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Non-Regulatory Incentives and Bank Behavior: the Stock-market, Taxes and Social Capital

José Maria Martin Flores

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ED 559

Non-Regulatory Incentives and Bank Behavior: the Stock-market, Taxes and Social Capital

THESE

En vue de l'obtention du
DOCTORAT ÈS SCIENCES DE GESTION

Par

José María MARTIN FLORES

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JURY

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Non-Regulatory Incentives and Bank Behavior: the Stock-market, Taxes and Social Capital
RESUME GENERAL (en français)

Après la crise financière de 2007-2009, les débats tant universitaires que politiques se sont focalisés sur la détermination des causes de la vulnérabilité des banques aux chocs et sur les moyens d'accroître la résilience du système bancaire. Une conséquence importante de la crise est la perte de confiance dans le secteur financier, à laquelle les actes répréhensibles des banques ont largement contribué. Comprendre les déterminants des actes déviants dans les banques est également devenu un enjeu central dans l'étude des institutions financières. Dans cette thèse, nous étudions comment certaines incitations non-réglementaires auxquelles sont soumises les banques peuvent influencer leur prise de risque et leur résistance aux chocs, ainsi que l'adoption de comportement déviants et frauduleux. Dans le premier chapitre de cette thèse, nous nous intéressons à l'impact du marché boursier sur la résistance des banques en temps de crise. Comme pour les banques cotées, nous observons que les banques privées qui sont vulnérables dans une crise financière tendent à le rester dans la crise suivante. Toutefois, pour les banques privées, il existe également un groupe de banques résilientes à des chocs successifs. L'examen des banques devenant cotées (par introduction en bourse ou acquisition par un groupe coté) révèle que les banques davantage exposées à des investisseurs institutionnels de court terme tendent à accroître leurs risques. Les banques résilientes aux chocs quand elles étaient privées, deviennent alors plus vulnérables aux crises lorsqu'elles sont soumises à certaines pressions à court terme du marché. Dans le deuxième chapitre de la thèse, nous nous intéressons aux incitations procurées par le système fiscal sur le niveau de capital des banques, qui est un facteur essentiel de leur résistance en cas de choc. Nous examinons l'impact d'un changement fiscal visant à réduire le traitement fiscal déséquilibré entre capitaux propres et dettes, en ce qui concerne la déductibilité de leurs coûts respectifs. Nous observons que l'octroi d'un bénéfice fiscal sur les capitaux propres marginaux des banques induit une augmentation de leurs fonds propres. La suppression de cette mesure

induit chez les banques un comportement inverse de réduction du capital. Ces résultats révèlent que le cadre fiscal est un facteur incitatif important des banques à être plus ou moins capitalisées, au-delà des contraintes réglementaires. Dans le troisième article de la thèse, nous nous intéressons aux déterminants des comportements déviants dans le secteur bancaire. Nous étudions comment l'environnement social et culturel, en particulier le niveau de capital social (mesuré par l'adhésion aux normes civiques et la densité des réseaux sociaux) peut influencer le comportement des banques. Nous mettons en évidence le fait que le capital social réduit la probabilité qu'une banque commette une fraude. Nous montrons également qu'une fois que la faute est révélée, les banques ont tendance à perdre davantage de parts de marché dans les zones géographiques caractérisées par des niveaux de capital social plus élevés. Dans ce travail, nous mettons ainsi en évidence que l'environnement des banques est un élément central de leur appétence au risque, leur stabilité et leur probité. Des éléments aussi divers que la pression exercée par le marché boursier, le cadre fiscal et l'environnement social et culturel jouent un rôle important sur le comportement bancaire.

Non-Regulatory Incentives and Bank Behavior: the Stock-market, Taxes and Social Capital
GENERAL ABSTRACT (in English)

Following the 2007-2009 financial crisis, the academic and policy debate has revolved around determining the causes that make banks vulnerable to shocks and how to increase the resilience of the banking system. An important consequence of the crisis is the decline of trust in the financial sector, something to which bank wrongdoing has significantly contributed. As such, understanding the factors that determine wrongdoing in banks has also become a central issue in the debate about financial institutions. In this dissertation, we study how non-regulatory incentives that banks face may influence banks' risk-taking and resilience to shocks, as well as misconduct. In the first chapter of this thesis, we study how stock-market forces determine the persistence of bank performance across crises. In this analysis, we observe that the persistence of business models that make banks more vulnerable across crises is not a specificity of publicly held banks but also applies to privately held institutions. However, for privately held banks, there is a group of banks that perform well across crises. This result suggests that stock-market listing may have adverse effects on the ability of banks to withstand crises well. To deepen the analysis of this problem, we look at banks that make a private-to-public transition between crises. Our results indicate that, after becoming publicly held, banks more subject to influences from short-term oriented institutional investors increase risk which makes top performer banks in one crisis more vulnerable to subsequent shocks. In the second chapter of the dissertation, we shift the focus to bank capital as a factor that makes banks more resilient to economic shocks. Particularly, we focus on the extent to which the tax system may provide incentives that lead to better capitalized financial institutions. We exploit a tax change that reduces significantly the unequal tax treatment between equity and debt with respect to interests and cost of equity deductibility and show that, when provided with a tax incentive on new equity increases, banks increase their equity ratios. We also show that the increase in capital ratios does not survive the removal of the tax

incentive. When the debt-equity tax bias is reestablished, banks reduce their equity ratios downwards. These findings document that tax incentives are an important determinant of bank capital ratios beyond regulatory aspects. In the third article of the thesis, we focus on bank misconduct. In this chapter, we study how social capital can shape the behavior of bank managers. In particular, we document that social capital (defined as strength of civic norms and density of social networks in an area) is negatively related to the probability that a bank is involved in a misconduct case. Moreover, social capital exerts some discipline on wrongdoers. Once misconduct is revealed, banks tend to lose higher percentages of deposits market-share in areas characterized by higher social capital levels. In this dissertation, we thus highlight that the banks' environment is a central element of their risk appetite, stability and probability to commit misconduct. Elements as diverse as stock-market pressure, tax rules, and social capital play an important role in bank behavior.

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1. General introduction

The 2007-2009 crisis had severe costs and triggered significant losses for the economy and the society. For the US, the estimates are around \$6 to \$14 trillion of output lost. In addition to these figures, there are other costs associated with the recession such as significant losses in terms of human capital due to the increase of unemployed people and unquantifiable costs of national trauma (Atkinson et al. 2013). The last financial crisis also had negative consequences for the financial sector. The severity of the economic shock jeopardized the stability of the financial system. Some institutions (some of them large and systemic) failed or were close to failure, and the lack of trust between banks caused a freeze in the interbank market. This led to an intervention of the US Government. During the last quarter of 2008, the government provided funding to US banks through the Troubled Asset Relief Program (TARP). The Federal Reserve also contributed to the recovery taking a very accommodative policy aiming at increasing liquidity in the financial system. These consequences and actions were not exclusive of the US. Similar events also took place in other European countries that were severely affected by the financial crisis.

The financial crisis was rooted in an excessive risk-taking by financial institutions (Jordà et al. 2013). The materialization of risks accumulated before the crisis triggered important losses for banks and their shareholders during the 2007-2009 period. Following the financial crisis, there has been an increasing interest in better understanding the factors that gave rise to the excessive risk-taking and imbalances. This issue has become important not only for academics but also for policy-makers and regulators during the last years.

Another topic that has been receiving a lot of attention from academics and regulators during the aftermath of the financial crisis is how to make banks more resilient to economic and financial shocks. There is agreement that higher bank capital enhances bank stability (Thakor 2014), however, there is a vigorous debate on how to make the banking system better capitalized. While increasing capital requirements in principle makes banks safer, it may also lead to a decrease in bank lending (Jiménez et al. 2017). This may have negative consequences for firms and households. Moreover, higher capital requirements may lead to regulatory arbitrage from banks that resist to enhance bank capital ratios (Acharya and Steffen 2015, Boyson et al. 2016). As a result, studying alternative factors that contribute to higher bank capital buffers has become an important issue.

With these aspects in mind, the first objective of this dissertation is twofold: First, we intend to shed further light on the pre-crisis factors that determine bank performance during crises, and second, we intend to provide additional evidence on policies, complementary to financial regulation, that would incentivize higher capital ratios in the banking sector.

Another negative consequence of the last financial crisis, not mentioned above, is the reduction of the confidence in financial institutions and markets (Sapienza and Zingales 2012). Banks behavior have significantly contributed to this drop in trust (Zingales 2015). The events related to bank stability during the financial crisis increased the fear of bank collapse; however, government interventions and post crisis regulations have made the financial sector more resilient. Despite becoming safer, the confidence in the financial sector has fallen by half over the last decade (Gallup 2016). This is to some extent explained by the high incidence of misconduct that has involved banks during the aftermath of the financial crisis (Chaly et al. 2017, Zingales 2015). In this respect, Mark Carney, Governor of the Bank of England, stated in 2016 that “*the incidence of financial sector misconduct has risen to a level that has the potential to create systemic risks and undermining trust in both financial*

institutions and markets.”¹ This significant rise in misconduct and its persistence has made bank wrongdoing a topic of great interest for academics during the aftermath of the crisis.² As a consequence, the second objective of this dissertation is to study bank misconduct analyzing which factors, beyond regulations, may contribute to or deter the occurrence of wrongdoing and its consequences.

The approach we take in this dissertation is to study how non-regulatory incentives provided by the environment in which banks operate can impact the three outcomes mentioned above (pre-crisis factors of bank performance during crisis, incentives to increase capital in the banking sector and determinants of misconduct in banks). Banks are special because of the systemic nature of their activities (organizing the payments system, transforming assets to match short-term demand of deposits with long-term demands of debt and monitoring borrowers). Because of this, they are subject to specific regulations and supervision. Despite being largely influenced by the financial regulatory context in which they operate, regulations may have a limited outreach (Jakovljević et al. 2015), which leaves some room for other environmental factors to influence bank behavior. Specifically, in this dissertation we study the following non-regulatory environmental factors: (i) pressure/influence from the stock-market, (ii) tax incentives and (iii) social capital. Our intention is to use these three factors to address the following questions:

1. Does stock-market influence explain bank performance across crises? Does stock-market influence push banks to increase risk?

¹ Mark Carney, “Building a Resilient and Open Global Financial System to Support Sustainable Cross-Border Investment,” Financial Stability Board (August 30, 2016).

² Zingales (2015) states that “As finance academics, we should care deeply about the way the financial industry is perceived by society. Not so much because this affects our own reputation, but because there might be some truth in all these criticisms, truths we cannot see because we are too embedded in our own world. And even if we thought there was no truth, we should care about the effects that this reputation has in shaping regulation and government intervention in the financial industry. Last but not least, we should care because the positive role finance can play in society is very much dependent upon the public perception of our industry.”

2. To what extent may the tax system provide incentives that lead to a better capitalized banking system and consequently make banks more resilient?
3. To what extent cultural and social norms provided by social capital can deter bank misconduct and what is its role in punishing banks involved in wrongdoing?

I dedicate one paper to each of these questions. Each of the three chapters of this dissertation makes a contribution on its own to their respective sub-fields of the literature as it is detailed in the respective chapters. We explain in the following paragraphs what our motivations to study these environmental factors are.

Regarding the first question, there is a consensus in the academic literature that excessive risk-taking by financial institutions is one of the main causes of bad performance of banks during crises (Beltratti and Stulz 2012, Ellul and Yerramilli 2013, Moussu and Petit-Romec 2017). Fahlenbrach et al. (2012) document that bad performance of banks is persistent across crises which is driven by the fact that some banks tend to stick to riskier business models that make them more vulnerable during crisis periods. Yet, we still know relatively little on how some environmental factors give rise to such risk-taking in the first place. A strand of the literature show that executive compensation schemes that allow managers to profit from high risk during good times (Boyllian and Ruiz-Verdú 2017, Fahlenbrach and Stulz 2011) and government or implicit guarantees (Acharya and Yorulmazer 2007, Farhi and Tirole 2012) boost risk-taking. However, the extent to which influence from an environmental factor, namely the stock-market, acts on the performance of banks during economic downturns has not been studied much in this context. We try to fill this gap by studying how stock-market listing affects bank performance across crises as well as risk.

Based on short-termism models (Holmström 1999, Stein 1989, Von Thadden 1995), some papers have studied how managers of non-financial firms place higher weight on short-term

earnings which leads to short-termist decisions that may not create value in the long-run. Two important papers in this literature have focused on how publicly held firms invest substantially less and are less responsive to changes in investment opportunities than private firms (Asker et al. 2014) and how making a private-to-public transition changes firm innovation policies (Bernstein 2015). There is also some empirical evidence showing the impact of market pressures in the banking sector. Falato and Scharfstein (2016) document that banks that become publicly held either through an IPO or because they are acquired by a publicly held Bank Holding Company undertake changes towards riskier business models. They argue that their results are consistent with the hypothesis that bank managers increase bank risk to boost short-term profits once they are subject to more stock-market driven short-termism.

Building on the findings provided by this literature, in Chapter 1, we look at the different behavior of publicly and privately held banks as well as banks making a private-to-public transition. In particular, we revisit and extend Fahlenbrach et al. (2012) to study whether there is a difference between publicly and privately held banks regarding the persistence of bank performance across crises. We focus on the two worst financial crises since the Great Depression: the 1998 crisis and the 2007-08 crisis³. We find that the persistence of bank performance across crises is not a specificity of publicly held banks but also applies to privately held institutions⁴. However, this result presents some nuances. For publicly held banks, we find that the correlation between crisis performances is driven by the quintile of bottom performers. Conversely, for privately held banks, we find that the correlation between crisis performances is driven by both top and bottom performers. The latter result indicates that for private banks there is also a persistence of good performance across crises that we do

³ A short summary of these two crises can be found in the Appendix of the first chapter.

⁴ We use a matched sample of publicly and privately held banks based on the size of these institutions. We remove the smallest banks from the sample of privately held banks to keep as many larger publicly held banks as possible.

not find in publicly held banks. This result suggests that stock-market listing is detrimental to the persistence of good bank performance across crises (at least for the periods we study). In order to shed more light on this empirical evidence, we exploit the fact that some banks that were private before the 1998 crisis made a private-to-public transition between the two crises. We conjecture that if stock-market listing is detrimental to the persistence of good performance across crises, becoming publicly held should have a negative impact on bank performance during the recent financial crisis mainly for banks that were good performers in 1998. We test this conjecture using a matched sample (based on size) of banks making a private-to-public transition between the two crises and private banks. We find that transitioning banks that were top performers in the 1998 crisis significantly underperform their counterparts that remained private during the 2007-2008 crisis. This result is found using a matched sample (based on size) of transitioning banks and banks that remain private. This finding provides further supporting evidence that stock-market listing is detrimental to the persistence of good performance across crises. Further tests confirm that these results are explained by stock-market driven short-termism. When we split the group of transitioning banks between banks subject to more short-termist influence from the market (i.e. banks whose institutional shareholders trade more actively the shares they hold in their portfolio) and banks less subject to short-termist influence from the stock-market, we observe that the banks in the former group are the ones actually underperforming their privately held counterparts. Finally, we study whether going through a private-to-public transition leads to changes in bank risk. A private-to-public transition leads to significant changes in ownership structure and eases the arrival of investors with different risk preferences and horizons. In line with Falato and Scharfstein (2016), we observe that transitioning banks more exposed to institutional investors with shorter horizons (i.e. higher short-termist market influence) increase risk between crises. We find that these banks had similar levels of non-deposit debt,

capital, asset growth and loan growth than top performers privately held banks before the 1998 crisis. However, before the 2007-2008 crisis, ‘transitioning’ banks more subject to stock-market driven short-termism have lower capital, higher asset and loan growth than their counterparts that remain private or their counterparts that make a private-to-public transition but are less subject to short-termist influences from the stock-market.

Regarding the second question we address in this dissertation, since the last 2007-2009 financial crisis, regulators have targeted bank capital ratios and imposed higher capital requirements on banks. However, a source of weakness, namely the tax system, subsidizes debt while taxing safer bank equity. This provides an incentive in a direction that goes against financial regulation (Thakor 2014). The debt-equity tax bias has been widely studied in the corporate finance literature since Modigliani and Miller (1963). The empirical papers in this field show that debt-tax-shields are an important determinant of non-financial firms capital structure (e.g. Faccio and Xu 2015, Feld et al. 2013, Graham 1996, 2003, Heider and Ljungqvist 2015). There is nothing special about banks in this regard as they are also subject to corporate income taxes. In this respect, the magazine *The Economist*⁵ reported the following: *“Firms without significant equity buffers are more likely to go broke, banks more likely to topple ... Leveraged global banks notched up losses of \$2 trillion in 2007-10 and the world economy imploded. Financial regulators have already gone some way to redressing the balance from debt by forcing the banks to fund themselves with more equity. But the bias remains—in large part because of the subsidy for debt. Under a more neutral tax system, firms would sell more equity and carry less debt. Investors would have to get used to greater volatility; but as equity buffers got thicker, shareholders would be taking less risk.”* This article argues that the effect of the tax bias is important for bank stability, and because banks are more systemic than non-financial firms, the unequal tax treatment between equity and

⁵ The great distortion, *The Economist*, May 16 2015, <https://www.economist.com/leaders/2015/05/16/the-great-distortion>

debt is particularly pernicious for them. Since bank capital provides some protection against failure in case of crisis, a tax bias that incentivizes the use of debt undermines to some extent the safeness of the banking system.

Studying the impact of the debt-equity tax bias in the banking sector is, therefore, important⁶. Academic papers have studied how the design of the tax system increases the probability of financial crises (De Mooij et al. 2013), reduces bank equity ratios (De Mooij and Keen 2016, Schandlbauer 2017), increases the tendency of banks to distribute dividends (Hemmelgarn and Teichmann 2014) and subsidizes risk-taking (Andries et al. 2017). Other papers have focused on alternative designs of the tax system that reduce the debt-equity tax bias. Schepens (2016) provides evidence on how the introduction of an equity tax-shield in Belgium (namely Allowance for Corporate Equity – ACE-) leads to an increase of bank equity ratios by around 1 percentage point. Célérier et al. (2018) provide empirical evidence showing that a reduction of the equity-tax bias leads to more bank lending (with unchanged levels of risk). Roe and Troege (2017) argue that banks should be subject to a special tax treatment that avoids the tax subsidy of leverage. They propose an ACE-like tax mechanism that grants deductibility for corporate income tax purposes of a notional interest computed on the portion of equity exceeding the minimum capital required.

Building on the above mentioned implications of the debt-tax bias for banks and due to its importance in the policy and academic debate, in Chapter 2 we investigate the extent to which bank equity ratios are responsive to tax incentives on marginal equity (i.e. new equity via capital increases or higher earnings retention) and whether the outcome of such a tax incentives lasts once the tax incentive is removed. Specifically, we exploit the implementation and removal of a tax reform in Italy that grants a tax shield on equity to banks that increase

⁶There have been other policy initiatives aiming at taxing banks via tax levies. This subject goes beyond to the questions addressed in this dissertation. Some references that study this issue are Buch et al. (2016), Capelle-Blancard and Havrylchuk (2017), Devereux et al. (2017)

their equity from a reference year. This tax shield on marginal equity reduces the unequal treatment between debt and equity that the corporate income tax systems provided before the reform. Banks (and other firms) apply a reduced tax rate (19% instead of 37%) on a notional return computed on equity increases. Using a difference-in differences setting, with Italian banks as treatment group and a matched sample of banks from other euro area states as control group⁷, we find that banks subject to this reform increase their capital ratios by around 0.6 percentage points relative to the control group. This result is consistent with Schepens (2016) who shows that an ACE-like tax shield applied on existing and new equity implemented in Belgium leads to an increase in bank equity ratios. The main criticisms to the system implemented in Belgium are that it has a high cost for the public finances of the country due to the high amount of tax proceeds lost because of the notional interest deductibility (Zangari 2014) and that it allows for tax planning opportunities for business groups (Hebous and Ruf 2017). Regarding the cost for public finances, our paper contributes to the academic and policy debate in this field showing that a ‘softer’ version of an ACE mechanism based on marginal equity increases (which in principle should be less costly for a government to implement)⁸ also leads to better capitalized banks. With respect to tax planning opportunities, the Italian ACE scheme includes an anti-avoidance clause that targets transactions between related parties in order to avoid abuses and tax planning (Zangari 2014). Therefore, another interesting contribution provided by the first set of results in Chapter 1 is that a positive effect on bank capital is also found in an ACE system that limits the scope for tax planning opportunities.

⁷ This setting is similar to the ones used by Devereux et al. (2017) and Schepens (2016). We execute several tests using different control groups to make sure of the robustness of our results.

⁸ Academic papers discussing this topic show that ACE mechanisms on marginal equity have, at least in the short-run, a lower impact in public finances in terms of tax revenue lost (Bordignon et al. 2001, De Mooij and Devereux 2010, Panteghini et al. 2012, Zangari 2014).

The Italian ACE tax incentive was withdrawn in 2002 which allows us to provide new evidence on whether the increase in equity ratios survives after the removal of the tax incentive. We find that that once the ACE mechanism is no longer applicable, banks stop increasing their equity and readjust downward their capital ratios relative to the control group. This finding shows that contrary to incentives to increase leverage (Heider and Ljungqvist 2015), there is no hysteresis in bank capital when a tax incentive to increase equity is removed. This is in line with a static perspective of capital structure. When a tax incentive on equity is introduced, the debt tax bias is reduced and banks rebalance their capital ratio upward. When the incentive is removed, the debt tax bias increases again, and bank capital is adjusted downward accordingly. These results shed more light on how the tax system can work as a complement of financial regulation with respect to capital levels and provide new evidence on how tax incentives determine bank capital structure.

In the last chapter of the thesis we study the role of social capital in bank misconduct. It is generally accepted in the academic research that human elements, such as the traits and preferences of managers play a role in how firms (including banks) are managed (Adhikari and Agrawal 2016, Bertrand and Schoar 2003, Hasan et al. 2017a, Hilary and Hui 2009). Firms do not make decisions on their own, people do, and the social interactions of managers may influence how they behave at work, and thus how they manage their firms. People incorporate as part of their identity cultural factors. Once incorporated, breaching them may lead to physical costs such as psychological distress (Akerlof and Kranton 2005). With respect to misconduct, the literature provides evidence consistent with the fact that the attitudes toward misconduct in a firm are likely to be influenced by the culture of the area from which it is managed (Parsons et al. 2018).

There is greater recognition of the idea that banks are socially accountable (Group of Thirty 2015) because their actions have externalities in the local economies and hence in the

society (Danisewicz et al. 2018, Jackson and Kotlikoff 2018). Thus, some social resistance against bank misconduct is expected, but the degree of intolerance may vary depending on the cultural and social factors of each community. Geographical areas characterized by higher degrees of trust and higher respect of civic norms would create a greater pressure on institutions to behave in line with the expected standards (Fukuyama 1997, Putnam 2000). Therefore, managers may anticipate greater degree of social stigmatization in those kinds of regions. Moreover, it is more likely that the cultural values derived from higher civic standards influence the social interactions of local managers, so they may anticipate greater physical or psychological costs from deviant behaviors related to them or their firms. These factors may reduce the scope for opportunistic behavior and enhance norm-compliant actions.

Taking the arguments above as a starting point, in Chapter 3, we study the extent to which social capital (which is an environmental factor that proxies for the level of trust and civic behavior in an area) plays a role in the likelihood that a bank is involved in misconduct and the consequences once misconduct is revealed. The way social capital and bank misconduct may interact is as follows: bank subsidiaries headquartered in areas characterized by higher social capital would be influenced by the social pressure surrounding them. If the social relationships in that region are of a higher quality (more trust and civic behavior, meaning higher social capital), managers would be more aware of the pro-social expectations in the area and at the same time would be subject to more social pressure to behave according to the established standards (Guiso et al. 2011). As a consequence, this will influence the approach towards misconduct they are going to implement in the firm. This culture permeates the bank subsidiary and therefore, there is the expectation of a lower probability of misconduct in bank subsidiaries located in higher social capital areas. In line with this argument, the recent Wells

Fargo misconduct case provides anecdotal evidence⁹. Related to this scandal, the Forbes magazine reported the following statement¹⁰: “...Mr. Stumpf’s¹¹ employees were doing precisely the things that they were asked to do by management – even if such asking was tacit, or implicit in shared group norms... Context drives conduct, the NY Fed’s Bill Dudley rightly asserts. And while this is true at every company, banks appear to struggle with this behavioral dynamic with a particular intensity... For humans, most social of all animals, group acceptance is perhaps the greatest informal incentive going. To win it, we’re prompted by evolution to mimic our peers’ behavior – and to refrain from doing anything they’re unlikely to approve...”. This assertion points to the fact that human behavior is significantly driven by social or group norms. Based on this, one could conjecture that whenever there is social pressure to behave in a civic way, the incentives to commit misconduct are reduced. This would explain lower propensity to misconduct in bank subsidiaries located in high social capital areas.

In this chapter, we provide empirical evidence of this hypothesis using a sample of US commercial banks and county-level measures of social capital. We follow Hasan et al (2017 a, b) and construct a social capital measure using the first principal component in an analysis of four variables: two proxies of civic norms (the US Census response rate and voter turnout in the presidential election) and two measures of the density of trust¹² (the number of social and civic associations and the number of non-government organizations). Using these data, we document a negative and statistically significant relationship between social capital and the probability that a bank is in violation of laws and regulations. Furthermore, we provide

⁹ Wells Fargo was sanctioned for opening “ghost accounts” without obtaining customers consent (CNN business September 9, 2016, “5,300 Wells Fargo employees fired over 2 million phony accounts” <https://money.cnn.com/2016/09/08/investing/wells-fargo-created-phony-accounts-bank-fees/index.html>)

¹⁰ Forbes, “Wells Fargo: What Drives Toxic Corporate Culture?” September 29, 2016 <https://www.forbes.com/sites/realspin/2016/09/29/wells-fargo-what-causes-toxic-corporate-culture/#73096e2349a1>

¹¹ Wells Fargo CEO at that time

¹² This is proxy by the density of networks in an area.

evidence of a stronger discipline against wrongdoing banks in high social capital markets. We show that banks affected by misconduct, lose higher portions of market share in counties characterized by higher social capital levels. This result is mainly significant during the aftermath of the financial crisis, a period characterized by low trust in the financial sector (Sapienza and Zingales 2012). The latter result is consistent with the fact that the events related to the banking sector during the crisis increased the social expectations on banks during its aftermath (Group of Thirty 2015). Social capital has the advantage of facilitating business relationships due to the higher trust between individuals. However, when trust is broken, discipline on deviants increases with the level of social capital (Spagnolo 1999). The effect of discipline may be even larger during a period of increasing expectations on banking institutions which explains why the result is mostly significant during the period after the financial crisis.

The rest of the dissertation is organized as follows. The second, third and fourth chapters correspond to the three empirical papers described above. The fifth chapter corresponds to the general conclusion of the thesis.

2. Chapter 1: Bank Performance across Crises and the Stock-market¹³

Abstract

This paper examines whether stock-market listing influences the persistence of bank performance across crises. We find that for both publicly and privately held banks, bank performance during the 1998 crisis is a strong predictor of bank performance during the 2007–2008 crisis. However, while for publicly held banks, the persistence is uniquely driven by bottom performers, for privately held banks the persistence is also driven by a group of top performers. Auxiliary analyses document that among privately held banks that were top performers in 1998, banks that transition from privately to publicly held between the two crises significantly underperform their counterparts that remained private. This effect is more pronounced when transitioning banks have a higher portion of investors that have short-term horizons. We also document that banks that undergo a private-to-public transitions increase risk. Specifically, we observe a decrease in capital ratios, increase in non-depository debt as well as increases in loan and asset growth, which are key determinants of banks' ability to persistently perform well during crises.

JEL Code: G21, G28, G32.

Key words: Tax, bank capital, debt-equity tax bias.

¹³ This chapter draws from an article authored by Alexandre Garel, Arthur Petit-Romec and Jose M. Martin Flores that at the moment of the submission of this dissertation is undergoing a second round revision at the Journal of Banking & Finance.

2.1 Introduction

Better understanding the performance of financial institutions during crises is an important issue that has received increased attention in the past decade. Empirical evidence highlights several key determinants of bank performance during the 2007–2008 crisis, including bank capital and funding structures (e.g., Beltratti and Stulz, 2012; Berger and Bouwman, 2013; Demirguc-Kunt et al., 2013), corporate governance (e.g., Erkens et al., 2012; Fahlenbrach and Stulz, 2011; Minton et al., 2014), risk-management practices (e.g., Aebi et al., 2012; Ellul and Yerramilli, 2013), and ownership structure (e.g., Garel and Petit-Romec, 2017). In a related article, Fahlenbrach et al. (2012) report that one of the most important determinants of bank performance during the 2007–2008 crisis is bank performance during the 1998 crisis. Their results are consistent with the existence of a persistent business model that makes some banks particularly vulnerable to crises.

In this paper, we examine whether the persistence of bank performance across crises is a specificity of publicly held banks or whether it also applies to privately held banks. Previous literature supports the view that stock-market listing influences various corporate policies in non-financial firms (e.g., Asker et al., 2014; Bernstein, 2015; Brav, 2009; Michaely and Roberts, 2012). However, empirical evidence on the effects of stock-market listing in banks is relatively scarce. We attempt to fill this gap by focusing on the links between stock-market listing and bank performance during crisis periods. We focus on the two worst financial crises since the Great Depression: the 1998 crisis and the 2007-08 crisis¹⁴. If stock-market listing influences the ability of a bank to perform well during crises, it would have potentially important implications for understanding the performance of banks during crises and how that performance can be improved through regulation.

¹⁴ These two crises had important effects on the real economy as bank borrowers were significantly affected by the performance of their banks during these crises (e.g., Chava and Purnanandam, 2011; Chodorow-Reich, 2014). We provide a summary of the two crises in Appendix 2.3.

Because privately held banks do not have any stock price data, we rely on accounting measures of bank performance throughout the analysis. We begin our empirical analysis by examining whether the results of Fahlenbrach et al. (2012) hold when we use accounting-based measures of bank performance instead of stock performance during the crisis. Using a sample of 385 publicly held U.S. banks,¹⁵ we show that the return on assets (ROA) of public banks in the 2007–2008 crisis is positively related to that of the 1998 crisis. The result is highly statistically significant and robust to the introduction of various pre-crisis characteristics of banks.¹⁶

Next, we examine the persistence of bank performance across crises for privately held banks. We reproduce the same regressions as for publicly held banks. One issue is that privately held firms are generally smaller than publicly held firms. To alleviate the concern that any observed differences between publicly and privately held banks may be driven by size differences rather than stock-market listing, we follow Asker et al. (2014) and Gao et al. (2013) and use a matching procedure based on size.¹⁷ This matching procedure leads to a sample of 385 privately held banks. Using this sample, we show that the ROA of privately held banks in the 1998 crisis is strongly associated to their ROA in the 2007–2008 crisis. As for publicly held banks, the result is highly statistically significant and robust to the introduction of various pre-crisis characteristics. These findings indicate that the persistence of bank performance across crises is not a specificity of publicly held banks but also exists for private banks. We then look at which banks drive this persistence. For publicly held banks, in line with Fahlenbrach et al. (2012), we find that the correlation between crisis ROAs is driven by the quintile of bottom performers. However, in the case of privately held banks, we find

¹⁵ As discussed in Section 2, we consider a bank as publicly held if it either is listed on a U.S. stock-market or is part of a publicly traded bank holding company (BHC).

¹⁶ We obtain similar results if we use return on equity (ROE) instead of ROA.

¹⁷ The matching procedure in the two aforementioned articles is based on size and industry. However, as our paper focuses on a single industry, we use only a matching based on size.

that the correlation between crisis ROAs is driven by both best and worst performers. This finding indicates that, in the case of private banks, there is a persistence of good performance (i.e., banks that did well during the 1998 crisis again do well during the 2007–2008 crisis).

So far, our results suggest that stock-market listing is detrimental to the persistence of good bank performance across crises. To shed further light on this issue, we exploit the fact that some banks that were private in 1998 made a private-to-public transition between the two crises. We match these banks with banks that were private in 1998 and remained private during the whole sample period. If stock-market listing is detrimental to the persistence of good performance across crises, becoming publicly held should have a negative impact on bank performance during the recent financial crisis mainly for banks that were top performers in 1998. This is exactly what we find. We show that among banks that made the private-to-public transition, only those that were top performers during the 1998 crisis significantly underperformed their counterparts that remained privately held during the 2007–2008 financial crisis. This finding provides further supporting evidence that stock-market listing is detrimental to the persistence of good performance across crises. Since the choice of making a private-to-public transition between the two crises may be related to unobservable changes in banks' risk environment and risk-taking incentives, we perform additional tests to alleviate concerns related to the private-to-public transition. Following Bernstein (2015), Falato and Scharfstein (2016) and Seru (2014), we compare the performance of 'transitioning' banks with banks that were targeted by a publicly held bank (or their BHC were targeted) in an M&A deal or attempted an IPO but the deals were withdrawn before completion (and the bank remained private until the end of 2006). The rationale for this approach is to compare banks that make a private-to-public transition with banks that had similar incentives to undergo this transition between the two crises. Our conclusions are unchanged when we use this alternative setting.

There is a disagreement in the literature on the effects that the stock-market may have on banks. A first view is that the stock-market imposes market discipline by providing banks with incentives to conduct their business safely and soundly (Basel Committee on Banking Supervision 1999, Bliss and Flannery 2002).¹⁸ A second view, mostly developed in the corporate finance literature, is that a stock-market listing can induce short-termist influences that bias managers' decisions towards the short-run (e.g., Asker et al., 2014; Davies et al., 2014; Graham et al., 2005; Poterba and Summers, 1995). The results in our paper contribute to the academic debate about the effects of stock-market influence by highlighting a negative consequence of stock-market listing on banks.

In line with the corporate finance literature studying the effect of stock-market listing on firm policies, a possible explanation for our results is that stock-market listing exposes some banks to stock-market driven short-termism that in turn affect their ability to perform well across crises. More precisely, short-termist influences could induce banks to change some aspects of their business model in a way that hinders their performance during crisis periods. In this respect, we analyze whether banks become riskier once they undergo a private-to-public transition. We observe that upon transition, banks tend to reduce their capital ratios, rely more into non-deposit debt funding and increase their asset and loan growth rates. This result is consistent with higher risk-taking after transitioning. The latter result does not seem to be driven by endogeneity. Similar to Bernstein (2015), we use a bank index returns after the announcement as an instrument in an IV treatment effects model for the transition treatment variable and a control group of banks that either withdraw their IPO or become the target of a listed BHC in an M&A deal that is cancelled (and the bank remains private until the end of 2006). The result of this test is in line with an increase in risk after banks make a private-to-public transition.

¹⁸ Market discipline is one of the pillars of bank regulation together with other two pillars (minimum capital standards and supervisor review of capital requirements).

The stylized facts we document are consistent with transitioning banks being unable to persistently perform well across crises. An increase in risk-taking when they become publicly held seems to be a potential explanation of this effect (for example, recent work emphasizes that a business model that favors higher leverage, more non-deposit debt funding, and stronger asset growth makes banks more vulnerable to crises (e.g., Fahlenbrach et al. 2012)). Thus, we conjecture that the effects of private-to-public transitions on banks should increase for banks facing higher short-termist influence from the stock-market¹⁹. We test this hypothesis in two steps. First, we assess whether, among banks that were top performers in 1998, the detrimental effect of becoming publicly held on bank performance during the 2007-2008 crisis is more pronounced for transitioning banks subject to greater stock-market driven short-termism. Second, we explore whether transitioning banks exposed to greater stock-market driven short-termism did change some key aspects of their business model (relative to banks that remain private and banks that face lower short-termist influence).

Regarding the first step, we assume that those banks whose institutional shareholders trade more actively the shares they hold in their portfolio are more subject to short-term influence from the stock-market.²⁰ When banks become publicly held, they undergo important changes in their ownership structure. However, the extent to which they face stock-market driven short-termism will differ depending on their level of short-term investor ownership after the transition. The stock-market driven short-termism hypothesis thus predicts that the detrimental effect of stock-market listing on bank performance should be more pronounced for transitioning banks with a higher exposure to short-term oriented investors. Consistent

¹⁹ A simple explanation of banks shifting to less conservative business models can be that a private-to-public transition shifts ownership from lowly diversified investors to more diversified ones that can bear more risk. Other option is that banks have more access to additional capital which allows them to increase risk more easily. Therefore, to rule out the possibility that these factors cause our result, we intend to address this point by distinguishing between banks that face higher and lower short-termist influence from the stock-market.

²⁰ Prior literature shows that short-term investor ownership is associated with short-termist behaviors and outcomes (e.g., Bushee, 1998; Chen et al., 2007; Gaspar et al., 2005; Stein, 1996; Harford et al., 2017). Consistent with these papers, we use institutional investors horizons as a proxy for short-term pressure from the stock-market.

with this prediction, our results indicate that among privately held banks that were top performers during the 1998 crisis, banks that made the private-to-public transition perform worse than their peers during the 2007–2008 crisis only when they have more short-term oriented institutional investors. In contrast, transitioning banks with a lower exposure to short-term institutional investors do not significantly underperform compared with banks that remained privately held.

Regarding the second step, we explore whether, among banks that were top performers in 1998, transitioning banks with more exposure to short-term oriented institutional investors and those remaining privately held have different pre-crisis characteristics. We do not observe any significant differences between the two groups in 1997. This indicates that before the 1998 crisis, when transitioning banks were still private, their risk indicators were not different. However, we find that transitioning banks subject to more short-term influence from the stock-markets have lower capital, more asset and loan growth, and more non-depository and short-term debt funding in 2006 (prior to the second crisis). Previous research shows that asset growth, lower capital, and non-depository (and short-term) funding have a negative impact on bank performance during crisis periods (e.g., Fahlenbrach et al. (2012)). We find similar results regarding capital as well as assets and loan growth when instead of using privately held banks as a comparison group, we compare transitioning banks facing higher short-termist influence from the market with transitioning banks facing lower short-termist influence from the market.²¹ Overall, our results provide evidence supporting the idea that short-termist influences from financial markets alter several aspects of bank business models in a way that hinders their performance during crisis periods.²²

²¹ However, we do not find that short-term funding and non-depository debt funding are statistically higher for banks facing more short-termist influences from the market than for banks facing lower short-termist influence.

²² This result also contributes to the findings of Barry et al. (2011) who show that changes in ownership structure of private banks, mostly when there are more institutional investors, tend to lead to an increase in risk indicators.

Our paper is related to several streams of research in banking and corporate finance. First, it contributes to the literature investigating the determinants of bank performance during crises. A large part of what is known about the determinants of bank performance during the 2007–2008 crisis is based on evidence from publicly held banks (e.g., Beltratti and Stulz, 2012; Ellul and Yerramilli, 2013; Erkens et al., 2012; Fahlenbrach and Stulz, 2011; Minton et al., 2014). Further research is necessary to assess whether the determinants of bank performance during crises highlighted for public banks also apply to private banks. Our results show that as for public banks, there is persistence in bank performance across crises for private banks. However, the results also highlight a specificity of private banks: banks that did well in the 1998 crisis do well again in the 2007–2008 crisis. Our findings suggest that publicly held banks may be subject to more short-termist influences that, in turn, affect their ability to maintain sound business models and persistently perform well during crises.

Our paper is also related to the corporate finance literature investigating the impact of stock-market listing on various corporate policies. Asker et al. (2014) show that compared with private firms, public firms invest substantially less and are less responsive to changes in investment opportunities. Bernstein (2015) documents that going public changes firms' strategies in pursuing innovation. Michaely and Roberts (2012) show that private firms smooth dividends less than public firms, and Brav (2009) shows that private firms rely more on debt financing. Our paper complements this literature by focusing on banks and showing that stock-market listing may induce a shift towards a less prudent business model that in turn affects the ability of a bank to perform well across crises. Our results support the findings of a contemporaneous paper by Falato and Scharfstein (2016) that present evidence that pressure to maximize short-term stock prices and earnings leads banks to increase risk-taking.

We show the effects of changes in ownership structure in transitioning banks and the effects that a higher presence of short-term oriented institutional investors has on risk indicators.

The rest of the paper proceeds as follows. Section 2.2 presents our sample of public and private banks and the empirical methodology. Section 2.3 presents the results. Section 2.4 concludes.

2.2 Data and methodology

2.2.1 Data sources and sample construction

Our data come from three main sources. Balance sheet and income statement data come from Call Reports. Data on whether or not a bank is publicly listed comes from CRSP, and data on investor horizons is based on Thomson Reuters Institutional (13F) Holdings database.²³ The starting point for the formation of our sample comprises all U.S. banking entities that are present in the Reports of Condition and Income (Call Reports) at the end of the second quarter of 1998. In line with Berger and Bouwman (2013), Kashyap et al. (2002) and Kashyap and Stein (2000), we work with data at the individual bank level.²⁴ We exclude banks that are not located in the United States and banks that are held by foreign owners, to keep our focus on U.S. banks. We also exclude banks that are not categorized as commercial banks²⁵ and banks that are out of the traditional banking business.²⁶ To ensure that our sample only includes “true” commercial banks, we further remove all entities for which loans or deposits are equal to zero. Bank organizations with total assets less than \$150 million are excluded²⁷. Finally, we restrict the sample to those banking entities that have the same

²³ Since 1978, the SEC requires all institutions with more than \$100 million of securities under discretionary management to report their holdings. All common-stock positions greater than 10,000 shares or \$200,000 must be disclosed.

²⁴ As Kashyap and Stein (2000) discuss, an alternative approach is to work with aggregated data at the level of the top holder entity. However, it is not obvious whether this approach is more appropriate. Given that the focus of the regulator tends to be on individual banks, we decide to study the performance across crises at this level. Nonetheless, in robustness tests, we reproduce our baseline analysis using data at an aggregated level and find that our results are unchanged.

²⁵ We exclude those banks for which the variable RSSD9331 is different from 1 and those for which IDs correspond to branches (RSSD9346 having the indicator DBR).

²⁶ We remove all entities that are not considered banks according to the variable RSSD9044.

²⁷ This threshold allows us to exclude from the sample the majority of rural banks that may have less exposure to the 1998 crisis and also let us have a larger portion of the larger publicly held banks. We consider that this treatment improves the quality of our tests. Nonetheless, the results hold if we do not apply this threshold.

identifier (RSSD) in 1998 Q2 and 2007 Q3. Following Fahlenbrach et al. (2012), we allow banks to merge and change names between 1998 Q2 and 2007 Q3. As long as the RSSD is the same in 1998 Q2 and 2007 Q3, we include the bank in our sample.²⁸

We then determine whether a bank is publicly held. More precisely, we consider a bank as publicly held if the bank itself is listed on a stock-market or if it is under the umbrella of a publicly listed Bank Holding Company (BHC) in 2006.²⁹ To determine whether an entity is public or belongs to a BHC that is public, we link Call Reports and CRSP data using the PERMCO-RSSD correspondence file available at the New York Fed website.³⁰ If the bank (or its BHC) has a stock price on CRSP in 2006, we classify it as publicly held. We do not take into account in the analysis those banks that delisted (or their BHC delisted) between 1998 and 2006. This classification leads to a sample of 2,450 banks, 509 of which are publicly held and 1,941 privately held.

We also identify banks that make a private-to-public transition between 1998 and 2006. These banks are those that are newly listed (or their BHC is newly listed) in an US stock-market (in NASDAQ, NYSE or AMEX) or are acquired by a publicly listed bank or BHC. Regarding new listings, we consider that a bank transitions via a new listing in the stock-market if it appears for the first time in CRSP during the years between 1998 and 2006. We corroborate the date of the IPO using Thomson-Financial's SDC New Issues database. For banks for which we do not find information on this database, we directly check the information on their 10-K reports and reports available on EDGAR³¹. Regarding M&A deals, we confirm the transition of the bank using changes in the ownership of commercial banks reflected in Call Reports (changes in the variables RSSD9346 and/or RSSD9348). We further

²⁸ We run our baseline tests excluding banks that are (or their BHC is) an acquirer in an M&A deal over the sample period. Our conclusions remain unchanged after running this test.

²⁹ To identify whether a bank is held by a BHC, we link subsidiaries to their top holder entity in Call Reports using the relationship file available at <https://www.ffiec.gov/nicpubweb/nicweb/DataDownload.aspx>.

³⁰ See https://www.newyorkfed.org/research/banking_research/datasets.html.

³¹ <https://www.sec.gov/edgar/searchedgar/companysearch.html>

corroborate that these changes correspond to an M&A deal using Thomson-Financial's SDC M&A database and/or the Merger Table from NIC data.³² We identify 226 private-to-public transitions corresponding to 141 new listings and 85 M&A deals. We also identify banks that submitted the IPO documentation but cancelled the deal before completion and banks that were the target of a public bank in an M&A deal but the deal was cancelled before completion. We use Thomson-Financial's SDC to identify these deals. We find 29 banks that are directly (or their BHCs are) affected by cancelled deals. We provide a summary statistics of these private-to-public transitions and cancelled deals in Appendix A.2.2.2.

2.2.2 Main dependent and independent variables

Our main dependent variable is the accounting performance of banks during the 2007–2008 crisis. While several studies on the determinants of bank performance during crises focus on stock performance (e.g., Aebi et al., 2012; Beltratti and Stulz, 2012; Fahlenbrach et al., 2012), we rely on accounting measures of bank performance throughout the analysis because our focus is on both publicly and privately held banks. Following Fahlenbrach and Stulz (2011), we use the crisis ROA and the crisis ROE as measures of accounting performance. The former is computed as the cumulative quarterly net income from 2007 Q3 to 2008 Q3 divided by total assets at the end of 2007 Q2. For ROE, we divide the cumulative quarterly net income by the book value of equity at the end of 2007 Q2. The 2007–2008 crisis obviously did not end in 2008 Q3. However, in line with previous studies (e.g., Fahlenbrach and Stulz, 2011), we stop the calculation of our measures of accounting performance at that point in time to avoid biases in the computation of our dependent variables due to the

³² Information regarding bank mergers is available at the National Information Center (NIC) using the following link: <https://www.ffiec.gov/npw/FinancialReport/DataDownload>

initiation of the Troubled Assets Relief Program (TARP) at the beginning of the fourth quarter of 2008.³³

Our main independent variable is the accounting performance of banks during the 1998 crisis. In line with Berger and Bouwman (2013) and Fahlenbrach et al. (2012), we consider that the 1998 crisis period corresponds to the last two quarters of 1998. We thus compute the ROA for the 1998 crisis as the cumulative net income over 1998 Q3 and 1998 Q4 divided by the total assets of each bank at the end of 1998 Q2. Alternatively, for the ROE we divide the cumulative net income over the crisis period by the book value of equity at the end of 1998 Q2.

Our control variables include various potential determinants of bank performance during the 2007–2008 crisis. We measure all the variables as of the end of fiscal year 2006. Specifically, we control for Tier 1 ratio, defined as Tier 1 capital to risk-weighted assets; size, defined as the natural logarithm of total assets; pre-crisis ROA (ROE), defined as the cumulative net income over the five quarters before 2006 Q4³⁴ divided by total assets (total equity) in 2005 Q3; the ratio of non-performing loans to total loans³⁵; the ratio of deposits to total assets; the loan concentration index; and the loan ratio. Finally, we also control for a dummy variable that captures whether or not a bank is held by a BHC. A detailed definition of all our variables is available in the Appendix 2.1. We winsorize all continuous variables at the 1% and 99% levels.

³³ Research on this topic shows that the behavior of banks was influenced by this program (Berger and Roman 2015, Fahlenbrach and Stulz 2011). Therefore, we find it appropriate to stop our measure of performance before the initiation of the TARP to avoid biases in the computation of our dependent variables. For the banks that disappear from the sample before 2008 Q3, we compute the cumulative net income until the last quarter they are in the sample.

³⁴ We compute pre-crisis ROA (ROE) in this way to be consistent with the measurement of our dependent variables.

³⁵ The ratio of non-performing loans is often used as a measure of asset risk.

