37)	(0.37)	(0.37)	(0.37)
ΔROA_{t-1}	25.0***	24.9***	24.9***	24.9***
	(7.64)	(7.64)	(7.64)	(7.64)
$\Delta liqratio_{t-1}$	1.27*	1.27*	1.27*	1.27*
	(0.75)	(0.75)	(0.75)	(0.75)
Year FE	Yes	Yes	Yes	Yes
N	93,006	93,006	93,006	93,006
R^2 (%)	61.7	61.7	61.7	61.7

Table 3.9. Taxes, MBS holdings and asset risk taking

This table shows how tax changes affect banks' holding of non-agency mortgage backed securities (cols. 1-4) and the riskiness of their asset using the regulatory measure of risk-weighted to total asset ratio (cols. 5-8). τ measures the effective tax rate on the respective tax base, weighted by the bank's deposits in the respective state. *debtbias* is the tax benefit of debt (specifically: deposit) funding compared to equity, scaled by the interest rate. Bank level controls include the log of total assets, ROA, and liquidity ratio, winsorized at the 1% and 99% level. See Appendix B for closer definitions. *Year2001-'06* is a dummy that takes the value 1 in said years; it cannot be interacted with the changes in deposit taxes since there are no such changes in those years. OLS estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Dep. var:	Δnon-agency MBS to assets ratio (ppt)				Δrisk-weighted to total assets ratio(ppt)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta\tau_{income}$	0.94	0.96	0.88		15.4	14.5	14.2	
	(0.85)	(0.86)	(0.87)		(34.6)	(35.1)	(35.1)	
$\Delta\tau_{income}\times$ *Year2001-'06*	0.64	0.62	0.70		179.9**	180.5**	180.9**	
	(4.12)	(4.11)	(4.12)		(89.4)	(89.2)	(89.1)	
$\Delta\tau_{equity}$		2.74	-5.38			-99.0	-134.6	
		(11.8)	(14.1)			(143.2)	(167.0)	
$\Delta\tau_{equity}\times$ *Year2001-'06*		187.5***	195.6***			-8330***	-8294***	
		(45.9)	(47.5)			(492.6)	(497.1)	
$\Delta\tau_{deposits}$			-224.1				-984.0	
			(207.4)				(1265.5)	
Δ*debtbias*				0.41				1.77
				(0.52)				(12.9)
Δ*debtbias*× *Year2001-'06*				2.48				84.5
				(3.41)				(154.1)
Bank-level controls & Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	93,006	93,006	93,006	93,006	93,006	93,006	93,006	93,006
R^2(%)	0.42	0.42	0.42	0.42	61.7	61.7	61.7	61.7
p-value of H_0: $\Delta\tau_{income}+\Delta\tau_{income}\times Year2001\text{-}'06=0$	0.715	0.714	0.714		0.052	0.052	0.052	
p-value of H_0: $\Delta\tau_{equity}+\Delta\tau_{equity}\times Year2001\text{-}'06=0$		0.00	0.00			0.00	0.00	

Table 3.10. The effect of taxes on bank survival (continued on next page)

Panel A. Classifications of years into normal, crisis, and pre-crisis

Year	'96-'97	'98	'99	'00-'01	'02-'03	'04	'05-'06	'07-'09
Def.	Pre-crisis	Crisis	Pre-crisis	Crisis	Pre-crisis	Normal	Pre-crisis	Crisis
Crisis note	Russian debt crisis, LTCM default		dot.com bust, 9/11 terrorist attack				Subprime lending crisis	

Panel B. Regression parameters

This table shows the coefficient estimates from a regression of bank survival on taxes. *Surv* is a dummy set to 1 if the bank remains in the sample or leaves because of a merger with another bank under the same bank holding company. *Crisis* is a dummy for crisis years, as defined in Panel A. Taxes and control variables are measured as averages in the pre-crisis years for the respective crisis/normal period. τ are effective tax rates; *debtbias* is the tax benefit of debt (specifically: deposit) funding compared to equity, scaled by the interest rate. Macro controls include state Unemployment, GSP growth, and budget balance; these and tax rates are weighted by the bank's deposits in the respective state. Bank level controls include the log of total assets, ROA, and liquidity ratio, winsorized at the 1% and 99% level. See Appendix B for closer definitions. Logit estimation; heteroskedasticity-consistent standard errors clustered at the state level are in parentheses. Statistical significance at the 10% level is denoted by *.

	Dependent variable: $\ln(\Pr(Surv_{it})/\Pr(1\text{-}Surv_{it}))$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta\tau_{income}$	-3.51	-4.12*	-4.05*		-1.75	0.017	0.091	
	(2.20)	(2.43)	(2.46)		(3.25)	(3.82)	(3.87)	
$\Delta\tau_{equity}$		-7.60	-8.20			26.3	25.0	
		(22.1)	(22.2)			(41.2)	(42.1)	
$\Delta\tau_{deposits}$			750.0				783.0	
			(826.7)				(1698.8)	
Δ*debtbias*				0.19				1.35
				(1.00)				(1.83)
Crisis $\times\Delta\tau_{income}$					-3.41	-5.75	-5.94	
					(3.49)	(4.76)	(4.78)	
Crisis $\times\Delta\tau_{equity}$						-34.0	-31.8	
						(38.3)	(39.0)	
Crisis $\times\Delta\tau_{deposits}$							-2389.9	
							(3634.8)	
Crisis $\times\Delta$*debtbias*								-0.99
								(1.59)
Crisis					0.65	1.59	1.67	-0.26
					(1.36)	(1.89)	(1.90)	(0.69)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Macro- & bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	31,831	31,831	31,831	31,831	31,831	31,831	31,831	31,831
Pseudo R^2(%)	4.31	4.32	4.32	4.26	5.28	5.3	5.3	5.22