2.2.3 Empirical methodology

We investigate the persistence of performance of individual banks across crises using cross-sectional regressions of the ROA (or ROE) of a bank in the 2007–2008 crisis on its ROA in the 1998 crisis. Formally, our baseline results come from running the following OLS regression for publicly held banks and privately held banks separately:

$$\begin{aligned} & \text{Crisis RoA 07/08}_i \text{ or Crisis RoE 07/08}_i \\ & = \beta_0 + \beta_1 \text{Crisis RoA98}_i \text{ or Crisis RoE98}_i + \beta_i X_i + u_i \end{aligned} \quad (1)$$

We also use the following regression to test whether the persistence of bank performance in crises is statistically different for privately and publicly held banks. The variable *publicly held* is a dummy variable that equals 1 if the bank is classified as publicly held and 0 otherwise:

$$\begin{aligned} & \text{Crisis RoA 07/08}_i \text{ or Crisis RoE 07/08}_i \\ & = \beta_0 + \beta_1 \text{Crisis RoA98}_i \text{ or Crisis RoE98}_i + \beta_2 \text{Publicly held} \\ & + \beta_3 \text{Crisis RoA98}_i \text{ or Crisis RoE98}_i * \text{Publicly held} + \beta_i X_i + u_i \end{aligned} \quad (2)$$

The main dependent variable is bank performance in the 2007–2008 crisis, and the main independent variable is bank performance in the 1998 crisis. X_i is a set of control variables, including several determinants of bank performance during the crisis. In addition to testing the persistence of bank performance across crises for public and private banks, we are interested in analyzing whether there are asymmetries in the relation between accounting performance during the 1998 crisis and during the 2007–2008 crisis. That is, we examine whether the persistence of bank performance across the two crises is driven by good performers (i.e., banks that did well in 1998 again doing well in the recent crisis) or bad performers (i.e., banks that did poorly in 1998 again doing poorly). To do so, we split banks into quintiles based on their performance in the 1998 crisis and create indicator variables for

each of the five groups. In some specifications, we therefore replace accounting performance during the 1998 crisis with these indicator variables.

2.2.4 Matching

As discussed in Section 2.2.1, our initial sample comprises 2,450 banks, 509 of which are publicly listed and 1,941 privately held. Private firms are generally smaller than public firms (e.g., Asker et al., 2014). This raises the concern that any observed differences between public and private banks may be driven by size differences rather than stock-market listing. Previous research documents differences across banks of different size in terms of portfolio composition (Kashyap et al. 2002), performance during crises (Berger and Bouwman 2013), and business models (Köhler 2015). To address this concern, we follow Asker et al. (2014) and Gao et al. (2013) and use a matching procedure. The aim of this procedure is to have a group of private banks and a group of public banks that are comparable. Figure 2.1 a) shows a kernel density graph of the size distribution for both groups measured by the natural logarithm of total assets at the end of 2006. We observe a large difference in size between publicly and privately held banks.

We use a propensity score matching based on size to minimize the difference between the two groups (Rosenbaum and Rubin 1985). We run a probit regression of a dummy equal to one if the bank is publicly held and zero otherwise on the natural logarithm of total assets. For each public bank in 2006, we find the private bank with the closest propensity score. We do the matching without replacement so we finish with the same number of publicly and privately held banks. The matched observations remain in the sample, while we remove the observations that are not matched. The matched sample has 385 banks for each group. Figure 2.1 b) shows a kernel density graph on the natural logarithm of total assets for both samples after matching. The differences observed in Figure 2.1 a) are widely reduced after matching.

Table 2.1 compares the characteristics of public banks and private banks. It shows that when we test whether the means of the two samples are different using a t-test, the null hypothesis that the means of the natural logarithm of total assets are equal between the groups of publicly and privately held banks cannot be rejected (p-value=0.62).

[Insert Figure 2.1 about here]

2.2.5 Summary statistics

Table 2.1 presents in Panel A descriptive statistics for our main dependent and independent variables and a correlation table in Panel B. The mean and median ROAs during the 2007–2008 crisis were 0.91% and 1.18%, respectively. The mean and median ROAs during the 1998 crisis were 0.63% and 0.64%, respectively. In contrast, pre-crisis ROAs were much higher, with a mean value of 1.75%. The statistics are similar for the ROE. The average bank has a ROE of 10.25% in the 2007–2008 crisis and 7.02% in the 1998 crisis, which is substantially lower than that in the pre-crisis period (mean value equal to 19.65%).

The mean and median Tier 1 ratios are 11.65 and 10.63, respectively, well above the minimum regulatory requirements. The difference between publicly and privately held banks is statistically significant. The average bank in our sample has a deposit ratio of 80.17% and a loan ratio of 70.73%. The minimums for these two variables are 53.60% and 25.35%, respectively, confirming that our sample focuses on commercial banks. Finally, some variables present statistically significant differences between publicly and privately held banks. On average, privately held banks have higher deposit ratios, lower asset growth, lower real estate loans ratios, lower loan concentration and higher non-performing loans ratios. However, after matching, we find no statistical differences between the two groups in terms of the proportion of liquid assets, short-term funding and the ratio of loans to total assets.

[Insert Table 2.1 about here]

2.3 Results

2.3.1 Persistence of accounting performance across crises for public and private banks

We begin by examining the persistence of performance across the 1998 and 2007–2008 crises for the group of publicly held banks. Using a sample of public banks and focusing on stock performance, Fahlenbrach et al. (2012) document a strong persistence of bank performance across crises. We are therefore interested in whether their results hold when using accounting-based measures of bank performance instead of stock performance.

Table 2.2, Panel A, presents regressions, where the dependent variable is accounting performance in the 2007–2008 crisis and the main independent variable is accounting performance in the 1998 crisis. Columns 1 and 4 present results for public banks, Columns 2 and 5 for private banks, and Columns 3 and 6 for public and private banks together. For public banks, the results indicate that the crisis ROA of 1998 has strong predictive power for the ROA in the 2007–2008 crisis. The effect appears statistically and economically significant. According to Column 1, in the cross-section of public banks, a one standard deviation increase in ROA in the 1998 crisis is associated with a 0.22% (0.620×0.35) increase in the ROA in the 2007–2008 crisis. Relative to a sample mean of 0.71, this corresponds to a 30.5% increase. Column 4 shows that the results are similar if we use ROE instead of ROA as a measure of accounting performance. Indeed, crisis ROE in 1998 has a strong and positive impact on ROE in the 2007–2008 crisis. This confirms that the persistence of bank performance across crises also holds when using accounting-based measures of bank performance instead of stock performance.

[Insert Table 2.2 about here]

The results from Columns 2 and 5 show that there is also a strong persistence of bank performance across crises for privately held banks. As in the case of publicly held banks, the ROA in the 1998 crisis of a privately held bank has strong predictive power for its ROA in the 2007–2008 crisis. The effect is also statistically and economically significant. In the cross-section of private banks, a one standard deviation increase in ROA in the 1998 crisis is associated with a 0.48% (1.462×0.33) increase in the ROA in the 2007–2008 crisis. Relative to a sample mean of 1.1, this corresponds to a 43.8% increase. Column 5 shows that the results are similar if we use ROE instead of ROA as a measure of accounting performance. Thus, the persistence of bank performance across crises is not a specificity of public banks, as it also exists for private banks.

In Columns 3 and 6, we examine whether the predictive power of the 1998 crisis ROA (ROE) is of similar magnitude for public and private banks. To do so, we pool public and private banks together and interact the 1998 crisis ROA with a publicly held indicator variable. According to these specifications, the interaction between 1998 crisis ROA and the publicly listed dummy has a negative significant effect and the 1998 crisis ROA remains positive and highly significant. This suggests that the persistence of bank performance across crises is of greater magnitude for privately than for publicly held banks. This result is robust to taking into account risk adjusted performance over crisis period. In Appendix A.2.2.1 we show that the persistence of performance across crises remains significant when we use a risk-adjusted measure of performance and remains different for publicly and privately held banks.³⁶

We further explore this difference. While we find strong persistence of bank performance across crises for both public and private banks, it is not necessarily driven by the same banks

³⁶ We compute a risk-adjusted ROA (ROA over crisis period divided by the standard deviation of the ROA before each crisis computed over three years using quarterly data).

in the two cases. We thus examine whether the persistence is driven by banks that did well in 1998 doing well again in 2007–2008 or by banks that did poorly in 1998 doing poorly again. Following Fahlenbrach et al. (2012), we split banks into quintiles based on their crisis ROA (ROE) in 1998 and create indicator variables for each quintile. *Crisis ROA 98-q1* contains all banks whose crisis ROA in 1998 is in the lowest quintile, while *Crisis ROA 98-q5* contains all banks whose crisis ROA in 1998 is in the highest quintile. We replace the crisis ROA in 1998 by the quintile indicator variables and rerun our main regressions from Panel A of Table 2. 2. the omitted quintile is *Crisis ROA 98-q3*. Panels B and C report the results for public and private banks, respectively.

In the case of public banks, the results from Panel B indicate that banks that performed poorly in 1998 (i.e., banks in the bottom quartile in 1998) did so again in the 2007–2008 crisis. *Crisis ROA 98-q2* and even more *Crisis ROA 98-q1* have a strong and negative impact on ROA in the 2007–2008 crisis. By contrast, the top quintile indicator variables are not statistically significant. We find similar results if we use ROE instead of ROA as a measure of accounting performance. In both cases, we observe that the persistence of bank accounting performance in the 1998 crisis and the 2007-08 crisis is driven by the group of worst performers.

In the case of private banks, the results from Panel C indicate that private banks that performed poorly in 1998 (i.e., banks in the bottom quartile in 1998) did so again in the 2007–2008 crisis. *Crisis ROA 98-q1* has a strong and negative impact on ROA in the 2007–2008 crisis. However, in sharp contrast with public banks, we also find that private banks that performed well in the 1998 crisis continued to do so in the 2007–2008 crisis. In the case of private banks, the persistence of bank performance across crises is thus at least partially driven by best performers and not only by worst performers, as is the case for the group of public banks. This difference is likely to explain the stronger persistence of bank performance

for privately held banks documented in Panel A. The results from Columns 3 and 4, in which we use ROE instead of ROA, confirm that in the case of private banks, the persistence is not only driven by worst performers but also by a group of best performers that did well in both crises.

So far, our empirical analysis of the persistence of bank performance in crises highlights two main findings. First, the persistence of bank performance across crises, previously documented by Fahlenbrach et al. (2012) for publicly held banks, also exists and if anything is even stronger for privately held banks. Second, while in the case of publicly held banks, the persistence is uniquely driven by worst performers, in the case of privately held banks, we find a persistence of good performance in crises (i.e., private banks that did well in 1998 again do well in the 2007–2008 crisis). These findings suggest that stock-market listing can be detrimental to the persistence of good bank performance across crises. In the next sub-section, we provide auxiliary analyses to further examine whether and how stock-market listing weighs on the ability of a bank to persistently perform well during crises.

2.3.2 Stock-market listing and the persistence of good performance across crises

In this sub-section, we aim to shed further light on the detrimental effect of stock-market listing on the ability of some banks to persistently perform well in crises. To address this issue, we exploit the fact that some banks that were privately held in 1998 made a private-to-public transition between the two crises. Specifically, we examine whether and for which banks transitioning to publicly held status between the two crises influences bank performance during the 2007–2008 crisis. If stock-market listing is detrimental to the persistence of good performance across crises, this would predict that becoming publicly held

has a negative impact on bank performance during the 2007–2008 crisis mainly for banks that were good performers in 1998.

To examine this issue, we begin by identifying banks that made a private-to-public transition between 1998 and 2006. In line with our identification of publicly held banks in Section 2.2, we consider that a bank becomes publicly held if it was private before 1998 and then either went public (or its BHC went public) or became part of a BHC that was publicly listed between 1998 and 2006³⁷. We further require that the stock-market listing status remains the same until 2006 Q4. We find that 226 banks in the initial sample that were privately held before the 1998 crisis and made a private-to-public transition between 1998 and 2006. We then match the sample of transitioning banks with banks that were private in 1998 and remained private over the whole sample period using a nearest-neighbor matching based on a propensity score as described in Section 2.2.4.³⁸ The matched sample has 225 banks that made the private-to-public transition and 225 banks that remained private. As Table 2.3, Panel A, shows, the t-test gives a p -value of 0.896 when we test whether the mean difference of the variable $\ln(\text{assets})$ is insignificant between the two groups. We additionally show in the last two columns of Panel A the characteristics of banks that “attempted” a private-to-public transition but the deal failed. This group of banks is used in our analysis in Panel C-1 and C-2 below.

In Table 2.3, Panel B, we examine whether making a private-to-public transition between the two crises mainly has an effect for banks that were good performers in 1998. We again split banks into quintiles based on their crisis ROA in 1998 and create indicator variables for each quintile using the matched sample of transitioning and privately held banks. *Crisis ROA 98-q1* takes the value one for all banks whose crisis ROA in 1998 is in the lowest quintile,

³⁷ We provide statistics of the transitioning banks and the methods of transition in Appendix A.2.2.2.

³⁸ We use size as the sole criterion for matching. We do the matching without replacement.

while *Crisis ROA 98-q5* takes the value one for all banks whose crisis ROA in 1998 is in the highest quintile, and zero otherwise. We then run cross-sectional regressions of bank performance during the 2007–2008 crisis on a *transition* dummy,³⁹ each quintile indicator variable of the performance during the 1998 crisis and interaction terms of these quintiles and the *transition* dummy. In Column 1, we run this regression without controls and in Column 2, we use the same control variables that we use in our baseline regression. The coefficients of the variables *Crisis ROA 98-q1* and *Crisis ROA 98-q5* are negative and statistically significant and positive and statistically significant, respectively. This is in line with previous findings showing that the persistence of performance across crises for private banks is driven by top performers doing well during the two crises and bottom performers doing poorly during the two crises. However, when we look at the interaction terms, we observe that the coefficient for the variable *Crisis ROA 98-q5*transition* is negative and statistically significant at the 10% level. This is consistent with the fact that banks that were top performers during the 1998 crisis and made a private-to-public transition, underperform their privately held counterparts. Their performance is between 0.6 and 0.7 points of *Crisis ROA 07/08* lower for transitioning banks that were top performers during the 1998 crisis. The coefficient of the variable *transition* is not statistically significant, meaning that on average transitioning banks do not underperform banks that remain private during the last financial crisis. This suggests that the ability of some privately held banks to persistently perform well across crises disappears once a bank makes a private-to-public transition.

Since the choice of making a private-to-public transition between the two crises may be related to unobservable changes in banks' risk environment and risk-taking incentives, we perform additional tests to alleviate concerns related to the private-to-public transition. Following Bernstein (2015), Falato and Scharfstein (2016) and Seru (2014), we compare the

³⁹ This variable takes the value of 1 if the bank transitioned from privately to publicly held and 0 for banks that remain privately held.

performance of ‘transitioning’ banks with banks that were targeted by a publicly held bank (or their BHC were targeted) in an M&A deal or attempted an IPO but the deals were withdrawn before completion (and the bank remained private until the end of 2006). The rationale for this approach is to compare banks that make a private-to-public transition with banks that had similar incentives to undergo this transition between the two crises. We use information on M&A and IPO withdrawn from Thomson-Financial’s SDC New Issues database (for withdrawn IPO) and Thompson SDC M&A database (for withdrawn M&A deals). We detect that among banks that remain in call reports between the 1998 and 2007/2008 crises, 29 banks (or their BHC) were involved in an IPO or were the target of a public bank in an M&A deal but the transactions were cancelled and the entities remained private during the whole period. In principle, this test should improve identification of the effect of stock-market listing on the persistence of bank performance across crises.

Another potential concern is the reason why this “attempted transition” is withdrawn. Consistent with Bernstein (2015), we document that the market returns following the announcement of the deal are a strong predictor of a deal success. In Table 2.3, Panel C-1 we use a sample of banks that make a private-to-public transition and a sample of banks that attempt this transition but the deal failed (the M&A deal or the IPO is withdrawn) and show, in Column 1, a linear probability regression in which the dependent variable is the a dummy variable taking the value one if the bank make a private-to-public transition and zero otherwise on a bank index return variable in the two months following the announcement of the deal⁴⁰ (a geometric mean of the two months returns) and a set of control variables that we used in our baseline tests measured at the year when the deal occurs and deal-year dummies. We find that the coefficient of the bank market returns is positively and highly significantly

⁴⁰ We use the 49 industry portfolio index from Kenneth French online data library (http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html) and obtain an index bank returns for the two months after the announcement. The number of observations is equal to 253 because we are unable to determine the exact month when the deal was announced for one of the transitioning banks.

related to the probability of transition success (either M&A or IPO). We provide further evidence of this association using two placebo tests. In Columns 2 and 3, we run a similar regression than in Column 1 but using bank returns measured one year before and one year after the deal, respectively. We find that the coefficient for the bank returns index is not significant in both regressions. This provides some assurance on the fact that returns following the deal announcements are likely to explain the probability of deal success. In the ideal case, one should use market returns as an instrument for the variable *transition* and run the cross-sectional regressions that we do in Table 2.3. Panel B using the sample of ‘transitioning’ banks and banks that whose ‘transitioning’ deal fails and remain private until 2006. However, given that market returns are measured at the time of transition and our main control variables are measured at right before the 2007/2008 crisis, running a two-stage least squares regression using the fitted value for *transition* from Table 2.3, Panel C-1, Column 1 is not possible for our setting. Thus, in Panel C-2 we run the same regressions that we run in Panel B Column 2 but using the group of transitioning banks as a treatment group and the group of 29 banks whose deals did not go through and remained private during the sample period as a control group. We add to this regression deal year dummies. We again find that, consistent with Table 2.3 Panel B, the coefficient for *Crisis ROA 98-q5* is positive and significant and the coefficient for the variable *Crisis ROA 98-q5*transition* is negative and statistically significant at the 5% level. Even if this test may not fully address the endogeneity related to a private-to-public transition, the fact that we find a result that is line with the findings in Table 2.3 Panel B provides some assurance on the robustness of our results.

[Insert Table 2. 3 about here]

2.3.3 Risk dynamics of banks following their transition

Previous literature supports the view that stock-market listing influences various corporate policies in non-financial firms (e.g., Bernstein, 2015; Brav, 2009; Michaely and Roberts, 2012) and banks (Falato and Scharfstein, 2016) and, in particular, that it can induce short-termist market influences that distort investment decisions (Asker et al. 2014, Davies et al. 2014, Graham et al. 2005, Poterba and Summers 1995). In banking, the easiest way to increase short-term profitability is by assuming higher risks, either on the asset side by growing faster (Fahlenbrach et al. 2017) or by changing the liabilities structure replacing capital by cheaper sources of funding (Demirgüç-Kunt and Huizinga 2010). In light of these previous findings, a possible explanation for our results in Table 2.3 is that banks that make a private-to-public transition increase some risk indicators. In Table 2.4 Panel A, we study the evolution of risk dynamics before during and after the transition takes place. We focus on capital (proxy by the Tier 1 capital ratio), non-deposit debt funding, asset growth and loan growth. We run a panel difference-in-differences regressions estimated as follows:

$$Risk_{it} = \beta_0 + \beta_1 PreTransition + \beta_2 Transition + \beta_3 PostTransition + \beta_4 X_{i,t-1} + \mu_i + \pi_t + \vartheta_r + \varepsilon_{i,t} \quad (3)$$

where *PreTransition* takes the value one for banks that make a private-to-public transition the year before a bank transitions and zero otherwise, *Transition* takes the value one for banks that make a private-to-public transition the year a bank transitions and zero otherwise and *PostTransition* takes the value one for banks that make a private-to-public transition the years after a bank transitions and zero otherwise, $X_{i,t-1}$ represents some bank level lagged control variables, μ_i represents bank fixed effects, π_t represents year dummies, and ϑ_r is a regulator (FDIC, Fed or OCC) dummy. $\varepsilon_{i,t}$ is an error term. The sample we use in this analysis is made of banks that make a private-to-public transition and private banks that are matched in the matching exercise we describe in section 2.2.4. The sample period is 1998-2006.

The coefficients of interest are *Transition* and *Post Transition*. A significant coefficient for these variables indicate that the year of the private-to-public transition and/or after the transition takes place banks change their risk indicators. In Column 1, our dependent variable is the Tier 1 ratio. Previous literature has documented that lower capital ratios increase bank risks and make bank more vulnerable during crisis times (e.g., Berger and Bouwman (2013)) We observe that the variable *Post Transition* is negative and statistically significant, suggesting that transitioning banks reduce their Tier 1 ratios with respect with the matched privately held counterparts after they become subject to market influence. In Column 2, our dependent variable is the non-deposit debt ratio over total assets. The literature in banking documents that higher non-deposit debt funding implies higher risks in banks (mostly related to debt roll-over) and makes banks less resilient to economic downturns (Demirgüç-Kunt and Huizinga 2010). In this regression we find that the coefficient for *Post Transition* is positive and significant, suggesting an increase in non-deposit debt financing with respect to the matched privately held banks after a bank transitions from private-to-public. In Column 3 and 4 we study asset growth and loan growth respectively. Fahlenbrach et al (2017) and Foos et al. (2010) document that higher loan growth on the asset side tends to be associated to higher risks. Fahlenbrach et al (2012) show that banks that grow faster before crises perform worse during crises. In Column 3, our dependent variable is the year-on-year growth of total assets. We observe in the regression that the coefficients for the variables *Transition* and *Post Transition* are positive and statistically significant. This implies that banks grow more once they make a private-to-public transition. In Column 4, we draw a similar conclusion for the year-on-year loan growth. We observe that upon transitioning, banks grow significantly more with respect to their privately held counterparts. The coefficient of the variable *Post*

Transition is not statistically significant although it is positive. Overall, these results suggest that banks increase risks once they make a private-to public transition⁴¹.

One concern with the results in Panel A can be that private-to-public transitions are endogenous and correlated with bank risk-taking. As a consequence, a matched group of private banks may not be a suitable control group. In Table 2.4 Panel B we address this potential endogeneity by using as a control group the 29 banks active during the period of study that “attempted” a private-to-public transition (as in Table 2.3 Panel C) but the deal was cancelled at some point. We collapse quarterly bank data around the transitioning deal (or the quarter in which a cancelled deal is announced) to perform these tests. Following Bayar and Chemmanur (2012) and Delis et al. (2017a), we use an instrumental variable (IV) treatment-effects model for our endogenous treatment. This model is a variant of the Heckman Inverse Mills Ratio (Heckman 1976, 1978). It is suited for cases in which endogeneity may arise due to unobservable factors that simultaneously affect both the probability of a private bank self-selecting its treatment (making a private-to-public transition) and the outcome variable. The econometric procedure we implement in this section is discussed in detailed in Maddala (1983), and it is a generalized version of the traditional two-step Heckman selection model, and therefore accounts for the effect of unobservables by using an error correction term⁴². This method has the advantage with respect to the standard instrumental variable framework that it takes into account the binary nature of the treatment variable (Clougherty and Duso 2015). In particular, we use a first-stage probit regression model to predict the probability that the bank makes a private-to-public transition (treatment equation) as follows:

⁴¹ We provide summary statistics of the characteristics of banks before transition in Appendix A.2.2.2

⁴² Cong and Drukker (2000) provide the technical details on how to implement this type of models in Stata.

$$\Pr(Transition) \tag{3}$$

$$= \gamma_0 + \gamma_1 BankReturnsIndex + \gamma_2 \Delta Risk_i^{Pre(t-6,t)} + \gamma_3 avgX_i^{Pre(t-6,t)} + \pi_t + \vartheta_r + \varepsilon_i$$

Where $\Delta Risk_i^{Pre(t-6,t)}$ is either the delta (average) of the outcome ratio (growth rate) between six quarters before the transition (the cancelled deal is announced) and the quarter in which the transition occurs (the cancelled deal is announced), $avgX_i^{Pre}$ is a vector of control variables. These variables are same that those used in Table 2.4., Panel A averaged over the six quarters previous to the transition (deal announcement for cancelled deals). The instrument that we use (*Bank Returns Index*) is the geometric mean of the returns of a bank index the two months following the deal initiation. From the treatment equation, the hazard for each observation is computed, serving as a correction term for the second-step. The correction term calculated for each firm in the first stage captures unobservable information related to enforcement actions and the outcome variable. In the second stage of the estimation procedure (the outcome equation), we estimate the following equation using OLS:

$$\Delta Risk_i^{Post(t,t+6)} \tag{4}$$

$$= \beta_0 + \beta_1 Transition + \beta_2 \Delta Risk_i^{Pre(t-6,t)} + \beta_3 avgX_i^{Pre(t-6,t)} + \delta lambda + \pi_t + \vartheta_r + \varepsilon_i$$

Where we regress the dependent variable of interest ($\Delta Risk_i^{Post(t,t+6)}$) which is either the change of the ratio used in Columns 1 or 2 or the average growth rate in Columns 3 and 4 in the six quarters following the transition (or the transition announcement for failed deals) on a set of independent variables that we use in the first stage ($\Delta Risk_i^{Pre(t-6,t)}$ and $avgX_i^{Pre(t-6,t)}$) and the treatment indicator *Transition* which is a dummy variable taking the value one for

banks that transition and zero for banks that initiate the IPO or are the target of a publicly held bank in an M&A deal but the transactions are cancelled. The hazard obtained in the treatment equation (λ is the parameter estimate on the hazard) is included as well as regulator and deal-year dummies.

Regarding the choice of the instrument, we follow Bernstein (2015) who instruments for private-to-public deal completion with an index of stock returns in the two months following the announcement of the deal. The economic intuition behind of this instrument is that stock returns over this short-term window affect the decision of a bank to go ahead with the deal or cancel it for both IPO and M&A. In line with the results we provide in Panel C-1 Table 2.3⁴³, we observe from the first stage regressions that the coefficient for *Bank Returns Index* is positive and statistically significant at the 1% level (in Columns 1 and 2) and at the 5% level (in Columns 3 and 4). This can be interpreted as that the probability of a “transitioning” deal completion is higher when market conditions are positive and vice versa. Given this result, we can assume that the instrument is relevant. Bank index returns over this short time window should satisfy the exclusion restriction (the instrument cannot be directly related to the outcome variable). Economically, bank index returns over this short-time window should not affect directly future risk-taking of the bank and if any, the effect would go through the transition itself.

In Table 2.4 Panel B we report the results of this test. We focus on the results of the second stages. In Columns 1 (and 2), we observe that transitioning banks reduce their Tier 1 ratios (increase their non-deposit debt financing) during the six quarters following the transition deal with respect to banks that “attempt” but do not undergo a private-to-public transition. In Column 3, we document that banks increase their asset growth over the six quarters after the

⁴³ Placebo tests using returns one year before and after the deal have a statistically insignificant coefficient which provides some additional assurance of the relevance of the instrument.

transition occurs with respect to banks in the control group. However, this coefficient is not statistically significant casting doubt on whether the result in Panel A may be driven by endogenous factors. Regarding loan-growth we observe that the coefficient for transition is positive and statistically significant, which implies that once we address the endogenous choice of making a private-to-public transition, banks' loan portfolio grows significantly more than for banks that do not become publicly held. Overall, these results point to the fact that the results reported in Table 2.4 Panel A are unlike to be driven by endogeneity (except for asset growth for which this conclusion is less clear).

[Insert Table 2. 4 about here]

2.3.4 Banks subject to influence from institutional investors with short-term horizons

We document in section 2.3.2 that private-to-public transitions of banks induce short-termist influences that are detrimental to the persistence of good performance across crises. In particular, transitioning from private to public between crises may impose an influence of stock-market forces that bias manager behaviors towards the short-term. This could make that banks that were good performers during 1998 crisis shift to riskier strategies before the last financial crisis in a way that hinders their performance during the crisis (once these increasing risks materialize). In the rest of this sub-section, we examine the relevance of this short-termist influence explanation. We do so in two steps. First, we assess whether, among banks that were top performers during the 1998 crisis, the detrimental effect of becoming publicly held on bank performance during the 2007-2008 crisis is more pronounced for transitioning banks subject to greater short-termist market influences. Second, we explore whether transitioning banks exposed to greater short-termist market influences change some key aspects of their business model (relative to banks that remain private and relative to banks that face lower short-termist influence from the stock-market).

Regarding the first step, we begin by differentiating banks that become publicly held according to the short-termist influences they face from financial markets. If a bias towards the short-term explains the detrimental effect of stock-market listing on the persistence of good performance across crises, this effect should be more pronounced for banks facing greater market short-termist influences. As a proxy for the extent to which a publicly held bank is exposed to short-termist influence from the stock-market, we use measures capturing whether the bank has institutional shareholders trading shares actively (more actively trading implies more short-termism). The motivation to use this proxy is twofold. First, bank stockholders can influence manager actions (Bliss and Flannery 2002). Second, previous research on investor horizons has shown that investors with short-term horizons impose pressure on managers to meet short-term earnings targets while long-term investors induce managers to invest for the long run (e.g., Bushee, 1998; Chen et al., 2007; Derrien et al., 2013; Gaspar et al., 2005; Stein, 1996). In addition to investor horizons, we focus on the presence of blockholders. Prior research shows that blockholders play a critical role in monitoring managers, deterring managerial myopia, and pushing managers to invest for the long run (Edmans 2009, Edmans and Holderness 2016).⁴⁴

To measure investor horizons, we follow recent literature in corporate finance and banking and compute several measures of investor horizons at the bank level (e.g., (Derrien et al. 2013, Garel and Petit-Romec 2017, Gaspar et al. 2005). Although it is impossible to directly observe the investment horizon of a given investor, it is revealed over time through the investor's trading behavior. To compute our measures of short-term institutional investor ownership, we use quarterly data from 13F Thomson Files. We compute the portfolio turnover of each institutional investor as the price-weighted share of stocks that have been sold over the last 12 quarters. We then obtain the weighted average of the portfolio turnover

⁴⁴ We provide characteristics of institutional shareholders variables in Appendix A.2.2.3.

of a firm's investors. This variable is named *MTurnover*. The second variable we use to measure short-term influence from the stock-market is the weighted average of the portfolio churn ratios of a firm's investors (*MChurnratio*) computed following Cella et al. (2013). Higher values of these variables imply, on average, a shorter investment horizon of a bank's investors. A detailed definition of these three variables is available in the Appendix 2.1. and summary statistics in Appendix 2.2.3. For some of our transitioning banks (or for their BHC), we are not able to find information in the 13F files so they are not present in this test. Finally, following Holderness (2003), we define as blockholders the institutional investors that hold at least 5% of a firm's stocks. We then sum the percentage of ownership held by blockholders to create the measure *blockholder ownership*.

We then use these three variables to classify banks that made a private-to-public transition according to the extent to which they face short-termist influence from the stock-market. Specifically, we classify banks with average turnover or with average churn ratio above the median, as facing greater stock-market driven short-termism. Similarly, we classify banks with blockholder ownership below the median as facing greater short-termist influence from the stock-market. Finally, we split the transition dummy into two dummies to capture whether a transitioning bank faces high or low stock-market driven short-termism. *Transition high ST influence* is a dummy variable that equals 1 if a bank makes a private-to-public transition between 1998 and 2006, and has a higher average turnover or churn ratio (i.e., above the median), or a lower portion of blockholders (i.e., below the median). Symmetrically, *Transition low ST influence* is a dummy variable that equals 1 if a bank becomes publicly held and has a lower average turnover or churn ratio (i.e., below the median), and a higher portion of blockholders (i.e., above the median). The median is computed for all transitioning banks with non-missing data on institutional ownership.

In Table 2.5, we regress performance during the last financial crisis on *Transition high ST influence* and *Transition low ST influence* dummies and the same control variables that we use in our baseline regressions. We only keep the banks in the top tercile of performance during the 1998 crisis (both transitioning and privately held). This is motivated by our findings in Table 2.3. In Table 2.3, Panel B we find that transitioning banks in the top quintile are the ones that underperform their privately held counterparts. However, in Table 2.5 we keep the top tercile in order to have a sufficient number of banks that allow us to run more meaningful regressions. If anything, taking the top tercile instead of the top quintile of banks according to their performance during the 1998 crisis would go against our conjecture and the results should, in principle, be stronger if we have kept only the top quintile.

The results show that among banks that were top performers in 1998, banks that become publicly held between the two crises do significantly worse than their counterparts that remained private only when they face greater short-termist influence from the stock-market. This is shown by the significant coefficient for the variable *Transition high ST influence*. By contrast, the dummy representing low stock-market driven short-termism is not significant for any of the three short termist measures in Columns 1, 2 and 3. When we test whether the difference between the coefficients for *Transition high ST influence* and *Transition low ST influence* is statistically significant, we find that the coefficients are different for the two measures of institutional investors horizon (*MTurnover* and *MChurnratio*) while the coefficients are not statistically different when we use *blockholder ownership* as splitting variable. Thus, top performer banks that become publicly held but face lower short-termist influence from the market do not tend to underperform compared with their counterparts that remain private during the last financial crisis. Overall, the results from Table 2.5 lend empirical support to the idea that stock-market listing induces short-termist influences that weighs negatively on the ability of a bank to persistently perform well across crises. We

present only the results for the group of top performers for brevity reasons. In line with the results in Table 2.3, unreported tests document that the transition dummies never turn out to be significant for any of the other groups (meaning first and second terciles of the variable *Crisis ROA 98*).⁴⁵

[Insert Table 2.5 about here]

Regarding the second step, we explore whether transitioning banks with greater short-termist influences from the market and their counterparts that remain private have different characteristics in the two pre-crisis periods. We also compare the former with banks that face lower short-termist market influence. As we find only significant differences for the sample banks that were among the best performers during the 1998 crisis (top tercile), we limit our analysis to these banks. We perform a t-test for the difference in means for some bank level variables at the end of 2006 and at the end of 1997. Table 2.6 reports the results for each of our three proxies for short-termist market influence (*MTurnover*, *MChurnratio* and *Blockholder ownership*). In Panel A, positive values of the mean difference imply that transitioning banks facing higher stock-market driven short-termism have a higher mean value for the specific variable than privately held banks and vice versa. We observe that banks facing stronger short-termist influence from the market tend to have significantly more asset and loan growth, lower Tier 1 capital ratios, more non-depository debt and short-term funding and less liquid assets in 2006 (prior to the 2007-2008 crisis) than their privately held counterparts. However, we find no significant differences between the two groups in terms of asset growth, bank capital, and short-term funding in 1997 (i.e., before the 1998 crisis).⁴⁶ These results are less pronounced when we use *blockholder ownership* as splitting variable. In Panel B, we compare the mean of the same ratios that we use above between transitioning

⁴⁵ The results of these regressions are available on request.

⁴⁶ For the variable *MTurnover*, we have 26 banks in the high pressure group and 27 for the variable *MChurnRatio*. For the variable *Blockholder ownership*, we have 32 banks in the high pressure group.

banks facing more short-term influence from the market and transitioning banks with lower short-term influence. Again, positive values of the mean difference imply that transitioning banks facing higher stock-market driven short-termism have a higher mean value for the specific variable than transitioning banks facing lower short-termist influence from the market and vice versa. We observe that the differences found above regarding higher asset and loan growth and lower Tier 1 ratios persist when we compare these two groups before the 2007/2008 crisis while these risk indicators are not statistically different before the 1998 crisis. However, in terms of short-term funding we do not find any statistically significant differences between these two groups. Regarding non-depository debt funding, we find that before the 2007/2008 crisis the mean difference is either not statistically significant (for MChurnratio or Blockholder ownership) or higher for banks facing lower short-term stock-market influence (for MTurnover).

Overall, the results from Table 2.6 provide suggesting evidence (in line with our findings in Table 2.5) that short-termist influences from financial markets induce banks to change some important aspects of their business model in a way that is likely to hinder their performance during crises. In particular, higher asset and loan growth and lower capital ratios are likely to be detrimental to a bank's ability to perform well during crises, as documented in previous studies (e.g., Berger and Bouwman, 2013; Fahlenbrach et al., 2012; Fahlenbrach et al., 2016; Laeven and Majnoni, 2003). This result is in line with the idea that becoming publicly held encourages banks to increase risks and the effect increases when stock-market driven short-termism is higher (Falato and Scharfstein 2016). The results for short-term debt funding and non-depository debt funding show that transitioning from privately to publicly held seems to lead to an increase in the reliance on this kind of funding by banks. However, our results do not point to the fact that the differences are significant for banks facing more

short-termist influence from the stock-market vis-à-vis banks that face lower short-termist influence regarding short-term debt and non-depository debt funding.

[Insert Table 2.6 about here]

2.3.5 Robustness tests

We conduct further empirical tests to examine whether our main results are sensitive to the empirical design that we have chosen. Table 2.7 shows the robustness tests. First, given that we compare public and private banks of similar size, a caveat is that our results may not generalize beyond large private banks.⁴⁷ In Table 2.7, Panel A, we therefore reproduce our regressions in which we use all the control variables from Table 2.2, Panel B, for public banks and Table 2. 2, Panel C, for private banks, without applying any matching. In the first two columns, we use Crisis ROA as a measure of performance, and in the last two columns, we use Crisis ROE. We find that our main results hold with this setting. In Column 1, the worst performers drive the persistence of bank performance across crises for the sample of public banks. In Column 2, both bottom and top performers drive the persistence of performance for private banks. These results are in line with our main results. The results using the ROE as a measure of performance in Columns 3 and 4 also confirm our main results. Second, we also run the same regression as in Panel A but aggregating the data at the level of the top holder using the variable RSSD9348 in call reports as in Acharya and Mora (2015). Data for banks that are held by the same BHC are aggregated at this level. Banks that are not held by a BHC remain in the sample. This procedure leads to 274 entities in the publicly held group and 1,521 in the privately held group. In line with our approach at the bank-entity level, we keep only the BHCs (or stand-alone banks) that have the same identifier between 1998 Q1 and 2007 Q3. Panel B shows the results. Again, we find that the persistence of performance for

⁴⁷ Alternatively, they may not generalize to large publicly listed banks. However, our matching procedure in Section 2.4 excludes a large fraction of private banks but only a small fraction of public banks.

publicly held banking groups is driven by the worst performers doing badly across crises, while worst performers doing poorly and best performers doing well across crises is the pattern we find for privately held entities.

[Insert Table 2.7 about here]

2.4 Conclusion

The objective of this paper is to explore empirically whether stock-market listing influences the persistence of bank performance across crises. We have two key results. First, we find that for both publicly and privately held banks, bank performance during the 1998 crisis is a strong predictor of its performance in the 2007–2008 crisis. This indicates that the persistence of bank performance across crises, first documented by Fahlenbrach et al. (2012) for a sample of U.S. publicly traded banks, also applies to privately held banks. Second, while for publicly held banks the persistence is uniquely driven by bottom performers, we show that in the case of privately held firms, the persistence is also driven by a group of top performers that did well during the 1998 crisis and continued to do so during the 2007–2008 crisis. We further show that among banks that were top performers in 1998, banks that made a private-to-public transition between the two crises significantly underperformed their counterparts that remained private. This effect is more pronounced when banks that become publicly held face greater short-termist influence from financial markets. Additional tests provide suggestive evidence that, among the banks that were top performers during the 1998 crisis, banks that become publicly held and face stronger stock-market driven short-termism change some important aspects of their business model (more asset growth, less capital, more loan portfolio growth) in a way that is likely to weigh negatively on their performance during crisis periods.

This result is consistent with the idea that short-termist influences from stock-market listing have an influence on managers that bias their decisions towards the short-term. The results of our paper suggest that when managers are subject to more stock-market driven short-termism (shareholders put more focus on short-term gains), their banks become riskier. We do not deny that market discipline may generate positive effects for banks (as foreseen by Pillar 3 of the Basel Accords), however, the influence from market forces may also have negative effects. Our paper contribute to this debate showing that that short-termist influence from the market reduces the learning of banking institutions from past achievements (top performance during crisis period) making them less able to perform well during shocks due to higher risk-taking before the crisis arrives. The results provided by our paper inform the regulatory debate about the effects that stock-market driven short-termism has on banks business models and performance during crisis periods. It also provides evidence consistent with the fact that shareholding stability (lower shareholders turnover) increases the ability of banks to better perform during economic shocks.

Table 2.1 Summary statistics

This table presents summary statistics of all banks in the matched sample in Panel A and a correlation table in Panel B. Variables are defined in the Appendix 2.1.

PANEL A : Summary statistics							Publicly held			Privately held			Mean diff.
All banks													
Variable	N	Mean	SD	p50	Min	Max	N	Mean	SD	N	Mean	SD	p-val
Ln(Assets)	770	13.42	0.81	13.38	11.93	17.24	385	13.41	0.78	385	13.44	0.84	0.62
Crisis ROA 98	770	0.63	0.34	0.64	-0.64	1.61	385	0.6	0.35	385	0.65	0.33	0.04
Crisis ROA 07/08	770	0.91	1.41	1.18	-5.41	3.5	385	0.71	1.41	385	1.1	1.39	0.00
Crisis ROE 98	770	7.02	3.77	7.11	-5.09	18.66	385	6.82	3.85	385	7.22	3.68	0.14
Crisis ROE 07/08	770	10.25	14.26	12.28	-48.03	41.27	385	8.41	14.21	385	12.09	14.1	0.00
NPL ratio	770	0.67	0.71	0.45	0	4.14	385	0.62	0.63	385	0.71	0.78	0.08
ROA pre-crisis	770	1.75	0.78	1.65	0.03	4.79	385	1.65	0.7	385	1.84	0.84	0.00
ROE pre-crisis	770	19.65	9.06	18.62	0.3	49.56	385	18.69	8.57	385	20.61	9.44	0.00
Liquid assets ratio	770	19.7	10.95	18.13	2.01	59.58	385	19.5	10.7	385	19.89	11.22	0.62
Loan concentration	770	0.62	0.15	0.63	0.26	0.95	385	0.64	0.14	385	0.61	0.16	0.01
Loan Ratio	770	70.73	12.15	72.35	36.19	92.23	385	71.11	12.71	385	70.34	11.57	0.38
Deposits ratio	770	80.17	7.54	81.38	53.6	92.38	385	79.02	7.32	385	81.33	7.59	0.00
Tier 1 ratio	770	11.65	3.3	10.63	7.97	31.39	385	11.22	2.42	385	12.09	3.95	0.00
Asset growth	770	3.38	3.23	2.57	-0.89	16.85	385	3.55	3.41	385	3.20	3.04	0.13
Loan growth	770	3.92	3.53	3.06	-0.89	20.30	385	4.10	3.65	385	3.73	3.40	0.15
ST funding	770	20.6	9.24	19.41	3.93	53.12	385	21.04	9.07	385	20.15	9.40	0.18
Non-deposit debt funding	770	10.24	8.65	8.49	0.45	34.20	385	11.20	8.53	385	9.29	8.69	0.00
RE loans ratio	770	74.84	13.41	77.11	29.91	97.39	385	76.21	12.25	385	73.47	14.36	0.00
BHC	770	0.95	0.22	1	0	1	385	0.97	0.16	385	0.93	0.26	0.00

Panel B : Correlations of main dependent and independent variables used in the empirical analysis

N=770	Crisis ROA 07/08	Crisis ROA 98	Crisis ROE 07/08	Crisis ROE 98	ROA pre- crisis	ROE pre- crisis	Tier 1 ratio	NPL ratio	Ln(Assets)	Deposits ratio	Loan concentration	Loan ratio	BHC
Crisis ROA 07/08	1												
Crisis ROA 98	0.2812	1											
Crisis ROE 07/08	0.9659	0.2783	1										
Crisis ROE 98	0.2507	0.8849	0.2852	1									
ROA pre-crisis	0.2112	0.3564	0.2005	0.3088	1								
ROE pre-crisis	0.2384	0.2869	0.2942	0.337	0.8798	1							
Tier 1 ratio	0.1128	0.1359	0.0163	-0.0791	0.0443	-0.2041	1						
NPL ratio	-0.1967	0.0111	-0.2344	-0.0146	-0.051	-0.1104	0.132	1					
Ln(Assets)	-0.1211	0.1105	-0.1022	0.1783	0.1231	0.1397	-0.1996	-0.0447	1				
Deposits ratio	0.1061	-0.0189	0.1146	0.0244	0.0872	0.1382	-0.1363	-0.0036	-0.2011	1			
Loan concentration	-0.2358	0.0068	-0.2556	0.0084	0.117	0.0956	-0.027	0.0655	0.0017	-0.0109	1		
Loan ratio	-0.0836	-0.0212	-0.0862	0.0421	0.2234	0.2394	-0.5121	0.0024	0.0266	0.148	0.2309	1	
BHC	-0.0034	-0.047	0.0284	0.029	-0.0201	0.0409	-0.2227	-0.0931	0.006	0.0747	-0.0403	0.0634	1

Table 2.2 Performance of banks in the 2008 crisis and performance during the 1998 crisis

This table shows results from cross-sectional regressions of the last financial crisis ROA and ROE on the Crisis ROA and ROE during the 1998 crisis in Panel A. In Panels B and C, the variable Crisis ROA 98 and Crisis ROE 98 are split into quintiles. ROA and ROE pre-crisis are computed over the five quarters before 2006 Q4. All other control variables are computed at 2006 Q4. The variable definitions are provided in the Appendix 2.1. The sample is indicated in the table. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

PANEL A	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Crisis ROA 07/08	Crisis ROA 07/08	Crisis ROA 07/08	Crisis ROE 07/08	Crisis ROE 07/08	Crisis ROE 07/08
	Publicly held	Privately held	All	Publicly held	Privately held	All
Crisis ROA 98	0.620*** (0.187)	1.462*** (0.312)	1.400*** (0.282)			
Crisis ROE 98				0.606*** (0.224)	1.282*** (0.248)	1.192*** (0.235)
Crisis ROA 98*Publicly held			-0.762** (0.314)			
Crisis ROE 98*Publicly held						-0.520* (0.277)
Publicly held			0.249 (0.231)			1.575 (2.332)
ROA pre-crisis	0.320*** (0.121)	0.225 (0.138)	0.261*** (0.095)			
ROE pre-crisis				0.462*** (0.117)	0.263** (0.113)	0.352*** (0.082)
Tier 1 ratio	0.014 (0.036)	0.005 (0.023)	0.012 (0.018)	0.047 (0.304)	0.021 (0.201)	0.086 (0.153)
NPL ratio	-0.547*** (0.139)	-0.264*** (0.095)	-0.378*** (0.081)	-5.652*** (1.294)	-3.025*** (0.898)	-4.078*** (0.769)
Ln(Assets)	-0.281*** (0.095)	-0.245*** (0.086)	-0.268*** (0.063)	-3.123*** (0.837)	-2.922*** (0.901)	-3.097*** (0.613)
Deposits ratio	-0.003 (0.008)	0.025*** (0.010)	0.012* (0.006)	-0.114 (0.073)	0.249** (0.099)	0.077 (0.063)
Loan concentration	-2.271*** (0.425)	-2.088*** (0.460)	-2.142*** (0.320)	-24.909*** (3.955)	-22.023*** (4.381)	-23.234*** (3.089)
Loan ratio	0.003 (0.007)	-0.017** (0.007)	-0.006 (0.004)	-0.030 (0.065)	-0.212*** (0.067)	-0.106** (0.044)
BHC	-0.119 (0.294)	0.093 (0.258)	0.030 (0.214)	-1.244 (3.005)	0.390 (2.410)	0.033 (2.045)
Constant	5.336*** (1.866)	3.473* (1.986)	0.012 (0.018)	68.738*** (16.414)	46.283** (19.672)	55.025*** (12.971)
Observations	385	385	770	385	385	770
Adj. R ²	0.162	0.292	0.231	0.247	0.304	0.273
P-val of the sum of ROA 98 (ROE 98) and the interaction term			0.00			0.00

PANEL B	(1)	(2)	(3)	(4)
VARIABLES	Crisis ROA 07/08	Crisis ROA 07/08	Crisis ROE 07/08	Crisis ROE 07/08
Sample :			Publicly held banks	
Crisis ROA 98-q1	-0.536*** (0.204)	-0.536*** (0.202)		
Crisis ROA 98-q2	-0.437* (0.229)	-0.439** (0.223)		
Crisis ROA 98-q4	-0.075 (0.202)	-0.189 (0.204)		
Crisis ROA 98-q5	0.213 (0.190)	0.093 (0.199)		
Crisis ROE 98-q1			-4.081* (2.228)	-3.939* (2.094)
Crisis ROE 98-q2			-3.172 (2.275)	-2.732 (2.122)
Crisis ROE 98-q4			0.995 (2.405)	0.980 (2.182)
Crisis ROE 98-q5			2.987 (2.249)	2.873 (2.140)
Constant	0.881*** (0.122)	5.855*** (1.914)	9.060*** (1.652)	70.801*** (16.300)
Control variables used in Panel A	No	Yes	No	Yes
Observations	385	385	385	385
Adj. R ²	0.029	0.160	0.024	0.212

PANEL C	(1)	(2)	(3)	(4)
VARIABLES	Crisis ROA 07/08	Crisis ROA 07/08	Crisis ROE 07/08	Crisis ROE 07/08
Sample	Privately held banks			
Crisis ROA 98-q1	-0.830*** (0.223)	-0.654*** (0.191)		
Crisis ROA 98-q2	-0.023 (0.160)	-0.142 (0.151)		
Crisis ROA 98-q4	0.151 (0.185)	0.070 (0.174)		
Crisis ROA 98-q5	0.598*** (0.202)	0.540*** (0.208)		
Crisis ROE 98-q1			-9.445*** (2.227)	-7.475*** (1.992)
Crisis ROE 98-q2			-3.443** (1.516)	-3.728*** (1.408)
Crisis ROE 98-q4			-1.251 (2.046)	-0.124 (1.780)
Crisis ROE 98-q5			3.724** (1.739)	4.395*** (1.621)
Constant	1.125*** (0.113)	4.260** (2.020)	14.173*** (0.944)	52.474*** (19.922)
Control variables used in Panel A	No	Yes	No	Yes
Observations	385	385	385	385
Adj. R ²	0.102	0.252	0.086	0.275

Table 2.3 Tests on the performance of banks that were privately held during the 1998 crisis but become publicly held or part of a public BHC before the 2008 crisis.

This table shows in Panel A the summary statistics for the sample of banks transitioning from private to public status between 1999 Q1 and 2006 Q4 and a matched sample of banks that remained privately held. In the last two columns we also show the summary statistics of banks that attempted a transition but the deal failed. Panel B shows in Columns 1 and 2 the cross-sectional regressions of the *Crisis ROA 07/08* on a dummy variable *Transition* that takes the value of 1 if the bank transitions from private to public between 1999 Q1 and 2006 Q4 and 0 otherwise, each *Crisis ROA 98* quantile indicator, the interaction term of each *Crisis ROA98* quantile indicator and *Transition*, and some pre-crisis control variables. In Panel C-1 we use a sample of banks that make a private-to-public transition and a sample of banks that attempt this transition but the deal fail (the M&A deal or the IPO is withdrawn) and show, in Column 1, a linear probability regression in which the dependent variable is the a dummy variable taking the value one if the bank make a private-to-public transition and zero otherwise on a bank index return variable in the two months following the announcement of the deal (a geometric mean of the two months returns) and a set of control variables measured at the year when the deal occurs. In Columns 2 and 3, we run a similar regression than in Column 1 but using bank returns measured one year before and after the deal, respectively. In Panel C-2 we run the same regression than in Panel B but using the sample of banks that make a private-to-public transition and banks that attempt this transition but the deal fail (the M&A deal or the IPO is withdrawn). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Panel A	Mean difference	N	SE	p-val	Privately held obs.	Privately held mean	Publicly held obs.	Publicly held mean	Failed transition obs.	Failed transition banks mean
Ln(Assets)	0.011	450	0.081	0.896	225	13.435	225	13.425	29	12.678
Crisis ROA 07/08	0.536	450	0.147	0.000	225	1.080	225	0.544	29	0.591
Crisis ROA 98	0.101	450	0.034	0.003	225	0.646	225	0.546	29	0.455
ROA pre-crisis	0.217	450	0.074	0.004	225	1.871	225	1.654	29	1.804
Tier 1 ratio	0.805	450	0.329	0.015	225	12.193	225	11.388	29	14.616
NPL ratio	0.182	450	0.062	0.004	225	0.744	225	0.562	29	1.137
Deposits ratio	2.471	450	0.684	0.000	225	81.402	225	78.931	29	82.529
Loan concentration	-0.040	450	0.014	0.003	225	0.603	225	0.643	29	0.657
Loan ratio	-0.462	450	1.139	0.686	225	70.214	225	70.676	29	65.759
BHC	-0.044	450	0.022	0.043	225	0.920	225	0.964	29	0.758

PANEL B	(1)	(2)
VARIABLES	Crisis ROA 07/08	Crisis ROA 07/08
Transition	-0.182 (0.275)	-0.087 (0.249)
Crisis ROA 98q1* Transition	0.267 (0.499)	0.094 (0.435)
Crisis ROA 98q2* Transition	-0.365 (0.388)	-0.334 (0.379)
Crisis ROA 98q4* Transition	-0.516 (0.456)	-0.380 (0.406)
Crisis ROA 98q5* Transition	-0.707* (0.403)	-0.642* (0.359)
Crisis ROA 98q1	-0.905** (0.386)	-0.707** (0.311)
Crisis ROA 98q2	0.147 (0.221)	0.128 (0.195)
Crisis ROA 98q4	0.348 (0.267)	0.199 (0.226)
Crisis ROA 98q5	0.925*** (0.266)	0.842*** (0.226)
ROA pre-crisis		0.307** (0.135)
Tier 1 ratio		0.019 (0.023)
NPL ratio		-0.523*** (0.128)
Ln(Assets)		-0.286*** (0.087)
Deposits ratio		0.016* (0.009)
Loan concentration		-2.211*** (0.468)
Loan ratio		-0.010 (0.007)
BHC		-0.167 (0.208)
Constant	0.909*** (0.182)	5.290*** (1.819)
Observations	450	450
Sample	Transition+private matched	Transition+private matched
Adj. R-squared	0.091	0.224
P-val of the sum of Crisis ROA 98q5 and Crisis ROA 98q5* Transition	0.47	0.49

PANEL C-1	(1)	(2)	(3)
VARIABLES	Deal Success	Deal Success Placebo 1 y before	Deal Success Placebo 1 y after
Bank Returns Index (transition)	0.015*** (0.004)		
Placebo Bank Returns Index		-0.213 (0.472)	0.555 (0.402)
Tier 1 ratio (transition)	-0.006*** (0.002)	-0.006*** (0.002)	-0.006*** (0.002)
ROA (transition)	-0.010 (0.014)	-0.012 (0.014)	-0.011 (0.014)
NPL ratio (transition)	-0.029 (0.018)	-0.044** (0.018)	-0.044** (0.018)
Ln(Assets) (transition)	0.077*** (0.021)	0.068*** (0.022)	0.070*** (0.022)
Deposits ratio (transition)	-0.001 (0.002)	-0.002 (0.002)	-0.001 (0.002)
Loan concentration (transition)	0.090 (0.118)	0.049 (0.122)	0.052 (0.121)
Loan ratio (transition)	-0.001 (0.001)	-0.000 (0.002)	-0.000 (0.001)
BHC (transition)	0.290*** (0.060)	0.315*** (0.061)	0.306*** (0.061)
Constant	-0.176 (0.399)	0.003 (0.409)	-0.047 (0.408)
Deal year dummies	YES	YES	YES
Observations	253 Transition + Failed deal banks	253 Transition + Failed deal banks	253 Transition + Failed deal banks
Adj. R-squared	0.383	0.344	0.349

PANEL C-2	(1)
VARIABLES	Crisis ROA 07/08
Transition	-0.046 (0.641)
Crisis ROA 98q1* Transition	-1.361 (0.997)
Crisis ROA 98q2* Transition	-2.075** (1.019)
Crisis ROA 98q4* Transition	-0.977 (1.521)
Crisis ROA 98q5* Transition	-2.293** (1.114)
Crisis ROA 98q1	-0.054 (0.889)
Crisis ROA 98q2	1.450 (0.946)
Crisis ROA 98q4	0.364 (1.464)
Crisis ROA 98q5	2.240** (0.962)
ROA pre-crisis	0.625*** (0.236)
Tier 1 ratio	-0.101 (0.070)
NPL ratio	-1.135*** (0.276)
Ln(Assets)	-0.738*** (0.195)
Deposits ratio	0.022 (0.021)
Loan concentration	-2.740** (1.328)
Loan ratio	-0.009 (0.016)
BHC	0.185 (0.671)
Constant	10.800** (4.598)
Deal year dummies	YES
Observations	254
Sample	Transition + Failed deal banks
Adj. R-squared	0.377
P-val of the sum of Crisis ROA 98q5 and Crisis ROA 98q5* Transition	0.92

Table 2.4 Risk dynamics upon transitioning

This table reports in Panel A panel difference-in-differences regressions estimated as follows: $Risk_{it} = \beta_0 + \beta_1 PreTransition + \beta_2 Transition + \beta_3 PostTransition + \beta_4 X_{i,t-1} + \mu_i + \pi_t + \vartheta_r + \varepsilon_{i,t}$ where *PreTransition* takes the value one for banks that make a private-to-public transition the year before a bank transitions and zero otherwise, *Transition* takes the value one for banks that make a private-to-public transition the year a bank transitions and zero otherwise and *PostTransition* takes the value one for banks that make a private-to-public transition the years after a bank transitions and zero otherwise, $X_{i,t-1}$ represents some bank level lagged control variables, μ_i represents bank fixed effects, π_t represents year dummies, and ϑ_r is a regulator (FDIC, Fed or OCC) dummy. $\varepsilon_{i,t}$ is an error term. The sample we use in this analysis is made of banks that make a private-to-public transition and banks that are matched in the matching exercise we describe in section 2.2.4. The sample period is 1998-2006. Robust standard errors, clustered at bank level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

This table reports in Panel B cross sectional regressions estimated in two steps using an Instrumental Variable treatment-effects model (Maddala 1983). The first stage is estimated as a probit model: $Pr(Transition) = \gamma_0 + \gamma_1 BankReturnsIndex + \gamma_2 \Delta Risk_i^{Pre} + \gamma_3 avgX_i^{Pre} + \pi_t + \vartheta_r + \varepsilon_i$. The second stage is estimated using OLS as follows: $\Delta Risk_i^{Post(t,t+6)} = \beta_0 + \beta_1 Transition + \beta_2 \Delta Risk_i^{Pre(t-6,t)} + \beta_3 avgX_i^{Pre(t-6,t)} + \delta lambda + \pi_t + \vartheta_r + \varepsilon_i$. *Transition* is a dummy variable taking the value one for banks that transition and zero for banks that initiate the IPO or are the target of a publicly held bank in an M&A deal but the transaction is cancelled. *BankReturnIndex* is the geometric mean return of a bank index during the two months following the announcement of the deal. $\Delta Risk_i^{Post(t,t+6)}$ is either the change of the ratio used in Columns 1 or 2 or the average growth rate in Columns 3 and 4 in the six quarters following the transition (or the transition announcement for failed deals). $\Delta Risk_i^{Pre(t-6,t)}$ is either the change of the ratio used in Columns 1 or 2 or the average growth rate in Columns 3 and 4 in the six quarters preceding the transition (or the transition announcement for failed deals). $avgX_i^{Pre(t-6,t)}$ is the average value computed over the six quarters preceding the transition (or the transition announcement for failed deals) for the control variables that are used in Panel A. *Lambda* is the term correction bias estimated from the first stage regression. , π_t represents deal-year dummies, and ϑ_r is a regulator (FDIC, Fed or OCC) dummy. $\varepsilon_{i,t}$ is an error term. The sample we use in this analysis is made of banks that make a private-to-public transition and a sample of banks that attempt this transition but the deal fail (the M&A deal or the IPO is withdrawn). The variables are collapsed to have their values before and after the quarter in which the transition deal takes place. *** p<0.01, ** p<0.05, * p<0.1.

Panel A	(1)	(2)	(3)	(4)
VARIABLES	Tier 1 ratio	Non-deposit debt to assets	Assets growth	Loan growth
Pre-transition	-0.328 (0.200)	0.068 (0.368)	-1.227 (1.415)	-2.684* (1.564)
Transition	-0.290 (0.283)	0.653 (0.435)	6.131*** (2.202)	4.262* (2.423)
Post-transition	-0.595** (0.274)	0.987* (0.503)	3.708* (2.180)	2.047 (2.202)
Tier 1 ratio		-0.133** (0.057)	1.294*** (0.229)	1.360*** (0.255)
ROA	0.325** (0.147)	0.453** (0.208)	-1.945* (1.083)	-2.979** (1.322)
NPL ratio	0.169* (0.094)	-0.195 (0.162)	-1.992*** (0.558)	-3.228*** (0.630)
Ln(Assets)	-0.978*** (0.336)	0.819 (0.542)	-19.189*** (1.490)	-17.486*** (1.798)
Deposits ratio	-0.084*** (0.019)		-0.123 (0.102)	-0.129 (0.118)
Loan concentration	4.459*** (1.220)	4.281** (1.924)	7.602 (7.803)	9.662 (9.015)
Loan ratio	-0.070*** (0.012)	-0.075*** (0.019)	0.447*** (0.084)	-0.341*** (0.099)
BHC	-0.527 (0.454)	-0.104 (0.513)	5.940** (2.949)	7.418** (3.275)
Constant	33.805*** (5.007)	1.523 (7.500)	223.877*** (24.510)	260.324*** (29.355)
Observations	4,050	4,050	4,050	4,050
R-squared	0.812	0.823	0.393	0.406
Year FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Regulator dummies	Yes	Yes	Yes	Yes
Sample	Transition+Matched private	Transition+Matched private	Transition+Matched private	Transition+Matched private
Adj. R-squared	0.787	0.800	0.313	0.328

Panel B	(1)	(2)	(4)	(4)
SECOND STAGE	D Post Tier 1 ratio	D Post Non-deposit debt to assets	Post Avg Assets growth	Post Avg Loan growth
Transition	-2.586** (1.19)	5.779** (2.32)	1.201 (1.50)	3.140* (1.90)
D Pre Tier 1 ratio	-0.181*** (0.04)			
D Pre Non-deposit debt to assets		-0.276*** (0.07)		
Pre Avg Assets growth			0.312*** (0.06)	
Pre Avg Loan growth				0.285*** (0.07)
Constant	5.835 (3.85)	-16.327** (7.70)	-0.971 (5.83)	1.527 (6.52)
FIRST STAGE	P(Transition)	P(Transition)	P(Transition)	P(Transition)
Bank Returns Index	0.099*** (0.04)	0.102*** (0.04)	0.087** (0.04)	0.089** (0.04)
D Lagged Tier 1 ratio	-0.043 (0.05)			
D Lagged Non-deposit debt to assets		0.054 (0.04)		
Lagged Avg Assets growth			0.257*** (0.08)	
Lagged Avg Loan growth				0.132** (0.06)
Constant	-1.898 (3.94)	-1.355 (4.04)	-2.561 (4.44)	-2.504 (4.08)
Lambda	0.609 (0.68)	-2.465* (1.32)	-0.212 (0.93)	-0.776 (1.12)
Control variables used in Panel A averaged over the six quarters prior to the deal used in the two steps	Yes	Yes	Yes	Yes
Regulator dummies (in the two steps)	Yes	Yes	Yes	Yes
Transition year dummies (in the two steps)	Yes	Yes	Yes	Yes
Observations	253	253	253	253
Wald-test	167.9708	78.12479	110.3461	125.7343
p-value	2.27e-20	9.90e-06	1.00e-11	4.71e-13

Table 2.5 Evidence from short-term stock-market influence

This table shows the cross-sectional regressions of the Crisis ROA 07/08 on some pre-crisis characteristics. We split the matched sample of transitioning and private banks into three using the terciles of the variable Crisis ROA 98. We then run regressions for banks in each tercile separately. We only show the regressions of the banks in the top tercile in line with the results in Table 2.3. The variable *Transition high ST influence* takes the value of 1 for banks for which the variable *MTurnover* is above the median of the sample of transitioning banks and 0 otherwise (Column 1), the value of 1 if *MChurnRatio* is above the median of the sample of transitioning banks and 0 otherwise (Column 2), and, the value of 1 if *Blockholder ownership* is below the median of the sample of transitioning banks and 0 otherwise (Column 3). The variable *Transition low ST influence* takes the value of 1 for banks that have a value of the variable *MTurnover* is below the median of the sample of transitioning banks and 0 otherwise (Column 1), the value of 1 if *MChurnRatio* is below the median of the sample of transitioning banks and 0 otherwise (Column 2), and the value of 1 if *Blockholder ownership* is above the median of the sample of transitioning banks and 0 otherwise (Column 3). We compute the median for each splitting variable using all the banks in the transitioning group. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

VARIABLES	(1) Crisis ROA 07/08	(2) Crisis ROA 07/08	(3) Crisis ROA 07/08
Short-term market influence is measured by	<i>MTurnover</i>	<i>MChurnRatio</i>	<i>Blockholder ownership</i>
Sample	Best tercile in 98	Best tercile in 98	Best tercile in 98
Transition high ST influence	-0.913*** (0.285)	-0.978*** (0.296)	-0.741*** (0.264)
Transition low ST influence	-0.372 (0.275)	-0.277 (0.231)	-0.439 (0.277)
ROA pre-crisis	0.476*** (0.124)	0.449* (0.236)	0.494** (0.235)
Tier 1 ratio	0.039 (0.032)	0.041 (0.025)	0.039 (0.026)
NPL ratio	-0.363** (0.147)	-0.362** (0.168)	-0.347** (0.168)
Ln(Assets)	-0.227* (0.122)	-0.210* (0.124)	-0.278** (0.130)
Deposits ratio	-0.000 (0.016)	0.002 (0.013)	-0.002 (0.014)
Loan concentration	-0.239 (0.691)	-0.391 (0.905)	-0.341 (0.924)
Loan ratio	-0.012 (0.009)	-0.012 (0.009)	-0.015* (0.009)
BHC	0.442 (0.472)	0.437 (0.308)	0.442 (0.328)
Constant	4.082 (2.675)	3.704* (2.198)	5.119** (2.213)
Observations	139	139	139
Adj. R ²	0.241	0.253	0.230
p-val testing the difference of coefficients for Transition high ST influence vs. Transition low ST influence	0.09	0.04	0.39

Table 2.6 Evidence from stock-market driven short-termism: pre-crisis characteristics

This tables compare the mean differences for bank-level variables measured at the end of 2006 and at the end of 1997 between top performer banks (top tercile of the variable *Crisis ROA 98*) that transition from privately to publicly held with higher short-term stock-market influence and their counterparts that remained private in Panel A and their counterparts that are subject to lower short-term stock-market influence in Panel B. The banks in this table are the same as those in Table 4. We have 26 banks in the high short-termist influence group for the variable *MTurnover* and 27 for the *MChurnRatio*. For the variable *Blockholder ownership*, we have 32 banks in the high short-termist influence group. Mean difference is computed as follows in Panel A: mean of banks that transition with high short-termis stock-market influence minus mean of banks that remained private. Mean difference is computed as follows in Panel B: mean of banks that transition with high short-termist influence minus mean of banks that transition with low short-term influence. Therefore, a positive value means that the banks that transition from privately held to publicly held with high short-termist influence have a higher value than banks that remained private in Panel A (transition and face lower short-termist influence in Panel B) and vice versa.

Panel A		<i>MTurnover</i>					<i>MChurnRatio</i>					<i>Blockholder ownership</i>			
Variables in:		2006		1997			2006		1997			2006		1997	
Mean difference : High ST influence – Private matched	N	Mean difference	p-value	Mean difference	p-value	N	Mean difference	p-value	Mean difference	p-value	N	Mean difference	p-value	Mean difference	p-value
Non-deposit debt funding	109	2.99	0.02	-1.63	0.12	110	2.36	0.06	-1.65	0.11	115	4.05	0.00	-1.02	0.29
ST funding	109	4.63	0.03	2.21	0.17	110	4.55	0.03	1.56	0.34	115	3.61	0.04	2.67	0.11
Liquid assets ratio	109	-5.17	0.05	-2.82	0.32	110	-6.46	0.01	-3.26	0.24	115	-0.04	0.99	0.22	0.94
Tier 1 ratio	109	-2.83	0.01	-0.87	0.63	110	-2.85	0.01	-1.03	0.57	115	-0.86	0.42	-0.75	0.62
Asset growth	109	1.63	0.01	0.01	0.99	110	1.69	0.01	0.14	0.86	115	0.49	0.36	-0.33	0.62
Loan growth	109	1.45	0.04	0.22	0.82	110	1.15	0.10	0.21	0.83	115	-0.19	0.76	-0.28	0.75
RE loans	109	5.94	0.06	0.73	0.83	110	2.80	0.38	-2.37	0.50	115	3.35	0.26	1.06	0.75

Panel B		<i>M</i> Turnover				<i>M</i> ChurnRatio				<i>Blockholder ownership</i>					
		2006		1997		2006		1997		2006		1997			
Variables in:															
Mean difference : High ST influence – Low ST influence	N	Mean difference	p-value	Mean difference	p-value	N	Mean difference	p-value	Mean difference	p-value	N	Mean difference	p-value	Mean difference	p-value
Non-deposit debt funding	56	-2.46	0.21	-2.04	0.05	56	-3.76	0.05	-2.15	0.04	56	0.60	0.76	-1.13	0.28
ST funding	56	0.00	1.00	-0.89	0.72	56	-0.15	0.95	-2.18	0.39	56	2.37	0.35	-0.03	0.99
Liquid assets ratio	56	-0.32	0.45	-3.11	0.31	56	-0.32	0.44	-4.06	0.18	56	0.23	0.59	3.20	0.30
Tier 1 ratio	56	-2.16	0.01	-0.79	0.70	56	-2.28	0.01	-1.12	0.58	56	-1.90	0.03	-0.71	0.73
Asset growth	56	1.44	0.07	0.46	0.52	56	1.62	0.04	0.71	0.31	56	0.85	0.30	-0.21	0.78
Loan growth	56	1.76	0.02	0.13	0.89	56	1.25	0.09	0.95	0.34	56	1.62	0.04	0.05	0.96
RE loans	56	7.22	0.05	1.67	0.69	56	1.40	0.71	-4.26	0.30	56	-2.98	0.44	2.86	0.50

Table 2.7 Robustness tests: no matching and aggregated data at BHC level

This table shows results from cross-sectional regressions of the last financial crisis ROA and ROE on the Crisis ROA and ROE during the 1998 crisis. The variables Crisis ROA 98 and Crisis ROE 98 are split into quintiles. ROA and ROE pre-crisis are computed over the five quarters before 2006 Q4. All the other control variables are computed at 2006 Q4. The variable definitions are provided in the Appendix 2.1. The sample is indicated in the table. In Panel A, we use the whole sample of banks. In Panel B, the sample contains bank data aggregated at the level of the top holder (RSSD9348) for multibank holding companies. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

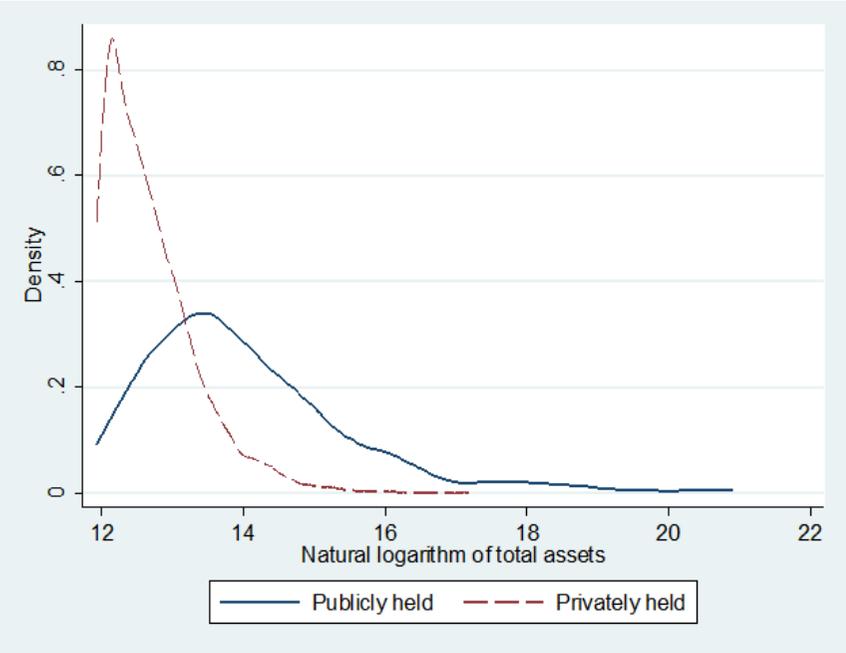
PANEL A VARIABLES	(1) Crisis ROA 07/08	(2) Crisis ROA 07/08	(3) Crisis ROE 07/08	(4) Crisis ROE 07/08
Crisis ROA 98-q1	-0.459** (0.187)	-0.531*** (0.075)		
Crisis ROA 98-q2	-0.478** (0.207)	-0.131** (0.055)		
Crisis ROA 98-q4	-0.063 (0.170)	-0.073 (0.067)		
Crisis ROA 98-q5	0.096 (0.170)	0.141* (0.082)		
Crisis ROE 98-q1			-5.781*** (1.892)	-4.473*** (0.783)
Crisis ROE 98-q2			-4.563** (1.852)	-0.419 (0.634)
Crisis ROE 98-q4			-0.694 (1.667)	0.601 (0.703)
Crisis ROE 98-q5			-1.467 (1.728)	2.915*** (0.848)
ROA pre-crisis	0.281** (0.137)	0.471*** (0.051)		
ROE pre-crisis			0.387*** (0.107)	0.437*** (0.045)
Tier 1 ratio	0.017 (0.030)	0.003 (0.008)	-0.011 (0.301)	-0.070 (0.073)
NPL ratio	-0.577*** (0.133)	-0.216*** (0.042)	-5.809*** (1.285)	-2.433*** (0.410)
Ln(Assets)	-0.168*** (0.049)	-0.182*** (0.045)	-2.246*** (0.508)	-2.001*** (0.467)
Deposits ratio	0.001 (0.008)	0.005 (0.004)	-0.063 (0.077)	0.089** (0.039)
Loan concentration	-2.223*** (0.373)	-1.660*** (0.174)	-23.424*** (3.996)	-16.004*** (1.775)
Loan ratio	-0.001 (0.006)	-0.011*** (0.002)	-0.046 (0.056)	-0.184*** (0.023)
BHC	-0.116 (0.290)	-0.078 (0.073)	-0.848 (3.047)	-0.588 (0.743)
Constant	4.343*** (1.413)	4.337*** (0.803)	0.387*** (0.107)	48.182*** (8.418)
Observations	509	1,941	509	1,941
Sample	Public	Private	Public	Private
Adj. R ²	0.147	0.261	0.193	0.266

PANEL B	(1)	(2)	(3)	(4)
VARIABLES	Crisis ROA 07/08	Crisis ROA 07/08	Crisis ROE 07/08	Crisis ROE 07/08
Crisis ROA 98-q1	-0.697*** (0.265)	-0.436*** (0.073)		
Crisis ROA 98-q2	-0.231 (0.199)	-0.133** (0.058)		
Crisis ROA 98-q4	-0.087 (0.185)	-0.058 (0.071)		
Crisis ROA 98-q5	-0.083 (0.213)	0.357*** (0.099)		
Crisis ROE 98-q1			-5.615** (2.735)	-3.071*** (0.808)
Crisis ROE 98-q2			0.307 (2.106)	-0.434 (0.641)
Crisis ROE 98-q4			0.431 (2.190)	0.366 (0.710)
Crisis ROE 98-q5			-2.023 (2.360)	2.686*** (0.906)
ROA pre-crisis	0.042 (0.067)	0.204*** (0.079)		
ROE pre-crisis			0.162 (0.114)	0.467*** (0.046)
Tier 1 ratio	0.016 (0.030)	0.019** (0.008)	0.104 (0.254)	0.088 (0.070)
NPL ratio	-0.511*** (0.149)	-0.224*** (0.038)	-5.628*** (1.520)	-2.164*** (0.396)
Ln(Assets)	-0.090 (0.063)	-0.061 (0.040)	-1.098 (0.685)	-1.035** (0.416)
Deposits ratio	0.007 (0.012)	0.012*** (0.004)	0.040 (0.118)	0.094** (0.045)
Loan concentration	-1.867*** (0.531)	-1.498*** (0.172)	-20.510*** (5.657)	-14.331*** (1.885)
Loan ratio	-0.005 (0.008)	-0.004 (0.003)	-0.078 (0.082)	-0.132*** (0.025)
BHC	-0.695 (0.453)	0.060 (0.086)	-4.130 (3.611)	-0.008 (0.875)
Constant	3.911** (1.728)	1.686** (0.774)	43.268** (17.079)	27.702*** (8.162)
Observations	274	1,521	274	1,521
Sample	Public	Private	Public	Private
Adj. R ²	0.116	0.272	0.133	0.273

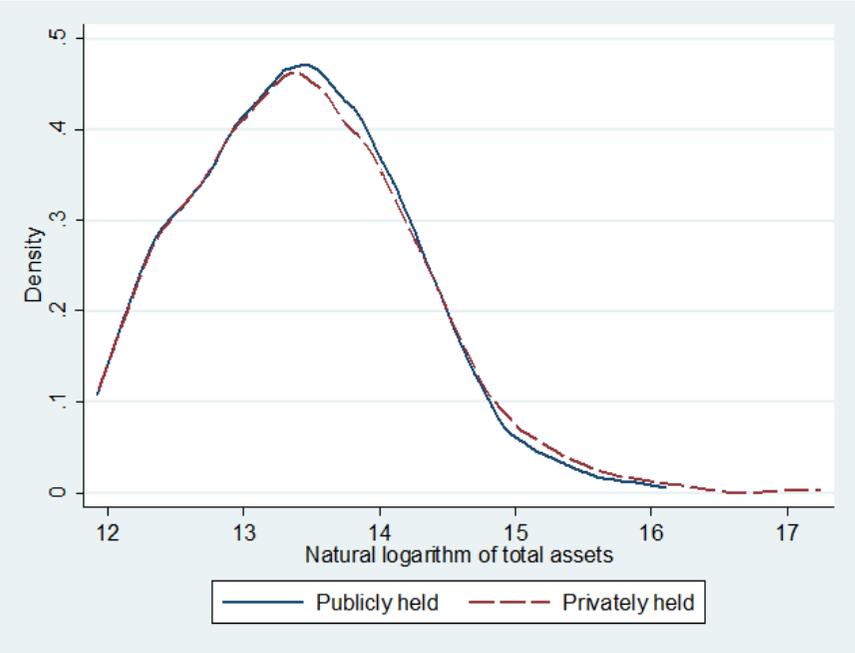
Figure 2.1 Comparing size distribution of private and public banks

This figure shows a kernel density distribution graph of the natural logarithm of total assets for the whole sample of banks before matching (on the left) and for the sample of matched banks (on the right).

a) Before matching



b) After matching



Appendix 2.1 Variable definitions

Variable name	Description
Crisis ROA 07/08 (%)	Cumulated quarterly net income (RIAD4340) for the period 2007 Q3–2008 Q3 divided by total assets (RCFD2170) as of 2007 Q2
Crisis ROE 07/08 (%)	Cumulated quarterly net income (RIAD4340) for the period 2007 Q3–2008 Q3 divided by total equity (RCFD 3210) as of 2007 Q2
Crisis ROA 98 (%)	Cumulated quarterly net income (RIAD4340) for the period 1998 Q3–1998 Q4 divided by total assets (RCFD2170) as of 1998 Q2
Crisis ROE 98 (%)	Cumulated quarterly net income (RIAD4340) for the period 1998 Q3–1998 Q4 divided by total equity (RCFD 3210) as of 1998 Q2
ROA pre-crisis (%)	Cumulated quarterly net income (RIAD4340) for the period 2005 Q4–2006 Q4 divided by total assets (RCFD2170) as of 2005 Q3
ROE pre-crisis (%)	Cumulated quarterly net income (RIAD4340) for the period 2005 Q4–2006 Q4 divided by total equity (RCFD 3210) as of 2005 Q3
Tier 1 ratio (%)	Core Tier 1 equity (RCFD8274) to the risk weighted assets (RCFDA223)
NPL ratio (%)	Non-performing loans (RCFD1407+ RCFD1403) to total loans (RCFD2122)
Ln(Assets)	Natural logarithm of total assets
Deposits ratio (%)	Total deposits (RCFD2200) to total assets
Loan Concentration	Loan share-based Herfindahl-Hirschman Index (loans secured by real estate, commercial and industrial loans, agricultural loans, consumer loans)
Liquid Assets ratio (%)	Liquid assets (RCFD1773+ RCFD0010) to total assets
Loan ratio (%)	Loans (RCFD2122) to total loans
BHC	Binary variable that takes the value of 1 if a bank is held by a bank holding company and 0 otherwise. In table 6 panel B this variable takes the value 1 when it is a Bank Holding Company and 0 when it is a standalone bank not held by a Bank Holding Company.
Asset growth (%)	Growth rate of the total assets
Loan growth (%)	Growth rate of the loan portfolio
RE loans ratio (%)	Real estate loans (RCFD1410) to total loans
Non-deposit debt funding (%)	Computed using call reports data as Total Assets – (Deposits + Total Equity)
ST Funding (%)	Short-Term debt (RCON2604+RCFN2200+RCFD2800+RCFD2332+RCFD3548) for 1997 and RCON2604+RCFN2200+RCONB993+RCFDB995+RCFDB571+RCFD3548 for 2006) divided by total assets
MTurnover	<p>Using quarterly data from 13-F Thomson Files, we compute the portfolio turnover as the price-weighted share of stocks that have been sold over the last 12 quarters (three-year period). Formally, we compute the portfolio turnover using the following formula:</p> $Turnover_{j,t} = \sum_{i=1}^n \frac{SoldShares_{i,t} * SharePrice_{i,t-12}}{TotalPortfolioValue_{i,t-12}},$ <p>where n is the number of different stocks contained in investor j at quarter t. We then average the final measure over four quarters. The final measure of investor j turnover for quarter t is defined as follows:</p>

	$AVGTURNOVER_{j,t} = \frac{1}{4} \times \sum_{t=-3}^0 Turnover_{j,t}.$ <p>Averaged investor portfolios turnover (TURNOVER) of a given bank:</p> $MTURNOVER_{i,t} = \sum_{j=1}^{j=n} \frac{AVGTURNOVER_{j,t} \times SHARESHELD_{i,t-1}}{SHARESOUT_{i,t-1}}.$
MChurnRatio	<p>Averaged j investor portfolios churn ratio of a given bank i. It measures how frequently institutional investors rotate the stocks in their portfolio and is constructed as in Gaspar et al. (2005). We compute it over a three-year period:</p> $CHURNRATIO_{j,t} = 2 \times \sum_{i=1}^n Shares_{i,t} \times Price_{i,t} - Shares_{i,t-12} \times Price_{i,t-12} - Shares_{i,t} \times (Price_{i,t} - Price_{i,t-12}) / (\sum_{i=1}^n Shares_{i,t} \times Price_{i,t} - Shares_{i,t-12} \times Price_{i,t-12})$ $MCHURNRATIO_{i,t} = \sum_{j=1}^{j=n} \frac{CHURNRATIO_{j,t} \times SHARESHELD_{j,t-1}}{SHARESOUT_{i,t-1}}.$
Blockholder ownership	<p>Institutional blockholding, or the percentage of shares outstanding held by a firm's institutional investors whose holdings are greater than 5% of the firm shares outstanding</p>

Appendix 2.2 Additional Tests and Summary Statistics

Table A.2.2.1 Additional Tests: Risk adjusted ROA

This table shows in Panel A the results from cross-sectional regressions of the last financial crisis Risk Adjusted ROA on the Risk Adjusted Crisis ROA and ROE during the 1998 crisis and the subset of control variables that we use over the chapter. In Panels B we show the summary statistics of the last financial crisis Risk Adjusted ROA on the Risk Adjusted Crisis ROA and ROE during the 1998 crisis. The Risk Adjusted ROA over crises periods is computed as the ROA over the crisis period divided by the pre-crisis standard deviation of the ROA computed over three years using quarterly data. All other control variables are computed at 2006 Q4. The variable definitions are provided in the Appendix 2.1. The sample is indicated in the table. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

PANEL A	(1)	(2)
VARIABLES	Risk Adjusted Crisis ROA 07/08	Risk Adjusted Crisis ROA 07/08
	Publicly held	Privately held
Risk Adjusted Crisis ROA 98	0.248** (0.115)	0.481*** (0.101)
ROA pre-crisis	3.778** (1.677)	4.154** (1.620)
Tier 1 ratio	-0.197 (0.633)	0.300 (0.361)
NPL ratio	-7.750*** (2.005)	-5.297*** (1.154)
Ln(Assets)	-3.473** (1.698)	-1.890 (1.492)
Deposits ratio	-0.249* (0.151)	0.355*** (0.124)
Loan concentration	-28.664*** (7.767)	-28.346*** (6.848)
Loan ratio	0.011 (0.110)	-0.230** (0.099)
BHC	0.714 (6.826)	1.508 (3.584)
Constant	94.894*** (34.233)	35.690 (29.303)
Observations	339	370
Adj. R-squared	0.100	0.182
P-val difference of coeff. for <i>Risk Adjusted Crisis ROA 98</i>	0.06	

PANEL B	Summary statistics								
Variable	N	mean	sd	min	max	p1	p50	p99	
Publicly held									
Risk Adjusted Crisis ROA 07/08	385	13.86	22.06	-58.60	75.18	-58.60	12.94	72.27	
Risk Adjusted Crisis ROA 98	339	14.90	11.28	-1.44	51.95	-1.11	11.92	47.75	
Privately held									
Risk Adjusted Crisis ROA 07/08	385	20.56	21.68	-58.60	75.18	-44.09	17.95	75.18	
Risk Adjusted Crisis ROA 98	370	13.33	10.02	-1.44	51.95	-1.44	11.32	51.95	

Table A.2.2.2 Summary statistics of banks making a private-to-public transition

This table reports in Panel A some summary statistics of the sample of banks that make a private-to-public transition and the sample of banks that are the target in an M&A deal in which the acquirer is a public bank or announce an IPO but the deal is cancelled. The mean and percentile values correspond to the variable Total Assets (in thousands) measured in 2006.

Panel A: Characteristics of transitioning banks regarding the method of transition

	<i>Banks that maket a private-to-public transition</i>										<i>Banks whose M&A or IPO deal is cancelled</i>		
	New listing in NYSE, AMEX or NASDAQ				M&A deal				Total		N	Mean Total Assets	P-value of mean difference of Ln(assets) between banks that transition and banks whose deal is cancelled
	N	Mean	p1	p99	N	Mean	p1	p99	Nb of banks (Nb of BHC)	Mean			
Total	154	1 039 766	151 879	5 982 196	71	943 318	157 544	7 567 937	225 (166)	1 009 331	29	505 586	0.00
Before or in 2002	96	1 192 301	151 879	9 181 474	44	1 257 267	162 499	7 567 937	140 (100)	1 212 719	19	406 686	0.00
After 2002	58	787 294	150 651	3 671 043	27	431 699	157 544	1 262 072	85 (66)	674 340	10	693 497	0.48

Panel B: Characteristics of transitioning banks and matched banks the year before transition

Summary statistics	All banks						Transitioning banks			Privately held			Mean diff. Transitioning > Private
	Variable	N	Mean	SD	p50	Min	Max	N	Mean	SD	N	Mean	
Ln(Assets)	450	12.66	0.84	12.62	10.71	14.83	225	12.56	0.79	225	12.76	0.87	0.99
NPL ratio	450	0.83	0.91	0.63	0.00	4.99	225	0.74	0.72	225	0.92	1.06	0.98
ROA	450	1.61	0.79	1.63	-0.94	3.76	225	1.58	0.70	225	1.64	0.87	0.78
ROE	450	18.13	8.34	18.33	-7.23	38.73	225	18.18	7.63	225	18.06	9.02	0.44
Liquid assets ratio	450	5.21	3.83	4.22	1.04	24.53	225	4.88	3.17	225	5.54	4.38	0.97
Loan concentration	450	0.54	0.14	0.54	0.28	0.90	225	0.57	0.14	225	0.52	0.15	0.00
Loan Ratio	450	66.30	12.52	67.72	26.13	90.21	225	66.87	12.05	225	65.73	12.99	0.17
Deposits ratio	450	83.06	6.86	85.00	61.97	92.54	225	83.36	6.44	225	82.75	7.25	0.17
Tier 1 ratio	450	12.72	4.40	11.27	7.55	31.37	225	12.30	3.56	225	13.14	5.08	0.98
Asset growth (annual)	450	16.51	19.89	10.82	-8.49	109.32	225	17.50	19.34	225	15.53	20.42	0.15
Loan growth (annual)	450	18.12	22.88	13.24	-12.85	127.20	225	18.52	22.00	225	17.71	23.77	0.35
ST funding	450	16.24	8.55	14.84	2.50	45.97	225	16.59	8.79	225	15.88	8.32	0.19
Non-deposit debt funding	450	7.89	7.52	5.82	0.29	28.42	225	7.68	6.36	225	8.11	8.53	0.73
RE loans ratio	450	67.23	15.24	69.13	25.86	94.73	225	69.92	13.52	225	64.54	16.37	0.00
BHC	450	0.86	0.35	1.00	0.00	1.00	225	0.82	0.39	225	0.89	0.31	0.99

Table A.2.2.3 Summary statistics of institutional investors' variables for banks that undergo a private to public transition

This table reports some summary statistics of the institutional investors variables corresponding to the banks or the BHC of banks that make a private-to-public transition. Definitions are provided in Appendix 2.1. The percentage of institutional investor ownership is given as provided by the Thomson 13-F files. The variables are measured in 2006 for all transitioning banks with information available in the sample.

Variable	N	Mean	sd	p50	p1	p99
Institutional ownership perc.	194	0.234	0.191	0.179	0.000	0.759
MChurnRatio	194	0.980	0.176	0.972	0.636	1.418
MTurnover	194	0.383	0.102	0.371	0.202	0.669
Blockholder ownership perc.	194	0.048	0.071	0.000	0.000	0.270

Appendix 2.3 Summary of the two crises studied in the chapter

The 1998 Crisis

According to Robert Rubin, then secretary of the Treasury of the US in 1998, the 1998 crisis was the “worst financial crisis in the last 50 years”. The crisis was ignited by the Russian default on its Sovereign debt in August, 1998 and it extended its tentacles to the US when the hedge fund Long-Term Capital Management (LTCM) collapsed. These events were contemporaneous with a financial downturn in other developing markets such as East Asia (mostly in Thailand, Indonesia and South Korea), Brazil, Turkey and Argentina. Upon the Russian default and LTCM collapse investors fled from government paper to safer US Treasuries. This shift of investors to safer liquidity caused unexpected tensions in the market. All these events caused significant losses to the US banks during late August and early September 1998. In September 1998, the Federal Reserve coordinated a private bailout of the LTCM fund which required \$3.5 billion of injection from a group of banks. The panic spread to other financial institutions that accumulated substantial write-offs due to losses in their investments during 1998Q3 and 1998Q4. According to Chava and Purnandam (2011) this crisis generated significant losses for banks and had significant negative real effects (they document a drop of lending to the real economy). These authors highlight the following reasons for banks losses: (1) direct exposure to Russian government bonds, (2) exposure to Russian borrowers, (3) losses in derivatives market, (4) losses on brokerage credit to LTCM and (5) increased counter-party risk in the interbank market.

The 2007/2008 Crisis

According to Berger and Bouwman (2013) the subprime lending crisis has been characterized by turmoil in financial markets as banks experienced difficulties in selling loans in the syndicated loan market and in securitizing loans. The supply of liquidity by banks dried up and the interbank market froze. Many banks started experiencing substantial losses by the end of 2007. Some banks were taken over due to the accumulated losses in capital (e.g., Countrywide was acquired by Bank of America and Bear Stearns by J.P. Morgan Chase). Large banks such as Washington Mutual and IndyMac failed during the third quarter of 2008 as well as Lehman Brothers, an investment bank. This meltdown in the financial sector led to the unprecedented intervention of the Federal Reserve extending safety-net benefits to investment banks and AIG and injecting money into the system through the purchase of commercial paper and mortgage backed securities (MBS). The US Treasury set up a bailout program called Troubled Asset Relief Program (TARP) that aimed at enhancing capital ratios of US banks. More than 700 banks received TARP monies. The inception of the program took place in October 2008. The crisis had substantial economic effects and the economic literature considers that it lasted until the end of 2009.

3. Chapter 2: Changes in the design of the tax system and bank capital ratios⁴⁸

Abstract

This paper studies how bank capital changes following the implementation and removal of a tax incentive on equity. We examine the impact of the introduction of a tax allowance in Italy granted to banks (and other firms) that increase their equity from a base year. Using a difference-in-differences setting, we observe an 8.83% increase in bank capital ratios following the implementation of this reform. When this tax mechanism is phased out, we observe an opposite effect on the equity ratio, showing the absence of a hysteresis effect in bank capital. We document a heterogeneous effect for large and small banks.

JEL Code: G21, G28, G32.

Key words: Tax, bank capital, debt-equity tax bias.

⁴⁸ This chapter draws from an article authored by Christophe Moussu and Jose M. Martin Flores accepted for publication in 2017 by the journal *European Financial Management*.

3.1 Introduction

Bank capital is important but imposing higher capital requirements on banks affects the offer of credit to the economy (Jiménez et al. 2017) and may result in a credit crunch with procyclical negative effects in crisis periods (Cornett et al. 2011, Dermine 2013). Another instrument available for policy makers to strengthen bank capital is to use tax incentives. Since interest payments are in general deductible from the corporate income tax base whereas equity returns are not, firms have an incentive to use higher leverage. The effect of interest tax shields on firm leverage has been widely studied for non-financial firms (e.g., Arena and Roper, 2010; Desai et al., 2004; Faccio and Xu, 2015; Feld et al., 2013; Graham, 1996; Graham and Tucker, 2006; Heider and Ljungqvist, 2015) and for banks (e.g. De Mooij and Keen, 2016; Hemmelgarn and Teichmann, 2014; Horváth, 2013; Milonas, 2016; Schandlbauer, 2017). However, banks are, in principle, more systemic than non-financial firms so the effect of this tax bias is particularly pernicious for them. Since bank capital provides some protection against failure in case of crisis, a tax bias that incentivizes the use of debt undermines to some extent the safeness of the banking system.

In this paper, we are interested in the impact of the implementation and removal of a tax reform which reduces this debt-equity tax bias. The tax scheme introduced in Italy in 1997 has similar characteristics to the Allowance for Corporate Equity (ACE) initially proposed by Devereux and Freeman (1991). This ACE-like mechanism grants a tax shield on equity to banks that increase their equity from a reference year. Banks (and other firms) apply a reduced tax rate (19% instead of 37%) on a notional return computed on equity increases. Using a difference-in differences setting, with Italian banks as treatment group and a matched sample of banks from other euro area states as control group, we find an 8.83% increase in bank capital ratios relative to the control group following the implementation of this tax

reform. We also observe that this increase is driven by a higher level of equity and not by a decrease in bank assets.

However, as the ACE tax incentive was withdrawn in 2002, we are able to provide new evidence on whether the introduction and removal of the tax allowance mechanism lead to symmetric effects. We find that once the ACE mechanism is no longer applicable, banks stop increasing their equity and readjust downward their equity ratios by 4.6% on average relative to banks in the control group. This finding documents that there is no hysteresis in bank capital when a tax incentive to increase equity is removed. This is in line with a static perspective of capital structure. When an ACE is introduced, the debt tax bias is reduced and banks rebalance their capital ratio upward. When the ACE is removed, the debt tax bias increases again, and bank capital is adjusted downward accordingly. Additional robustness tests show that our results are unlikely to be driven by changes in monetary policy or other economic events. Also, we show that our findings are robust to different control groups and the introduction of various control variables.

Our paper contributes to the existing literature on the effects of ACE mechanisms on capital structure. The most studied ACE mechanism in the finance and economics literature is the one introduced by Belgium in 2006. Panier et al. (2013) and Princen (2012) document a positive effect of the tax reform on equity ratios for non-financial firms, and Schepens (2016) for banks. However, the Italian ACE scheme differs from the Belgian reform of 2006 in three important respects. First, in Italy, the notional interest is computed only on new equity after the reform is in place and not on the existing stock of equity as in the Belgian case. This is particularly interesting as there are no “windfall rents” associated with reduced taxation on the existing equity. It therefore provides a great opportunity to test the effect of a tax incentive to increase equity for banks, as there are no tax-shields on existing equity in the Italian case. Our results confirm that a positive impact on bank capital ratios is also observed for a “softer”

version of the ACE. Second, the Italian ACE scheme includes an anti-avoidance clause that targets transactions between related parties in order to avoid abuses and tax planning (Zangari 2014). In particular, as documented by Hebous and Ruf (2017), multinational non-financial companies used the Belgian ACE mechanism to implement tax planning structures combining the benefits from the Belgian ACE with interest deductions. Thus, our result on the introduction of the ACE in Italy is important because it shows that a positive effect on bank capital is also found in an ACE system that limits the scope for tax planning opportunities. Third, an interesting feature of the ACE experiment in Italy is that it was phased out in 2002. This phasing-out allows us to provide new evidence on the existence of symmetric reactions of bank capital to the introduction and removal of a tax shield on equity increases.