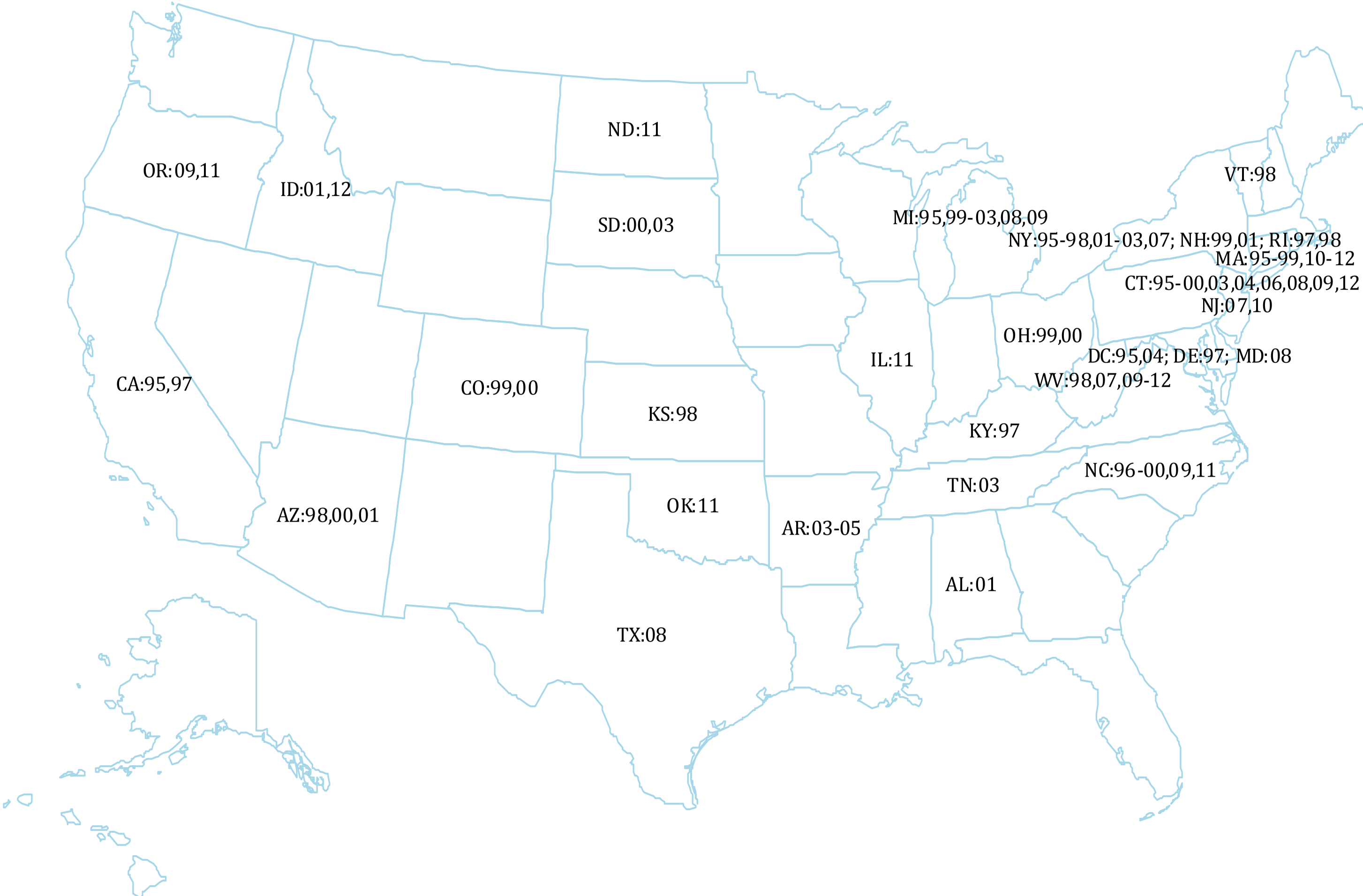
Table 3.10. The effect of taxes on bank survival (continued)

Panel C. Marginal effects of taxes on survival probability

This table shows the marginal effects on survival probabilities in crisis and normal years, using the classification in Panel A above. The marginal effects are calculated from the estimates presented in Panel B. Marginal effects are evaluated at the mean levels of the variables (other than the Crisis variable), calculated using the method of Norton, Wang, and Ai (2004). Crisis is a dummy for crisis years as per the classification in Panel A. Taxes and control variables are measured as the averages in the pre-crisis years for the respective crisis/normal period. τ are effective tax rates; *debtbias* is the tax benefit of debt (specifically: deposit) funding compared to equity, scaled by the interest rate. Statistical significance at the 10% level is denoted by *.

Tax variable	*State*	(1)	(2)	(3)	(4)
$\Delta\tau_{income}$	Normal	-0.08	-0.07	-0.07	
		(0.15)	(0.14)	(0.14)	
	Crisis	-0.32*	-0.30*	-0.29*	
		(0.15)	(0.17)	(0.17)	
$\Delta\tau_{equity}$	Normal		0.19	0.18	
			(1.10)	(1.11)	
	Crisis		0.25	0.24	
			(1.44)	(1.46)	
$\Delta\tau_{deposits}$	Normal			9.73	
				(46.51)	
	Crisis			12.79	
				(60.99)	
$\Delta debtbias$	Normal				0.06
					(0.09)
	Crisis				0.02
					(0.06)

Figure 3.1. Geography of state tax changes, 1994-2010



Note: Numbers indicate years when bank taxation was changed