Our paper also contributes to the bank capital structure literature. Previous work on this topic mainly focuses on the determinants of bank capital structure (e.g. Berger et al., 2008; Gropp and Heider, 2010) and their adjustments towards the optimal ratio (e.g. De Jonghe and Öztekin, 2015). Another stream of the bank capital structure literature has focused on the extent to which tax rates determine equity ratios. These papers show a negative (positive) relationship between tax rates and bank capital (leverage) ratios for international (e.g. De Mooij and Keen, 2016; Hemmelgarn and Teichmann, 2014; Horváth, 2013; Milonas, 2016; Schandlbauer, 2017) and single-country samples (Bond et al. 2016, Gambacorta et al. 2017, Milonas 2016, Schandlbauer 2017). Our paper differs from these papers in two aspects. First, instead of looking at tax rate changes, we focus on a specific tax incentive targeting equity. We show that an exogenous reduction of the debt-equity tax bias leads to better capitalized banks. Second, we demonstrate that the effects of a tax incentive targeting equity are unlikely to last once the incentive is removed. We provide evidence for a symmetric reaction of bank equity ratios once the tax incentive is phased out. Given that accumulating higher capital in normal periods determines the ability of a bank to withstand economic shocks (Berger and

Bouwman 2013), it is interesting to know whether a tax incentive on new equity leads to better capitalized institutions and whether this effect remains after the incentive disappears. However, we are not the only ones to document a symmetric reaction of bank capital to tax incentives. Bond et al. (2016) document that bank capital ratios react symmetrically to corporate income tax cuts and tax increases. In contrast, our paper provides new evidence about the effects of implementing and removing an explicit policy tool addressing the debt-equity tax bias on the use of equity financing by banks. This is, to some extent, valuable information for regulators and policy makers who may consider a different taxation of banks that does not provide incentives that go against capital adequacy regulations⁴⁹.

Finally, we observe that the introduction (removal) of the ACE incentive has a positive (negative) and significant impact on bank capital only for small banks⁵⁰. This result is in line with De Mooij and Keen (2016), who document that the capital ratio of larger banks is not sensitive to tax changes at local level. They attribute this effect to “too big to fail” considerations. However, another potential explanation is the ability of multinational banks to exploit tax arbitrage opportunities at group level. Gu et al. (2015) provide evidence that international banks shift debt from one country to another to exploit differences in the tax codes between their home countries and other countries where their subsidiaries operate. Our results can be consistent with the second line of reasoning. However, they are less supportive of the “too big to fail” argument since medium size banks capital ratios do not change significantly when the ACE incentive is implemented or removed.

From a financial regulation perspective, our results show that eliminating the tax distortion against equity can contribute to creating capital buffers in the upside of the

⁴⁹ See Roe and Troege (2017) for an extensive discussion on bank taxation.

⁵⁰ The Italian banking sector has a large number of small cooperative banks that are subject to specific regulatory requirements. We also test (in unreported tests) whether the effect on smaller banks persists when we remove those cooperative banks. Our conclusions remain unchanged after applying this treatment.

economic cycle⁵¹. However, its effect on bank capital is likely to be significant for small banks only, suggesting that such a tax reform has a limited reach for larger banks. The negative effect of the phasing-out of ACE on bank capital that we document also provides evidence that the effect of the tax incentive is unlikely to last once it is removed.

The remainder of the paper is structured as follows. Section 3.2 provides the details of the reform introduced in Italy. Section 3.3 describes the data and methodology employed in the empirical analysis. Section 3.4 describes the results as well as the different robustness tests performed. Section 3.5 concludes.

3.2 Background: The Italian Allowance for Corporate Equity tax reform

The identification strategy of this paper relies on the introduction of a tax allowance on equity increases in Italy. A common feature among corporate income tax systems is that the cost of debt is deductible from the taxable basis whereas the cost of equity is not. This lack of neutrality between the tax treatment of equity and debt financing creates an economic distortion and leads to higher firm leverage. A system to correct for this bias is the Allowance for Corporate Equity initially proposed by Devereux and Freeman (1991). This mechanism aims at increasing the neutrality of the tax system by granting an allowance to companies that compensates for the opportunity cost of equity financing. This scheme grants a notional interest deduction against the corporate income tax base computed as a percentage of the qualifying equity.

⁵¹ The kind of equity used in the paper is the book value of common equity. From a regulatory perspective, this type of equity is loss absorbing capital of the highest quality. Due to the scarcity of data regarding the regulatory ratios Tier 1 and Tier 2, we cannot analyze how these ratios change around the ACE reform.

As part of the 1998 tax reform⁵², Italy introduced a tax scheme to reduce the tax advantage of debt. This mechanism, known as Dual Income Tax, allowed the computation of a notional interest on the book value of new equity (common stock and reserves) in excess of the 1996 level⁵³. This notional return was initially set by the Ministry of Finance at 7% and then decreased to 6% in 2001. Rather than being fully deductible, the amount corresponding to the notional interest on new equity became taxable at 19% instead of the ordinary tax rate of 37%.⁵⁴ Even though this mechanism is not exactly the same as the tax scheme proposed by Devereux and Freeman (1991), it has the same purpose which is the reduction of the debt-equity tax bias (Bordignon et al. 2001). This mechanism was initially applicable to non-financial companies and from 2000 onwards financial companies also benefitted from the reduced taxation on equity increases from 1997 (Zangari, 2014). However, the decree approved in December 1997 included this latter extension of the ACE scheme to financial companies. We can therefore consider that the economic incentive was established for both banks and non-financial companies when the decree was enacted⁵⁵.

In order to give stronger incentives for companies to use equity, the book value of equity eligible for the tax allowance was increased by a factor of 1.2 in 2000 and 1.4 in 2001. However, after a new government took office in Italy, the ACE scheme was phased out from

⁵² This piece of legislation also included the introduction of a regional tax (known as IRAP) that replaced the former local tax mechanism (known as ILOR). We deal with tax rate changes and confounding effects from other tax reforms in the robustness tests section.

⁵³ Since the tax allowance was granted on book equity increases from September 1996 level, when the law was first presented (see Bordignon et al. (2001) for further details), the scope for strategic behavior before the implementation of the law was very limited.

⁵⁴ The rest of the taxable profits of the Italian firms remained taxable at 37%. Bordignon et al. (2001) provides a detailed description of the institutional aspects of the reform. After the application of the ACE, the average tax rate of the company could not be lower than 27%.

⁵⁵ The final decree detailing the ACE scheme was approved on 18 December 1997 and published in the Official Gazette on 5 January 1998. However, the reform was presented by the end of 1996 and subject to discussions and uncertainties before the final decree was approved in December 1997. According to the latter document, book equity increases accomplished in 1997 were rewarded for ACE purposes. In theory, banks could have already started to change their funding structure in that year but it is likely to be too short notice to observe any substantial change in bank capital in 1997. Therefore, we analyze the impact of the introduction of this scheme from 1998 onwards. Actually, in Figure 2.1 we observe that a drastic change in the equity ratio takes place precisely in 1998 where equity funding grows significantly more compared to previous years and this trend continues until the phasing-out of the ACE (see also Appendix A for a graphical analysis).

2002 onwards. Italian firms could still benefit from the measure in 2002 and 2003 but only equity increases until July 2001 were taken into account for the computation of the tax allowance⁵⁶. Therefore, we consider that the economic incentive to increase equity disappears from 2002 onwards.

3.3 Data and methodology

3.3.1 Data

Bank accounting data are retrieved from Bankscope and country level data are obtained from the World Economic Indicators at the World Bank and OECD statistics. We collect annual data on banking institutions that are categorized as commercial, savings and cooperative banks in all EU countries. To avoid double counting and given the fact that we are measuring a tax impact at the country level, we work with unconsolidated accounts following De Mooij and Keen (2016).⁵⁷ We keep all banks with data available for our main variables for each year over the two regression periods used in the empirical analysis in Table 3.3 plus one additional pre-treatment year.

We use a difference-in-differences methodology. Following Schepens (2016) and Devereux et al. (2017), we define a group of treated banks (Italian banks) that are subject to the tax reform and a group of control banks that are not affected by the treatment (banks from other EU member states). However, for the selection of our control group, we take into account other developments that occurred in the same period potentially affecting our control group. Our concern is that the period we use for our empirical analysis coincides with the creation of the euro area. Therefore, in order to have treated and control banks equally affected by this event, we only keep banks from countries that first became euro area member

⁵⁶ The notional interest was also reduced to 3% for the years 2002 and 2003.

⁵⁷ Bankscope distinguishes between consolidated and unconsolidated accounts. We keep those accounts that have the code U1, U2 or U*. We use unconsolidated statements, which terminate at the border and to which national tax policies and regulations apply.

states⁵⁸ and Greece, which entered the euro area during the period of study. We do not include Austria as this country also applied an ACE mechanism between 2000 and 2004. Finally, we eliminate those banks with negative equity value and those with an annual growth or decrease in total assets higher than 50% to avoid polluting our results with mergers and acquisitions, spinoffs or similar events.

We work with two different samples for each of the two events that we analyze in this paper (introduction and phasing-out of the ACE mechanism). This is due to the fact that the number of European banks with data available in Bankscope is significantly lower before 1997. For the first treatment (i.e. the introduction of the ACE), 168 treated banks and 1,178 banks in the control group have data available each year for the required variables during the period 1993-2000. Regarding the phasing-out of the ACE, 414 treated banks and 1,395 banks in the control group have non-missing data available each year for the required variables during the period 1998-2003⁵⁹. All variables are winsorized at the 1st and 99th percentiles to avoid the impact of outliers.

The main dependent variable of our analysis is the natural logarithm of the ratio of equity to total assets. We use determinants of the bank equity ratio as control variables, notably the logarithm of assets (as a measure of size), the loan to assets ratio (as a measure of asset diversification), the return on assets (as a measure of profitability), and the non-interest income ratio (as a measure of income diversification). To capture the effect of macroeconomic differences at the country level, we include the growth of GDP per capita, the logarithm of GDP per capita and the annual inflation rate. The definition of the variables is provided in Table B.1. in the Appendix 3.1.B.

⁵⁸ These countries are Austria, Belgium, Finland, France, Germany, Ireland, Italy, Luxembourg, Netherlands, Portugal and Spain.

⁵⁹ We show in Figure A.1 in the Appendix that the restrictions that we impose in the selection of the sample are unlikely to drive our results. Using the whole sample of Italian banks available in Bankscope, we show that the average equity over total assets ratio increases sharply during the years in which the ACE incentive is in place and decreases drastically once the incentive is removed in 2002. This is in line with the findings of our empirical analysis.

3.3.2 Methodology

The empirical methodology presented in this section seeks to identify the causal impact of the introduction and removal of an incremental ACE in Italy on the equity ratio of Italian banks. Ideally, the impact of a policy change is assessed using a random experiment. Since the tax reform is an exogenous event affecting a specific group of banks, we can determine a treatment group (Italian banks) and a control group (banks from other euro area countries) and assume a quasi-natural experiment to test causality.

In order to establish causality on the use of new equity financing by Italian banks after the ACE tax reform and its repeal, we employ a difference-in-differences (DID) approach. This approach allows us to examine the effect of a tax subsidy granted on marginal equity for banks on the evolution of bank capital before and after the introduction of the ACE. As we are also interested in the effect of the removal of this tax incentive, we reproduce the same DID regressions using the phasing-out of the ACE mechanism as a shock to observe how the equity ratio evolves after the tax incentive is removed.

Because treated banks (Italian) and non-treated banks (from other EU countries) may differ along some characteristics, we need to find a group of non-treated banks that is as similar as possible to our treated group. The main requirement of the DID methodology is that prior to the treatment the dependent variable follows a parallel trend for both control and treatment group (Roberts and Whited 2013). In the absence of “treatment”, the average change in the dependent variable should be the same for both treated and non-treated groups. As a result, any significant difference in the two groups after the treatment can be attributed to the tax change.

In order to obtain two comparable samples of banks before both treatments (introduction of the ACE mechanism and its phasing-out), we apply a propensity score matching procedure, using a nearest neighbor matching rule (Rosenbaum and Rubin 1983). A propensity score procedure aims at balancing the observed covariates of both treated and non-treated banks. We treat each event, the introduction and the phasing-out of the ACE separately and therefore do the matching, first to test the effect of the introduction of the ACE, and then for its phasing-out. The steps of the propensity score matching are the following. First, we run a probit regression for each sub-sample of euro area and Italian banks in the last year before the introduction and phasing-out of the ACE scheme. The dependent variable in these regressions is a dummy that takes the value one if a bank is located in Italy and zero otherwise. We use as independent variables the present and lagged growth of the equity to assets ratio and the present equity ratio. We also include size, profitability, the ratio of loans over total assets and the ratio of non-interest income over total gross income. After running the probit regression, we predict the estimated model for each bank in order to assign a propensity score. Second, following Rosenbaum and Rubin (1985a), we match every treated bank using the 3 nearest available matching banks in the control group, based on the propensity score obtained from the probit regression. The matching is done with replacement. This means that a non-Italian bank can be matched with several Italian banks. The matching procedure using the 3 nearest neighbors leads to 4,032 bank-year matches for the sample we use to study the effects of the introduction of the ACE and 6,624 bank-year matches for the phasing-out⁶⁰. This leaves us with 182 unique banks in the control group for the introduction of the ACE and 240 unique banks⁶¹ for the phasing-out. The banks that are not matched are removed from the sample. Table 3.1 provides descriptive statistics of the matched sample of Italian banks and the control

⁶⁰ As a consequence, there are 672 matches (4,032/6) for the sample in which we study the introduction of the ACE and 1,656 matches (6,624/4) for the phasing-out sample.

⁶¹ The term unique banks means the number of banking entities that are actually matched regardless of the number of times a bank is matched.

group for the two periods we analyze⁶². Panel A covers the period 1995-2000 used to test the impact of the introduction of the ACE and Panel B covers the period 2000-2003 used to test the effect of the phasing-out of this tax mechanism.

[Insert Table 3.1 about here]

In Table 3.2, we investigate the characteristics of the treatment and control group before the treatment for the two subsample periods. We compare each variable for the pre-treatment period, reporting the means for the treated (Column 1) and control (Column 2) groups and the significance of the difference in their means using a t-test (Column 3).

In panel A we observe that before the matching the difference in means of the annual change in the key dependent variable is not significant between the control and treatment groups. This difference is equal to 0.04 (p-value=0.42). However, this difference in means slightly decreases after the matching is performed. Regarding the second event, the t-test in Panel B of the annual change of the equity ratio over the period 1998-2001 documents that there was a difference equal to 0.07 in the average annual change of the equity ratio between the treatment and control groups that the matching corrects. After matching, this difference becomes equal to 0.04 and is not statistically significant (p-value=0.56).

[Insert Table 3.2 about here]

We also perform a graphical analysis to test more rigorously the parallel trend assumption. Figure 3.1 shows the difference of average year-on-year growth of the equity ratio between the treatment and control groups for the introduction of the ACE (a) and for the phasing-out (b). In Figure 3.1 a), we observe that there is no significant difference in the growth of the

⁶² The number of unique banks (regardless of the number of times a bank is matched) per country is as follows (introduction of the ACE, phasing-out of the ACE): Belgium (10, 10); Finland (3, 0); France (54, 49); Germany (57, 106); Greece (2, 5); Italy (168, 414); Luxembourg (19, 7); the Netherlands (1, 1); Portugal (2, 6); Spain (34, 56).

equity ratio between treated and non-treated banks before the treatment and we only find significant differences in the growth of the equity over total assets ratio after the treatment takes place (4 percentage points higher growth for the treated banks in 1998). Regarding the phasing-out, in Figure 3.1 b) we observe that after the matching, a significant difference in the year-on-year growth of the equity ratio occurs in 2002 (first post-treatment year after the ACE is phased out) when the ratio decreased roughly 5 percentage points more for the treated than for the control group banks. These analyses give us some confidence that the parallel trend requirement is satisfied in the two shocks we study.

[Insert Figure 3.1 about here]

Although the DID methodology mainly requires that the dependent variable follows a parallel trend in the pre-treatment period for both groups (Roberts and Whited 2013)⁶³, we also test whether after matching the difference in means of other bank variables that we use in our regressions is statistically significant before each treatment. In Table 3.2 Panel A, we observe that after matching, banks are comparable for the pre-treatment period in terms of size, profitability, loan ratio, and loan loss provisioning. There are still some statistically significant differences in terms of the non-interest income ratio. Regarding Panel B, even after matching, there are still some differences in terms of size⁶⁴, profitability, non-interest income and loan-loss provisioning before the phasing-out of the ACE. The significant differences in the loan ratio are corrected after matching. However, the DID methodology does not require banks to be similar over all dimensions.

⁶³ Roberts and Whited (2013) highlight that the key assumption for the consistency of a DID estimator is the zero correlation assumption of the dependent variable. This condition means that in the absence of treatment, the average change in the dependent variable (equity to assets ratio in our case) would have been the same in both the treatment and control group. Therefore, we have to verify that before the treatment the average change of our dependent variable follows a similar trend for both control and treatment groups.

⁶⁴ We address this issue in the robustness tests (Section 4.3).

Given the construction of a satisfactory control group, we set up the econometric model as in equation 1.

$$y_{i,t} = \alpha + \beta_1 ACE_t + \beta_2 X_{i,t-1} + \beta_3 Z_{i,t-1} + \beta_4 post + c_i + \gamma_t + \varepsilon_{i,t} \quad (3.1)$$

$y_{i,t}$ is either the natural logarithm of the equity to assets ratio ($\frac{equity_t}{total\ assets_t}$), the equity to assets ratio or the natural logarithm of common equity. $X_{i,t-1}$ is a subset of time-varying lagged bank control variables. These variables are the natural logarithm of total assets and the return on assets, which have been identified in the existing literature as bank capital determinants (Berger et al. 2008, Gropp and Heider 2010, De Jonghe and Öztekin 2015). Given the high number of non-listed banks in our sample, we are not able to include the market-to-book ratio in our regressions. Additionally, we add the loan to assets ratio and the ratio of non-interest income over total gross income. $Z_{i,t-1}$ represents the lagged value of GDP per capita growth, the inflation rate and the natural logarithm of GDP per capita. The inclusion of these macroeconomic variables aims at controlling for differences in the countries' development level. Our variable of interest is the treatment variable ACE .⁶⁵ This variable is defined in two different ways depending on whether we study the effect of the introduction of the ACE or its phasing-out. For the introduction, ACE is a dummy variable that takes the value one for Italian banks after the reform is implemented (1998, 1999 and 2000) and 0 otherwise. We extend the post-treatment period until 2000 to include the first year in which banks were actually deducting the ACE benefit from their tax return (as mentioned in Section 3.2)⁶⁶. For the phasing-out, we replace the variable ACE by ACE_{pho} which is a dummy variable that takes the value one for Italian banks after the reform is phased-out (2002 and 2003) and 0 otherwise. $post$ is a dummy variable that takes the value

⁶⁵ This variable results from the interaction of the dummy *treat* taking the value one if the bank is Italian and zero otherwise and *post* that takes the value one in the years after the shocks and zero otherwise.

⁶⁶ Results are qualitatively similar if we reduce the pre- and post-treatment period to two years. The results of these regressions are available on request.

one for the years after the reform is implemented (or phased out for the second shock) and zero otherwise. This variable is removed whenever time fixed effects are included. c_i stands for bank fixed effects and γ_t stands for time effects. Standard errors are clustered at the bank level unless otherwise indicated.

In this specification (i.e. introduction of the ACE), the coefficient β_1 represents the causal effect of the tax change on our measure of equity $y_{i,t}$ for a given bank located in Italy relative to the control group during the period after the implementation of the tax reform. When the phasing-out of the ACE is studied, β_1 represents the causal effect of removing the tax incentive on the measure of bank equity for Italian banks relative to banks in the control group. A positive (negative) and significant coefficient should be interpreted as an unusual increase (decrease) in equity financing relative to what it would have been in the absence of the shock. A potential concern of the DID approach is that bank capital could change due to a change in bank characteristics or another simultaneous event. However, adding bank-specific and macroeconomic country-specific variables in the regressions should alleviate this concern. Likewise, we use time dummies as in other papers using DID methodology for tax shocks affecting the banking sector (e.g. Buch et al., (2016); Capelle-Blancard and Havrylchyk, (2017)) to capture unobservable global economic and financial conditions that are year specific. Moreover, we perform several robustness tests shown in Section 3.4.3 to make sure that other events, potential omitted variables or the choice of the control group are not driving our results.

3.4 Results

3.4.1 Baseline results

Table 3.3 presents the baseline results of our DID analysis after matching for both the introduction (Panel A) and the phasing-out (Panel B) of the ACE mechanism. We begin the

discussion with the results shown in Panel A. In Column 1, we regress the natural logarithm of the equity ratio on a subset of bank control variables and macroeconomic variables for the period 1995-2000. The pre-treatment period is 1995-1997 and the post-treatment period is 1998-2000. We observe that the coefficient of the treatment variable (*ACE*) is positive and statistically significant. The coefficient of 0.085 indicates an increase of 8.83% in the equity ratio for the average Italian bank in the sample after the *ACE* is applicable relative to what it would have been in the absence of the *ACE* reform.⁶⁷ This result indicates that treated banks respond to the introduction of the *ACE* increasing their equity financing relative to control group banks. Using a different length for the pre- and post-treatment periods does not lead to a different conclusion. In unreported tests, we run the same regression as in Column 1 using two pre- and post-treatment years. We obtain a coefficient for the variable *ACE* equal to 0.07 (SE=0.033) that is significant at the 5% level. Nonetheless, we decide to use three post-treatment years to include the year 2000, when Italian banks could benefit from the *ACE* tax allowance on their tax return. In Column 2, we do the same regression as in Column 1, clustering the standard errors at the country level. Given that we analyze a treatment at the country level, it is advisable to test whether the significance of the result holds when we cluster standard errors at this level. The treatment coefficient is significant at the 1% level. In Column 3, we run the same regression as in Column 1 using the equity over total assets ratio as dependent variable instead of the natural logarithm of this ratio. This regression gives a coefficient for the treatment variable equal to 0.53. This result means that the equity over total assets ratio of the average treated bank increases by 0.53 percentage points after the *ACE* is applicable relative to banks in the control group. In Column 4, we use the natural logarithm of common equity as a dependent variable to assess whether the increase in the equity ratio is

⁶⁷ As the variable *ACE* is a dummy variable and the dependent variable is a natural logarithm, we should compute the effect on the ratio using the following formula; $\Delta \ln Y = 100 * (\exp[\hat{\beta} - \frac{1}{2} * \hat{V}(\hat{\beta})] - 1)$ where $\Delta \ln Y$ indicates the percentage change of the dependent when the dummy *ACE* is equal to one, $\hat{\beta}$ is the estimated coefficient and $\hat{V}(\hat{\beta})$ is the estimated variance of the coefficient of the treatment dummy.

driven by an increase in the level of equity. The treatment coefficient is equal to 0.063 and statistically significant (at the 10% level), meaning that the increase in the equity ratio is driven by a higher use of equity by Italian banks. In Column 5 we follow the approach suggested by Bertrand et al. (2004) and collapse the equity ratio into pre-treatment and post-treatment period averages at bank level. This approach should correct for potential correlations in the error term and bad estimations of the standard errors. The coefficient of the treatment variable remains positive and significant (at the 10% level) and is slightly higher than in Column 3.

In Table 3.3 Panel B, we discuss the effects of the removal of the ACE tax incentive on Italian banks equity ratios. In these regressions the treatment variable (ACE_{pho}) takes the value one for Italian banks after the removal of the ACE incentive and zero otherwise. We use the same model as in equation (1). However, for these regressions the coefficient β_1 represents the causal effect of the phasing-out of the ACE on our measure of equity ($y_{i,t}$). The period we use for these regressions is 2000-2003. The pre-treatment period is 2000-2001 and the post-treatment period is 2002-2003. We keep two pre- and post- treatment years for two reasons. First, our pre-treatment period includes the two years during which banks could fully benefit from the ACE allowance before it was phased out. Second, our post-treatment period includes the two first years after the ACE incentive was removed. This allows a sufficient number of years to test the effect of the phasing-out of the ACE without going further away from this event, avoiding confounding effects from other factors⁶⁸. In the robustness test section, we show that the results do not change if we extend the length of the pre- and post-treatment periods. In Column 1, we run a fixed effect regression of the equity ratio on the same bank and macroeconomic variables that we use in Panel A. We observe that the coefficient of the treatment variable (ACE_{pho}) is negative and statistically significant. The

⁶⁸ For instance, a potential confounding factor would be the Corporate Income Tax reform that entered into force in Italy in 2004. For details about this tax reform, Oropallo et al. (2005) provides an extensive summary.

coefficient is equal to 0.047 indicating a decrease of 4.6% in the equity ratio for the average Italian bank in the sample (relative to the control group) after the ACE incentive is removed. This result documents that the positive effect of the ACE on bank equity ratio reverses once the tax incentive is removed. This provides empirical evidence of the absence of a hysteresis effect in bank capital associated with equity tax incentives. We observe that once the tax incentive that motivated the increases in bank capital ratios is removed, the ratio of equity over total assets decreases significantly. In Column 2, we do the same regression as in Column 1, clustering the standard errors at the country level. We observe no change in the level of significance of the treatment coefficient. In Column 3, we run the same regression as in Column 1 using the equity over total assets ratio as dependent variable instead of the natural logarithm of this ratio. This regression gives a coefficient for the treatment variable equal to -0.64. This result means that the equity over total assets ratio of the average treated bank decreases by 0.64 percentage points after the ACE incentive is removed relative to banks in the control group. In Column 4, we use the natural logarithm of common equity as a dependent variable. The treatment coefficient is equal to -0.006 and statistically insignificant. This is in line with the results that we observe on the equity ratio. As banks do not have an incentive to use more common equity (there is no incentive to decrease it either), we observe no change in the level of equity once the ACE is phased out. The documented negative change in the equity ratio thus comes from higher leverage. In Column 5, we collapse the equity over total assets ratio into pre- and post-treatment period averages at bank level. The coefficient is negative, statistically significant and very close to the one in Column 3.

[Insert Table 3.3 about here]

3.4.2 Other balance sheet subcomponents and risk

In Table 3.4, we analyze the evolution of bank total assets, retained earnings and some bank risk characteristics after the implementation (Panel A) and phasing-out (Panel B) of the ACE reform using a DID setting. We begin the discussion with the results shown in Panel A. In Column 1, the dependent variable is the natural logarithm of total assets. The coefficient of the treatment variable is not significantly different from zero, which confirms that the effect observed in the equity ratio upon the introduction of the ACE is not triggered by a reduction in bank assets after the treatment but by an increase in equity financing (as shown in Column 4 of Table 3.3 Panel A). In Column 2, we test for the evolution of retained earnings using the natural logarithm of the retained earnings as a dependent variable. The ACE mechanism gives an incentive to retain more profits so we should observe a positive effect. The number of data points available in Bankscope for this variable for banks in the control group is very low (16 in the year with the highest number of non-missing values). As a consequence, we just analyze the impact of the ACE on this variable for the treatment group. In this regression, all banks in the control group are removed so the coefficient of interest is *post* a dummy variable taking the value one after 1997 and zero otherwise. The coefficient of this variable is 0.28 and statistically significant, providing evidence that Italian banks increased their retained earnings after the introduction of the ACE. The absence of control group does not allow us to make a causal claim, although the positive and significant coefficient that we find gives us some confidence in the fact that the introduction of the ACE leads to higher profit retention.

In Columns 3, 4, 5 and 6, we aim at analyzing the evolution of bank risk variables. As risk is an important determinant of bank capital (Berg and Gider 2016), we want to analyze whether the increase in bank equity is accompanied with an increase in bank risk. Observing a significant increase in bank risk after the tax reform could cast doubt on the causal effect of the introduction of the ACE on bank capital. In Column 3, we use the ratio of non-performing loans over total loans (NPL) as a measure of credit risk. For this variable, the data are scarce

so we just run a regression on the sample of Italian banks (as in Column 2). The coefficient on the *post* variable is negative and highly significant, which means that credit risk decreased for Italian banks after the introduction of the ACE. In Column 4, we do a standard DID regression without control variables and observe a negative but insignificant coefficient for the treatment variable, documenting no change in the ratio of loan loss provisions (LLP) around the introduction of the ACE. In Column 5, we use the natural logarithm of the standard deviation of the return on assets as a proxy for bank operating risk. The treatment coefficient *ACE* is negative but not statistically significant which means that there is no change in earnings volatility around the tax reform. In Column 6, we use the natural logarithm of the Z-score as dependent variable. This is a measure of bank solvency and is computed as the ratio of the sum of the equity ratio and the return on assets divided by the standard deviation of the return on assets. We find a negative but insignificant coefficient for the treatment variable, documenting no change in bank solvency risk after the reform. Overall, our results document that bank risk does not increase after the reform. This gives us confidence that bank risk factors are not causing the increase in the equity ratios observed in Table 3.3 Panel A.

In Table 3.4 Panel B, we reproduce the regressions that we do in Panel A using the period 2000-2003 to test the effect of the evolution of the variables before and after the phasing-out of the ACE. Our treatment variable is *ACE_{pho}*. In Column 1, we observe a significant positive change of the size of Italian banks relative to banks in the control group. As a reminder, we showed in Column 4, Table 3.3 Panel B that common equity did not change around the removal of the ACE incentive. This is not surprising since the removal of the ACE does not provide an incentive to reduce equity, but to use more debt financing instead. Thus, our results show that after the ACE incentive is removed banks finance their growth more with leverage than equity. This finding provides evidence of a symmetric reaction of banks

once a tax incentive targeting equity is removed. When the ACE incentive is applicable, banks increase their equity financing accordingly, but once this incentive disappears their capital ratios do not remain at the same level but rather decrease as more debt is used. The re-introduction of the debt-equity tax bias leads to less capitalized banks which is in line with the findings of Schandlbauer (2017). In Column 2, the negative coefficient associated to the variable *post* indicates lower earnings retention, although the coefficient is not statistically significant. This documents that once the ACE incentive is no longer in place, banks do not finance their growth with more earnings retention.

In Columns 3, 4, 5 and 6, we aim at analyzing the evolution of bank risk variables when the ACE is phased out. In Column 3, we observe that the NPL ratio decreases by 0.78 percentage points after the removal of the ACE incentive. In Column 4, we run a standard DID regression and observe a negative and significant (at the 10% level) coefficient associated to the treatment variable. This means that the LLP ratio decreases after the ACE incentive disappears. These results could lead to think that credit risk decreases after the ACE removal. We, therefore, run further robustness tests to verify whether our baseline results are unchanged when we take into account credit risk. We discuss these robustness tests in Section 3.4.3. In Column 5, we run a DID regression using as dependent variable the natural logarithm of the standard deviation of the return on assets and in Column 6 the natural logarithm of the Z-score. The results of these regressions document that neither variable changes significantly after the removal of the ACE incentive.

[Insert Table 3.4 about here]

3.4.3 Robustness tests

In Table 3.5, we test the robustness of our baseline results for the introduction (Panel A) and the phasing-out (Panel B) of the ACE. The natural logarithm of the ratio of equity over total assets is the dependent variable in all regressions. In Columns 1 and 2, we test whether our results are driven by the design of our matching procedure. In Column 1, we redo the matching for the pre-treatment period using one matched bank (neighbor) instead of three. We use the same variables and procedure that we described in Section 3.3. Then we run a regression of the natural logarithm of the equity ratio on the treatment variable and a subset of bank-specific and macroeconomic variables. As before, we use bank fixed effects and robust standard errors clustered at bank level. The coefficient of the treatment variable is positive and significant at the 5% level. In Column 2, we do the same exercise but using five matched banks instead. Again, the results of the regression show that the treatment variable has a positive and significant coefficient. These results document that our conclusions are robust to a change in the matching procedure. In Column 3, we remove the Italian banks that are above the 95th percentile in terms of the mean equity to assets ratio growth over the post-treatment period (1998-2000) and redo the matching using three neighbors. After doing this, we run a fixed effects regression and observe that the treatment variable has a coefficient that is positive and significant. In Column 4, we remove all banks in the control group that are below the 5th percentile in terms of the mean equity to assets ratio growth over the post-treatment period and redo the matching using three neighbors. Again, the fixed effects regression reveals a positive and significant coefficient for the treatment variable. This gives us confidence that the effect that we observe is not triggered by a group of Italian banks that have drastically increased their equity or by a group of banks in the control group that have experienced a large drop in their equity ratio. The Italian government scaled-back its ownership and influence in the banking sector during the 1990s. As this process described in Appendix 3.1.C could have had an impact on the behavior of Italian banks involved in it, in

Column 5 we remove the banks identified as former state-controlled banks as described in Appendix 3.1.C. Then, we redo the matching and run the DID regressions on this smaller sample. The coefficient of the treatment variable remains positive, significant and close to the baseline results.

In Table 3.5 Panel B, we run similar tests for the phasing-out of the ACE. In Columns 1 and 2, we show that when we do the matching using one or five matched banks instead of three, we still observe a negative and significant impact of the removal of the ACE incentive on the equity ratio. In Column 3, we remove the Italian banks that are below the 5th percentile in terms of the mean equity to assets ratio growth over the post-treatment period (2002-2003) and redo the matching using three neighbors. After doing this, we run a fixed effects regression and observe that the treatment variable has a coefficient that is negative and significant. Therefore, the effect that we observe once the ACE incentive is removed is not driven by just a few Italian banks decreasing their equity ratios drastically. In Column 4, we remove the control group banks that are above the 95th percentile in terms of the mean equity to assets ratio growth over the post-treatment period (2002-2003) and redo the matching using three neighbors. The treatment variable is negative and significant showing that the effect is not driven by a few banks in the control group increasing their equity ratio drastically after the phasing-out of the ACE. In Column 5, we remove Italian state-controlled banks as described in Appendix 3.1.C, redo the matching and run the DID regressions. Results remain similar to the baseline results.

[Insert Table 3.5 about here]

In Table 3.6, we run the baseline regressions shown in Table 3.3 Column 1 for both the introduction and phasing-out of the ACE but adding some additional controls that could impact the significance and size of the treatment variable coefficient. Firstly, we add the LLP

ratio as control for credit risk. This test is motivated by two aspects. First, our findings described in Section 3.4.2 regarding the evolution of this ratio before and after the phasing-out of the ACE show a statistically significant reduction of credit risk after the phasing-out of the ACE. Second, the banking literature (e.g. Berg and Gider (2016)) shows that risk can be an important determinant of bank capital structure. In Columns 1 and 2, we show that our findings are unchanged when we add this control variable. In unreported results, we also include the loan growth rate as a proxy for bank risk (Fahlenbrach et al. 2017, Laeven and Majnoni 2003). The results are unchanged as well. Secondly, we add the short-term interest rate as an additional control. We intend with this test to rule out the possibility that changes in the interest rate occurring at the same time as the tax changes are driving the equity ratio adjustments.⁶⁹ Results in Columns 3 and 4 show that our previous conclusions remain unchanged when we add this variable. We just observe that for the analysis in Column 3, the size of the treatment coefficient is smaller. The data regarding short-term interest rates are missing for some years for Luxembourg and Greece. This may distort our results since the control group is modified and some assumptions of the DID model may then be violated. Nonetheless, we observe a positive and significant coefficient associated to the treatment variable. Thirdly, we add the statutory corporate income tax rate to control for changes in the tax rate affecting either the control or treatment group. The banking literature has documented that bank capital has a lagged response to tax rate changes (e.g. Bond et al., 2016; De Mooij and Keen, 2016; Hemmelgarn and Teichmann, 2014). During the period of analysis some corporate income tax changes took place (e.g. Germany cut its corporate income tax rate from 51.6% to 38.3% in 2000 and Italy from 37.8% to 36.8%). Therefore, in order to rule out the possibility that those tax rate changes drive our results, we also control for the tax rate in Columns 5 and 6. The results remain similar to the baseline results when we control for this

⁶⁹ We also tackle this aspect running a placebo test that we show in Table 7, Column 6.

variable. The coefficient of the corporate income tax rate variable remains non-significant in both regressions. This is likely to be due to the short time-frame used in the DID regressions.

[Insert Table 3.6 about here]

As stated in the introduction, the main contribution of our paper is the symmetric effect on bank capital that we document once a tax incentive targeting equity is removed. Therefore, we further test the robustness of this result in Table 3.7 to ensure that the effect is actually driven by the new equilibrium provided once the debt-equity tax bias is reestablished and not by the choice of our econometric setting or other confounding factors. As shown in Table 3.2 Panel B, banks still differ in terms of size after the matching. As size is an important determinant of capital structure (Gropp and Heider 2010, Lemmon et al. 2008), we address this potential bias by performing a matching procedure based on total assets. We use a nearest neighbor matching method as described in section 3.3.2, but using total assets as the only variable to compute the propensity score. We are left with 613 banks in the control group. Column 1 documents that the treatment coefficient remains negative and statistically significant after this matching. This alleviates the concern that differences in size are driving our results. In Column 2, we show that our results hold when we only use banks located in Southern European euro area states (Portugal, Spain and Greece). The reason why we use the latter is because the process of convergence towards the creation of the euro area may have had similar effects in terms of interest rates and indebtedness in Southern European Economies that are different from other euro area economies (Gopinath et al. 2017). The coefficient of the treatment variable is negative and very close to the one in the baseline results when we use this alternative control group. In Column 3, we use Austrian banks as a control group. Austrian banks have been removed from the control group in our baseline results because this

country applied an ACE mechanism between 2000 and 2004.⁷⁰ However, for the phasing-out of the ACE we can use banks located in this country as control group to test the effect of the removal of the ACE incentive in Italy. The reasoning is the following: during the pre-treatment period, both Italian and Austrian banks could benefit from a tax allowance on equity increases while only in the case of Italy this incentive was removed as from 2002. Therefore, we can test the size of the effect between banks that keep being subject to the tax incentive and banks for which this incentive is no longer applicable. We find a treatment coefficient that is negative, very close to our baseline results and statistically significant. In Column 4, we show the same DID regression as in Table 3.3 Panel B on an extended period taking three pre- and post-treatment years leads to a coefficient of the treatment variable that is similar to our baseline result.

In Columns 5 and 6, we check the internal validity of our model by estimating the treatment effect on a placebo treatment variable. We run the placebo tests using the periods 2002-2005 and 2004-2007 respectively. In these regressions, we assume that an ACE reform took place in 2004 or 2006 instead of the actual years. The treatment variable *ACE_{pho}* takes the value one for Italian banks in 2004 and 2005 (in Column 5) or 2006 and 2007 (Column 6) and zero otherwise. Our placebo pre-treatment period is 2002-2003 in Column 5 and 2004-2005 in Column 6. We run a fixed effect regression of the natural logarithm of the equity ratio on the bank-specific and macroeconomic variables that we use throughout the paper. As the treatment did not occur in either 2004 or 2006 the treatment variables are not statistically significant.

This result gives us some confidence in the validity of our main results, not only because the placebo test gives further support to the validity of the DID test, but also because it removes other potential confounding effects. The result in Column 5 gives us some

⁷⁰ Klemm (2007) provides details of the characteristics of the ACE mechanism introduced in Austria.

confidence that the equity ratio went down after the ACE was phased out and did not catch up again to the old level. This means that the higher equity ratio that we observe in our baseline results in Table 3.3 Panel B is due to the fact that the tax change led to a new optimal equity ratio and then, once the ACE was phased out, banks found a new optimal ratio at a lower level. It is worth mentioning that a new corporate income tax reform entered into force in Italy in 2004 (Oropallo and Parisi 2005). The absence of significance of the treatment variable for this placebo test gives us confidence on the fact that other simultaneous tax reforms do not drive the equity ratio changes that we observe in our main results. Furthermore, our result in Column 6 (which complements the result in Table 3.6 Columns 3 and 4) eliminates the possibility that changes in interest rates are driving our results on the equity ratio. Between April 1999 and October 2000 the ECB gradually increased its benchmark interest rate from 2.5% to 4.75%. A similar increase took place between December 2005 and December 2006 when the ECB benchmark interest rate was increased from 2% to 3.50%. The fact that we do not find a significant change in the equity ratios of Italian banks in another period with a drastic increase in interest rates indicates that the observed changes in equity ratios are unlikely to be driven by changes in monetary policy.

[Insert Table 3.7 about here]

3.4.4 Heterogeneous effects

The literature has documented heterogeneous responses to tax incentives depending on the ex-ante capitalization (Devereux et al. 2017) and bank size (De Mooij and Keen, 2016). In Table 3.8, we check whether better- and worse-capitalized banks respond differently to the introduction (Column 1) and the phasing-out (Column 2) of the ACE mechanism. We introduce an interaction term between the ACE dummy and the pre-treatment equity to assets

ratio (in 1996 in Column 1 and in 2000 in Column 2). The ratio is normalized (zero mean and variance equal to one) so the variable *ACE* shows the impact for the average bank and the interaction term *ACE * Equity/Assets pre-treatment* shows the impact of one standard deviation change in the pre-treatment equity over total assets ratio. In Column 1, we observe that the value of the coefficient of the treatment variable (*ACE*) is positive and significant, in line with the results in Table 3.3, but that the interaction term is not statistically significant. This indicates that there is no difference in the reaction to the introduction of the ACE for better- and worse-capitalized banks. In Column 2, we observe that the ACE coefficient is negative and statistically significant (at the 10% level). The coefficient for the interaction term is negative but statistically insignificant showing that there is not a different reaction between better- and worse-capitalized banks once the ACE incentive is removed.

[Insert Table 3.8 about here]

Finally, from a regulatory perspective, it is interesting to test whether a tax incentive on capital generates different effects for larger and smaller banks. Previous papers have shown that larger banks tend to be less reactive to tax changes (De Mooij and Keen 2016, Heckemeyer and De Mooij 2013). As in Berger and Bouwman (2013) or Köhler (2015), we split the sample into three groups: smaller banks, with total assets below €1 billion; medium sized banks, with total assets between €1 and €3 billion and larger banks with total assets greater than €3 billion. For the introduction of the ACE, we are left with 71 treated banks in the smaller group, 54 treated banks in the medium size group and 43 treated banks in the larger size group. Regarding the phasing-out, we are left with 284 treated banks in the smaller group, 67 treated banks in the medium size group and 63 treated banks in the larger size group. We redo the matching for the Italian banks in each size group and the DID regressions for the introduction and the phasing-out of the ACE. The regression in Column 1 shows that smaller banks significantly increase their equity ratio upon the introduction of the ACE

relative to the control group. The coefficient is equal to 0.07 and statistically significant. In Column 2, we observe that once the ACE is phased out, in line with prior findings, smaller banks reduced their equity ratios significantly relative to the control group. The treatment coefficient is equal to -0.05 and statistically significant. In Columns 3 and 4, we do the same tests for medium sized banks. We find that there is no statistically significant reaction to the introduction or removal of the ACE incentive. The treatment variable is not significant in any case. The same results are found in Columns 5 and 6 for the sample of larger banks. These results lead to an additional test. In Italy, we find some heterogeneity regarding the type of banking institutions. There are a large number of small cooperative banks that have asset values that, in most cases, are lower than €1 billion, and other types of commercial banks. These cooperative banks are subject to some product, investment, regulatory and territorial restrictions as well as some tax rebates (even though they remain taxable entities for corporate income tax purposes). Since we observe that the result comes from smaller banks, we may be capturing an effect that is linked to the special nature of these banks and not to the fact that there is a reaction to the introduction and removal of the ACE incentive. We use the institutional classification list provided by the Bank of Italy to identify banks that are classified as credit cooperatives⁷¹. When we reproduce the regressions that we show in Table 3.9 removing the cooperative banks, results remain qualitatively similar to the ones we show in Table 9. We do not report them for brevity.

[Insert Table 3.9 about here]

⁷¹ Credit cooperative banks under the denomination BCC (*Banche di Credito Cooperativo*) have mutualistic features as they grant credit primarily to their members, they must do business in their region and can only expand under certain circumstances, and they must allocate 70% of their annual profit to a non-distributable reserve. The latter profit is not taxable whereas the profit not allocated to this non-distributable reserve is fully taxable under the corporate income tax rules. Gutierrez (2008) provides a review of the features of the Italian cooperative banking sector. We classify banks depending on their legal classification as provided by the Bank of Italy in its website (<https://infostat.bancaditalia.it/giava-inquiry-public/flex/Giava/GIAVAFEInquiry.html#>).

Our results tend to prove that only small banks capital react significantly to both the implementation and the withdrawal of the ACE. This is in line with Gu et al. (2015), who argue that larger international banks have mechanisms available to reduce their tax burden at group level. Another possible explanation is the fact that banks that are “too big to fail” benefit from an implicit creditor insurance and therefore, their decisions regarding capital structure are less influenced by tax changes at local level but rather by other considerations related to the cost of bankruptcy or capital requirements (De Mooij and Keen 2016). However, we observe that medium sized banks do not increase their equity ratios significantly once the ACE is implemented. This result reduces support for the “too big to fail” hypothesis as the only explanation for this heterogeneity. From this subsection, we can conclude that even if the use of equity tax shields creates incentives for banks to use more capital, the effects are unlikely to incentivize larger banks. This suggests that an ACE mechanism on marginal equity has a limited reach in terms of regulatory perspective.

3.5 Conclusions

The existence of an unequal tax treatment of debt and equity has been identified as one of the factors explaining leverage in the banking sector. Recent empirical evidence documents that this tax distortion affects banks capital structure. Our paper contributes to this literature by exploiting an exogenous variation in the Italian corporate tax system where a tax shield on incremental equity was introduced for banks in 1997. This reform reduces the tax distortion between equity and debt financing, which should provide incentives for banks to increase their level of equity.

Using a difference-in-differences setting, we document that the introduction of a tax shield on incremental equity results in an increase in bank capital. We document an 8.83% increase in bank capital ratios relative to the control group upon introduction of the equity tax shield.

This mechanism was phased out in 2002 which gives us the opportunity to test whether the introduction and removal of the equity tax shield lead to symmetric effects. We find that once the tax incentive is no longer applicable, banks readjust downward their equity ratios by 4.6% on average relative to banks in the control group. This finding reveals that there is no hysteresis in bank capital when tax incentives on equity are removed. The positive effect on equity ratios generated by the tax incentive does not last once it is removed.

We also document heterogeneous reactions to both the introduction and the removal of the tax incentive. Once we split the sample between large, medium size and small banks; we find that these effects are significant only for small banks.

Overall, our results show that reducing the tax distortion against equity can contribute to creating buffers in the upside of the economic cycle. However, its effect on bank capital, at least in the short-run, is likely to be significant only for small banks, suggesting that such a tax incentive on marginal equity has a limited reach for larger banks.

Table 3.1: Summary statistics after matching

This table presents the summary statistics after matching. The matching procedure using the 3 nearest neighbors leads to 4,032 bank-year matches for the introduction of the ACE and 6,624 bank-year matches for the phasing-out. The matched sample for the introduction of the ACE scheme (Panel A) includes 350 unique banks. 168 are Italian banks and 182 are banks from other euro area countries. The matched sample for the phasing out of the ACE scheme (Panel B) includes 654 unique banks. 414 are Italian banks and 240 are banks from other euro area countries. The distribution of unique banks per country can be found in the Appendix 3.1.B. We compute the statistics variable excluding repeated observations. The variables *NPL ratio* and *Ln(Retained Earnings)* are only used for treated banks which explains its lower number of observations.

1995-2000	Variables	mean	sd	P1	P50	P99	2000-2003	mean	sd	P1	P50	P99
<u>PANEL A</u>							<u>PANEL B</u>					
<u>Introduction</u>	Equity/Assets	8.90	4.32	2.09	8.16	21.25	<u>Phasing-out</u>	10.92	4.84	3.20	10.08	25.20
	Ln(Total Assets)	7.43	1.66	4.20	7.21	11.18		6.29	1.72	3.67	6.00	11.27
	Ln(Equity)	4.91	1.45	1.84	4.77	7.95		3.99	1.56	1.33	3.73	8.17
	RoA	0.60	0.61	-0.87	0.49	2.46		0.71	0.62	-0.78	0.66	2.50
	Loan ratio	49.90	19.79	4.91	51.68	91.67		58.75	18.32	6.07	61.25	93.12
	Nii ratio	31.86	16.28	-8.61	29.45	75.79		25.38	13.38	-3.83	24.05	75.64
	NPL ratio	3.83	3.58	0.20	2.57	17.81		2.53	2.93	0.16	1.58	15.10
	LLP ratio	0.43	0.49	-0.42	0.33	2.47		0.35	0.42	-0.34	0.28	2.09
	Ln(Sd RoA)	-2.59	1.76	-9.21	-2.43	0.55		-2.74	2.48	-9.21	-2.13	0.37
	Ln(Z-Score)	4.65	1.41	1.38	4.63	8.77		4.86	1.82	1.69	4.47	9.37
	Ln(Retained Earnings)	1.58	1.27	0.23	1.33	5.30		1.13	1.10	0.23	0.93	5.96
	GDPpc growth	3.07	1.44	0.02	3.18	6.79		1.71	1.71	-1.31	1.35	4.73
	Inflation	2.26	1.25	0.53	1.98	5.24		2.63	0.82	1.03	2.61	4.39
	Ln(GDPpc)	10.46	0.36	10.01	10.47	11.38		10.55	0.39	10.09	10.55	11.49

Table 3.2: Matching

This table compares the features between the matched and unmatched sample before treatment. The unmatched sample is the sample of banks before applying the matching and the matched sample after the matching is done. The nearest-neighbor matching procedure is performed with replacement using the propensity score for each bank in the control group. Each treated bank is matched with the 3 closest banks in the control group using the propensity score matching. Non-matched banks are removed. Column 1 is the mean of each variable for Italian banks (treated group). Column 2 is the mean of the variables for non-treated banks (control group). Column 3 shows the p-values for the significance of the difference in means. The parallel trend test is the first difference of the Equity/Assets over the four years period before the treatment (1994-1997 for the introduction of the ACE and 1998-2001 for the phasing out). The panel at the bottom compares the pairwise means of each variable the year before each treatment (1997 and 2001 respectively). The sample in Panel A has 672 matches and Panel B 1,656 matches.

PANEL A					PANEL B						
<u>Introduction of the ACE</u>		Number of unique banks	Unmatched	168	1178	<u>Phasing-out of the ACE</u>		Number of unique banks	Unmatched	414	1395
			Matched	168	182				Matched	414	240
Variable			Mean Treated Group	Mean Control Group	Difference p-value				Mean Treated Group	Mean Control Group	Difference p-value
			(1)	(2)	(3)				(1)	(2)	(3)
Parallel trend test					Parallel trend test						
ΔEquity/Assets		Unmatched	0.03	0.07	0.42		Unmatched	0.02	0.09	0.09	
		Matched	0.03	0.067	0.62		Matched	0.02	0.06	0.56	
Pair wise mean comparison					Pair wise mean comparison						
Ln (Assets)		Unmatched	7.31	7.00	0.00		Unmatched	5.92	6.99	0.00	
		Matched	7.31	7.49	0.31		Matched	5.92	6.85	0.00	
RoA		Unmatched	0.59	0.38	0.00		Unmatched	0.82	0.32	0.00	
		Matched	0.59	0.59	0.89		Matched	0.82	0.67	0.01	
Loan ratio		Unmatched	48.95	56.72	0.00		Unmatched	57.33	58.76	0.06	
		Matched	48.95	47.31	0.41		Matched	57.33	56.91	0.81	
Nii ratio		Unmatched	29.44	23.3	0.00		Unmatched	23.99	24.90	0.08	
		Matched	29.44	34.65	0.00		Matched	23.99	27.12	0.01	
LLP ratio		Unmatched	0.48	0.41	0.03		Unmatched	0.26	0.50	0.00	
		Matched	0.48	0.36	0.02		Matched	0.26	0.43	0.00	

Table 3.3: Baseline regressions

This table documents the change of bank capital after the introduction (Panel A) and phasing-out (Panel B) of the equity tax shield. These are the results of estimating equation 1. In columns 1 and 2 the dependent variable is the natural logarithm of the equity to assets ratio. In column 3 the dependent variable is the equity to total assets ratio. In column 4 the dependent variable is the natural logarithm of common equity. In column 5 we compare the average of the equity over assets ratio before and after the treatment for the treated and control groups. The regression period used in Panel A is 1995-2000 and 2000-2003 in Panel B. The variable of interest is *ACE* (*ACEpho*) that is a dummy taking the value 1 for Italian banks after 1997 in Panel A (and after 2001 in Panel B) and zero otherwise. *post* takes the value one after 1997 in Panel A and after 2001 in Panel B and zero otherwise. All regressions are done using the sample of matched banks. Control variables are lagged one period. Bank and time fixed effects as well as the cluster level of standard errors are indicated in the table. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

PANEL A VARIABLES	(1) Ln(Equity/Assets)	(2) Ln(Equity/Assets)	(3) Equity/Assets	(4) Ln(Equity)	(5) Average Equity/Assets
ACE (treat x post)	0.085*** (0.028)	0.085*** (0.023)	0.526** (0.247)	0.063* (0.035)	0.667* (0.369)
Ln (Assets)	-0.265*** (0.065)	-0.265** (0.086)	-2.547*** (0.607)	0.442*** (0.083)	-3.251*** (0.761)
RoA	0.066*** (0.017)	0.066*** (0.017)	0.446*** (0.150)	0.079*** (0.019)	0.909** (0.369)
Loan ratio	0.000 (0.001)	0.000 (0.001)	-0.003 (0.008)	0.003** (0.001)	0.023 (0.017)
Nii ratio	-0.001* (0.001)	-0.001*** (0.000)	-0.015** (0.007)	-0.001* (0.001)	-0.028 (0.019)
GDPpc growth	0.029** (0.015)	0.029** (0.012)	0.173** (0.088)	0.021 (0.016)	0.340** (0.160)
Inflation	-0.002 (0.010)	-0.002 (0.008)	-0.059 (0.074)	0.005 (0.009)	-0.062 (0.208)
Ln(GDPpc)	-0.360 (0.759)	-0.360 (0.903)	-2.952 (4.758)	-0.024 (0.839)	-7.951 (6.593)
Constant	7.632 (7.890)	7.632 (9.192)	57.998 (49.552)	1.589 (8.657)	114.110* (68.120)
Post					0.721 (0.539)
Bank FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	NO
No. of matched obs	4,032	4,032	4,032	4,032	1,344
No. of unique banks	350	350	350	350	350
No. of unique obs	2,100	2,100	2,100	2,100	700
Cluster SE	Bank	Country	Bank	Bank	Bank
Adj. R-squared	0.083	0.083	0.082	0.395	0.171

PANEL B	(1)	(2)	(3)	(4)	(5)
VARIABLES	Ln(Equity/Assets)	Ln(Equity/Assets)	Equity/Assets	Ln(Equity)	Average Equity/Assets
ACEpho	-0.047*** (0.013)	-0.047*** (0.009)	-0.641*** (0.124)	-0.006 (0.009)	-0.644*** (0.125)
Ln (Assets)	-0.301*** (0.053)	-0.301*** (0.069)	-2.835*** (0.490)	0.303*** (0.057)	-1.442** (0.676)
RoA	0.032*** (0.007)	0.032*** (0.008)	0.317*** (0.079)	0.049*** (0.012)	0.055 (0.131)
Loan ratio	-0.003*** (0.001)	-0.003** (0.001)	-0.023*** (0.007)	-0.000 (0.001)	-0.017** (0.008)
Nii ratio	0.000 (0.001)	0.000 (0.000)	0.002 (0.006)	-0.000 (0.000)	0.000 (0.006)
GDPpc growth	-0.026*** (0.007)	-0.026** (0.011)	-0.290*** (0.072)	-0.018* (0.008)	-0.409** (0.198)
Inflation	0.013 (0.020)	0.013 (0.025)	0.159 (0.208)	0.001 (0.024)	0.226 (0.251)
Ln(GDPpc)	-0.188 (0.408)	-0.188 (0.346)	-1.791 (3.550)	0.314 (0.579)	-10.209 (10.374)
Constant	6.311 (4.141)	6.311 (3.424)	48.880 (36.162)	-1.207 (6.037)	128.784 (107.915)
Post					-0.280 (0.487)
Bank FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	NO
No. of matched obs	6,624	6,624	6,624	6,624	3,312
No. of unique banks	654	654	654	654	654
No. of unique obs	2,616	2,616	2,616	2,616	1,308
Cluster SE	Bank	Country	Bank	Bank	Bank
Adj. R-squared	0.183	0.183	0.216	0.378	0.103

Table 3.4: Other balance sheet subcomponents and risk

This table presents the analysis for different balance sheet components (columns 1 and 2) and risk characteristics (columns 3 to 6) around the tax changes. Panel A shows the results for the introduction of the ACE and Panel B for the phasing-out. In column 1, we test the change in the natural logarithm of total assets before and after the tax changes. In column 2, the dependent variable is the natural logarithm of retained earnings. As data for this variable are available for the majority of banks in the treatment group but only for a few banks in the control group, we only analyze the evolution of retained earnings before and after the treatment for the Italian banks (in this case the treatment variable is *post*). In columns 3 to 6 we analyze the evolution of some risk characteristics around the tax change. We proceed in column 3 as in column 2 and we analyze how the Non-Performing Loans ratio evolves before and after the tax change for Italian banks due to the scarcity of data for the NPL ratio for the control group banks. In column 4, we use the ratio of loan loss provisions over total assets (as another measure of credit risk). The dependent variable used in column 5 is the natural logarithm of the standard deviation of RoA (as a measure of earnings volatility) and in column 6 the natural logarithm of the Z-score (as a measure of bank stability). *ACE* (*ACEpho*) is a dummy taking the value 1 for Italian banks after 1997 in Panel A (and after 2001 in Panel B) and zero otherwise. *post* takes the value one after 1997 in Panel A and after 2001 in Panel B and zero otherwise. The regression period used in Panel A is 1995-2000 and 2000-2003 in Panel B. Bank and time fixed effects as well as the cluster level of standard errors are indicated in the table. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

PANEL A	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Ln(Assets)	Ln(Retained Earnings)	NPL ratio	LLP ratio	Ln(Sd-RoA)	Ln(Z-score)
	Other B/S subcomponents		Risk characteristics			
ACE	0.013			-0.001	0.144	-0.102
	(0.022)			(0.037)	(0.138)	(0.122)
Post		0.280***	-0.979***			
		(0.051)	(0.142)			
Constant	7.285***	1.437***	4.315***	0.521***	-2.420***	4.475***
	(0.009)	(0.026)	(0.070)	(0.019)	(0.086)	(0.072)
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	NO	NO	YES	YES	YES
No. of matched obs	4,032	897	953	3,985	3,901	3,873
No. of unique banks	350	166	167	347	350	350
No. of unique obs	2,100	897	953	2,070	2,008	2,001
Cluster SE	Bank	Bank	Bank	Bank	Bank	Bank
Sample	Matched	Only Italian banks	Only Italian banks	Matched	Matched	Matched
Adj. R-squared	0.363	0.067	0.108	0.030	0.005	0.006
<hr/>						
PANEL B	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Ln(Assets)	Ln(Retained Earnings)	NPL ratio	LLP ratio	Ln(Sd-RoA)	Ln(Z-score)
	Other B/S subcomponents		Risk characteristics			
ACEpho	0.095***			-0.059*	-0.161	0.057
	(0.012)			(0.032)	(0.144)	(0.115)
Post		-0.012	-0.778***			
		(0.026)	(0.124)			
Constant	6.160***	1.131***	2.947***	0.333***	-2.616***	4.810***
	(0.004)	(0.012)	(0.067)	(0.012)	(0.057)	(0.046)
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	NO	NO	YES	YES	YES
No. of matched obs	6,624	1,318	1,263	6,371	6,534	6,515
No. of unique banks	654	372	397	650	654	654
No. of unique obs	2,616	1,318	1,263	2,570	2,580	2,579
Cluster SE	Bank	Bank	Bank	Bank	Bank	Bank
Sample	Matched	Only Italian banks	Only Italian banks	Matched	Matched	Matched
Adj. R-squared	0.564	0.001	0.079	0.009	0.005	0.002

Table 3.5: Robustness tests

This table presents some robustness tests for the previous results. The regression period used in Panel A is 1995-2000 and 2000-2003 in Panel B. In column 1, we redo the matching using 1 matched bank (neighbor) instead of 3 and the same variables that we use in table 2 for the computation of the propensity score. In column 2, we redo the matching using 5 matched banks (neighbors) instead of 3 and the same variables that we use in table 2 for the computation of the propensity score. In column 3 Panel A we remove all banks in the treatment group whose average equity ratio growth rate over the post-treatment period (1998-2000) is above the 95th percentile and redo the matching. In column 3 Panel B we remove all banks in the treatment group whose average equity ratio growth rate over the post-treatment period (2002-2003) is below the 5th percentile and redo the matching. In column 4 Panel A we remove all banks in the control group whose average equity ratio growth rate over post-treatment period (1998-2000) is below the 5th percentile and redo the matching. In column 4 Panel B we remove all banks in the control group whose average equity ratio growth rate over post-treatment period (2002-2003) is above the 95th percentile and redo the matching. In Column 5 (Panel A and B), we remove the banks identified as former state-controlled banks that are involved in the process described in Appendix 3.1.C, redo the matching and run the DID regressions. *ACE* (*ACE_{pho}*) is a dummy taking the value 1 for Italian banks after 1997 in Panel A (and after 2001 in Panel B) and zero otherwise. Bank and time fixed effects as well as the cluster level of standard errors are indicated in the table. All control variables are lagged one period. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

PANEL A	(1)	(2)	(3)	(4)	(5)
VARIABLES	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)
	1 neighbor	5 neighbors	Remove treated banks if ratio growth >p 95	Remove control banks if ratio growth <p 5	Remove state-controlled banks
ACE	0.089** (0.044)	0.086*** (0.026)	0.073** (0.031)	0.085*** (0.028)	0.077** (0.034)
Ln (Assets)	-0.338*** (0.077)	-0.271*** (0.057)	-0.285*** (0.067)	-0.265*** (0.065)	-0.274*** (0.060)
RoA	0.062*** (0.020)	0.058*** (0.015)	0.067*** (0.017)	0.066*** (0.017)	0.057*** (0.015)
Loan ratio	-0.000 (0.001)	-0.000 (0.001)	0.001 (0.001)	0.000 (0.001)	-0.001 (0.001)
Nii ratio	-0.001 (0.001)	-0.001** (0.001)	-0.001** (0.001)	-0.001* (0.001)	-0.002*** (0.001)
GDPpc growth	0.041** (0.016)	0.021 (0.013)	0.040** (0.016)	0.029** (0.015)	0.044*** (0.015)
Inflation	-0.019 (0.012)	0.003 (0.008)	-0.010 (0.009)	-0.002 (0.010)	-0.025*** (0.009)
Ln(GDPpc)	-1.356** (0.682)	0.412 (0.568)	-0.893 (0.669)	-0.360 (0.759)	-1.495** (0.719)
Constant	18.610*** (7.036)	-0.347 (5.951)	13.341* (6.982)	7.632 (7.890)	19.597*** (7.482)
Bank FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
No. of matched obs	2,016	6,048	3,816	4,032	2,424
No. of unique banks	259	403	338	350	245
No. of unique obs	1,554	2,418	2,028	2,100	1,470
Cluster SE	Bank	Bank	Bank	Bank	Bank
Adj. R-squared	0.101	0.086	0.088	0.083	0.105

PANEL B	(1)	(2)	(3)	(4)	(5)
VARIABLES	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)
	1 neighbor	5 neighbors	Remove treated banks if ratio growth <5p	Remove control banks if ratio growth >p 95	Remove state-controlled banks
ACEpho	-0.033**	-0.055***	-0.042***	-0.047***	-0.047***
	(0.015)	(0.012)	(0.013)	(0.013)	(0.015)
Ln (Assets)	-0.324***	-0.293***	-0.260***	-0.301***	-0.276***
	(0.060)	(0.049)	(0.054)	(0.053)	(0.061)
RoA	0.029***	0.032***	0.032***	0.032***	0.029***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Loan ratio	-0.004***	-0.003***	-0.003***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Nii ratio	-0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
GDPpc growth	-0.019**	-0.026***	-0.021***	-0.026***	-0.020**
	(0.008)	(0.007)	(0.008)	(0.007)	(0.009)
Inflation	0.002	-0.000	0.003	0.013	-0.005
	(0.024)	(0.019)	(0.022)	(0.020)	(0.024)
Ln(GDPpc)	0.431	-0.302	-0.419	-0.188	-0.447
	(0.504)	(0.410)	(0.439)	(0.408)	(0.494)
Constant	0.062	7.446*	8.466*	6.311	8.837*
	(5.149)	(4.175)	(4.478)	(4.141)	(5.055)
Bank FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
No. of matched obs	3,312	9,936	6,304	6,624	5,744
No. of unique banks	558	722	630	654	555
No. of unique obs	2,232	2,888	2,520	2,616	2,220
Cluster SE	Bank	Bank	Bank	Bank	Bank
Adj. R-squared	0.200	0.181	0.158	0.183	0.185

Table 3.6: Robustness tests: Including additional controls

This table depicts the results for the introduction and the phasing out of the ACE scheme in Italy. In these tables we run the baseline regressions in Table 3 but add some additional control variables that may have an impact on the equity over assets ratio. The sample period is 1995-2000 (as in Table 4.1 Panel A) when we analyze the effect of the introduction of the ACE on the Equity/Assets ratio and 2000-2003 (as in Table 3.1 Panel B) when we analyze the effect of the phasing-out. In all cases the dependent variable is the natural logarithm of the equity over total assets ratio. The additional control variables are detailed in the table. The variable of interest is *ACE* which is a dummy taking the value 1 for Italian banks after the treatment and zero otherwise. All regressions are done using the sample of matched banks. Control variables are lagged one period. Bank and time fixed effects as well as the cluster level of standard errors are indicated in the table. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)	Ln(Equity/Assets)
Additional control variables (lagged):	Risk (LLP ratio)		ST-Interst rate		CIT Rate	
ACE is defined as :	ACE introduction	ACE phasing-out	ACE introduction	ACE phasing-out	ACE introduction	ACE phasing-out
ACE	0.083*** (0.029)	-0.050*** (0.014)	0.049* (0.029)	-0.050*** (0.014)	0.096*** (0.027)	-0.038** (0.018)
Ln (Assets)	-0.268*** (0.066)	-0.303*** (0.055)	-0.296*** (0.062)	-0.296*** (0.054)	-0.264*** (0.065)	-0.297*** (0.054)
RoA	0.066*** (0.018)	0.029*** (0.007)	0.070*** (0.017)	0.031*** (0.007)	0.066*** (0.017)	0.032*** (0.007)
Loan ratio	0.000 (0.001)	-0.003*** (0.001)	-0.000 (0.001)	-0.003*** (0.001)	0.001 (0.001)	-0.003*** (0.001)
Nii ratio	-0.001* (0.001)	-0.000 (0.001)	-0.001 (0.001)	-0.000 (0.001)	-0.001* (0.001)	0.000 (0.001)
GDPpc growth	0.032** (0.015)	-0.026*** (0.007)	-0.005 (0.010)	-0.024*** (0.007)	0.029** (0.015)	-0.026*** (0.007)
Inflation	-0.003 (0.010)	0.003 (0.021)	0.002 (0.009)	0.011 (0.020)	-0.001 (0.010)	0.008 (0.023)
Ln(GDPpc)	-0.487 (0.763)	-0.414 (0.419)	0.531 (0.332)	-0.357 (0.435)	-0.402 (0.801)	-0.032 (0.463)
ADDITIONAL CONTROLS	0.008 (0.016)	-0.013 (0.011)	0.006 (0.030)	0.014 (0.013)	0.001 (0.002)	-0.001 (0.002)
Constant	8.973 (7.925)	8.697** (4.244)	-1.215 (3.244)	8.006* (4.381)	8.023 (8.260)	4.714 (4.754)
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
No. of matched obs	3,984	6,410	3,865	6,624	4,032	6,624
No. of unique banks	347	650	350	654	350	654
No. of unique obs	2,069	2,572	2,003	2,616	2,100	2,616
Cluster SE	Bank	Bank	Bank	Bank	Bank	Bank
Adj. R-squared	0.085	0.180	0.092	0.184	0.083	0.184

Table 3.7: Robustness tests: Alternative settings for the phasing out of the ACE and placebo tests

This table presents additional robustness test for the phasing-out of the ACE when we use different control groups (Columns 1, 2 and 3), an extended period (column 4) and placebo tests (Columns 5 and 6). In Column 1, we redo the matching using 3 neighbors but the only matching variable is size. In Column 2, we select Italian banks as the treatment group and banks from Southern Euro Area countries (Spain, Portugal and Greece) as a control group. Then we match the sample as described in section 3.3 of this paper. In Column 3, we use Austrian banks as control group (no matching is applied) because this country applied a similar ACE scheme between 2000 and 2004. In column 4, we use the sample used for our baseline results but we run the regressions over an extended pre-treatment (1999-2001) and post-treatment (2002-2004) periods. In Column 5 and 6 we run two placebo tests as if the tax change had taken place in 2004 and 2006 respectively. *ACEpho* is a dummy taking the value 1 for Italian banks after 2001 and zero otherwise. Bank and time fixed effects as well as the cluster level of standard errors are indicated in the table. All control variables are lagged one period. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Dep. Var.: Ln(Equity/Assets)					
	Matching on size	Southern EA	Other ACE	Extended periods (99-04)	Placebo: 2004	Placebo: 2006
ACEpho	-0.080*** (0.013)	-0.044** (0.017)	-0.045*** (0.015)	-0.040*** (0.010)	-0.018 (0.015)	0.016 (0.017)
Ln (Assets)	-0.254*** (0.045)	-0.365*** (0.071)	-0.336*** (0.072)	-0.279*** (0.034)	-0.224*** (0.044)	-0.165*** (0.050)
RoA	0.039*** (0.006)	0.027*** (0.007)	0.027*** (0.007)	0.045*** (0.009)	0.028*** (0.010)	0.027*** (0.009)
Loan ratio	-0.002*** (0.001)	-0.003** (0.001)	-0.004*** (0.001)	-0.003*** (0.001)	-0.001 (0.001)	-0.000 (0.001)
Nii ratio	-0.001 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.000)	-0.000 (0.001)	-0.000 (0.001)
GDPpc growth	-0.044*** (0.008)	-0.038*** (0.010)	0.079*** (0.029)	-0.035*** (0.008)	-0.022** (0.011)	0.009 (0.011)
Inflation	0.007 (0.022)	0.064*** (0.022)	0.276*** (0.078)	-0.009 (0.017)	0.000 (0.014)	-0.006 (0.014)
Ln(GDPpc)	0.262 (0.802)	0.607 (0.471)		-0.064 (0.321)	-1.539** (0.675)	0.177 (1.379)
Constant	1.020 (8.396)	-1.615 (4.966)	3.930*** (0.525)	4.888 (3.329)	19.924*** (6.989)	1.497 (14.478)
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
No. of matched obs	6,624	6,624	2,084	9,736	6,087	5,297
No. of unique banks	1,027	496	521	654	654	632
No. of unique obs	4,108	1,984	2,084	3,889	2,509	2,341
Cluster SE	Bank	Bank	Bank	Bank	Bank	Bank
Adj. R-squared	0.179	0.211	0.208	0.200	0.070	0.047

Table 3.8: Heterogeneous effects: Capitalization

This table analyzes the existence of heterogeneous reactions around the introduction and phasing-out of the ACE scheme. The sample period in Column 1 is 1995-2000 (as in Table 3.1 Panel A) when we analyze the effect of the introduction of the ACE on the Equity/Assets ratio and in Column 2 is 2000-2003 (as in Table 3.1 Panel B) when we analyze the effect of the phasing-out. We modify the classic difference-in-differences setting to test for heterogeneous effects between banks depending on their initial level of capitalization (Equity/Assets). We add the interaction term $ACE * Equity/Assets \text{ pre-treatment}$ that captures whether banks in Italy respond differently depending on their level of capital before the introduction of the ACE and before the phasing out. The variable $Equity/Assets \text{ pre-treatment}$ is the standardized Equity/Assets ratio of each bank in 1996 and 2000 respectively. We do these regressions using the matched sample of banks (as in Table 3). Bank and time fixed effects as well as the cluster level of standard errors are indicated in the table. Control variables are lagged one period. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

VARIABLES	(1) Ln(Equity/Assets)	(2) Ln(Equity/Assets)
ACE is defined as :	ACE introduction	ACE phasing-out
ACE	0.090*** (0.028)	-0.025* (0.015)
ACE *Equity/Assets pre-treatment	0.011 (0.029)	-0.012 (0.014)
Post*Equity/Assets pre-treatment	-0.059** (0.023)	-0.032*** (0.008)
Ln (Assets)	-0.272*** (0.067)	-0.312*** (0.051)
RoA	0.054*** (0.016)	0.026*** (0.007)
Loan ratio	0.000 (0.001)	-0.003*** (0.001)
Nii ratio	-0.002** (0.001)	0.000 (0.001)
GDPpc growth	0.019 (0.014)	-0.029*** (0.007)
Inflation	-0.002 (0.010)	0.008 (0.021)
Ln(GDPpc)	-0.133 (0.749)	-0.050 (0.396)
Constant	5.369 (7.779)	4.945 (4.012)
Bank FE	YES	YES
Time FE	YES	YES
No. of matched obs	4,032	6,624
No. of unique banks	350	654
No. of unique obs	2,100	2,616
Cluster SE	Bank	Bank
Adj. R-squared	0.111	0.211

Table 3.9: Heterogeneous effects: Size

This table analyzes the existence of heterogeneous reactions upon the introduction of ACE and its phasing-out. The sample period is 1995-2000 (as in Table 3.1 Panel A) when we analyze the effect of the introduction of the ACE on the Equity/Assets ratio and 2000-2003 (as in Table 3.1 Panel B) when we analyze the effect of the phasing-out. We run different regressions splitting the group of Italian banks by size to test whether we observe heterogeneous effects. In Columns 1 and 2 we keep all Italian banks with total assets equal or below €1 billion and redo the matching. Column 1 tests the effect of the introduction of the ACE and Column 2 the effects of its removal. In Columns 3 and 4 we keep all Italian banks with total assets between €1 and €3 billion and redo the matching. We run the same regressions as in Columns 1 and 2. In Columns 5 and 6 we keep all Italian banks with total assets equal or above €3 billion and redo the matching. Bank and time fixed effects as well as the cluster level of standard errors are indicated in the table. Control variables are lagged one period. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Smaller		Medium		Larger	
ACE is defined as :	ACE introduction	ACE phasing-out	ACE introduction	ACE phasing-out	ACE introduction	ACE phasing-out
ACE	0.070** (0.035)	-0.050*** (0.016)	0.009 (0.058)	-0.013 (0.034)	0.086 (0.055)	0.004 (0.022)
Ln (Total Assets)	-0.259*** (0.075)	-0.247*** (0.053)	-0.121 (0.078)	-0.344*** (0.111)	-0.289*** (0.084)	-0.231*** (0.060)
RoA	0.036** (0.016)	0.020*** (0.006)	0.066** (0.033)	0.038** (0.015)	0.091*** (0.029)	0.049** (0.025)
Loan ratio	-0.001 (0.001)	-0.001 (0.001)	-0.000 (0.002)	-0.001 (0.001)	0.001 (0.002)	-0.002 (0.001)
Nii ratio	-0.002*** (0.001)	0.001 (0.001)	-0.001 (0.001)	0.002 (0.002)	0.000 (0.001)	-0.001 (0.001)
GDPpc growth	-0.006 (0.017)	-0.021** (0.010)	0.008 (0.013)	-0.018* (0.011)	0.023 (0.022)	-0.008 (0.009)
Inflation	-0.005 (0.011)	-0.007 (0.028)	-0.027 (0.026)	-0.059* (0.031)	-0.021 (0.018)	-0.028 (0.026)
Ln(GDPpc)	1.500 (0.968)	-1.310 (0.945)	-0.517 (0.740)	-0.270 (0.736)	-0.236 (0.868)	-0.410 (0.627)
Constant	-11.732 (9.988)	17.502* (9.833)	8.296 (7.670)	7.392 (7.418)	6.636 (9.116)	8.237 (6.428)
Bank FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
No. of matched obs	1,704	4,544	1,296	1,072	1,032	1,008
No. of unique banks	155	400	151	183	141	202
No. of unique obs	930	1,600	906	732	846	808
Sample	<=1 billion	<=1 billion	>1 billion and <3 billion	>1 billion and <3 billion	>=3 billion	>=3 billion
Cluster SE	TA+Control	TA+Control	TA+Control	TA+Control	TA+Control	TA+Control
Adj. R-squared	Bank	Bank	Bank	Bank	Bank	Bank
	0.089	0.260	0.036	0.143	0.107	0.074

Figure 3.1. Evolution of the equity ratio before and after the treatment

The figure plots the difference in the mean annual growth rate of the equity ratio (measured by the first difference of the natural logarithm) between the treatment and the control group. Each bar represents the difference of the mean of the within firm year-on-year growth rate of the equity ratio around the introduction of the ACE tax reform (Figure 3.1.A.) and its phasing-out (Figure 3.1.B.). The sample used is the matched sample of banks. Positive values mean that on average the equity ratio of Italian banks grows more than the control group relative to the year before. Negative values mean that on average the equity ratio of Italian banks grows less (or decreases more) than the control group relative to the year before. The significance of the difference in means between the treatment and control group is only shown when the p-value is smaller than 5%. The vertical red line separates the pre-treatment and post-treatment periods for both the introduction (Figure 3.1.A) and phasing-out (Figure 3.1.B.) of the ACE.

Figure 3.1.A: Introduction

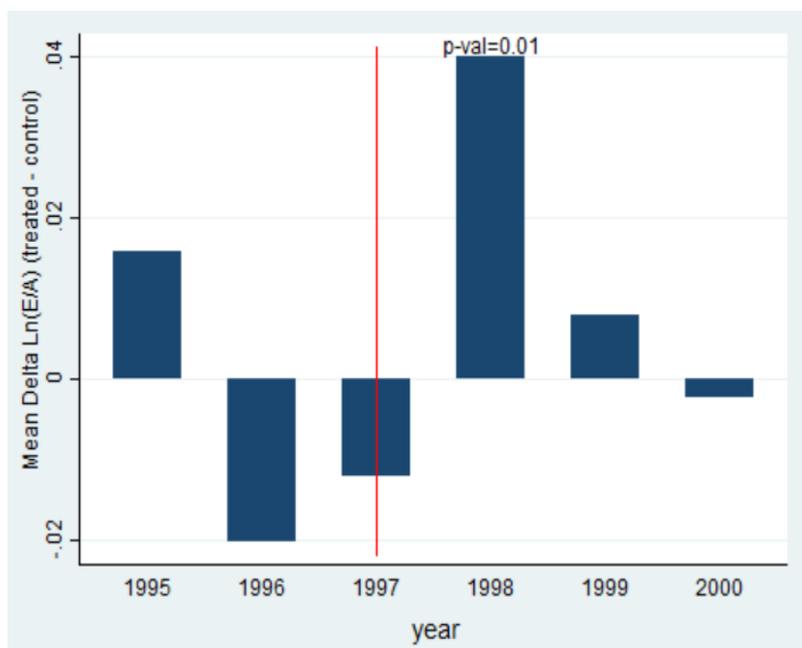
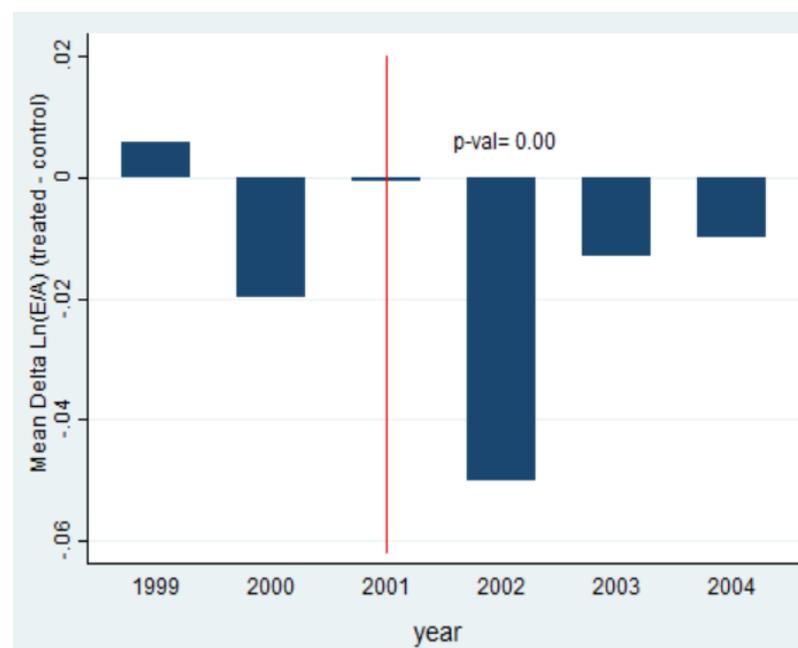


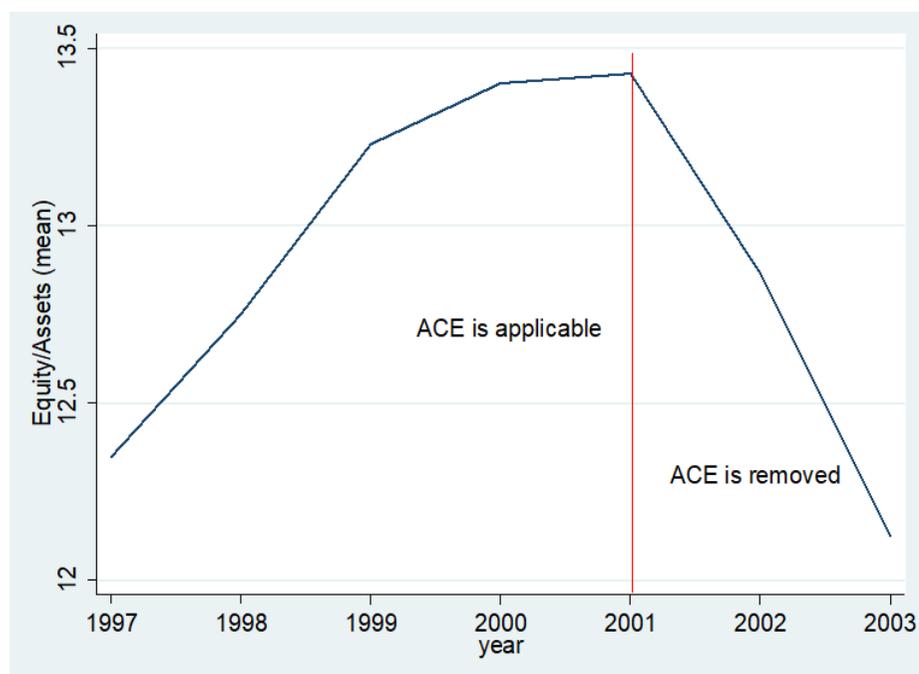
Figure 3.1.B: Phasing out



Appendix 3.1.A

Figure A.1. Evolution of the equity ratio for all Italian banks in Bankscope between 1997 and 2003.

This graph presents the evolution of the mean equity ratio (in %) for whole sample of Italian banks available in Bankscope between 1997 and 2003. The period on the left of the red line is the period during which the ACE scheme was fully applied. The period on the right side of the vertical red line is the period corresponding to the phasing-out of the ACE incentive. The mean value of the equity over total assets ratio for each year is computed using all Italian banks with information available on Bankscope each year. The equity over total assets ratio is winsorized at the 1 and 99 percent levels before computing the mean.



Appendix 3.1.B

Table B.1: Variable definitions and data sources

Variable name	Description	Source
Equity/Assets	Ratio of equity (defined as common equity) over total assets	Bankscope
Ln(Assets)	Natural logarithm of total assets	Bankscope
Ln(Equity)	Natural logarithm of common equity	Bankscope
RoA	Net income over total assets	Bankscope
Loan ratio	Total loans over total assets	Bankscope
Nii ratio	Non-interest income over total gross revenue	Bankscope
NPL ratio	Non-performing loans over total loans	Bankscope
LLP ratio	Loan loss provision over total assets	Bankscope
Loan Growth	100 multiplied by the natural log of the ratio loans to loans lagged one year	Bankscope
Ln(Sd RoA)	Natural logarithm of the standard deviation of the RoA over 3 years	Bankscope
Ln(Z-Score)	Natural logarithm of the Z-score. The Z-score is computed as follows: (Equity/Assets + RoA)/ Sd RoA)	Bankscope
Ln((Retained Earnings)	Natural logarithm of the retained earnings	Bankscope
GDPpc growth	Annual growth of the GDP per capita	WDI – World Bank
Inflation	Annual growth of the CPI index	WDI – World Bank
Ln(GDPpc)	Natural logarithm of the GDP per capita	WDI – World Bank
ST interest rate	Annualized short-term interest rate for each country	OECD statistics
CIT rate	Top statutory corporate income tax rates (including regional tax)	OECD statistics

Appendix 3.1.C

Summary of the scale-back process of the Italian state in the banking sector and strategy to identify banks potentially involved in the process

Two categories of state-controlled banks co-existed before 1990; savings banks (*Casse di Risparmio*), and larger banks in which the state had large stakes because they were considered as banks of national interest or public law banks (Natoli et al. 2016). Before 1990, savings banks were non-for profit organizations that devoted their earnings to projects of public interest. Since the enactment of the so-called Amato law in 1990, and the new banking law in 1993, savings banks became joint stock companies held by foundations. These newly created joint-stock banks became for profit organizations as their commercial peers and the previous restrictions on lending and access to capital markets were removed. It is complicated to assess how private an institution is when it is controlled by a foundation with a public character over which political parties and regional governments have some influence (Sapienza 2004). Subsequent laws in 1998 and 1999 required foundations to shed controlling stakes in banking institutions. In the same vein, the Italian state undertook a privatization process of the financial sector after the enactment of the new banking law of 1993. The state reduced progressively over the 1990s their stakes in larger banks that were considered of public interest. These banks became held by bank foundations and in some case became publicly listed. According to Ayadi, Schmidt, & Carbó Valverde (2009) these foundations significantly reduced their controlling stakes by the end of the 1990s and the beginning of the 2000s. In order to identify state-controlled banks for our test in Table 3.5, Column 5, we follow the following strategy: Firstly, we classify as state-controlled banks those banks that are state-controlled banks prior to the 1990 reform. These are savings banks (*Casse di Risparmio*) and banks resulting from mergers of savings banks. If the term *Casse di Risparmio* is in the name of the institution, we classify it as state-controlled. We also do internet researches to make sure that our classification is correct. We also check for the other banking institutions whether they result from the merger of two or more savings banks. If a bank is resulting from the merger of two *Casse di Risparmio*, we classify it as state-controlled. Secondly, we classify as state-controlled institutions categorized as banks of national interest or public law banks in which the state or any state agency had some stakes according to the information available in Mediobanca Research (1997), the

website of the Italian Treasury⁷² and Natoli et al. (2016). Once this classification is performed, we end up with 67 state-controlled banks for the period 1995-2000 (introduction of the ACE) and 55 state-controlled banks for the period 2000-2003 (phasing-out).

⁷²http://www.dt.mef.gov.it/export/sites/sitodt/modules/documenti_it/finanza_privatizzazioni/finanza_privatizzazioni/Riepilogo_delle_privatizzazioni_del_Ministero_dal_01-01-1994_al_30-09-2016.pdf

4. Chapter 3: Social Capital and Bank Misconduct⁷³

Abstract

Using enforcement actions issued by US bank regulators to identify wrongdoing, I investigate the effect of local levels of social capital (as captured by strength of civic norms and density of social networks) on bank misconduct. I find that banks headquartered in counties with higher levels of social capital are less likely to be involved in misconduct. This effect is concentrated in less geographically diversified banks. These results stand up to several robustness and endogeneity checks and are supported by an analysis of bank executives' social capital. I also show that once misconduct is detected, banks lose more market share in counties with higher levels of social capital. The latter result is only significant during the aftermath of the financial crisis and for non-technical misconduct cases. My paper sheds light on the role of social capital in bank misconduct and how social norms surrounding banks can discipline their behavior.

JEL Codes: G21, G28.

Keywords: banks, social capital, misconduct, market share.

⁷³ This chapter draws from a working paper authored by Jose M. Martin Flores which is available on the repository SSRN (Social Science Research Network) at the moment of the submission of this dissertation.

4.1 Introduction

Misconduct cases discovered by bank regulators over the past years have significantly deteriorated the image of the banking sector (Nguyen et al. 2016, Thakor 2016) and reduced the trust in banks (Chaly et al. 2017, Gallup 2016). For instance, Wells Fargo was sanctioned for opening customer accounts fraudulently⁷⁴, and JPMorgan, HSBC and Citi for their lack of money laundering controls⁷⁵. Even though misconduct cases concerning larger banks attract more headlines, this is a problem that affects the banking sector as a whole, given the number of actions for misconduct taken by US bank regulators against financial institutions of all sizes⁷⁶.

Violations of laws and regulations committed by banks have costs for financial stability (Jackson and Kotlikoff 2018), shareholder value (Armour et al. 2017, Köster and Pelster 2017) and generate negative real effects on local economies (Danisewicz et al. 2018)⁷⁷. Given the negative consequences associated with bank wrongdoing, it is important to study which factors may determine misconduct in the banking sector. The literature has focused on how internal governance factors affect the probability that a bank is involved in a misconduct case (Altunbas et al. 2018, Nguyen et al. 2016, Sakalauskaite 2018). However, there is a lack of empirical evidence on how informal institutions in the area surrounding the bank can influence wrongdoing. Studying the role of informal institutions in bank misconduct is motivated by the idea that social norms are needed to sustain legal norms (Guiso et al. 2015). In actuality, organizations do not make decisions, people do, and consequently, the social interactions that managers have are likely to influence what they do inside work (Hilary and

⁷⁴ "Wells Fargo uncovers up to 1.4 million more fake accounts", CNN, August 31, 2017.

⁷⁵ "Fed to Citi: Improve Anti-Money Laundering Controls" Fox News, March 26, 2013.

⁷⁶ For a total of 3,114 cases obtained from US bank regulator websites between 2001 and 2015, 90% are related to banks with less than \$3 billion of total assets.

⁷⁷ After taking into account endogeneity, Danisewicz et al. (2018) show that local economies with local banks that receive enforcement actions for violations of banking laws and regulations experience negative macroeconomic outcomes.

Hui 2009). Social factors - *culture* - play an important role in the individual's decision to break or follow the rules and the level of social tolerance for misconduct in a firm headquarters' area is likely to influence the approach towards misconduct in a firm (Parsons et al. 2018). In this paper, I analyze how a specific social factor, namely social capital, affects the probability that a bank is involved in misconduct and the consequences once misconduct is revealed.

The concept of social capital is based on the levels of mutual trust and shared civic norms and beliefs in a society (Woolcock 1998, 2001). A central idea that emerges in studies of social capital is that it facilitates norm-consistent and constrains norm-deviant actions (Guiso et al. 2011, Hasan et al. 2017a). The existence of informal values or norms shared by the members of a community and the creation of social ties (e.g. through social and civic associations) increase trust, which favors cooperation among members of the community (Fukuyama 1997, Putnam 1993). This leads to denser networks that facilitate the transmission of civic norms. Because of denser networks and respect for civic norms, residents in the community might fear a greater cost of breaching this trust by committing actions that deviate from the established standard (Spagnolo 1999). There is considerable evidence showing how social capital affects the behavior of individuals (e.g. Buonanno et al. 2009, Cialdini and Trost 1998, Guiso et al. 2004, Putnam 2001), however, it is interesting to study how social capital can affect the decisions of local firms regarding norm-deviant behaviors. This is particularly important in the context of banking due to the crucial role of trust in the industry (Fungáčová et al. 2017, Merton and Thakor 2018) and the interactions that banks have with the community through continuous banking relationships (e.g. Berger et al. 2014, Bolton et al. 2016, Canales and Nanda 2012). The existence of higher respect for civic norms and denser networks (i.e. higher social capital) in an area should, in theory, lead to lower misconduct. Managers affect bank decisions and are subject to the influences of social peers surrounding

bank headquarters. Given that bank misconduct is socially undesirable, high social capital communities are more likely to perceive this norm-deviant behavior as incongruent with the standards associated with higher respect for civic norms and trust. Accordingly, managers in high social capital areas should anticipate higher psychological distress associated with behaviors in their firms that deviate from social norms (Akerlof and Kranton 2005). Moreover, bank managers in higher social capital areas expect higher social sanctions associated with misconduct than in places with lower social capital levels. Because of higher standards set in high social capital communities (Pretty 2003) and rapid propagation of information in places with denser networks (Buonanno et al. 2009), bank managers' reputations are more at risk in areas with higher social capital levels. Therefore, they will have lower incentives to engage in opportunistic behaviors and be more likely to have higher concerns about misconduct. As a consequence, they will be more prone to adopt policies that decrease the probability of wrongdoing. Likewise, bank misconduct damages the trust of customers in banks (Chaly et al. 2017). Consequently, once a deviant behavior is revealed, a bank will be subject to a harsher punishment in markets characterized by higher levels of social capital due to the greater importance of mutual trust in those areas. Based on these assertions, I conjecture that banks headquartered in counties with a higher level of social capital have a lower probability of being involved in misconduct and that the discipline exerted over banks once wrongdoing is revealed increases in areas with higher social capital.

I use US county-level data to test these two conjectures. As in Hasan et al (2017 a, b), I construct a social capital measure using the first principal component in an analysis of four variables: two proxies of civic norms (the US Census response rate and voter turnout in the presidential election) and two measures of the density of networks (the number of social and civic associations and the number of non-government organizations). I use data from the Northeast Regional Center for Rural Development (NRCRD) at Pennsylvania State

University to compute these variables. To identify bank misconduct, I use enforcement actions issued by US banking regulators against banks that engage in unsafe, unsound or illegal practices. Enforcement actions are suitable for studying the relation between social capital and bank misconduct since they provide an unambiguous external indicator of unwanted behavior in the industry (Nguyen et al. 2016) and are publicly announced which allows bank stakeholders to be aware of them (Delis et al. 2018).

Using a sample of 101,669 bank-year observations during the period 2001-2015, I document a negative and statistically significant relationship between social capital and bank misconduct.⁷⁸ The economic effect is sizeable. Relative to an average probability of misconduct equal to 3.1%, a one standard deviation increase in the measure of social capital corresponds to a reduction of the probability that a bank is involved in misconduct of 11.8%. This result is mainly driven by less geographically diversified banks.⁷⁹ More complex and geographically dispersed banks tend to have decision centers that are located in different areas and the cultural values (i.e., social capital) of those areas may not be correlated with each other. Therefore, the impact of social capital measured at the level of the headquarters gets diluted.⁸⁰ Another explanation for this result is that less geographically dispersed banks tend to be managed locally and have greater portions of local investors and employees. These stakeholders are likely to be influenced by the ‘same social capital’ and therefore, the

⁷⁸ I run probit regressions controlling for bank and county variables as well as time (to capture the effect of aggregate shocks) and regulator fixed effects (to capture differences across regulators).

⁷⁹ I identify geographically diversified banks as those operating in five or less states.

⁸⁰ If one could determine the location of every decision center for more geographically diversified banks (hence more complex) and identify misconduct at that level, one could measure whether local levels of social capital surrounding these decision centers affect the probability of misconduct. However, this cannot be done with the existing data. Enforcement actions against banks that engage in misconduct are issued against commercial banks and not against specific branches of the bank. Also, it is difficult to identify the decision centers of each bank. In order to do so, proprietary information from the internal organization of each bank would be needed. Therefore, my estimates are not interpreted as that social capital does not determine bank misconduct for more geographically dispersed banks but rather that I can better identify the effect of social capital on misconduct for less geographically dispersed banks because decisions leading to misconduct are more likely to emanate from the headquarters.

probability that subcultures arise is lower in less geographically dispersed banks (Thakor 2016).⁸¹

These results are robust to controlling for bank and regional unobserved heterogeneity, using organ donation as an alternative social capital measure and are incremental to the effect of local religiosity. I also run regressions using the level of social capital of the state where bank executives grew up instead of the level of social capital in the county where a bank is headquartered. Using executives cultural traits is consistent with the fact that key decision-makers in a firm create and disseminate their cultural values within the organization (Liu 2016). This test is based on the idea that people are likely to be affected not only by the level of social capital of the place where they live, but also by the social capital of where they grew up (Guiso et al. 2004). For this test I use a smaller sample of banks for which I can identify the state where bank executives grew up.⁸² In line with the baseline results, I find that bank executives' social capital is negatively related to misconduct.

One of the methodological challenges that I face in this paper is that I can only observe detected misconduct (once an enforcement action against a financial institution has been issued). This poses a problem of partial observability since wrongdoing may have occurred even in the absence of an enforcement action. To address this aspect, I follow Nguyen et al. (2016) and run additional tests using a bivariate probit model (Poirier 1980). I also run additional tests using mean and lagged values of social capital to ensure that my model is actually explaining the relation between social capital and committing bank misconduct. I show that these tests confirm my baseline results.

⁸¹ Another possibility is that geographically diversified banks are more likely to be publicly held so they may be subject to stronger monitoring by stock-market actors (e.g. market supervisors, more sophisticated investors, etc) reducing the effect of social capital as a monitoring force. However, I find that the effect of social capital on the probability of misconduct is significant for both publicly and privately held banks.

⁸² I thank Scott Yonker for sharing his data on CEOs origins.

I identify the causal effect of social capital on bank misconduct using a two-stage instrumental variable regression to address the endogeneity of social capital due to omitted variables (potentially correlated with social capital and misconduct). For this test, I follow Hasan (2017 a, b) and use racial homogeneity and the natural logarithm of the distance from the Canadian border as instruments for social capital. The results obtained confirm that my baseline model is unlikely to be biased due to endogeneity.

Finally, I study the consequences that misconduct may have for banks depending on the level of social capital of the areas where they operate. I use percentage changes in deposits market share in each county where a bank has branches to analyze this outcome. I observe that once misconduct is publicly known, banks lose higher percentages of market share in counties with higher levels of social capital. When the social capital measure is one standard deviation above the mean, I observe a decrease of 1.2% in deposits market share. This represents 18.5% of the average growth of deposits market share for a bank in a given county. The latter result is only significant for the aftermath of the financial crisis (2011-2015) and mainly holds for misconduct cases of a non-technical nature⁸³.

My paper makes relevant contributions. First, it contributes to the literature studying the determinants of bank misconduct. Previous papers have provided evidence how internal governance factors determine bank misconduct (Nguyen et al. 2016, Altunbas et al. 2018, Sakalauskaite 2018). My article adds to this literature, providing empirical evidence on how an external factor, social capital surrounding bank headquarters, provides environmental influences constraining misconduct.

Second, my study is related to the stream of the literature that analyzes the impact of informal institutions and culture on firm behavior. Some papers study how religiosity affect

⁸³ Technical and non-technical misconduct are defined in Appendix 4.1.

firm level outcomes such as risk-taking, earnings manipulation and executive compensation schemes, among others (e.g., Adhikari and Agrawal 2016, Hilary and Hui 2009, Grullon et al. 2009, Hilary and Huang 2015). Contrary to these papers, I focus on social capital that is a source of civic values, instead of moral values (as it is the case of religion) and provide evidence about how it constraints opportunistic behaviors related to misconduct. Other papers have studied how social capital surrounding the headquarters of non-financial firms determine debt contracting, tax avoidance, executive compensation, the quality of financial information and corporate social responsibility (Hasan et al. 2017a, b, Hoi et al. 2019, Jha 2017, Jha and Chen 2014, Jha and Cox 2015). My paper diverges from the above-mentioned studies. I look at banks and study whether social capital deters wrongdoing in the banking sector. Looking at how an external factor such as social capital influences misconduct in banks is economically important given the negative impact that misconduct has on trust in banks (Jackson and Kotlikoff 2018, Zingales 2015) and the adverse macroeconomic consequences of bank sanctions (Danisewicz et al. 2018). In a paper related to mine, Parsons et al. (2018) provide evidence of how social factors explain the geographical distribution of misconduct committed by US firms. However, that paper does not discuss the role of social capital in wrongdoing and does not focus on the particularities related to bank misconduct. Therefore, my paper further contributes to this literature providing evidence on how local-level social capital can deter misconduct in banks.

Finally, my paper is related to the literature that analyzes the consequences of regulatory actions due to violations of laws and regulations. Papers focusing on financial institutions show sharp decreases in market values (Armour et al. 2017), adverse reputational effects in the syndicated loans market (Delis et al. 2018), changes in credit conditions (Roman 2017), worse performance (Köster and Pelster 2017) and a significant decrease of bank deposits (Delis et al. 2017b, Martinez Peria and Schmukler 2001). I contribute to this stream of the

literature by documenting a heterogeneous effect for the consequences of bank misconduct. I show that banks lose deposit market shares after bank misconduct is revealed, with this effect more pronounced in higher social capital areas. I also contribute to the literature studying the deterioration of firm-customer and other business relationships with adverse real effects when a company is caught in misconduct (e.g. Chava et al. 2017, Johnson et al. 2014, Karpoff et al. 2008, Klein and Leffler 1981). I contribute to this literature showing that during periods of low trust in the financial sector, bank customers exert a harsher punishment on sanctioned institutions in higher social capital areas, causing a reduction in county-level bank market shares.

The remainder of the paper is structured as follows. Section 4.2 discusses in more detail the theoretical background behind my study and motivates the hypotheses I test. Section 4.3 describes the data and empirical methodologies used in the paper. Section 4.4 explains the results of the paper. Section 4.5 concludes.

4.2 Hypothesis development

4.2.1 How social capital affects bank misconduct

Social capital is an environmental factor affecting the behavior of individuals (Guiso et al. 2004, Rupasingha et al. 2006) and organizations (Bloom et al. 2012, La Porta et al. 1997). However, in their review of the literature, Guiso et al. (2011) argue that definitions of social capital used in the economic literature tend to be vague and very broad, and suggest a narrower definition that focuses on values and shared beliefs that help cooperation in a society. In line with this suggestion, I define social capital as the civic norms and networks that determine the performance in a society (Woolcock 2001).

A central idea that emerges in studies of social capital is that the set of existing common beliefs and standards to judge behaviors make that civic norms facilitate actions that are

consistent with the prescribed norms. At the same time, respect for civic norms constrains individual and organizational actions that are socially undesirable (Hasan et al. 2017a). Nonetheless, how civic norms are spread and enforced depend on the interactions among people within a specific community. The existence of informal values or norms shared by the members of the community facilitates the creation of social ties through associative movements (i.e. social and civic associations) which increases trust and favors cooperation (Putnam 1993). Frequent social interactions in a dense network lead to more information sharing that boosts the communication and enforcement of social norms (Coleman 1988).

Given how social capital can shape the expectations and behaviors of people in a specific area, one could establish a natural (negative) link between social capital and bank misconduct. There is evidence that managers affect firm outcomes (Bertrand and Schoar 2003) and they are themselves influenced by the social environment surrounding companies' headquarters (Hilary and Huang 2015, Hilary and Hui 2009). Bank misconduct is undesirable from a social perspective (Group of Thirty 2015), so one should expect some social resistance against it. This resistance may even be stronger in higher social capital areas where higher trust and respect for civic norms increase the pressure from peers regarding civic and socially positive behaviors. Accordingly, managers in high social capital areas should anticipate higher social stigmatization for improper behavior connected to their firms (Cialdini and Trost 1998). Also, managers should anticipate higher psychological distress associated with behaviors that deviate from the expected standards set by social norms in a community (Akerlof and Kranton 2005).⁸⁴ These internal and external channels reduce the room for opportunistic behaviors by managers.⁸⁵ and provide them with a more positive attitude towards compliance with laws

⁸⁴ When social norms become part of the persons' identity, breaching them may lead to psychological distress (Akerlof and Kranton 2005, Parsons et al. 2018). The more someone deviates from the self-imposed benchmark, the guiltier the person feels.

⁸⁵ Some opportunistic behaviors from managers are related to specific policies that could boost the profits of the bank in the short-run: assuming temporary extreme risks, operating with low capital levels (due to the reliance on return on equity in the banking sector), delaying required provisions, setting loose internal controls and credit

and regulations because violating their civic duties can be seen as incongruent with the prescribed values of civic norms (Hasan et al. 2017a).⁸⁶ Accordingly, I formulate the following hypothesis.

Hypothesis 1a: *A bank headquartered in a county with a higher level of social capital has a lower chance of being involved in misconduct.*

Locally oriented banks have their business activities more concentrated in a specific area (Hakenes et al. 2014, Ostergaard et al. 2015) and bank officers are more likely to have local origins.⁸⁷ The diffusion of social and cultural factors from the headquarters towards other parts of the firm in less geographically diversified banks is easier since internal controls can be easily implemented and, if needed, managers can walk through branches or subsidiaries and have conversations with local employees. Furthermore, less geographically diversified banks are more likely to have local ownership and employees. This can contribute to a higher pressure and incentives to avoid deviant behaviors coming from investors and bank workforce in areas with higher social capital as the ‘same social capital’ aligns the incentives of bank stakeholders and managers. As a consequence of these factors, less complex organizations can be more successful in spreading cultural values from the headquarters, affecting individual and group behavior within the organization. However, as banking organizations get more complex and geographically diversified, they are likely to develop subcultures in different

assessment methods, reducing necessary expenditures to comply with regulations (e.g. compliance team), selling inappropriate financial products to customers, charging unfair commissions to customers, working with customers whose funds come from questionable, illegal or unethical sources, manipulating markets to obtain short-term gains, etc.

⁸⁶

⁸⁷ The literature in CEO-firm matching shows that firms hire local managers more frequently (Yonker 2017). Anecdotal evidence from the financial sector is in line with this argument when board members of Bank of America valued positively some potential CEO candidates because they have local origins (“North Carolina native might please board” as a pro for John J. Mack when listing the pros and cons for CEO candidates of Charlotte-based Bank of America (Corkery 2009)). Other papers point to the evidence that smaller and/or regional oriented firms are more likely to have local officers (“Firms with less visibility are expected to face greater challenges in attracting nonlocal directors, forcing them to rely on the local labor market for directors” (Knyazeva et al. 2013)). Thus, one can conjecture that regional banks are, in principle, more likely to have local officers.

divisions or areas, making more difficult the transmission of a uniform culture from the headquarters (Thakor 2016).

Consequently, endowed social and cultural factors surrounding bank headquarters are likely to be more significant for less geographically diversified banks. This conjecture should not necessarily mean that social capital does not determine bank misconduct in more geographically dispersed banks. If one could determine the location of every decision center for more geographically diversified banks (hence more complex) and identify misconduct at that level, one could measure whether local levels of social capital surrounding these decision centers affect the probability of misconduct. However, enforcement actions against banks that engage in misconduct are issued against commercial banks and not against specific branches of the bank. Also, it is difficult to identify decision centers unless one has proprietary information about the internal organization of each bank. Therefore, I conjecture that the negative effect of social capital (measured at the level of the headquarters) on bank misconduct will mostly be significant for less geographically dispersed banks. This is so because I can better identify empirically the effect of social capital on misconduct for these institutions as decisions leading to misconduct are more likely to emanate from the headquarters. This is consistent with other papers studying the effect of social capital on corporate outcomes that find that the effect of social capital attenuates with geographical diversification (Hasan et al. 2017a, Jha and Cox 2015). In line with these arguments, I hypothesize the following.

***Hypothesis 1b:** The negative relationship between social capital and bank misconduct is more pronounced for less geographically diversified banks.*

4.2.2 Banks involved in misconduct are punished more in high social capital regions, mainly when trust in the financial sector is low

Enforcement actions against banks for violations of laws and regulations are a publicly announced indicator of norm-deviant behavior that may have negative reputational effects (Delis et al. 2018). A negative reputation may disincentivize customers from contracting banking services with sanctioned banks.⁸⁸ Therefore, once misconduct is publicly known, banks may be subject to some degree of punishment in markets where they operate, losing, as a consequence, some market-share. However, all customers do not necessarily share the same belief and consequently the levels of social intolerance for bank misconduct may vary from one place to another. Misconduct is a way banks can breach the trust granted by the community in which they operate. Social capital constrains norm-deviant behaviors and therefore it will amplify the social penalty against misbehavior committed by banks.⁸⁹ Thus, a harsher punishment from customers against banks involved in misconduct is expected in higher social capital areas.

During the last financial crisis, the overall level of trust in corporations dropped significantly (Lins et al. 2017, Sapienza and Zingales 2012, Servaes and Tamayo 2017) and the bank misconduct cases discovered during its aftermath reinforced this negative view about the banking sector (Chaly et al. 2017). During the aftermath of the financial crisis, social accountability heightened for banks and society's expectations increased regarding how they conduct business. Therefore, the incremental effect of social capital in punishing deviant banks is expected to be more relevant during the aftermath of the financial crisis. Based on the above, I formulate the following hypothesis.

Hypothesis 2: Banks involved in wrongdoing will be subject to harsher punishment in high social capital counties and consequently lose more market share. The incremental effect of

⁸⁸ This is consistent with the market discipline literature. Being sanctioned for misconduct implies that private sector agents will perceive the bank as more risky (Delis et al. 2018). Bank customers who are exposed to the bank risk taking may penalize riskier banks by withdrawing their deposits, even in the presence of deposit insurance. Martinez Peria and Schmukler (2001) provide empirical evidence in this respect.

⁸⁹ The literature in social capital provides support to these arguments. There is considerable evidence showing that a community's social capital affects the behavior of individuals (Buonanno et al. 2009, Guiso et al. 2004).

social capital on sanctioned banks is more relevant in the aftermath of the financial crisis because of the lower trust in the financial sector during this period.

4.3 Data and methodology

4.3.1 Sample construction

I collect data on regulatory enforcement actions for the period 2001-2015 from the websites of the three main banking supervisors in the US: the Federal Reserve System (Fed), the Federal Deposit Insurance Corporation (FDIC) and the Office of the Comptroller of the Currency (OCC). All insured commercial banks have one of the above agencies as their primary bank regulator⁹⁰. The supervisory bodies conduct full scope on-site examinations every 12 or 18 months depending on the size and financial condition of the bank⁹¹. This involves an audit process to evaluate the bank. If as a result of this process the supervisors find that the bank has breached any regulation, they issue an enforcement action.

Following Delis et al. (2018), I focus on bank enforcement actions that are related to technical aspects⁹² such as violations of capital adequacy and liquidity, asset quality, lending, provisions and reserves, and non-technical aspects such as failures of the bank's internal control and audit systems, risk management systems, anti-money laundering violations, violations of consumer protection laws (Federal Trade Commission Act, the Equal Credit Opportunity Act, etc.), breaches of the requirements concerning the competency of the bank personnel (board members, senior managers or other affiliated parties) and cases related to

⁹⁰ OCC supervises National Banks. State Banks are supervised by their chartering state banking departments, in conjunction with the FDIC for banks that are not members of the Federal Reserve System or the Fed for banks that are members of the Federal Reserve System. Agarwal et al. (2014) provides a detailed explanation on how banking supervision is organized in the US.

⁹¹ Exams are conducted every 18 months for banks under a specific total assets threshold. This threshold has changed over time and is set at \$500 million since 2007. The reader may refer to Agarwal et al. (2014) for further details.

⁹² These enforcement actions are related to the Basel Committee Core Principles for Effective Banking Supervision (Basel Committee 2012). They deal with the issues that are of the greatest interest for regulators since they are related to bank safety and soundness and should bear reputational risk since non-observation of these issues could lead to future bank difficulties (Delis et al. 2017a, 2018).

fraud and insider abuse. Either because they pose a risk to safety and soundness of the bank or because they tend to have media coverage, these kinds of breaches of the law and regulations have reputational effects and are of a great concern for both managers and supervisors (Delis et al. 2018)⁹³. In the next step, I match the enforcement actions using name, city and state with each bank that received an enforcement action using the Reports of Condition and Income (hereinafter Call Reports). If there are multiple enforcement actions related to a single misconduct case, I aggregate them so only one case is identified. My final sample contains 3,114 enforcement actions. 53% of these enforcement actions are of a technical nature and 47% of them are related to non-technical issues. I observe that the proportion of technical enforcement actions is higher during the financial crisis (2008 - 2010), while non-technical enforcement actions are more numerous after 2011 when Dodd-Frank regulations were enacted and right before the financial crisis in 2005 and 2006. The differences between these types of misconduct are detailed in Appendix 4.1 and 4.2. Even though I see some clusters, enforcement actions are not just a feature of the last financial crisis, as they spread over the sample period. Table 4.1 provides a detailed summary of the enforcement actions used in the paper.

[Table 4.1 about here]

I obtain balance sheet and income statement information from Call Reports. Banks (both public and private) must file every quarter their Call Reports. However, as I work with annual data, I keep the year-to-date information at the end of every fiscal year for the period 2000-2015. I remove from my sample banks that have fewer than two observations, banks that have negative equity values, no deposits or loans, failed (the year they fail) and acquired banks (the

⁹³ Other enforcement actions deal with reporting issues (Call Reports filing or Home Mortgage Disclosure Act) or violations of laws such as Flood Disaster Protection Act. These violations are considered as less severe and they have little reputational consequences according to the literature dealing with bank enforcement actions in the US (Danisewicz et al. 2018, Delis et al. 2017a, 2018, Lambert 2017). Therefore, I do not consider them as bank misconduct.

year they are acquired), banks that are non-insured or have been insured for less than 2 years, banks with no information on their primary regulator, banks whose federal regulator is the Office of Thrift Supervisor (OTS), and banks that are not located in the continental territory of the US and Alaska⁹⁴. My final sample contains 8,953 commercial banks.

Finally, I use some county-level variables. I obtain income per capita and employment from the Bureau of Economic Analysis and the median age, the percentage of people older than 25 years old with higher education and the percentage of rural population (to define the dummy variable *rural*) from the US Census. Continuous bank accounting and county-level socioeconomic variables are winsorized at the top and bottom 1% to avoid the impact of outliers. Table 4.2 provides descriptive statistics for the variables used in the paper.

[Table 4.2 about here]

4.3.2 Social capital measure

I use data from the Northeast Regional Center for Rural Development (NRCRD) at Pennsylvania State University to estimate the level of social capital in US counties in the years 1997, 2005, 2009 and 2014. The measure of social capital I use is based on Rupasingha and Goetz (2008) who use a principal component analysis (PCA) to construct a county-level index of social capital in the United States. The variables used for the computation of the social capital index (based on a PCA) are the number of non-profit organizations without including those with an international approach divided by population per 10,000 (NCCS), the number of social and civic associations⁹⁵ divided by population per 1,000 (ASSN), the voter

⁹⁴ Because I use distance from the Canadian border (in Km) as an instrumental variable, keeping banks located in remote (i.e. Alaska) or non-continental (e.g. Hawaii, Puerto Rico) areas could add unnecessary noise to my estimation.

⁹⁵ Associations included in this category are civic and associations, sports teams and clubs, bowling centers, physical fitness facilities, public golf courses, religious organizations, political organizations, business associations, professional associations and labor organizations in each county.

turnout in presidential elections (PVOTE) and the census response rate (RESPN).⁹⁶ In line with the theoretical development, NCCS and ASSN are measures of networks and capture the density of these networks at county-level. PVOTE and RESPN are measures of social norms. Higher values of these factors mean higher social capital levels. Because the social capital index is solely computed in the years 1997, 2005, 2009 and 2014, and consistent with other papers using county-level variables that are not measured every year (Hasan et al. 2017a, Hilary and Hui 2009, Jha and Cox 2015), I use linear interpolation to fill-in the data for the periods 2000-2004, 2006-2008, 2010-2013.

Figure 4.1 presents the spatial distribution of the variable *social capital* for all counties in the US in 2014. A darker shade represents higher social capital values. The map shows that social capital is higher in upper Midwest/Northwest counties and lower in Southern counties (with some exceptions). This map shows similar patterns to the ones reported in Rupasingha et al. (2006) for 1997 and Hasan et al. (2017a, b) for 2005. This explains the high correlation of the variable *social capital* over time (roughly 90% on average). This is consistent with the fact that social capital is highly persistent (Guiso et al. 2011).

4.3.3 Empirical design

I use the following probit model (or baseline model) to test Hypotheses 1a and b.

$$Prob(Y_{i,t} = 1) = F(\alpha + \beta_1 SocialCapital_{c,t-1} + \beta_2 X_{i,c,t-1} + \varphi_r + \varphi_t + \varepsilon_{i,t}) \quad (1)$$

Where, $Y_{i,t}$ is a variable indicating the presence of misconduct. This variable is a dummy variable equal to one if bank i receives an enforcement action for committing misconduct at time t and zero otherwise. α is a constant term, $SocialCapital_{c,t-1}$ is the main independent variable, $X_{i,c,t-1}$ contains a variety of variables including bank level covariates and a set of

⁹⁶ Before computing the PCA, these four factors are standardized to have a mean of zero and a standard deviation equal to one.

county-level variables. All these time-varying variables are measured at $t-1$ in the baseline model. The bank level covariates used in the regressions contain a set of variables that previous studies have shown to influence misconduct in banks (Nguyen et al. 2016). I include some proxies for capital strength, earnings, asset quality and liquidity as managers of poorly performing or distressed banks may be more likely to commit wrongdoing to inflate earnings. As a proxy for capital strength I use the equity over total assets ratio, I use the return on assets as a proxy for earnings, I use the allowance for loans and lease losses to control for asset quality, for liquidity I use the ratio of cash and cash equivalents over total assets. I also include other bank level variables such as the ratio of risk-weighted assets to total assets, deposit ratio, age, size and size squared, a proxy for competition (a deposit weighted HHI index computed for all areas in which a bank has branches), a dummy that takes the value one if the bank is publicly held and zero otherwise and a dummy that takes the value one if the bank is held by a bank holding company and zero otherwise.

The choice of the county-level variables is based on papers studying the effect of social capital on firm policies (e.g., Hasan et al. 2017 a).⁹⁷ These variables are income per capita, employment rate, median age, percentage of people with higher education and a dummy variable taking the value one if the county is rural (less than 50% of the county population living in urban areas) and zero otherwise⁹⁸. These county-level control variables have been highlighted in the literature as determinants of social capital. For instance, Rupasingha et al. (2006) highlights that more income leads people to join social groups and participate in non-income earning social activities. Furthermore, these authors suggest that education and age are the most important determinants of social capital. Putnam (1995) states that employed

⁹⁷ In the Online Appendix, I add other county-level control variables to the baseline regression to rule-out that other missing county-level variables are biasing our results.

⁹⁸ Definition of rural counties is provided by the US census in its rural lookup table <https://www.census.gov/geo/reference/urban-rural.html>. Counties with less than 50 percent of the population living in rural areas are classified as mostly urban; 50 to 99.9 percent are classified as mostly rural; 100 percent rural are classified as completely rural.

people belong to more social groups than those outside the labor force. Putnam (1995) and Rupasingha et al. (2006) also show that urban communities present a lower level of civic engagement than rural areas. These county-level variables may also influence misconduct. Parson et al. (2018) identify these socio-economic variables as determinants of corporate misconduct and Egan et al. (2017) show that individuals working in the financial sector (i.e. financial advisers) are more likely to be involved in misconduct in areas with low education, elderly populations and high incomes. As I want to isolate the effect of social capital on bank misconduct from other county-level socioeconomic characteristics, it is important to include these control variables. I also include regulator fixed effects (φ_r) to control for differences across regulatory agencies and time fixed effects (φ_t) to control for the effect of aggregate shocks. In Appendix 4.1 I describe the construction of the variables used in the paper as well as the sources. Table 4.2 provides descriptive statistics. It is evident from the summary statistics that enforcement actions are a rare event (3.1% of observations). Probit models may underestimate the probability of rare events. To verify the robustness of my results, I follow King and Zeng (2001) 's recommendations and correct these downward biases by analyzing the data using a rare events logit model. The results delivered by this procedure are in line with the rest of the results of the paper (Online Appendix, Table A.3.C.). However, I report probit models because it is a more conservative estimation. Standard errors are clustered by bank or county as indicated in the tables.

However, as I try to estimate the probability of bank misconduct, I face an empirical challenge. Misconduct is not observed until it is detected. Therefore, the outcome I observe when studying regulatory enforcement actions is the product of the commission of misconduct and the detection of misconduct. As the process of detection is not perfect (misconduct may be committed and not detected), I face a problem of partial observability. In order to address this aspect, I run additional tests using a bivariate probit model (Poirier

1980). This methodology allows me to be sure that what I am detecting is a lower probability of committing misconduct and not lower chance of detection or enforcement. This model, initially created to address the partial observability problem studying crime (Becker 1968), and used in other papers studying corporate misconduct (e.g. Nguyen et al. 2016, Wang 2011, Wang et al. 2010), assumes that the probability of misconduct is determined by two sets of variables. The first set is derived from the benefit of committing fraud and the second is derived from the probability of being detected. According to my hypotheses, *social capital* should have a negative impact on the probability of committing misconduct and positive or no impact on the probability of detection (conditional on misconduct being committed). Empirically, the bivariate probit model addressing partial observability separates detection from commission processes. Let $M_{i,t}$ and $D_{i,t}$ be whether bank i commits wrongdoing in year t and whether misconduct is detected, respectively:

$$M_{i,t}^* = X_{M,i,t-1}\beta_M + \varepsilon_{i,t} \quad (2)$$

$$D_{i,t}^* = X_{D,i,t-1}\beta_D + \mu_{i,t} \quad (3)$$

Where, $X_{M,i,t-1}$ is a vector of variables explaining the incentive to commit misconduct, and $X_{D,i,t-1}$ is a set of variables that explain the banks' likelihood of being detected by the regulator. These two vectors do not contain the same variables and one of them has one or more variables that are absent in the other. $X_{M,i,t-1}$ contains the same variables that I use in my baseline model. However, $X_{D,i,t-1}$ does not contain the *equity ratio* and the proxy for earnings (*ROA*). I instead use the regulatory capital ratio and the Z-score. Since the regulatory capital ratio is one of the target ratios for regulators, a low level of this ratio will put the bank under the radar of supervisors. Similarly, Z-score⁹⁹ is a proxy for bank stability. Low levels of this measure will indicate high earnings volatility and closeness to insolvency (e.g. Berger et

⁹⁹ I use in the regressions the natural logarithm of Z score. The Z score is computed as the average mean of the equity ratio plus the mean of the ROA divided by the standard deviation of the ROA. Low values of the Z score imply lower bank stability and closeness to insolvency. The Z-score indicates the number of standard deviations that a bank's return on assets has to drop below its expected value before equity is depleted and the bank becomes insolvent.

al. 2017, Laeven and Levine 2009). Therefore, low values of these ratios will draw the attention of supervisors, increasing the chances of misconduct detection. $\varepsilon_{i,t}$ and $\mu_{i,t}$ are the disturbances that I assume that follow a bivariate normal distribution.

I represent $M_{i,t} = 1$ if $M_{i,t}^* > 1$ and $M_{i,t} = 0$ otherwise. I denote $D_{i,t} = 1$ if $D_{i,t}^* > 1$ and $D_{i,t} = 0$ otherwise. As the realizations of $M_{i,t}$ and $D_{i,t}$ are not observed directly, I define a variable $Z_{i,t} = M_{i,t} * D_{i,t}$. $Z_{i,t} = 1$ if a bank engages in misconduct and is detected, and $Z_{i,t} = 0$ if a bank does not engage in misconduct or if it engages in wrongdoing and is not detected by the regulator.

If I denote ϕ the bivariate standard normal cumulative distribution function, ρ the correlation between $\varepsilon_{i,t}$ and $\mu_{i,t}$ from (2) and (3), then:

$$P(Z_{i,t} = 1) = P(M_{i,t} = 1, D_{i,t} = 1) = \phi(X_{M,i,t-1}\beta_M, X_{D,i,t-1}\beta_D, \rho) \quad (3)$$

$$P(Z_{i,t} = 0) = P(M_{i,t} = 0, D_{i,t} = 0) + P(M_{i,t} = 1, D_{i,t} = 0) = 1 - \phi(X_{M,i,t-1}\beta_M, X_{D,i,t-1}\beta_D, \rho) \quad (4)$$

Therefore, the log-likelihood function allowing me to estimate the coefficients of the model¹⁰⁰ is as follows:

$$L(\beta_M, \beta_D, \rho) = \sum \log(P(Z_{i,t} = 1)) + \sum \log(P(Z_{i,t} = 0)) \quad (5)$$

In final part of the paper, I assess the consequences of bank misconduct as stated in Hypothesis 2. In this case, I use a county-level measure of market share. I am interested in studying how market share changes when misconduct is detected by the regulator, depending on the level of social capital of the county. In this respect, I run the following OLS model:

$$\begin{aligned} \% \Delta MarketShare_{i,c,t} &= \gamma_0 + \gamma_1 ZSocialCapitalMkt_{c,t-1} + \gamma_2 misconduct_{i,t} \\ &+ \gamma_3 ZSocialCapitalMkt_{c,t-1} * misconduct_{i,t} + \gamma_4 X_{i,c,t-1} + \varphi_r + \pi_t + \mu_s + u_{i,t} \end{aligned} \quad (6)$$

¹⁰⁰ The model includes bank regulator and time fixed effects.

Where, $\% \Delta MarketShare_{i,c,t}$ is the percentage change of deposits market share of bank i in county c between time $t-1$ and t . I compute $MarketShare_{i,c,t}$ using branch level data available at the FDIC Summary of Deposits. These data are available annually and measured as of 30 June every year. I compute the share of deposits of each bank in each county every year using these data and then the annual percentage change. $ZSocialCapitalMkt_{c,t-1}$ is the standardized value of the social capital variable at $t-1$ computed in each market (each county in which the bank operates), and $misconduct_{i,t}$ is a dummy variable taking the value one if bank i receives an enforcement action between year $t-1$ and t and zero otherwise¹⁰¹. Therefore, the coefficient of interest is γ_3 and captures the impact of misconduct detection on market share changes when social capital is one standard deviation above the mean. In other words, this interaction term captures whether banks are more penalized in high social capital counties once misconduct is revealed. $X_{i,c,t-1}$ represents a vector of bank-level (equity ratio, allowance for loan and lease, ROA, liquidity, size, deposit ratio, age, publicly held dummy and bank holding company dummy) and county-level variables (bank competition measured at county-level, income per capita, population, employment and average wage growth) measured for each county in which a bank is present. φ_r , π_t and μ_s represent regulator, time and state fixed effects¹⁰², respectively. $u_{i,t}$ is an error term.

4.4 Results

4.4.1 Baseline results

¹⁰¹ Since the market share using branch level data are measured as of 30 June every year, enforcement actions issued during the third and fourth quarter of $t-1$ are moved to year t . Therefore, the variable $misconduct_{i,t}$ takes the value one if an enforcement action has been issued against bank i during the third or fourth quarter of the fiscal year $t-1$ or during the first or second quarter of the fiscal year t , and zero otherwise.

¹⁰² State fixed effects aim at controlling for state-level unobserved factors that could affect changes in deposit holdings from customers (e.g. state-level policies that make holding deposits less or more attractive) and/or market shares.

In Table 4.3, I test the effect of *social capital* on the probability that a bank is involved in misconduct using the probit model detailed in (1). I provide the regression coefficients for each variable and the marginal effects of the main variable of interest in square brackets. Column 1 reports a probit regression of a bank misconduct dummy on *social capital* and year and regulator fixed effects. I observe a negative impact of *social capital* on misconduct. The marginal effect of *social capital* is equal to -0.007 and significant at the 1% level. In Column 2, I show my baseline model including the full set of control variables. I observe that the economic significance of *social capital* gets reduced but the coefficient remains negative and statistically significant at the 1% level. These results are in line with my predictions in Hypothesis 1a. They show that a higher level of social capital is associated with a lower chance that a bank is involved in misconduct. The economic effect is sizeable. I find that a one standard deviation increase in the county-level measure of social capital is associated with a statistically significant 0.36% (-0.003×1.201) lower probability of misconduct. Relative to an average probability equal to 3.1%, this corresponds to significant decrease in the probability that a bank is involved in misconduct of 11.8% ($-0.0036/0.0305$)¹⁰³. In Column 3, I run the same regression but cluster the standard errors on the county-level. Given that the main dependent variable is measured at this level, it is advisable to test whether the results hold when applying this clustering in order to correct the standard errors for the non-independence of the observations within the same county. I show that the coefficient of the variable *social capital* remains statistically significant at the 1% level.

Some tests in the Online Appendix provide additional evidence regarding the baseline results of the paper. In Table A.3.A in the Online Appendix, I use all the components of social capital as regressors; instead of using the social capital variable computed using a principal

¹⁰³ In Online Appendix, Table A.3.A I show that both networks and civic norms components of social capital have a negative and significant impact on the probability that a bank is involved in misconduct. In Table A.3.B, I show that the effect of *social capital* in my baseline regression is negative and significant for the pre-crisis, crisis and post-crisis periods.

components analysis. The results of this test show that the effect comes from the two components of social capital, networks and trust. In Table A.3.B. I show that the effect documented in the baseline tests holds for the pre-crisis, crisis and post-crisis periods.

[Table 4.3 about here]

4.4.2 Bivariate probit model with partial observability, average over the sample period and further lags

I employ regulatory enforcement actions issued by US bank regulators against banks that engage in unsafe, unsound and illegal banking practices to detect the banks that have committed misconduct. Generally, one can only observe detected misconduct (once an enforcement action has been issued) but not the set of all committed cases of misconduct. That is, even in the absence of enforcement actions, a bank may have engaged in misconduct without being detected. This problem is known as partial observability. I follow Wang (2013), Wang et al. (2010), and Nguyen et al. (2016) and address this problem running a bivariate probit model (Poirier 1980). I describe the results of this test in section 4.4.2.1. Additionally, in section 4.4.2.2., I run some additional tests using mean values and lagged values of the main independent variable.

4.4.2.1 Bivariate probit model with partial observability

To address the partial observability issue, I run the model detailed in section 4.3.3. (5) states the function that is used for this model. Table 4.4 reports the estimation. Column 1 reports the prediction results for banks committing misconduct $P(M=1)$ and Column 2 show the prediction results for banks that were detected to have committed misconduct, conditional upon misconduct having been committed $P(D=1 | M=1)$.

The coefficients of the variable *social capital* have the expected signs. In Column 1, I observe that the coefficient of my key variable of interest is negative and statistically significant. This shows that a higher level of social capital is associated with a lower chance of committed misconduct. In Column 2, the estimated coefficient of *social capital* is positive. This suggests that social capital is positively associated with detection of misconduct. However, the coefficient is not statistically significant.

These results are in line with my baseline results using a probit model. They confirm that banks located in high social capital areas are less likely to commit misconduct when I take into account the partial observability problem associated with corporate wrongdoing.

4.4.2.2 Additional tests

In my study, I use enforcement actions issued against banks that commit misconduct. These data do not allow me to determine with enough certainty the exact date when wrongdoing has taken place for all cases. However, the measure of social capital is persistent and highly sticky. Due to this persistence, I argue that even if I do not measure misconduct at the time it is committed (but rather when there is an enforcement action)¹⁰⁴, my empirical analysis is capturing a lower chance of receiving an enforcement action because misbehavior is lower in banks headquartered in counties with higher social capital levels. In this subsection, I run some additional tests that strengthen my argument.

In Columns 3 and 4, given the high persistence of *social capital*, I run a cross-sectional probit regression using the mean values of the main independent variable and the rest of controls. I further distinguish between technical and non-technical cases to test whether the results hold for both types of misconduct¹⁰⁵. In Column 3, I run a probit regression of a

¹⁰⁴ This is the same approach that Nguyen et al. (2016) and Altunbas et al. (2018) follow in their studies.

¹⁰⁵ Using a cross-sectional probit regression on the mean values is better suited to test whether the results hold for both types of enforcement actions due to the fact that the number of misconduct events overall is very low in

dummy taking the value one if the bank is involved in technical misconduct over the sample period and zero otherwise on the mean value of *social capital* and the rest of independent variables I use in my baseline model. I show that the coefficient for *social capital* is negative and significant at the 1% level. I do the same exercise in Column 4 with respect non-technical enforcement actions. The coefficient for *social capital* is negative, statistically significant at the 1% level.

A potential concern in my study is that there may be some delay between the issuance of the enforcement action and the moment when the wrongdoing takes place. In this respect, I run two additional regressions taking 2 lags (in Column 5) and 3 lags (Columns 6) of the variable *social capital*¹⁰⁶. The reported coefficients for *social capital* are negative and highly statistically significant. These results support my baseline findings.

[Table 4.4 about here]

4.4.3 Robustness tests

In Table 4.5, I run several regressions to test the robustness of my results. I only show the coefficients and/or marginal effects of the variable *social capital* or other specific variables of interest for brevity. The control variables in this table are the ones I use in my baseline model. In Column 1, I run a robustness check that aims at testing whether bank-specific unobserved heterogeneity influences my results. I would ideally control for unobserved heterogeneity across banks in the model including bank fixed effects. However, implementing such a model in my econometric setting poses two challenges. First, the level of community's social capital

whole sample (3.1% of the total observations). In the cross-section, 15.6% of banks are involved in technical misconduct at least once during the sample period and 13% are involved in non-technical misconduct. However, when I run regressions using the whole panel data set (the one I use in my baseline model) for technical and non-technical misconduct, separately (unreported tests), I obtain negative and significant coefficients for the variable *social capital*.

¹⁰⁶ Parsons et al. (2018) indicate that, on average, misconduct is detected after 2 years with a median value of 3 years.

tends to change very slowly, making impractical to implement a bank fixed effects model to examine the relation between year-by-year variation in within bank social capital and misconduct. In this respect, Zhou (2001) notes that when temporal variation in the key dependent variable is small, firm fixed effects eliminates too much of the variation in the variable to accurately estimate its coefficient. In order to get around this issue, Wintoki et al. (2012) suggests a sampling using period-spaced observations¹⁰⁷. I follow this approach and use a sample using year intervals. To allow the maximum variation possible of *social capital*, I reduce the sample to the years in which this variable is measured in the NRCRD database (years 2005, 2009 and 2014) plus the year 2001 which is the first year of my sample. The second challenge is that the estimation of the bank fixed effects coefficients in a non-linear model could introduce an incidental parameters problem (Lancaster 2000, Neyman and Scott 1948). This problem of finding consistent estimators in non-linear models occurs because the number of bank fixed effects grows without bound, but the amount of information for their estimation is limited. Both fixed effects and coefficient estimates may become biased in such a setting. Even though the bias may get reduced as the number of periods in the panel data increases, there is some uncertainty on whether it would disappear in my setting. It is also worth mentioning that in non-linear panels, taking first differences of the dependent variable to get rid of fixed effects is not possible. Despite this second challenge, I decide to go ahead with a linear probability bank fixed effects model using a period-spaced sample. The result in Column 1 shows that the coefficient of *social capital* is negative and only statistically

¹⁰⁷ Wintoki et al. (2012) study the effect of board structure on firm performance. Because board structure is highly persistent, it can reduce the power of any panel data estimator (Zhou 2001). To mitigate this concern, Wintoki et al. (2012) sample at two or three-year intervals instead of every year.

significant at the 10% level. This result offers some assurance that bank-level unobserved heterogeneity is not driving the results.¹⁰⁸

In the baseline tests, I control for regulator and time fixed effects. This choice follows previous literature in finance that use social capital or other variables measured at county-level that have low variation across time (Adhikari and Agrawal 2016, Hasan et al. 2017a, Hilary and Huang 2015, Hilary and Hui 2009, Jha and Cox 2015). A potential concern is the existence of a time-invariant regional heterogeneity that affects social capital and bank misconduct. To alleviate this concern, I include state fixed effects. However, there is low variation in the *social capital* variable within a state. The inclusion of state fixed effects can take away part of the *social capital* variable effect on bank misconduct. Nonetheless, in Column 2, I show that the coefficient of *social capital* remains negative and statistically significant at the 5% level after running the baseline model with state fixed effects. This result suggests that the baseline findings are not driven by omitted regional unobserved heterogeneity.

In Column 3, I exclude rural banks from the sample. I run a regression on a sample of banks that have at least one branch located in a Metropolitan Statistical Area (MSA), excluding all banks that have branches only in rural counties. This test allows me to rule out the possibility that my result is driven by differences in enforcement intensity between rural and urban banks¹⁰⁹. The variable *social capital* remains negative and significant and the economic effect is close to my baseline results. In Column 4, instead of using linear interpolation to complete the data for missing years, I back-fill using estimates of the last year

¹⁰⁸ I also run the baseline model using a random effects probit regression. In this test the coefficient of the variable social capital is negative and statistically significant and the size of the coefficient is similar to the one of my baseline result. I do not report the results for brevity.

¹⁰⁹ Nguyen et al. (2016) show that bank enforcement intensity does not vary between rural and urban areas. However, they have a sample of publicly held banks that have a low number of institutions located in rural areas. Therefore, I believe that it is convenient to run my tests using a sample of banks located in non-rural areas to completely rule out any concern about differences in enforcement between these two types of territories.

for which *social capital* data are available. For example, I fill in missing data for *social capital* from 2001-2004 using social capital estimates in 1997. My conclusions remain unchanged when I run this robustness test. In Column 5, I adjust the component of *social capital* ASSN in 1997¹¹⁰. As in Hasan et al. (2017b), I use only the 10 types of social organizations that are consistently reported in NRCRD to account for social organizations. Then, I compute the PCA and come up with a modified social capital index. I then use linear interpolation to complete the data for the period 2001-2004 and run the same regression that I run in my baseline tests. As expected, the correlation between the modified social capital index and the one I use in the rest of the paper is very high. I observe in the regression in Column 5 that my results are similar to the baseline regression.

Following Guiso et al. (2011) and Hasan et al. (2017), I use organ donation as an alternative measure of social capital. Since donation of organs has no economic payoff and there is no legal obligation to donate, the decision to donate can be seen as a measure of how much people internalize the common good, providing a proxy for civic capital. I show in Column 6 the result of my baseline regression using *organ donation* as a proxy for social capital. The variable *organ donation* has a negative coefficient and is statistically significant at the 1% level. Using this variable as an alternative proxy for social capital leaves my conclusions unchanged.

In Column 7, I run a regression using a variable that captures the number of religion adherents at county-level as in Hilary and Hui (2009). Religion has an effect on corporate decisions and is also a potential source of both moral values and an engine of socialization (Adhikari and Agrawal 2016, Dyreng et al. 2012, Guiso et al. 2011). The literature has shown that religion may have an impact on corporate misbehavior (Grullon et al. 2009). As a

¹¹⁰ ASSN in 1997 includes 12 categories instead of 10. I eliminate the categories that are not represented in the subsequent years (i.e. non classified membership organizations and sports membership organizations) and compute the modified value of ASSN.

consequence, I add the lagged variable *religion* together with the social capital measure and the set of control variables. My results show that *social capital* remains negative and highly significant while the variable *religion* is not statistically significant. This test shows that the measure of social capital captures the effect of altruistic and social values on bank misconduct beyond the effects of religion.

In Column 8, I add a proxy for less severe enforcement actions as a control variable. Less severe enforcement actions are related to violations of some reporting obligations or other laws less related to bank safety and soundness (see Appendix 4.1 for a more precise definition). However, these enforcement actions may be precursors to more severe bank problems and may trigger an increase of the attention of regulators on these banks, increasing the likelihood that an enforcement action is issued (Danisewicz et al. 2018). In order to be sure that omitting this variable does not lead to biased results, I use a variable that takes the value one if a bank has received a less severe enforcement action during the period $t-2$ and t and zero otherwise. I observe that the coefficient of less severe enforcement actions is positive and significant as predicted. What it is reassuring is that the coefficient of *social capital* remains positive and statistically significant and is the same as the one in my baseline regression. This gives me confidence that my baseline result not biased due to an enhanced supervision from the bank's examiners/supervisors for institutions that receive less severe enforcement actions.

[Table 4.5 about here]

In The Online Appendix, I run some additional tests to rule-out that other potential missing variables are biasing my baseline results. In Figure 4.1, we observe that the higher levels of social capital are concentrated in some specific areas of the US. As a consequence, in Table A.3.F I run separate regressions for different regions of the US to rule-out that the baseline

result is driven by banks located in a specific area of the US. *Social capital* remains significant in all regressions. In Table A.3.G. I show that when I add other additional county-level variables to the model, the negative effect of social capital on the probability that a bank is involved in misconduct remains significant. In Table A.3.H. I take into account other variables that affect the behavior of banks (and that could potentially be related to misconduct) such as distance to regulator (Wilson and Veuger 2017), board characteristics (Nguyen et al. 2016) or bank complexity (Alessandrini et al. 2009). The results delivered by those regressions do not change the main conclusions drawn from the baseline tests.

As an addition to the robustness tests described above, I run a simulation exercise that works as a placebo test. I show the results of this exercise in Figure 4.2. The histogram plots the distribution of the coefficients of the variable *social capital* obtained from 1000 linear probability model regressions in which the dependent variable is a dummy variable randomly simulated (taking the value one or zero), the main independent variable is the measure of social capital and the rest of control variables and fixed effects are the same as those that I use in Table 4.3, Column 2. I generate 3,114 events every time, which are equal to the number of misconduct events identified in the baseline regressions. The vertical red line in Figure 4.2 represents the actual coefficient obtained for the variable *social capital* in the model estimated using a linear probability model (coefficient equal to -0.003)¹¹¹. The results show that, on average, the effect of social capital on false misconduct is zero. I observe that the point estimate of my actual result is never reached by the simulated coefficients. Finally, from the distribution of the t-statistics, I observe that in less than 5% of the cases the t-statistic is lower than -1.65 . This result suggests that the likelihood of capturing pure spurious results in the actual estimates is statistically negligible.

¹¹¹ I use a linear probability model because is less computationally demanding than a probit model to run this simulation exercise. As expected the coefficient of the baseline linear probability model is similar to the marginal effect obtained in the baseline test in Table 3.

[Figure 4.2 about here]

4.4.4 Evidence from the social capital level of the CEO

The baseline result of the paper shows that local levels of social capital are negatively related to the probability that a bank is involved in misconduct. This effect persists even after controlling for numerous county-level variables and state fixed effects. Despite this, a potential concern is that the results I find might be driven by local area fixed effects (unobserved heterogeneity) related to bank locations that are not captured by the variables and empirical settings I have used so far. As a consequence, I intend to alleviate concerns regarding local area fixed-effects by using the level of social capital of the CEO and other executives instead of the local level of social capital surrounding bank headquarters. The approach to measuring CEOs' and other executives personal social capital is motivated by the fact that people not only incorporate the values associated with the level of social capital of the place where they live, but also the level of social capital of the place where they grew up (Guiso et al. 2004). The key idea is that when individuals move to a different location, their cultural values (i.e. social capital) travel with them. Using executives cultural traits is consistent with the fact that key decision-makers in a firm create and disseminate their cultural values within the organization (Liu 2016).

I obtain data on the state where a CEO and other executives grew up for banking groups in the S&P 1500 from Scott Yonker¹¹². The state where an executive grew up is identified as the state in which an executive acquires her Social Security number. I calculate the mean value of social capital at the state-level using the level of social capital of each county within a state. As I am not able to measure the level of social capital in the state where an executive grew up at the time she was a child, I rely on the persistence of social capital over time (Guiso et al.

¹¹² In Israelsen and Yonker (2017), Jiang et al. (2018) and Yonker (2017), the authors use data on the state where the executives of a firm grew up. They identify the state of origin for the executives that appear on Execucomp.

2011, 2016) and use the value of social capital in 1997 (which is the first year for which social capital can be computed in a reliable manner using NRCRD data)¹¹³.

It is worth mentioning some features of the data I use for the analysis in Table 4.6. First, 47% of the bank-year observations present a CEO that is from the state where the largest bank within a bank holding company is located. This figure is similar to the one found by Adhikari and Agrawal (2016) for banks and larger than the 30% rate for non-financial firms found in the literature (Yonker 2017). For banks operating in two or fewer states this figure increases to 54% while for banks operating in five or more states the percentage of bank-year observations with a local CEO is 39%. This is consistent with the fact that less geographically diversified banks are more likely to have local CEOs. Second, as the data that identifies the state where a CEO and other executives grew up relies on the sample of banking groups available on Execucomp, I can only compute the level of social capital of the CEO and other executives for bank subsidiaries of banking groups present in the S&P 1500. This reduces the sample to fewer observations than my baseline model (2,326 bank-year observations) and for banks that are of a larger size (total assets roughly equal to \$49 billion on average instead of \$1.2 billion for the baseline sample). Also banks in this group operate on average in more than three states while in the baseline sample the average is 1.3 states. These features may bias my results against finding significant results as I hypothesize that the effect of social capital is more important for less geographically diversified banks.

Despite these odds, in Table 4.6, Column 1, I run a probit regression of a dummy variable taking the value one in the presence of misconduct and zero otherwise on the social capital level of the CEO of the banking group to which each bank belongs (*CEO social capital*) and the set of bank and county-level variables that I use in the baseline test. I find a negative and

¹¹³ NRCRD data provides social capital values for 1990. However, Hasan et al. (2017b) report in the appendix of the paper that they re-estimate the value of social capital in 1990 due to inconsistencies in the number of non-profit organizations reported in the NRCRD data. To avoid biases in the estimation, I use the value of social capital in 1997 as a proxy for the CEO level of social capital.

statistically significant coefficient at the 5% level for the variable *CEO social capital*. In Column 2, I add some CEO-level control variables (CEO tenure, CEO age, CEO delta and CEO total compensation), and in Column 3, I add controls for board size and independence. In both Columns 2 and 3, I find negative and significant coefficients for the variable *CEO social capital*. Alternatively, in Column 4, I follow Liu (2016) and compute the average social capital of all executives with data available on Execucomp for each banking group to have a proxy for the average social capital of bank insiders (*Executive social capital*). I run a probit regression of the misconduct indicator on this variable and the baseline set of control variables. I obtain a coefficient that is negative and statistically significant at 10% level.

These results from the level of social capital of the CEO and other executives alleviate the concerns about geographic fixed effects. They also provide further evidence on how social capital, measured as a personal trait of the firm executives, can introduce cultural values in the bank that reduce the probability that the institution is involved in wrongdoing. This is consistent with the idea that managers are not only influenced by the social environment in which they live, but also from the values related to social capital they acquired during their childhood.

[Table 4.6 about here]

4.4.5 Addressing endogeneity

I study social capital as an external informal institution that exerts exogenous pressure through social mechanisms reducing the incentive to commit misconduct in the banking sector. However, even if *social capital* is exogenously determined, my empirical analysis may be affected by endogeneity due to omitted variables that are correlated with *social capital* and bank misconduct. Therefore, I use a two-stage instrumental regression as an identification strategy to address this concern. As an instrument, I need a variable that is correlated with

social capital and is unrelated to the probability of being involved in a bank misconduct case (exclusion restriction). Regarding the first requirement, I follow Putnam (2007), who provides evidence showing that ethnic homogeneity increases cooperation and social solidarity, which should imply higher social capital. Thus, I compute an index of racial homogeneity (*HHI race*) at county-level using race data from the US Census as in Hasan et al. (2017a, b). This variable is computed every year for every county as a Herfindahl index calculated across the US Census ethnic categories of Hispanic, non-Hispanic black, non-Hispanic white, Asian and a category for other races. Regarding the exclusion restriction, I find no prior theoretical argument or empirical evidence that relates racial homogeneity and bank misconduct. Therefore, I consider that *HHI race* satisfies the two requirements of an instrumental variable. Additionally, I use a second instrumental variable used in Hasan et al. (2017a, b), the natural logarithm of the distance from the bank headquarters to the Canadian border. The reasoning for this instrument is based on Putnam (2001, p48) who states that the distance to the Canadian border is the best single predictor of social capital in American states and discusses that this is because “slavery as a system and the post-slavery reconstruction period were institutionally designed to destroy social capital.”

Table 4.7, Column 1 reports two-stage least square estimates of a linear model. Panel B, Column 1 reports the first stage of the instrumental variable regression. *social capital* is the dependent variable in this model. *HHI race* and the natural logarithm of *distance to Canadian border* are the instruments and main regressors in the first stage. Since the dependent variable in the first stage and the time-varying instrument, *HHI race*, are measured at the county-level, I use within county clustering for the standard errors in the two stages¹¹⁴. Along with the instrumental variables, I include all control variables specified in the baseline model. The coefficient of *HHI race* is, as expected, positive and highly significant in the first stage.

¹¹⁴ I obtain similar significant levels for the coefficients if I cluster standard errors at bank level

Likewise, as I forecasted, *distance to Canadian border* has a negative coefficient and is also significant at the 1% level. I test for the overidentifying restriction and the p-value of the Hansen J test is above 10%. The F-statistic (testing for weak instruments) is well above to the critical cutoff of 10 stated in Stock et al. (2002) and Stock and Yogo (2002). The second-stage regression is based on the baseline model, except that the key independent variable is the fitted value of *social capital* obtained from the first-stage regression. The coefficient on the fitted value of *social capital* is negative and statistically significant at the 5% level. Moreover, the magnitude of the coefficient is comparable to the one of the baseline model. This finding suggests that my previous estimates are not severely biased, mitigating the concern that my results are affected by the omitted variable bias.¹¹⁵

In Column 2, I fit an IV probit model using Maximum Likelihood Estimation. This method of estimation fits a probit model when one (or more) of the regressors are affected by endogeneity. This model is an alternative to two-stage least square estimations when the outcome variable is binary. I observe that my instruments are highly significant in the first-stage and the coefficient of *social capital* in the outcome regression is negative and significant, with a marginal effect close to the one in my baseline regression. The Wald test at the bottom of the table testing whether the correlation coefficient ρ is equal to zero, reports an insignificant statistic¹¹⁶. The result of the IV probit is therefore consistent with the result in Column 1.

[Table 4.7 about here]

¹¹⁵ As an additional piece of evidence, I run a test (Online Appendix, Table A.3.E) that exploits bank headquarters relocations to other counties, resulting in either a decrease or increase in social capital. Using these relocations, I find that social capital-increasing relocations lead to a lower chance that a bank is involved in a misconduct case. This result further corroborates that my results are unlikely to be plagued by endogeneity.

¹¹⁶ A Wald test that fails to reject the null hypothesis of no endogeneity is an indicator of the absence of endogeneity in IV probit models.

4.4.6 Social capital and bank policies

In this section, I explore a possible channel through which social capital can reduce bank misconduct. I examine whether banks located in higher social capital counties tend to have more prudent business models and better internal control systems.

Social capital can be linked to more prudent business models or better internal control due to three reasons. First, many cases of technical misconduct are issued when bank fundamentals indicate increased risks. Thus, the deterrent effect of social capital could reduce technical misconduct by contributing to more prudent (less risky) bank policies. Second, managers in higher social capital areas have higher concerns regarding misconduct and are less resistant to compliance due to higher civic capital and altruism (Hasan et al. 2017a, Jha 2017). As a consequence, the robustness of internal control systems and reporting are, in principle, of a better quality for banks located in higher social capital areas (Jin et al. 2017). Third, bank distress imposes negative externalities on society (Caiazza et al. 2018, Danisewicz et al. 2018, Jackson and Kotlikoff 2018). Presumably, higher social capital makes that bank managers internalize bank externalities. As a consequence, banks in higher social capital areas have more conservative business models.

Table 4.8 provides the results of my analysis. The first outcome I consider is a measure of default risk, namely the Z-score. I complement the analysis with bank measures that are linked to less prudent business models such as earnings volatility, off-balance sheet commitments growth, lending growth, non-performing loans and wholesale funding (e.g. Beltratti and Stulz 2012, Berger et al. 2017, Demirgüç-Kunt and Huizinga 2010, Fahlenbrach et al. 2012, 2017). I also use accounting restatements as a proxy for the strength of the internal control system (Dechow et al. 2010, Doyle et al. 2007).

In Column 1, I show that *social capital* is associated with lower default risk (higher Z-scores). An increase in one standard deviation of *social capital* leads to an increase of 9.3% in

the Z-score. This is consistent with the idea that banks in higher social capital areas have more prudent business models. The rest of coefficients are consistent with this finding. Higher level of social capital is associated with lower earnings volatility (Column 2), lower growth of unused commitments (Column 3), lower loans growth (Column 4), lower non-performing loans as a portion of total loans (Column 5) and lower wholesale funding (Column 6). In Column 7, I show that higher social capital is associated with a lower probability of accounting restatements¹¹⁷. Prior literature indicates that restatements reflect weaknesses of the internal control system (Dechow et al. 2010, Doyle et al. 2007). This result provides evidence that is consistent with better internal control systems for banks located in higher social capital areas which may lead to a lower incidence of non-technical misconduct.

[Table 4.8 about here]

4.4.7 Geographically diversified and less geographically diversified banks

My empirical setting captures the influence of social peers surrounding bank headquarters on the probability of regulatory misconduct. However, the negative relationship between social capital and bank misconduct could significantly attenuate for banks that are geographically diversified as I hypothesize in section 4.2 of the paper. I use information on the number of states in which a bank has branches using data from the FDIC summary of deposits to measure geographical dispersion.

I test Hypothesis 1b in Table 4.9. I modify the baseline regression by adding indicators of geographical diversification and interactions of these indicators with *social capital*. This setting allows me to test whether the effect of the social capital surrounding the headquarters on misconduct attenuates with geographical dispersion. Since testing for the presence of

¹¹⁷ Consistent with previous columns in Table 4.8, I run this regression using OLS. Results are similar if I use a non-linear model such as probit.

interaction effects in non-linear models is problematic (Greene 2010), I use a linear probability model instead of probit. In Column 1, I run a linear probability model of an indicator of misconduct on the social capital variable, a dummy variable (*two to five*) taking the value one if the bank has branches in two or more states and five or less states and zero otherwise; another dummy variable (*six or more*) if the bank has branches in six or more states and zero otherwise and interaction terms of *social capital* and these dummies. I also include the variables that I use in the baseline model, except size squared. I remove this variable as my indicator of geographical dispersion may already be capturing the different effect that a very large size may have on misconduct. The coefficient of the variable *social capital* is statistically significant at the 1% level meaning that for banks operating in one state (the benchmark group in this regression) the negative relationship between social capital and misconduct holds. The coefficient for *social capital*two to five* tests whether there is a different effect of social capital on misconduct for banks with branches in two or more states and five or less states. The coefficient for this interaction term is virtually zero and statistically insignificant. When I perform a Wald test summing *social capital* and this interaction term, I find that the relationship is different from zero (p-value=0.05). This result documents that the effect of *social capital* on bank misconduct for banks operating in two to five states is not different from the effect for banks operating in one single state. The coefficient for the interaction term *social capital*six or more* is positive and statistically insignificant. When I perform a Wald test summing *social capital* and the latter interaction term, I obtain a p-value equal to 0.14. This means that the sum of the two coefficients is, from a statistical perspective, equal to zero. The results in Column 1 document that the effect of social capital on bank misconduct attenuates with geographical dispersion and becomes zero for banks operating in six or more states. This is consistent with hypothesis 1b. Given that more geographically diversified banks may have dispersed decision centers, the social capital

of those areas and the social capital of the headquarters region may not be correlated. I, therefore, find the effect of social capital measured at the level of the headquarters on misconduct for those banks for which it is supposed to be more relevant.

The regressions in Columns 2 and 3 provide further evidence on the fact that the findings in Column 1 is explained by geographical diversification and not by other forces such as banks being publicly held or size. In Column 2, I provide further evidence on whether a difference between publicly and privately held banks exists with respect to the relation between social capital and bank misconduct. The motivation to run this test stems from the fact that publicly held banks may be subject to stricter monitoring from investors (i.e. institutional investors) and the stock-market (i.e. market regulators, analysts) which may reduce the effect of social capital as an external disciplinary force. I define a dummy *publicly held* taking the value one if a bank is public or held by a public bank holding company and zero otherwise. I also define an interaction term *social capital*publicly held*. I use the information provided in call reports and in the bank relationships database available at the National Information Center website¹¹⁸ to identify the banks' top holding company. I then merge the call reports with CRSP using the RSSD-PERMCO file available at the New York Fed website¹¹⁹. I then determine whether a bank (or its bank holding company) is publicly held if I find stock price information on CRSP for each bank-year observation. In line with Column 1, I run a linear probability model. I find a negative and significant coefficient for the variable *social capital*. The interaction term *social capital*publicly held* has a negative and non-significant coefficient. Therefore, this result suggests that with regards to misconduct, social capital plays an external deterrent role for both publicly and privately held banks¹²⁰. In

¹¹⁸ This website is a repository of financial data and institution characteristics collected by the Federal Reserve System. The information is available in the following link
<https://www.ffiec.gov/nicpubweb/nicweb/DataDownload.aspx>

¹¹⁹ https://www.newyorkfed.org/research/banking_research/datasets.html

¹²⁰ I also test whether I observe different effects for National banks (regulated by the OCC) and State banks (regulated by either FDIC or Fed). The result of this test is available in the Online Appendix (Table A.3.D.) and

Column 3, I intend to provide evidence that allows me to rule-out the possibility that the different impact of social capital on bank misconduct for geographically diversified banks found in Column 1 is solely explained by size. In Column 3, I define a dummy variable *larger than 1 bill* taking the value one if a bank has total assets greater than \$1 billion¹²¹ and zero otherwise and create an interaction term *social capital*larger than 1 bill*. I show that the coefficient for social capital remains negative and highly significant and the coefficient for the interaction term is negative and statistically insignificant. The Wald tests for the sum of *social capital* and the interaction terms give p-values lower than 0.10 (not reported). This reinforces the argument that the effect of social capital on the probability of misconduct declines with geographical diversification and not only with size.

In the Online Appendix, I show that the different impact of social capital on misconduct between banks that are geographically dispersed and more regionally focused banks is unlikely to be explained by differences in enforcement exerted by bank regulators. In Table A.3.D, I show that the negative effect of social capital on bank misconduct is significant for both National and State banks (for the whole sample and for the sample of less geographically dispersed banks). This allows me to rule-out the possibility that the results in Table 4.9 are driven by differences in enforcement between supervisors (Agarwal et al. 2014).

[Table 4.9 about here]

4.4.8 Consequences of bank misconduct and the role of social capital

Hypothesis 2 states that in high social capital regions the civic norms and networks are such that there is a greater punishment for deviant behavior. Misconduct is a way banks can

documents a non- statistically significant difference in the impact of social capital on misconduct for these two types of banks.

¹²¹ This threshold is widely used in the banking literature to differentiate between smaller and larger banks in the US (e.g. Berger et al. 2017, Berger and Bouwman 2013)

breach the trust granted by the community in which they operate. I forecast that this effect can be amplified during the aftermath of the financial crisis since during this period the overall level of trust in corporations (and especially in the banking sector) significantly dropped (Lins et al. 2017, Sapienza and Zingales 2012, Servaes and Tamayo 2017). Therefore, revelations of misconduct are expected to lead to worse outcomes for financial institutions during the period after the financial crisis in areas with high social capital levels.

I perform a formal test of Hypothesis 2. For this purpose I use the model described in (6). My outcome variable is $\% \Delta \text{Market share of deposits}$ and my variable of interest in this model is $\text{misconduct} * Z \text{ social capital Mkt}$ that is capturing the incremental effect that social capital may have on the percentage change of deposits market shares upon misconduct revelation.

Table 4.10 shows the results of these tests. In Columns 1 to 3, I explore whether the incremental effect of social capital on percentage changes of deposit market shares once misconduct is revealed occurs in the period before the financial crisis (Column 1), during the financial crisis (Column 2) and/or during the aftermath of the financial crisis (Column 3). For this purpose, I divide my sample into three periods and run OLS regressions for each period using my bank-county sample. Pre-crisis period lasts from 2001 to 2006, crisis period from 2007 to 2010 and post-crisis period from 2011 to 2015. The dummy variable *misconduct* captures the average effect of misconduct revelation on the percentage change deposit market shares. In the three regressions, the coefficients of *misconduct* are significant revealing that, on average, banks lose some market share upon misconduct revelation. However, I am particularly interested in the interaction term $\text{misconduct} * Z \text{ social capital Mkt}$. I observe that in Column 1, the coefficient is positive and non-significant and in Column 2 even if the coefficient turns negative, it is not statistically significant. Only in Column 3 this coefficient is statistically significant (at the 1% level). This is consistent with my hypothesis 2. Banks get punished when misconduct is discovered and social capital has an incremental effect.

However, as expected, this effect is significant only during the aftermath of the financial crisis, when trust in corporations (especially in financial institutions) decreased and social capital exerts a stronger effect on deviant firms. The economic interpretation of the result is as follows: during the aftermath of the crisis, banks lose, on average, 0.42% of their deposit market share once misconduct is revealed. The coefficient of -0.75 for the interaction term implies a decrease of roughly 1.2% in deposits market share upon misconduct revelation when *Z social capital Mkt* is one standard deviation above the mean. This represents, in absolute value, 18.1% of the average growth of deposits market share for a bank in a given county. The addition of the two coefficients (-0.42 + -0.75) is statistically significant (p-value=0.00, not reported).

[Table 4.10 about here]

In Table 4.11, I test whether I observe different consequences for technical and non-technical misconduct events. In Columns 1 to 4, I test whether the effect I identify in this section is different for technical and non-technical misconduct cases. I run OLS regressions on the post-crisis sample similar to the one in Table 4.11, Column 3 but replacing *misconduct* by a dummy variable *technical misconduct* (columns 1 and 3) or *non-technical misconduct* (Columns 2 and 4)¹²². In Columns 1 and 2 I keep all bank-county observations and in Columns 3 and 4 I keep counties that have at least 3 banks present in their territory. The results on technical misconduct show that upon revelation of technical misconduct, banks lose, on average, 3.8% of deposits market share. However, social capital attenuates this effect instead of increasing it.

When I study non-technical misconduct, the coefficient for *non-technical misconduct* shows that, on average, there are not significant changes in bank deposits market share upon

¹²² These variables are defined in Appendix.

revelation of non-technical misconduct¹²³. However, the interaction term *non-technical misconduct * Z social capital mkt* is negative and highly significant. Taken jointly, the coefficient *non-technical misconduct* and the interaction term provide evidence that the revelation of non-technical misconduct leads to a decrease of deposit market share of roughly 1% when *social capital* is one standard deviation above the average (the sum of the two coefficients is statistically significant at the 1% level in both Column 2 and Column 4). Taken together, these results show that the incremental effect of social capital on percentage changes of deposits market share occurs only for cases of non-technical misconduct.

[Table 4.11 about here]

4.5 Conclusions

Using enforcement actions issued by US bank regulators to identify bank wrongdoing, I provide a novel analysis of the impact of social capital, as captured by strength of civic norms and density of social networks, on the probability that a bank is involved in misconduct. The results of the paper indicate that banks headquartered in areas with higher social capital levels are less likely to commit misconduct. This result is consistent with the idea that social capital deters bank practices that may end up in a violation of bank laws and regulations. The effect of social capital surrounding the bank physical location on misconduct is mostly significant for banks that are less geographically dispersed. The latter result is consistent with the fact that more complex and geographically dispersed banks tend to have decision centers that are located in different areas and the cultural values (i.e., social capital) of those areas may not be correlated with each other. Therefore, the impact of social capital measured at the level of the headquarters gets diluted. Another explanation for this result is that less geographically

¹²³ This result is consistent with Delis et al. (2017b) that find that depositors, on average, react less after the issuance of non-technical enforcement actions.

dispersed banks tend to be managed locally and have greater portions of local investors and employees. These stakeholders are likely to be influenced by the ‘same social capital’ and therefore, the probability that subcultures arise is lower in less geographically dispersed banks

Furthermore, I show that once misconduct is revealed, banks are more punished in counties with higher social capital levels. This punishment translates into greater percentages of deposits market share lost in higher social capital areas. This effect is concentrated during the aftermath of the financial crisis and mainly for misconduct cases that are linked to non-technical aspects.

These results provide interesting implications about how social capital helps to cultivate a social environment surrounding bank headquarters that deter undesirable behaviors such as bank misconduct. Specifically, I interpret these findings as indicating that social influences emanating from informal secular norms and networks, constrain opportunistic managerial behaviors associated to misconduct. The findings of the paper also show how social capital, a factor that is considered to boost trust in business relationships, becomes a disciplinary force when there is a breach of trust.

This study contributes to the nascent body of literature that finds that cultural attributes and social norms influence managerial behavior. Whereas prior studies have focused on how internal mechanisms affect misconduct and other managerial behaviors, I rather focus on external mechanisms, such as social norms, that could deter undesired managerial actions. This paper also contributes to the existing literature with new evidence on how cultural factors shape bank-customer relationships in the presence of a reputational shock.

Figure 4.1 Spatial distribution of social capital measure in 2014

This figure presents the spatial distribution for the variable *social capital* in the United States. For brevity, I depict only the variable based on social capital data in 2014. The counties with higher social capital measures are displayed with a darker shade, whereas the counties with lower social capital measures are displayed with a lighter shade.

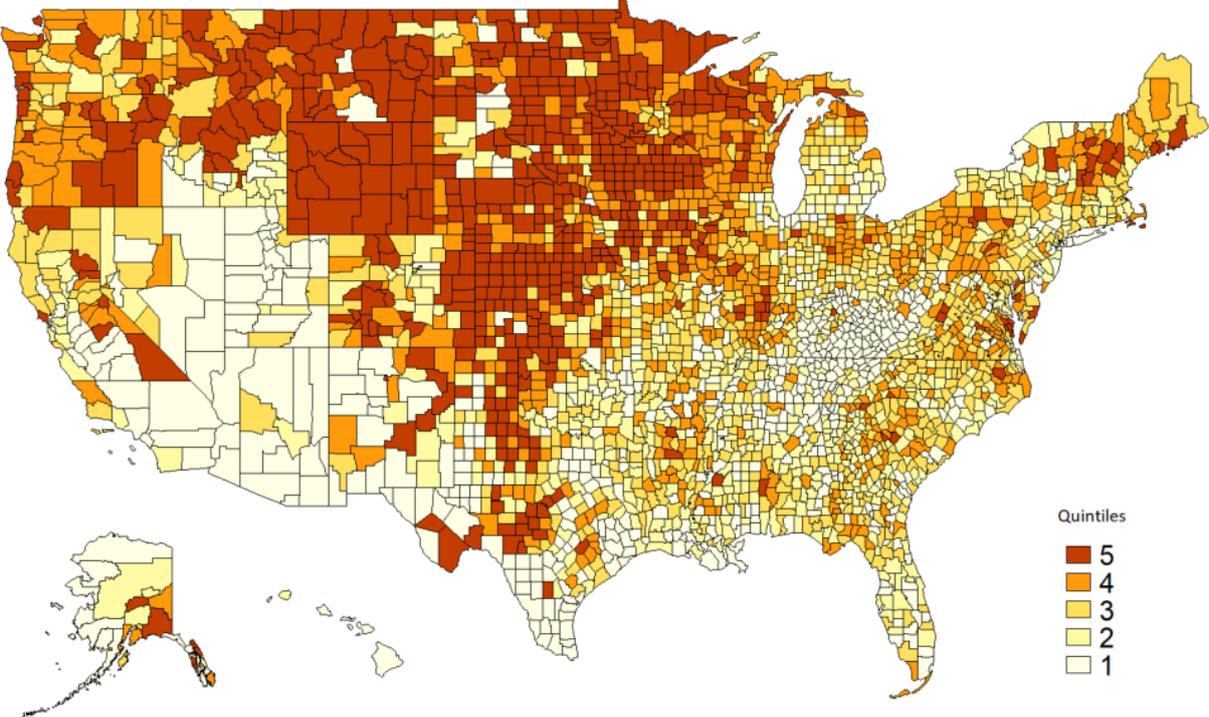


Table 4.1 Summary of misconduct cases

This table reports the bank enforcement actions issued by the three main banking regulators in the US (FDIC, OCC and FRB) each year. Technical enforcement actions refer to those enforcement actions related to violations of capital adequacy and liquidity, asset quality, lending, provisions, and reserves. Non-technical enforcement actions are related to failures of the bank's internal control and audit systems, risk management systems, anti-money laundering violations, violations of consumer protection laws (Federal Trade Commission Act, the Equal Credit Opportunity Act, etc.), breaches of the requirements concerning the competency of the bank personnel and cases related to fraud and insider abuse.

Year	All	Technical (%)	Non-Technical (%)	FDIC	OCC	FRB
2001	95	53%	47%	37	51	7
2002	118	61%	39%	47	64	7
2003	121	49%	51%	53	52	16
2004	127	43%	56%	70	48	9
2005	148	21%	79%	81	61	6
2006	142	24%	76%	86	50	6
2007	125	34%	66%	74	37	14
2008	202	53%	47%	89	87	26
2009	391	76%	24%	221	125	45
2010	577	78%	22%	351	152	74
2011	334	61%	39%	233	77	24
2012	254	50%	50%	186	47	21
2013	184	32%	68%	128	37	19
2014	150	24%	76%	101	36	13
2015	146	14%	86%	94	34	18
Total	3114	53%	47%	1851	958	305

Table 4.2 Descriptive statistics

This table reports descriptive statistics for the main variables of my empirical analysis. Please refer to Appendix 4.1 for variable definitions and details about their computation. For each variable, the mean difference between banks without misconduct and banks with misconduct is calculated. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively.

Variables	N	mean	sd	p25	p50	p75	Misconduct=0 - Misconduct=1
misconduct	101669	0.031	0.172	0	0	0	-
social capital	101669	0.004	1.201	-0.823	-0.133	0.638	0.327***
equity ratio	101669	10.668	3.276	8.493	9.883	11.923	1.031***
allowance loan lease	101669	1.517	0.792	1.044	1.316	1.747	-0.584***
ROA	101669	1.081	1.012	0.673	1.187	1.648	1.191***
liquidity	101669	6.713	6.435	2.841	4.432	7.914	-0.324**
RWA	101669	67.515	13.235	58.988	68.265	76.838	-4.272***
deposit ratio	101669	83.239	7.105	80.123	84.856	88.316	0.195
age (ln)	101669	3.641	0.935	3.178	4.190	4.277	0.181***
size (ln)	101669	11.880	1.265	11.023	11.749	12.564	-0.625***
size sq	101669	142.740	31.444	121.517	138.048	157.845	-16.10***
competition	101669	0.193	0.119	0.111	0.158	0.236	0.015***
BHC	101669	0.796	0.403	1	1	1	-0.016*
publicly held	101669	0.091	0.288	0	0	0	-0.064***
income pc (ln)	101669	10.399	0.285	10.195	10.379	10.579	-0.085***
employment	101669	56.317	13.956	46.137	55.791	64.955	-1.866***
education	101669	21.370	9.981	13.777	18.500	27.200	-2.89***
median age (ln)	101669	2.091	0.112	2.079	2.079	2.197	0.018***
rural	101669	0.414	0.492	0	0	1	0.134***
Other variables used							
Z score (ln)	97744	3.865	1.049	3.249	3.910	4.540	1.005***
regulatory capital	101645	17.118	7.321	12.372	14.961	19.189	2.834***
loans growth	101669	5.340	15.202	-3.138	3.100	10.362	9.334***
unused comm growth	88364	16.560	54.586	-11.182	7.120	29.550	18.560***
sd(ROA) (ln)	99871	-1.424	1.003	-2.067	-1.460	-0.817	-0.985***
NPL ratio	90251	1.551	2.048	0.263	0.836	1.973	-2.907***
wholesale funding	101669	19.838	10.317	12.247	18.443	25.845	-3.103***
accounting restatements	101669	0.045	0.207	0	0	0	-0.014***
religion	100402	61.739	17.508	49.730	60.079	71.766	3.761***
less severe	101669	0.029	0.167	0	0	0	-0.036***
organ donation	68330	0.375	0.361	0.195	0.276	0.433	0.059***
Executives & board-level variables							
CEO social capital	2326	-0.121	0.761	-0.689	-0.193	0.347	0.209***
Executives social capital	2326	-0.061	0.601	-0.498	-0.193	0.258	0.159***
CEO age (ln)	2260	4.032	0.108	3.970	4.025	4.094	-0.00349
CEO tenure (ln)	1907	1.906	0.879	1.386	2.079	2.565	0.244***
total compensation (ln)	2300	7.841	1.186	6.928	7.691	8.710	-0.542***
CEO delta (ln)	2174	5.265	1.678	4.060	5.204	6.540	-0.267**
board size (ln)	5802	2.664	0.222	2.565	2.708	2.803	-0.078***
board independence	5802	69.854	16.782	58.333	72.727	81.818	3.187**
Instrumental variables							
HHI race	101669	0.694	0.199	0.518	0.725	0.879	-
distance to Canadian border (ln)	101359	6.441	0.836	5.985	6.568	7.058	-
Bank-county-level variables							
%ΔMarket share of deposits	321332	6.512	32.736	-5.631	0.331	7.793	-
Z social capital mkt	321332	0	1	-0.665	-0.118	0.503	-
county competition	321332	0.215	0.135	0.128	0.178	0.258	-

Table 4.3 Baseline regressions

This table reports the results of probit regressions of a dummy variable taking the value one in the presence of misconduct and zero otherwise on a social capital variable and control variables. I represent the marginal effect of the variable *social capital* in square brackets. All independent variables are lagged one period. Fixed effects and the cluster level of standard errors are specified on the table. Standard errors are robust to heteroscedasticity. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1.

	(1) No control variables	(2) Baseline	(3) Cluster by county
Dependent var. :	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)
social capital	-0.096***[-0.007] (0.009)	-0.056***[-0.003] (0.012)	-0.056***[-0.003] (0.013)
equity ratio		-0.032*** (0.004)	-0.032*** (0.004)
allowance loan lease		0.157*** (0.011)	0.157*** (0.010)
ROA		-0.268*** (0.009)	-0.268*** (0.011)
liquidity		0.000 (0.002)	0.000 (0.002)
RWA		0.008*** (0.001)	0.008*** (0.001)
deposit ratio		0.001 (0.002)	0.001 (0.002)
age		0.042*** (0.011)	0.042*** (0.011)
size		-0.505*** (0.101)	-0.505*** (0.103)
size sq		0.025*** (0.004)	0.025*** (0.004)
competition		0.105 (0.098)	0.105 (0.106)
BHC		-0.019 (0.026)	-0.019 (0.025)
publicly held		-0.066* (0.035)	-0.066* (0.036)
income pc		-0.119* (0.071)	-0.119 (0.074)
employment		0.002 (0.001)	0.002 (0.001)
education		0.002 (0.002)	0.002 (0.002)
median age		-0.052 (0.111)	-0.052 (0.116)
rural		0.009 (0.027)	0.009 (0.026)
Constant	-2.071*** (0.043)	1.097 (0.924)	1.097 (0.956)
Regulator FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Cluster	Bank	Bank	County
Pseudo-R2	0.054	0.168	0.168
Observations	101669	101669	101669

Table 4.4 Bivariate probit with partial observability and alternative settings

This table reports the results of the estimated relations between social capital and committing misconduct (M=1) in Column 1 and detection given misconduct (D=1 | M=1) in Column 2. In Column 3, the dependent variable is *technical misconduct* taking the value one if a bank receives a technical enforcement action during the sample period and zero otherwise. The control variables are measured at the mean value for each bank. In Column 4, the dependent variable is *non-technical misconduct* taking the value one in the presence of non-technical misconduct over the whole sample period and zero otherwise. The independent variables are measured at their mean value for each bank. The main dependent variable (*social capital*) is lagged 2 and 3 years in Columns 5 and 6, respectively. The rest of independent variables remain lagged 1 year in these columns. Standard errors (in parentheses) are clustered by banks and robust to heteroscedasticity. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. I represent the marginal effect of the variable *social capital* in square brackets. Definitions for all variables are provided in Appendix 4.1.

	(1)	(2)	(3)	(4)	(5)	(6)
	Bivariate probit with partial observability		Technical EA	Non-technical EA	2 years lag	3 years lag
Dependent var. :	P(M=1)	P(D=1 M=1)	P(Technical Misconduct=1)	P(Non-technical Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)
social capital	-0.084***[-0.002]	0.047[0.001]	-0.098***[-0.016]	-0.085***[-0.016]	-0.048***[-0.002]	-0.044***[-0.002]
	(0.030)	(0.034)	(0.023)	(0.022)	(0.012)	(0.012)
Z score		-0.116*** (0.014)				
regulatory capital		-0.003 (0.004)				
equity ratio	-0.019*** (0.007)		-0.104*** (0.011)	-0.000 (0.008)	-0.030*** (0.004)	-0.029*** (0.004)
ROA	-0.242*** (0.021)		-0.729*** (0.036)	-0.229*** (0.027)	-0.274*** (0.009)	-0.275*** (0.010)
allowance loan lease	0.103*** (0.032)	0.031 (0.031)	0.541*** (0.036)	0.198*** (0.029)	0.150*** (0.012)	0.148*** (0.012)
liquidity	-0.001 (0.004)	0.001 (0.004)	0.007 (0.005)	0.001 (0.004)	-0.000 (0.002)	-0.001 (0.002)
RWA	0.003 (0.002)	0.004 (0.002)	0.031*** (0.002)	0.005*** (0.002)	0.008*** (0.001)	0.008*** (0.001)
deposit ratio	-0.004 (0.004)	0.005 (0.004)	-0.008** (0.004)	0.006* (0.003)	0.001 (0.002)	0.001 (0.002)
age	0.025 (0.039)	0.002 (0.042)	0.225*** (0.024)	0.195*** (0.022)	0.021* (0.012)	0.009 (0.013)
size	0.254 (0.384)	-2.875*** (0.310)	1.139*** (0.236)	-0.431** (0.180)	-0.518*** (0.101)	-0.517*** (0.103)
size sq	0.016 (0.016)	0.094*** (0.011)	-0.046*** (0.009)	0.028*** (0.007)	0.026*** (0.004)	0.026*** (0.004)
competition	0.139 (0.250)	-0.084 (0.283)	0.187 (0.191)	0.029 (0.183)	0.112 (0.098)	0.103 (0.099)
BHC	-0.092 (0.072)	0.076 (0.081)	0.105* (0.058)	0.132** (0.052)	-0.025 (0.026)	-0.030 (0.027)
publicly held	-0.201* (0.113)	0.085 (0.087)	-0.348*** (0.079)	-0.162** (0.066)	-0.068* (0.035)	-0.064* (0.036)
income pc	0.213 (0.216)	-0.248 (0.229)	-0.052 (0.145)	0.266* (0.141)	-0.121* (0.072)	-0.133* (0.072)
employment	-0.001 (0.003)	0.002 (0.003)	-0.000 (0.002)	0.004** (0.002)	0.001 (0.001)	0.001 (0.001)
education	-0.001 (0.006)	0.000 (0.006)	0.004 (0.004)	-0.007* (0.004)	0.002 (0.002)	0.002 (0.002)
median age	0.129 (0.317)	-0.199 (0.339)	0.077 (0.247)	-0.221 (0.236)	-0.084 (0.112)	-0.100 (0.114)
rural	-0.063 (0.059)	0.094 (0.066)	0.001 (0.051)	0.028 (0.051)	0.010 (0.026)	0.012 (0.026)
Constant	-7.457** (2.992)	22.918*** (2.932)	-8.863*** (1.904)	-4.268** (1.673)	1.387 (0.931)	1.569* (0.943)
athrho	-0.994*** (0.150)	-0.994*** (0.150)	-	-	-	-
Regulator dummies	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	-	-	Yes	Yes
Cluster	Bank	Bank	Bank	Bank	Bank	Bank
Log-Likelihood	-10930.68	-10930.68	-2920.986	-3188.799	-11407.58	-11167.98
Pseudo-R2	-	-	0.250	0.083	0.168	0.168
Observations	97720	97720	8952	8952	99740	97850

Table 4.5 Robustness tests

This table reports the results of probit regressions of a dummy variable taking the value one in the presence of misconduct and zero otherwise on a social capital variable and control variables. In Column 1, I run a linear probability model with bank fixed effects sampling in 2001, 2005, 2009 and 2014. In Column 2, I add state fixed effects to the baseline model. In Column 3, I remove rural banks from the sample. In Column 4, I use the social capital variable without applying linear interpolation. In Column 5, I eliminate the discrepancies in the measurement of the non-profit organization variable (NCCS) in 1997 following Hasan et al (2017b) and compute a social capital score using a modified value of the NCCS variable. In Column 6, I use the variable organ donation (measured as the number of organ donors divided by county population per 10,000) obtained from United Network for Organ Sharing (UNOS) as a proxy for social capital. In Column 7, I add the religion variable as a regressor. In Column 8, I add a proxy for less severe enforcement actions as a regressor. Fixed effects and the cluster level of standard errors are specified in the table. Standard errors are robust to heteroscedasticity. All independent variables are lagged one period. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. I represent the marginal effect of the variable *social capital* and *organ donation* in square brackets. Definitions for all variables are provided in Appendix 4.1. Constant terms are included in the regression although they are not shown in the table for brevity.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Linear Bank FE	State FE	No-rural	SC without interpolation	SC Modified	Organ donation	Religion	Less severe EA
Dependent var. :	P(Misconduct=1)							
social capital	-0.006* (0.003)	-0.038**[-0.002] (0.015)	-0.056***[-0.003] (0.015)	-0.049***[-0.002] (0.011)	-0.075***[-0.003] (0.016)		-0.055***[-0.002] (0.012)	-0.057***[-0.003] (0.012)
organ donation						-0.130***[-0.007] (0.043)		
religion							-0.000 (0.001)	
less severe								0.209*** (0.042)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	No	No	No	No	No	No
Bank FE	Yes	No	No	No	No	No	No	No
Cluster	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj. R2	0.054							
Pseudo-R2		0.173	0.155	0.167	0.168	0.159	0.168	0.168
Observations	27302	101669	65970	101669	101632	68502	100402	101669

Figure 4.2 Placebo Test

This histogram reports the distribution of the coefficients of the variable social capital obtained from 1000 linear probability model regressions in which the dependent variable is a dummy variable randomly simulated, the main independent variable is the measure of social capital and the rest of control variables, fixed effects and clustering of standard errors are the same as those that I use in Table 4.3, Column 2. I generate 3,114 events every time, which are equal to the number of misconduct events in the baseline regressions. The vertical red line in the graph represents the actual coefficient obtained for the variable *social capital* in the baseline model estimated using a linear probability model (coefficient equal to -0.003). In less than 5% of the cases the t-statistic is smaller than -1.65.

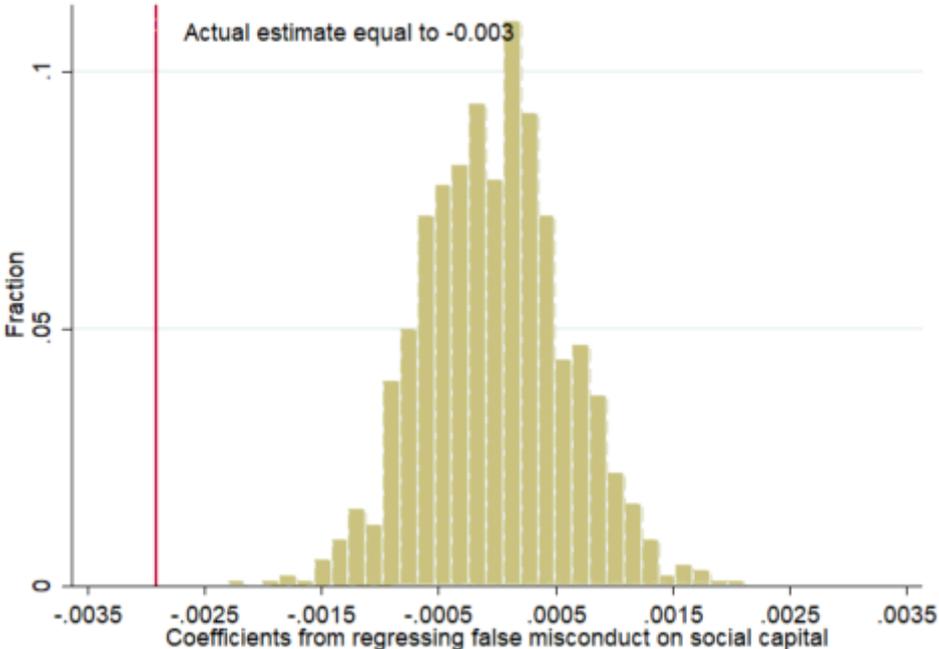


Table 4.6 Evidence from the social capital level of the state where the CEO and other executives grew up

This table reports the results of probit regressions of a dummy variable taking the value one in the presence of misconduct and zero otherwise on a social capital variable and control variables. In Column 1, the main dependent variable is *CEO social capital* which is the average level of the social capital measure in 1997 in the state where the CEO of a banking group grew up. In Column 2, I add some CEO-level control variables. In Column 3, I add board-level variables. In Column 4, I compute a social capital measure for all executives of the bank reported on Execucomp (usually the top 5 earners). The sample I use in these tests is determined by the availability of the state where an executive grew up in the data provided by Scott Yonker used in Israelsen and Yonker (2017), Jiang et al. (2018) and Yonker (2017). This sample only includes bank subsidiaries that belong to banking groups present in the S&P 1500. In all columns, I use the same control variables that I use in the baseline model in Table 4.3 except *BHC* and *publicly held* because all banks in this smaller sample are held by publicly traded bank holding companies. All independent variables are lagged one period. Fixed effects and the cluster level of standard errors are specified in the table. Standard errors are robust to heteroscedasticity. All independent variables are lagged one period. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. I represent the marginal effect of the variable *social capital* in square brackets. Definitions for all variables are provided in Appendix 4.1.

	(1) CEO SC	(2) CEO variables	(3) Board variables	(4) Executives SC
Dependent var. :	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)
CEO social capital	-0.182**[-0.012] (0.078)	-0.279***[-0.011] (0.089)	-0.194**[-0.014] (0.078)	
Executives social capital				-0.187*[-0.013] (0.107)
equity ratio	-0.005 (0.017)	-0.017 (0.018)	-0.006 (0.017)	-0.006 (0.017)
allowance loan lease	0.106* (0.058)	0.068 (0.064)	0.112* (0.060)	0.107* (0.060)
ROA	-0.051 (0.042)	-0.069 (0.053)	-0.040 (0.043)	-0.050 (0.043)
liquidity	-0.001 (0.007)	-0.008 (0.008)	-0.002 (0.007)	-0.001 (0.007)
RWA	0.003 (0.004)	0.009** (0.004)	0.002 (0.004)	0.003 (0.004)
deposit ratio	-0.004 (0.006)	-0.006 (0.007)	-0.004 (0.006)	-0.003 (0.006)
age	0.070 (0.079)	0.136 (0.087)	0.072 (0.077)	0.072 (0.081)
size	-0.837 (0.788)	-0.303 (0.906)	-0.783 (0.780)	-0.897 (0.801)
size sq	0.039 (0.027)	0.018 (0.032)	0.037 (0.027)	0.041 (0.028)
competition	-0.220 (0.516)	-0.558 (0.553)	-0.344 (0.521)	-0.144 (0.513)
income pc	-0.545 (0.466)	-1.225** (0.506)	-0.503 (0.475)	-0.546 (0.470)
employment	0.021*** (0.005)	0.026*** (0.006)	0.020*** (0.005)	0.021*** (0.005)
education	-0.002 (0.012)	0.002 (0.013)	-0.002 (0.012)	-0.001 (0.012)
median age	0.543 (0.618)	0.590 (0.777)	0.354 (0.626)	0.565 (0.621)
rural	0.208 (0.199)	0.171 (0.207)	0.230 (0.199)	0.188 (0.203)
CEO age		0.070 (0.667)		
CEO tenure		-0.233*** (0.088)		
total compensation		0.050 (0.087)		
CEO delta		0.101* (0.059)		
board size			-0.338 (0.487)	
board independence			0.594 (0.411)	
Constant	3.941 (7.233)	8.343 (8.323)	3.279 (7.324)	4.321 (7.379)
Regulator FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Cluster	Bank	Bank	Bank	Bank
Pseudo-R2	0.218	0.244	0.224	0.215
Observations	2326	1687	2238	2326

Table 4.7 Endogeneity

This table reports the estimates from IV regressions explaining the likelihood of a bank being involved in misconduct. Panel A shows the results of the second stage regressions of misconduct on social capital and a set of bank and county-level control variables. In Column 1, I report the results of a 2SLS regression. In Column 2, I report the results from an IV probit regression using Maximum Likelihood Estimation. I use the variables *HHI race* and the natural logarithm of *distance to Canadian border* as instruments in both models. Panel B shows the first stage regressions. In both columns the dependent variable is *social capital*. Fixed effects and the cluster level of standard errors are specified on the table. Standard errors are robust to heteroscedasticity and clustered as indicated in the table. *social capital*, *HHI race* and *distance to Canadian border* are lagged one period. Control variables are lagged one period. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1. I represent the marginal effect of the variable *social capital* in square brackets. Constant terms are included in the regression although they are not shown in the table for brevity.

	(1)	(2)
<i>Panel A : Second Stage</i>	2SLS	IVprobit
Dependent var. :	Misconduct	P(Misconduct=1)
social capital (instrumented)	-0.004**	-0.062**[-0.003]
	(0.002)	(0.029)
Controls	Yes	Yes
Regulator FE	Yes	Yes
Time FE	Yes	Yes
Cluster	County	County
Observations	101359	101359
<i>Panel B : First Stage</i>		
Dependent var. :	social capital	social capital
HHI race	2.145***	2.145***
	(0.116)	(0.116)
distance to Canadian border (ln)	-0.074***	-0.074***
	(0.021)	(0.021)
Controls	Yes	Yes
Regulator FE	Yes	Yes
Time FE	Yes	Yes
Cluster	County	County
Adj-R2	0.580	
Observations	101359	101359
F test of weak instruments	255.53	
p -Value of Hansen test	0.37	
Wald test of $\rho=0$ (p-value)		0.840

Table 4.8 Bank policies

This table reports the results of OLS regressions explaining several bank level variables. In Column 1 the dependent variable is the natural logarithm of the Zscore, in Column 2 the standard deviation of the Return on Assets, in Column 3 the annual growth of unused commitments, in Column 4 the annual growth of gross loans, in Column 5 the ratio of Non-performing loans over total net loans, in Column 6 the ratio of wholesale funding over total assets and in column 7 an indicator variable taking the value one if the bank restated its accounts during the year and zero otherwise. Fixed effects and the cluster level of standard errors are specified on the table. Standard errors are robust to heteroscedasticity. All independent variables are lagged one period. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent var. :	Zscore	sd(ROA)	Gr. unused commitments	Gr. loans	NPL	Wholesale funding	Accounting Restatements
social capital	0.090*** (0.006)	-0.088*** (0.006)	-1.336*** (0.212)	-0.388*** (0.075)	-0.080*** (0.013)	-1.442*** (0.079)	-0.005*** (0.001)
equity ratio	0.052*** (0.002)	0.020*** (0.002)	0.693*** (0.079)	0.285*** (0.030)	0.002 (0.004)	-0.953*** (0.024)	0.001* (0.000)
allowance loan lease	-0.255*** (0.007)	0.248*** (0.007)	-2.602*** (0.324)	-2.601*** (0.103)	0.703*** (0.021)	0.090 (0.100)	-0.000 (0.001)
ROA	0.325*** (0.006)	-0.244*** (0.006)	0.252 (0.263)	0.706*** (0.098)	-0.528*** (0.014)	-0.375*** (0.074)	-0.006*** (0.001)
liquidity	-0.002** (0.001)	0.002*** (0.001)	-0.007 (0.040)	0.036*** (0.012)	-0.004** (0.002)	-0.100*** (0.012)	0.000 (0.000)
RWA	-0.011*** (0.000)	0.011*** (0.000)	-0.185*** (0.017)	0.030*** (0.006)	0.015*** (0.001)	0.084*** (0.007)	0.000 (0.000)
deposit ratio	0.004*** (0.001)	-0.004*** (0.001)	-0.049 (0.030)	0.020* (0.011)	-0.002 (0.002)	-0.763*** (0.011)	-0.000 (0.000)
age	0.110*** (0.007)	-0.108*** (0.007)	-2.776*** (0.224)	-3.586*** (0.098)	0.119*** (0.015)	-1.824*** (0.088)	0.006*** (0.001)
size	0.476*** (0.060)	-0.479*** (0.060)	-22.073*** (2.154)	-3.254*** (0.718)	0.480*** (0.124)	5.027*** (0.837)	-0.064*** (0.012)
size sq	-0.018*** (0.002)	0.018*** (0.002)	0.800*** (0.086)	0.140*** (0.029)	-0.015*** (0.005)	-0.209*** (0.034)	0.003*** (0.000)
competition	-0.174*** (0.050)	0.193*** (0.049)	5.774*** (1.763)	3.073*** (0.590)	-0.100 (0.107)	5.809*** (0.644)	0.014 (0.009)
BHC	-0.051*** (0.014)	0.044*** (0.014)	0.045 (0.529)	0.758*** (0.181)	-0.027 (0.033)	-0.700*** (0.195)	-0.016*** (0.003)
publicly held	-0.093*** (0.021)	0.091*** (0.021)	1.681*** (0.636)	1.315*** (0.288)	-0.112*** (0.041)	-0.222 (0.266)	0.001 (0.003)
income pc	-0.053 (0.038)	0.053 (0.038)	2.219* (1.245)	4.719*** (0.480)	-0.565*** (0.085)	0.335 (0.491)	0.001 (0.007)
employment	-0.001** (0.001)	0.001** (0.001)	0.012 (0.019)	0.011 (0.007)	-0.005*** (0.001)	0.036*** (0.008)	0.000*** (0.000)
education	-0.005*** (0.001)	0.005*** (0.001)	-0.001 (0.031)	-0.043*** (0.012)	0.017*** (0.002)	-0.020 (0.012)	-0.000 (0.000)
median age	0.048 (0.063)	-0.073 (0.062)	-1.675 (2.132)	-3.463*** (0.755)	0.550*** (0.131)	-1.083 (0.776)	0.003 (0.012)
rural	0.031** (0.014)	-0.028** (0.014)	0.446 (0.445)	0.236 (0.165)	-0.033 (0.029)	1.192*** (0.181)	0.001 (0.003)
Constant	0.896* (0.527)	0.736 (0.524)	173.806*** (17.721)	-7.115 (6.406)	0.718 (1.122)	59.549*** (6.924)	0.354*** (0.096)
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj. R2	0.309	0.244	0.040	0.123	0.283	0.396	0.015
Observations	99764	99871	88364	101669	90251	101669	101669

Table 4.9 Geographically and less geographically diversified banks

This table reports the results of linear probability model regressions of a dummy variable taking the value one when a bank is involved in a misconduct case and zero otherwise on a social capital variable, interaction terms of the social capital variables and dummy variables indicating whether the bank is present in two to five states (*two to five*), six or more states (*six or more*), an indicator of whether the bank is publicly held (*publicly held*) or larger than \$1billion (*larger than 1bill*) and a set of control variables. In Column 1, the variable *4 or more states* takes the value one if a bank has branches in 4 or more states and zero otherwise. In Column 2, I restrict the sample to banks that are present in five or less states. The variable *publicly held* takes the value one if the bank is public or held by a public BHC and zero otherwise. In Column 3, I restrict the sample to banks that are present in five or less states. The variable *larger than 1 bill* takes the value one if the bank has total assets higher than \$1billion and zero otherwise. Fixed effects and the cluster level of standard errors are specified on the table. Standard errors are robust to heteroscedasticity. Control variables are those that I use in the baseline regressions except the variable *size sq*. Control variables are lagged one period. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1. Constant terms are included in the regression although they are not shown in the table for brevity.

	(1) Geographical dispersion	(2) Publicly held	(3) Larger than 1 bill
Sample :	All banks	Banks present in 5 or less states	Banks present in 5 or less states
Regression method	OLS	OLS	OLS
Dependent var. :	Misconduct	Misconduct	Misconduct
social capital	-0.006*** (0.001)	-0.003*** (0.001)	-0.003*** (0.001)
two to five	0.017*** (0.003)		
six or more	0.152*** (0.026)		
social capital *two to five	0.000 (0.003)		
social capital * six or more	0.034 (0.027)		
publicly held		-0.002 (0.003)	
social capital*publicly held		-0.000 (0.003)	
Larger than 1bill			0.005 (0.004)
social capital*larger than 1bill			-0.003 (0.004)
Controls	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Cluster	Bank	Bank	Bank
Adj.-R2	0.066	0.065	0.065
Observations	101669	101163	101163
p-val social capital+ social capital *two to five=0	0.05		
p-val social capital+ social capital * six or more=0	0.14		

Table 4.10 Consequences of bank misconduct and the role of social capital

This table reports the results of OLS regressions of a variable that measures the annual percentage change of the market share of deposits in each county on a misconduct variable, a standardized measure of social capital measured in each market (i.e. county in which the bank has a branch), the interaction of both and a subset of bank level and county-level control variables. The unit of observation is the county-bank level. In Column 1, I use the pre-crisis period sample (2001-2006), in Column 2, I use a crisis period sample (2007-2010) and in Column 3, I use a post-crisis period sample (2011-2015). Fixed effects and the cluster level of standard errors are specified in the table. Standard errors are robust to heteroscedasticity. All independent variables are lagged one period. County-level variables are measured for each county in which a bank is present. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. All variables are defined in Appendix 4.1.

	(1)	(2)	(3)
Sample :	Pre-crisis	Crisis	Post-crisis
Dependent var. :	%ΔMarket share of deposits		
misconduct	-2.023*** (0.525)	-3.855*** (0.516)	-0.424* (0.229)
Z social capital mkt	-1.625*** (0.183)	-0.964*** (0.227)	-0.398** (0.171)
misconduct * Z social capital mkt	0.466 (0.505)	-0.402 (0.419)	-0.750*** (0.223)
equity ratio	-0.127** (0.052)	0.002 (0.060)	-0.299*** (0.043)
allowance loan lease	-3.181*** (0.193)	-2.010*** (0.223)	-2.039*** (0.133)
ROA	-1.388*** (0.186)	-0.173 (0.156)	2.131*** (0.118)
liquidity	-0.118*** (0.029)	0.040 (0.035)	0.031* (0.016)
RWA	0.091*** (0.012)	0.092*** (0.015)	-0.002 (0.009)
deposit ratio	-0.000 (0.019)	0.055** (0.025)	-0.068*** (0.021)
age	-3.024*** (0.206)	-3.962*** (0.255)	-1.981*** (0.173)
size	0.094 (0.124)	0.681*** (0.152)	0.297*** (0.090)
BHC	2.944*** (0.396)	0.653 (0.531)	0.585* (0.338)
publicly held	-2.726*** (0.450)	-3.333*** (0.505)	-1.741*** (0.327)
county competition	2.313** (1.145)	0.951 (1.462)	0.303 (0.900)
income pc	5.824*** (1.092)	7.703*** (1.148)	-0.861 (0.736)
employment	0.006 (0.014)	-0.042*** (0.016)	-0.001 (0.010)
education	0.180*** (0.029)	0.236*** (0.031)	0.134*** (0.015)
median age	-4.935*** (1.541)	-4.890*** (1.781)	-2.199** (0.994)
rural	2.062*** (0.331)	2.107*** (0.399)	0.981*** (0.254)
Constant	-36.649*** (10.559)	-61.231*** (11.650)	28.922*** (7.811)
Regulator FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Cluster	Bank-county	Bank-county	Bank-county
Adj. R2	0.029	0.030	0.021
Observations	118209	85668	117483

Table 4.11 Consequences of bank misconduct and the role of social capital: heterogeneous effects

This table reports the results of the OLS regressions of a variable that measures the annual percentage change of the market share of deposits in each county on a misconduct variable, a standardized measure of social capital, the interaction of both and a set of bank and county-level control variables. The unit of observation is the county-bank level. In all columns, I use a post-crisis period sample (2011-2015). In Columns 1 and 2, I use the whole sample of county-banks and in Columns 3 and 4 I use the sample of county-banks for those counties that have at least two different banking institutions present in the county. Fixed effects and the cluster level of standard errors are specified on the table. Standard errors are robust to heteroscedasticity. All independent variables are lagged one period. County-level variables are measured for each county in which a bank is present. The control variables included in regressions are those that I use in Table 4.10. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1. Constant terms are included in the regression although they are not shown in the table for brevity.

	(1) Technical misconduct	(2) Non-technical misconduct	(3) Technical misconduct	(4) Non-technical misconduct
Dependent var. :	%ΔMarket share of deposits			
Sample :	Post-crisis: All banks	Post-crisis: All banks	Post-crisis: Counties with >2 banks	Post-crisis: Counties with >2 banks
	OLS	OLS	OLS	OLS
Z social capital mkt	-0.598*** (0.159)	-0.350** (0.170)	-0.641*** (0.167)	-0.379** (0.181)
technical misconduct	-3.824*** (0.436)		-3.826*** (0.446)	
non-technical misconduct		0.083 (0.245)		0.116 (0.250)
technical misconduct * Z social capital mkt	1.168*** (0.417)		1.261*** (0.443)	
non-technical misconduct * Z social capital mkt		-1.027*** (0.233)		-1.064*** (0.249)
Controls	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Cluster	Bank-county	Bank-county	Bank-county	Bank-county
Adj. R2	0.022	0.022	0.022	0.022
Observations	117483	117483	114378	114378

Appendix 4.1 Variable definitions

<u>Variables</u>	<u>Definitions</u>	<u>Source</u>
<i>social capital</i>	The first principal component based on data from the Northeast Regional Center for Rural Development (NRCRD) at Pennsylvania State University. I estimate the level of social capital in US counties in the years 1997, 2005, 2009 and 2014. The measure of social capital I use is based on Rupasingha and Goetz (2008) who use a principal component analysis (PCA) to construct a county-level index of social capital in the United States. The variables used for the computation of the social capital index are the number of non-profit organizations without including those with an international approach divided by population per 10,000 (NCCS), the number of social and civic associations divided by population per 1,000 (ASSN), the voter turnout in presidential elections (PVOTE) and the census response rate (RESPN). In line with the theoretical development, NCCS and ASSN are measures of networks and capture the density of these networks at county-level. PVOTE and RESPN are measures of social norms. Higher values of these factors mean higher social capital levels. Because the social capital index is solely computed in the years 1997, 2005, 2009 and 2014, and consistent with other papers using county-level variables that are not measured every year (Hasan et al. 2017a, Hilary and Hui 2009, Jha and Cox 2015), I use linear interpolation to fill-in the data for the periods 2000-2004, 2006-2008, 2010-2013.	NRCRD (Northeast Regional Center for Rural Development)
<i>equity ratio</i>	The ratio of total equity divided by total assets (in percentage)	Call reports
<i>allowance loan lease</i>	The loan and lease allowance scaled by total loans (in percentage)	Call reports
<i>ROA</i>	The ratio of the pre-tax income over total assets (in percentage)	Call reports
<i>liquidity</i>	The ratio of cash and cash equivalents divided by total assets (in percentage)	Call reports
<i>RWA</i>	The ratio of risk weighted assets over total assets (in percentage)	Call reports
<i>deposit ratio</i>	The ratio of total deposits over total assets (in percentage)	Call reports
<i>age</i>	The natural logarithm of the number of years since the bank is an FDIC insured institution	Call reports
<i>size</i>	The natural logarithm of total assets	Call reports
<i>size sq</i>	The variable <i>size</i> squared	Call reports
<i>BHC</i>	A binary variable taking the value one if the bank is held by a bank holding company and zero otherwise	Call reports
<i>publicly held</i>	A binary variable taking the value one if the bank is publicly held or held by a bank holding company that is publicly held and zero otherwise	Call reports
<i>competition</i>	The bank-level Herfindahl-Hirschman Index (HHI) of deposit concentration for the local markets in which the bank is present. I define the local market as the Metropolitan Statistical Area (MSA) or non-MSA county in which the bank branches are located. I weight the local market measure of competition by the portion of bank total deposits that each bank has in each local market to obtain a bank-level variable	FDIC summary of deposits
<i>income pc</i>	The natural logarithm of the income per capita measured at county-level	Bureau of Economic Analysis
<i>rural</i>	Dummy variable taking the value one if the county is designed as a rural county and zero otherwise	US Census
<i>median age</i>	Natural logarithm of the median age group per county according to the age groups provided by the US Census	US Census
<i>employment</i>	The total number of jobs in the county divided by total population (in percentage)	Bureau of Economic Analysis
<i>education</i>	Number of people older than 25 years old that have a bachelor degree or higher divided by total population in the county (in percentage)	US Census
<i>Z score</i>	The natural logarithm of the Z score. The Z score is computed as the average mean of the equity ratio plus the mean of the ROA divided by the standard deviation of the ROA. The mean values and standard deviation are computed over a three years period	Call reports

<i>regulatory capital</i>	Tier 1 capital divided by risk-weighted assets (in percentage)	Call reports
<i>loans growth</i>	The year on year growth of gross loans (in percentage)	Call reports
<i>unused comm growth</i>	The ratio of unused commitments divided by total assets (in percentage). Unused commitments are measured using the following call reports variables: RCFD3814 + RCFD3816 + RCFD3817 + RCFD3818 + RCFD6550 + RCFD3411	Call reports
<i>sd(ROA)</i>	The natural logarithm of the standard deviation of the ROA computed over a three years period	Call reports
<i>NPL ratio</i>	The ratio of loans 90 days or more past due but still accruing interest plus nonaccrual loans divided by total loans (in percentage)	Call reports
<i>wholesale funding</i>	The ratio of uninsured deposit and non-capital funding over total assets (in percentage). Wholesale funds (also known as managed liabilities in the Federal Reserve Bulletin) are the sum of large time deposits, deposits booked in foreign offices, subordinated debt and debentures, gross federal funds purchased, repos, and other borrowed money	Call reports
<i>Accounting Restatements</i>	This indicator variable equals 1 if the call report variable RIAD B507 (Restatements due to corrections of material accounting errors and changes in accounting principles) is either positive or negative for the bank-year and 0 otherwise.	Call reports
<i>religion</i>	number of religion adherents at county-level divided by total population of the county (in percentage)	Association of Religion Data Archives (ARDA)
<i>less severe</i>	A binary variable taking the value one if the bank receives a less severe enforcement action related to a reporting issue (Call report, HDMA disclosure, etc.) or a violation of Flood Insurance requirements between $t-2$ and t .	Regulators'websites
<i>organ donation</i>	Number of organ donations in the county divided by county population per 10,000	Obtained by email from Network for Organ Sharing (UNOS)
<i>CEO social capital</i>	The social capital level of the CEO of the banking group to which each bank belongs. This is measured using the state-level value of the level of social capital in 1997 where the CEO of the banking group grew up. The state value of social capital is computed as the mean value of the social capital index of all counties in a state in 1997	Scott Yonker and own calculations
<i>Executives social capital</i>	The social capital level of the executives reported on Execucomp of the banking group to which each bank belongs (usually the top 5 earners of the firm). This is measured using the state-level value of the level of social capital in 1997 where the executives of the banking group grew up. The state value of social capital is computed as the mean value of the social capital index of all counties in a state in 1997. Then I average the value of all executives to obtain a bank-level variable	Scott Yonker and own calculations
<i>CEO tenure</i>	The natural logarithm of the number of years the CEO is served in the position	Execucomp
<i>CEO age</i>	The natural logarithm of the CEO age	Execucomp
<i>CEO total compensation</i>	The natural logarithm of the CEO total compensation	Execucomp
<i>CEO delta</i>	The natural logarithm of the expected dollar change in CEO wealth for a 1% change in stock price	Lalitha Naveen website
<i>Board independence</i>	The fraction of directors that are outsiders	BoardEx
<i>Board size</i>	The natural logarithm of the number of directors sitting on the board	BoardEx
<i>misconduct</i>	It is a binary variable taking the value one in the presence of bank misconduct and zero otherwise. Misconduct cases are identified using bank enforcement actions issued by bank regulators in the US. They can be cease and desist orders, prompt corrective actions, written agreements or prohibitions from banking. These actions may imply financial penalties in certain cases.	Regulators'websites
<i>technical misconduct</i>	It is a binary variable that takes the value one in the presence of misconduct related to bank violations of capital adequacy and liquidity,	Regulators'websites

	asset quality, lending, provisions, and reserves, and zero otherwise.	
<i>non-technical misconduct</i>	It is a binary variable that takes the value one in the presence of misconduct related to failures of the bank's internal control and audit systems, risk management systems, anti-money laundering violations, violations of consumer protection laws (Federal Trade Commission Act, the Equal Credit Opportunity Act, etc.), breaches of the requirements concerning the competency of the bank personnel or related parties and cases related to fraud and insider abuse, and zero otherwise.	Regulators' websites
Instrumental variables		
<i>HHI race</i>	The Herfindahl index calculated across the Census Bureau ethnic categories of Hispanic, non-Hispanic black, non-Hispanic white, Asian and other races for a county in a given year. This is a measure of ethnic homogeneity	US census
<i>distance to Canadian border (ln)</i>	Natural logarithm of the distance in kilometers from the bank headquarters to the closest point of the Canadian border	www.internationalbordercommission.org/fr/
Other variables (used in Appendix)		
<i>avg salary resid</i> (used in the Online Appendix)	The residual of a regression of the natural logarithm of the average salary of bank examiners on state dummies and a time trend. For this refining, I use two different samples one for each regulatory agency and quarterly information (for the period before 2009Q3 I attribute the value of the only observation available per year to the four quarters as it is done in Delis et al. (2017a) for data on bank examiners). Then I keep the last annual observation available for each year to use it as an instrument. The average salary of bank examiners is not available for banks regulated by the Federal Reserve. Average salaries of bank examiners are obtained for each agency and each state on a yearly basis until 2009Q3 and on a quarterly basis onwards. Each bank is matched with the <i>avg salary resid</i> obtained for the federal agency that regulates it (FDIC or OCC) in its own state (for OCC banks) or the state in which the corresponding field office is located (FDIC banks). I cannot compute this variable for OCC regulated banks that are located in states where there is not an OCC field office because I did not feel confident on how to allocate the responsibility for on-site examinations.	FedScope Employment Cubes from the Office of Personnel Management
<i>distance to reg (ln)</i> (used in the Online Appendix)	Natural logarithm of the distance from the bank headquarters to the closest bank regulator's office in the state where the bank is located. The distance is measured in kilometers. For State Banks, the distance is computed from the headquarters of the bank to the closest state regulator office. For National Banks, the distance is computed from the headquarters of the bank to the closest OCC office in the state. Following Delis et al. (2017a), I only compute the distance from the closest OCC office for banks located in states where the OCC has at least one office. Banks with total assets higher than \$50 billion tend to have a team of examiners that work permanently on-site. Therefore, I set the value of this variable equal to 0.01 for these banks	Author's computation using data from FDIC Summary of deposits, Call reports, OCC website and Wilson and Veuger (2017)
<i>HQ deposits</i>	Percentage of deposits held at the main office	FDIC summary of deposits and own calculations
<i>avg distance to HQ</i>	Average distance in Km between the headquarters and the branches (natural logarithm)	FDIC summary of deposits and own calculations
<i>avg SC distance</i>	distance (in absolute value) of social capital surrounding the head-office and the level of social capital of branch locations (natural logarithm)	FDIC summary of deposits and own calculations
Bank-county-level variables		
<i>%ΔMarket share of deposits</i>	Annual percentage change of the share of deposits of a bank in a county	FDIC summary of deposits

<i>Z social capital mkt</i>	Standardized value of <i>social capital</i> computed for the markets (counties) in which a bank operates	NRCRD (Northeast Regional Center for Rural Development)
<i>county competition</i>	The county-level Herfindahl-Hirschman Index (HHI) of deposit concentration. I compute the market share of each bank in each county and compute the HHI based on those market shares	FDIC summary of deposits

Appendix 4.2 Examples of misconduct

Type	Sub-classification	Description (Date, Regulator, Bank, Text)
<i>Technical</i>	violations of capital adequacy and liquidity	September 2011, OCC, Citizens First National Bank : "...(1) The Bank shall achieve within ninety (90) days of the date of this Order and thereafter maintain the following capital levels (as defined in 12 C.F.R. Part 3): (a) Total risk-based capital at least equal to twelve percent (12%) of risk-weighted assets; and 6 (b) Tier 1 capital at least equal to eight percent (8%) of adjusted total assets..1 (2) The requirement in this Order to meet and maintain a specific capital level means that the Bank may not be deemed to be "well capitalized" for purposes of 12 U.S.C. § 1831o and 12 C.F.R. Part 6 pursuant to 12 C.F.R. § 6.4(b)(1)(iv)..."
	asset quality and lending	March 2009, FDIC, Heartland Bank : "...The FDIC and the OSBC considered the matter and determined that they had reason to believe that the Bank had engaged in unsafe or unsound banking practices. The FDIC and the OSBC, therefore, accepted the CONSENT AGREEMENT and issued the following: IT IS HEREBY ORDERED, that the Bank, its institution affiliated parties, as that term is defined in section 3(u) of the Act, 12 U.S.C. § 1813(u), and its successors and assigns, cease and desist from the following unsafe or unsound banking practices: ... C. Operating with an excessive level of adversely classified assets and non-accrual loans; D. Engaging in hazardous lending and lax collection practices..."
	provisions and reserves	February 2009, FRB, Sun American Bank : "...Within 60 days of this Agreement, the Bank shall review and revise its allowance for loan and lease losses ("ALLL") methodology consistent with relevant supervisory guidelines..."
<i>Non-Technical</i>	bank's internal control and audit, risk management or IT systems	June 2014, FRB, Regions Bank : "...The Federal Reserve Board on Wednesday announced that Regions Bank, Birmingham, Alabama, will pay a \$46 million penalty for misconduct related to the process followed by the bank for identifying and reporting non-accrual loans. The Federal Reserve also issued a consent order requiring Regions Bank to continue to improve its relevant policies and procedures(...)The enforcement actions are based on deficiencies in the controls and procedures in place at Regions Bank ..."
	anti-money laundering violations	March 2008, FDIC, First Regional Bank : "...IT IS HEREBY ORDERED, that the Bank, its institution-affiliated parties, as that term is defined in section 3(u) of the Act, 12 U.S.C. § 1813(u), and its successors and assigns, cease and desist from the following unsafe and unsound banking practices and violations of law and/or

		regulation, as more fully set forth in the FDIC’s Report of Examination dated April 23, 2007: (a) operating in violation of section 326.8 of the FDIC’s Rules and Regulations, 12 C.F.R. § 326.8, regarding a satisfactory Bank Secrecy Act (“BSA”) and Anti-Money Laundering (“AML”) compliance program with respect to Individual Retirement Accounts administered by third parties...”
	violations of consumer protection	April 2011, FRB, 10 different banks: The Federal Reserve Board on Wednesday announced formal enforcement actions requiring 10 banking organizations to address a pattern of misconduct and negligence related to deficient practices in residential mortgage loan servicing and foreclosure processing. These deficiencies represent significant and pervasive compliance failures and unsafe and unsound practices at these institutions. The Board is taking these actions to ensure that firms under its jurisdiction promptly initiate steps to establish mortgage loan servicing and foreclosure processes that treat customers fairly, are fully compliant with all applicable law, and are safe and sound.
	compliance issues	September 2011, FDIC, Northwest Savings Bank: “...2. (a) The Bank shall develop and implement an effective CMS that is commensurate with the level of complexity of the Bank’s operations and a comprehensive written compliance program (“Compliance Program”). (b) Within 60 days from the effective date of this ORDER, the Board shall submit to the Regional Director of the New York Regional Office of the FDIC (“Regional Director”) for non-objection a Compliance Program that, at a minimum: (i) includes policies, controls, procedures, and processes that ensure consistent compliance with all consumer laws, regulations and regulatory guidance to which the Bank is subject...”
	breaches of the requirements concerning the competency of the bank personnel	November 2011, OCC, American Bank & Trust Company, N.A.: “...the Currency of the United States of America (“Comptroller”) intends to initiate prohibition, cease and desist, and civil money penalty proceedings against Harry S. Coin (“Respondent”) pursuant to 12 U.S.C. §§ 1818(b), (e), and (i) on the basis of Respondent’s activities while serving as chief executive officer...” “...the Comptroller of Respondent caused the Bank to purchase approximately twenty acres of land in Rock Island, Illinois (“Rock Island Property”) without conducting any formal analysis or obtaining prior Board approval, as required by the Bank’s Branching Policy...” “Respondent caused the Bank to deposit \$970,000 in Bank funds into the Bank’s account at a correspondent bank in exchange for receiving preferential terms on a personal loan from the correspondent bank, in violation of 12 U.S.C. § 1972(2)(A). The Bank sustained a lost opportunity cost of approximately \$30,526 because the Bank’s account did not earn any interest at the correspondent bank. Respondent received personal gain in the form of a lower interest rate on his personal loan, which resulted in lower payments...”

	<p>cases related to fraud and insider abuse</p>	<p><u>July 2009, FDIC, Harleysville Savings Bank:</u> “..The FDIC considered the matter and determined it had reason to believe that: (a) The Respondent has engaged or participated in violations, unsafe or unsound banking practices and/or breaches of fiduciary duty as an institution-affiliated party of Harleysville Savings Bank, Harleysville, Pennsylvania; (b) By reason of such violations, unsafe or unsound banking practices and/or breaches of fiduciary duty, the Bank has suffered financial loss or other damage, the interests of the bank’s depositors have been prejudiced, and/or Respondent received financial gain or other benefit; and (c) Such violations, unsafe or unsound banking practices and/or breaches of fiduciary duty involve personal dishonesty on the part of the Respondent or demonstrate the Respondent's willful and/or continuing disregard for the safety or soundness of the Bank...”</p>
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ONLINE APPENDIX

Online Appendix Supplementary results

A.3.A Variables of social capital used in the computation of the social capital index

In Online Appendix Table A.3.A, I present probit regressions in which I replace the social capital index as main independent variable by each of the variables I use in the computation of the Principal Component. These variables are related to civic norms RESPN (US Census response rate) and PVOTE (voter turnout) and related to networks ASSN (number of associations divided by population per 1,000) and NCCS (number of non-profit organizations divided by population per 10,000). In Column 1, I use all variables in the regression and show that the effect of social capital on bank misconduct is driven by both networks and respect for civic norms. In Columns 2 to 5, I introduce each variable separately in the regression. I observe that all variables forming the social capital index are negative and have a significant impact on bank misconduct.

A.3.B Pre-crisis, Crisis and Post-crisis periods

In Online Appendix Table A.3.B, I present probit regressions in which the baseline test is run for the pre-crisis (2001-2006), crisis (2007-2010) and post-crisis (2011-2015) periods. I find negative and significant coefficients for the variable *social capital* in the three periods.

A.3.C Rare events logit model

It is evident from the descriptive statistics that the cases of misconduct (3.1% of the total observations) are a rare event. Statistical procedures, such as probit regressions may underestimate the probability of rare events. To verify the robustness of my results, in Online Appendix Table A.3.C, I follow King and Zeng (2001) and correct this potential downward bias using a rare events logit procedure. The result of this test is in line with the baseline model.

A.3.D Different regulators

In Online Appendix, Table A.3.D, I intend to rule-out the possibility that my result is driven by different degree of enforcement from different regulators. The fact that some banks are subject to examinations by a state level body and other banks exclusively by a federal body may lead to inconsistencies in the enforcement and supervisory process (Agarwal et al. 2014). In order to address this aspect, I run separate regressions for National Banks (regulated by the OCC and subject to examinations by this supervisory body) and State Banks (regulated by either FDIC or Fed and subject to examinations by state and federal regulators under a periodical rotation policy). In Columns 1 and 2, I include all banks in the sample and in Columns 3 and 4, only less geographically dispersed banks. In all cases I find results that are consistent with my baseline regressions. I find that the coefficient of *social capital* is negative and statistically significant in all cases.

A.3.E Headquarter relocations

In Online Appendix, Table A.3.E, I report a cross-sectional linear probability model of a dummy variable representing misconduct on a dummy (*SCincreasing*) taking the value one if a bank is relocating its headquarters to a county that has a higher level of social capital and zero otherwise, *post* that takes the value one the third, fourth and fifth year after the headquarter relocation and zero otherwise, and interaction term of the two and the rest of bank and county-level controls that I use in the baseline model. I choose a linear probability model because using non-linear models such as logit or probit with interaction terms may be problematic (Greene 2010). I run this regression on a sample of banks that relocate their headquarters once during the sample period (292 banks are affected). I remove from the sample banks that relocate more than once as in Hasan et al. (2017b). I follow Parsons et al. (2018) that indicate that the median time of misconduct detection is 3 years (for the cases in my sample for which I can identify the year when the misconduct –mostly non-technical

cases- is committed I find that the average period between commission and announcement of misconduct is roughly 3 years). Consequently, I remove the three first years after the relocation, and run the regression on the three years previous to the relocation (t-3, t-2 and t-1) and the third, fourth and fifth year (t+3, t+4 and t+5) after the relocation. I only use three years pre- and post-event periods to avoid to the extent possible the influence of other events taking place as I get further away from the relocation event. I restrict the sample to relocation events taking place between 2002 and 2012 to have a sufficient number of pre- and post-event years. There are 141 relocation events that are related to social capital increasing relocations and 151 events related to social capital decreasing relocations.

The interaction term *SCincreasing* post* captures the effect of social capital on bank misconduct after a relocation event takes place. The coefficient for this interaction is negative and statistically significant (at the 10% level), showing that moving to a county with greater social capital reduces the chances that a bank is involved in misconduct.

A.3.F Different geographical areas

In Online Appendix, Table A.3.F, I replicate the baseline regression for different geographical areas in the US. In Figure 4.1 I observe that the highest levels of social capital tend to cluster in the northern part of the country. Therefore, in Panel A, I run additional regressions for the different geographical areas to rule-out the possibility that the fact that social capital clusters in a specific area is the sole reason why I find a negative association between social capital and bank misconduct. I run separate regressions for northern states (Column 1), southern states (Column 2), eastern states (Column 3) and western states (Column 4). Panel B shows which states fall in the different geographical areas. The results of the four regressions display a negative and significant impact of social capital on bank misconduct. This is in line with my baseline result.

A.3.G Other county-level variables

In Online Appendix, Table A.3.G, I provide further details regarding other county level variables that could be missing in the baseline tests. The choice of the county level variables in the main tests of the paper are motivated by the choices made in other papers studying the role of social capital on firm outcomes (e.g. Hasan et al. 2017 a, b, Jha and Cox 2015, etc.). However, there could potentially be other variables that influence the process of misconduct and be related to social capital. Even though the tests in Table 4.5 in which I control for unobserved bank-level and regional heterogeneity, the test in Table 4.6 in which I use CEOs' levels of social capital as well as the endogeneity test in Table 4.7 provide some assurance that the main results of the paper are unlikely to be biased by omitted variable factors, in Table A.3.G I add other county-level control variables to my baseline model to provide additional evidence that potential omitted variables are not biasing my results. In Column 1¹²⁴, I add county-level variables that may have a significant impact in the process of misconduct and other related firm outcomes (e.g. Parsons et al. 2018, Hilary and Huang, 2015, Di Giuli and Kostovetsky 2014). Besides the control variables used in the baseline model, I add population growth, wages growth, the natural logarithm of total population, the natural logarithm of population density, number of firms in the same industry (i.e. number of banks) located in the same county, political orientation (the variable *republican* is the percentage of republican voters in each presidential election) and the percentage of religious people. When I add all these variables to the model, the effect of social capital on bank misconduct remains negative and statistically significant. Including these additional variables in the model may be problematic because multicollinearity issues may arise as some of the county-level variables are highly correlated. Therefore, in Columns 2 to 7, I add each variable (except religion

¹²⁴ Population related variables are collected or computed using US Census data. Wage growth is computed using BEA data. The number of banks in a county is computed using call reports data. Political orientation of the county is obtained using the percentage of votes for the Republican Party in each US presidential election between 2000 and 2016 (I use linear interpolation to fill-in the gaps between elections). *Religion* is defined in Appendix 4.1.

because I added it in the robustness test section) separately. In all cases *social capital* is negative and statistically significant.

A.3.H Board variables and distance from the regulator

In this table I intend to address other concerns regarding omitted variables in the tests we run throughout the paper. One potential interpretation of our baseline results is that less geographically dispersed banks take advantage of denser networks associated with social capital to collude with them, what could explain the negative effect of social capital on misconduct. Under this hypothesis, banks that are located closer to the regulator office have better access to communicate with them (Wilson and Veuger 2017). Therefore, this conjecture can be tested checking whether the effect of social capital on misconduct is different for banks located closer from the regulators office than for banks located further away. In order test this alternative interpretation of the results, in Column 1 of Table A.3.H, I run a linear probability model including *social capital* the standardized variable *distance to regulator*, an interaction term of this variable with *social capital* and the rest of control variables I use in the baseline tests. I limit the sample to state banks as the literature has shown that state banks' regulators tend to be more lenient than federal regulators (Agarwal et al. 2014). I find that the coefficient for social capital remains negative and highly significant; the coefficient for *distance to regulator (std)* is not significant. The key variable here is the interaction term of *social capital* and *distance to regulator (std)*¹²⁵. A negative and significant support would provide support for the alternative explanation that I mention above. However, the coefficient is zero and not significant. Therefore, I conclude that there is not empirical evidence of "collusion" between banks and regulators.

¹²⁵ The number of observations in Table A.3.D Column 4 and Table A.3.H Column 1 should be identical. However, this is not the case because I have not been able to determine the distance from the regulator for a few banks which drives the different in the number of observations between these two regressions.

In Column 2, I include board variables as board characteristics may have an impact on the probability of misconduct in corporations (Marra et al. 2011, Nguyen et al. 2016). We add *board size* and *board independence* as controls. The main concern of this test is that the information for board composition is only available for sample publicly held banks and mostly for the larger ones. These features may bias my results against finding significant results as I hypothesize that the effect of social capital is more important for less geographically diversified banks (and hence smaller). Despite the odds, I run a test on this sample using board variables and find a negative and statistically significant coefficient (at the 10% level) for *social capital*.

In Columns 3 and 4, following Alessandrini et al. (2009), I include some additional controls to the baseline test to control for bank organizational complexity. I include the percentage of deposits held at the main office (a higher percentage of deposits in the head office means lower complexity) and the natural logarithm of the average distance in Km between the headquarters and each branch (higher average distance means more complexity) in Column 3. In Column 4, I replace the natural logarithm of the average distance in Km between the headquarters and each branch by the natural logarithm of the mean distance (in absolute value) of social capital surrounding the head-office and the level of social capital of each branch location. I find in all cases a negative and statistically significant coefficient for *social capital*.

A.3.1 Consequences of bank misconduct and the role of social capital: IV treatment effects model

I address the potential endogeneity of my tests in Table 4.10. The issuance of enforcement actions for misconduct is not a random event. Banks self-select into this treatment which potentially leads to endogeneity and biased estimators when using OLS. I follow Delis et al. (2017a, 2018) and run an instrumental variable treatment-effects model. This model is a

variant of the Heckman Inverse Mills Ratio (Heckman 1976, 1978). It is suited for cases in which endogeneity may arise due to unobservable factors that simultaneously affect both the probability of a private firm self-selecting its treatment (receiving an enforcement action for misconduct) and the outcome variable (percentage changes in deposit market share).

The econometric procedure I implement in this section is discussed in detailed in Maddala (1983), and it is a generalized version of the traditional two-step Heckman selection model, and therefore accounts for the effect of unobservables by using an error correction term¹²⁶. This method has the advantage with respect to the standard instrumental variable framework that it takes into account the binary nature of the treatment variable (Clougherty and Duso 2015).

In particular, I use a first-stage probit regression model to predict the probability that the bank supervisor sanctions a bank for misconduct (treatment equation). The independent variables in the first stage are the same as those in the regressions in Table 4.10 plus the two instruments, *avg salary resid* and the natural logarithm of *distance to reg*. From the treatment equation, the hazard for each observation is computed, serving as a correction term for the second-step. The correction term calculated for each firm in the first stage captures unobservable information related to enforcement actions and the outcome variable. In the second stage of the estimation procedure (the outcome equation), I regress the *%ΔMarket share of deposits* on a set of independent variables that include the *misconduct*, the interaction term *misconduct * Z social capital mkt*, the independent bank and county-level covariates used in the first stage and the hazard obtained in the treatment equation (*lambda* is the parameter estimate on the hazard). I also include regulator, state and year fixed effects.

¹²⁶ Cong and Drukker (2000) provide the technical details on how to implement this type of models in Stata.

I select my instruments using factors that may affect the probability of issuing an enforcement action for misconduct. First, I use the residual of the average salary of bank examiners as my first instrument. The reasoning for using this variable as an instrument for my treatment is the following. Bank examiners are skilled professionals with analytical skills and solid knowledge in finance and accounting. In principle, the salaries paid by the regulatory agencies are set according to pay bands depending on the skills and experience of the examiner plus supplements that vary locally. To be able to lure competent professionals, regulatory agencies intend to pay salaries that are locally competitive¹²⁷. Wages paid by regulatory agencies tend to increase with the tenure of the bank examiner within the institution or with the experience that the person may have acquired at other jobs. However, these agencies, in special circumstances, may pay salaries that are outside the pre-specified bands if they need to recruit someone who has a specific set of skills (FDIC 2018, OCC 2015). Also, regulatory agencies may use salary incentives such as promotional incentives or special pay increases in order to retain or attract talented workers or even to encourage them to improve their skills. Given these characteristics, after removing local and time effects, higher salaries may be related to more skilled and experienced examiners on average. A better set of skills and more experience are likely to be related with a stronger capacity of detecting deficiencies in banks and violations of laws and regulations during the examination process. Therefore, higher salaries will then be linked with a higher likelihood that examiners produce findings and conclusions that lead to enforcement actions on banks.

I obtain information on bank examiners average salaries from Fed Scope Employment Cubes at the Office of Personnel Management¹²⁸. This database includes aggregated

¹²⁷ This information has been consulted on the websites that provide information on the pay and benefits of the employees of the regulatory agencies, FDIC (<https://www.fdic.gov/about/jobs/offer.html>) and OCC (<https://careers.occ.gov/pay-and-benefits/salary/index-occ-salary-structure.html>)

¹²⁸ The information can be obtained from this website <https://www.fedscope.opm.gov/employment.asp>. The variable average salary is defined in this website as the “sum of an employee’s rate of basic pay and any locality

information such as the average salary, length of service, gender¹²⁹, age, occupation and official duty station (i.e., state) of the bank examiners on a yearly basis (quarterly after 2009Q3). This website only provides information on the average salary of bank examiners for the OCC and the FDIC. As a consequence, banks regulated by the Fed are excluded in this test¹³⁰. I refine the average salary variable by using the residuals from the regression of the natural logarithm of the average examiners' salary in local offices on an annual trend (to capture the annual increase in the share average salary of examiners) and state fixed effects (to capture local conditions that may determine the salaries)¹³¹. Then, each bank is matched with the residual variable *avg salary resid* obtained for the federal agency that regulates it (FDIC or OCC). I cannot identify the field office in charge of the supervision of each bank for OCC regulated banks. As a result, OCC regulated banks are matched every year with the value of *avg salary resid* in the state where they are headquartered. I remove the observations for OCC regulated banks that are located in states where there is not an OCC field office because I did not feel confident on how to allocate the responsibility for on-site examinations. On the other hand, for FDIC regulated banks, I can identify the FDIC field office in charge of their supervision using the information available in the FDIC Summary of Deposits. Thus, I

comparability payment and/or special pay adjustment". I obtain information for the OCC and the FDIC on the occupation code 05700570-Financial Institution Examining. This information is available for each state and agency annually until 2009Q3 and quarterly onwards.

¹²⁹ Delis et al. (2017a) use the fedscope database to compute the portion of female bank examiners. In that paper, the authors use the residual of a regression of the portion of female bank examiners on a time trend and state fixed effects as an instrument for enforcement actions. I have tried the same variable as an instrument, but it presents a low statistical significance (results are available upon request). This could affect the validity of my test and I decide not to use this instrument. This lack of significance may be due to the fact that I run an instrumental treatment-effects model for the period 2011-2015, as it is the one of interest to test hypothesis 2. If I run the same regression for the whole period of my study (2001-2015), the residual value of the portion of female bank examiners turns out statistically significant in the first stage.

¹³⁰ As a consequence of this treatment, I lose approximately 10% of the banks. This issue is also faced in Delis et al. (2017a). Given that my sample of banks remains large enough; this aspect should not affect the validity of my results.

¹³¹ I refine my variable using two different samples, one for each regulatory agency using quarterly data (for the period before 2009Q3 I attribute the value of the only observation available per year to the four quarters of the year as it is done in Delis et al. (2017a) for data on bank examiners). Then, for every year, I keep the last annual observation available for each pair of state-agency observations to use them as an instrumental variable.

match each bank with the value of *avg salary resid* in the state where its FDIC field office is located.

The exclusion restriction for *avg salary resid* is satisfied in my context because contemporaneous changes in market shares are not directly formulated by bank examiners; and they could only be influenced by the supervisory process and the enactment of an enforcement action.

Second, I use as an additional instrument the natural logarithm of the distance (in kilometers) between the bank headquarters and the closest regulatory office in the state where the bank is located. I match each bank with the closest OCC office in the state where the bank is located for National banks¹³² (OCC regulated banks) and with the closest State regulator field office for State banks¹³³ (FDIC regulated banks). The reasoning for this instrument is as follows. First, various papers establish that geographical proximity reduces information asymmetries (e.g. Degryse and Ongena 2005, Wilson and Veuger 2017). Sensitive information about bank misconduct is more likely to be detected when supervisory agencies are located near the bank¹³⁴. Also, bank examiners tend to travel to bank premises so transportation costs may have an impact on the quality of monitoring. I may then expect some delay in the issuance of enforcement actions by bank supervisors since the information flows will tend to be slower for banks further away from the supervisor. Therefore, I may expect a

¹³² I use the location of the offices provided in the OCC website <https://www.occ.treas.gov/about/who-we-are/district-and-field-offices/index-organization.html>. I then obtain the coordinates (longitude and latitude) for each office and compute the distance between the bank headquarters and the closest office using the stata commands *geodist* and *geonear*. It is worth reminding that banks located in states where there is no OCC field office are not in the sample.

¹³³ I use the information on State Regulator offices' addresses provided by Stan Veuger that is used in Wilson and Veuger (2017). The authors use public documents to construct a database of regulatory field office locations of state bank regulators and confirm each location by email or phone survey. I then obtain the coordinates (longitude and latitude) for each office and compute the distance between the bank headquarters and the closest office using the stata commands *geodist* and *geonear*.

¹³⁴ I refine this instrument and set the value of the distance to regulator equal to 0.01 for banks that have total assets above \$50 billion. The reason why I perform this additional test is that large banks have a team of supervisors that work on-site constantly. Therefore, the distance from the headquarters to the regulatory office matters less in these cases. The result of unreported test using a non-refined measure gives results that are similar from a statistical and economical perspective to the reported ones.

negative relationship between the distance to the regulator and misconduct. However, supervisors could compensate for the reduced monitoring of faraway banks by increasing the number of enforcement actions. Thus, the direction of the effect is mostly an empirical question. The exclusion restriction for this instrument states that the closeness to the bank regulator only determines changes in deposit market share through the supervisory process (issuance of an enforcement action). Bank location is time-invariant and therefore exogenous with respect to contemporaneous changes in deposit market shares since bank location decisions are made back in time and rarely change over time ¹³⁵.

The result of the instrumental variables treatment-effects model is shown in the Table A.3.I in Column 1. I run this test on the post-crisis sample as it is my result of interest. I observe that the interaction term *misconduct * Z social capital mkt* is negative and highly significant at the 1% level in this model. This is in line with the result I obtain using OLS (Table 4.10, Column 3)¹³⁶. Moreover, the parameter *lambda* is not statistically significant, revealing that, from a statistical perspective, the unobservable factors in the treatment-assignment errors are less important than the observable ones (the variables for which I can control for). This result suggests that my previous estimate is unlikely to be biased because of the self-selection issue.

¹³⁵ The only concern is that regulators tend to have their offices in urban areas, and banks may potentially have more flexibility in those areas to gain market share. I address this concern in unreported tests. I run the same treatment-effects model using banks located in non-rural counties (as defined by the US Census). Results remain consistent.

¹³⁶ I also run the IV treatment-effects regression for the pre-crisis and crisis periods. I obtain results that are consistent with the findings shown in Table 4.10, Columns 1 and 2. I do not show the results for brevity.

Table A.3.A Different components of social capital

This table reports the results of probit regressions of a dummy variable taking the value one in the presence of misconduct and zero otherwise on the different variables used to compute *social capital* and control variables. I represent the marginal effect of each variable of interest in square brackets. In Column 1, I add the four components of *social capital*. In Columns 2 and 3, I include the variables related to civic norms RESPN (US Census response rate) and PVOTE (voter turnout), respectively. In Columns 4 and 5, I include the variables related to networks ASSN (number of associations divided by population per 1,000) and NCCS (number of non-profit organizations divided by population per 10,000), respectively. All independent variables are lagged one period. Fixed effects are specified on the table. Standard errors are robust to heteroscedasticity and clustered by bank. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1. Constant terms are included in the regression although they are not shown in the table for brevity.

	(1)	(2)	(3)	(4)	(5)
	All components	RESPN	PVOTE	ASSN	NCCS
Dependent var. :	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)
RESPN	-0.262*[-0.012] (0.152)	-0.383***[-0.017] (0.137)			
PVOTE	0.010[-0.001] (0.139)		-0.221*[-0.010] (0.127)		
ASSN	-0.111***[-0.004] (0.028)			-0.121***[-0.005] (0.024)	
NCCS	0.000[-0.000] (0.001)				-0.001**[-0.000] (0.001)
Controls	Yes	Yes	Yes	Yes	Yes
Regulator and Time FE	Yes	Yes	Yes	Yes	Yes
Pseudo-R2	0.168	0.167	0.167	0.168	0.167
Observations	101669	101669	101669	101669	101669
p-value test : RESPN+PVOTE=ASSN+NCCS	0.41				

Table A.3.B Pre-crisis, Crisis and Post-crisis periods

This table reports the results of probit regressions of a dummy variable taking the value one in the presence of misconduct and zero otherwise on a social capital variable and control variables. I represent the marginal effect of the variable *social capital* in square brackets. In Column 1, I run the baseline regression for the pre-crisis period (2001-2006). In Column 2, I run the baseline regression for the crisis period (2007-2010). . In Column 3, I run the baseline regression for the post-crisis period (2011-2015). All independent variables are lagged one period. Fixed effects are specified on the table. Standard errors are robust to heteroscedasticity and clustered by bank. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1.

	(1) Pre-crisis	(2) Crisis	(3) Post-crisis
Dependent var. :	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)
social capital	-0.048**[-0.001] (0.020)	-0.039**[-0.002] (0.020)	-0.110***[-0.006] (0.024)
equity ratio	-0.037*** (0.007)	-0.051*** (0.007)	-0.022*** (0.007)
allowance loan lease	0.222*** (0.020)	0.181*** (0.021)	0.125*** (0.017)
ROA	-0.311*** (0.019)	-0.274*** (0.014)	-0.243*** (0.015)
liquidity	0.002 (0.004)	-0.001 (0.003)	0.003 (0.002)
RWA	0.008*** (0.001)	0.011*** (0.001)	0.006*** (0.001)
deposit ratio	0.002 (0.003)	-0.001 (0.002)	-0.001 (0.003)
age	0.048** (0.019)	0.034* (0.018)	0.063*** (0.021)
size	-0.240 (0.154)	-0.012 (0.179)	-0.927*** (0.167)
size sq	0.015** (0.006)	0.004 (0.007)	0.043*** (0.006)
competition	0.011 (0.164)	0.152 (0.159)	0.131 (0.166)
BHC	0.011 (0.046)	0.023 (0.045)	-0.101** (0.042)
publicly held	-0.108* (0.063)	-0.165*** (0.060)	0.075 (0.060)
income pc	0.104 (0.139)	-0.180 (0.118)	-0.156 (0.122)
employment	-0.001 (0.002)	0.000 (0.002)	0.005*** (0.002)
education	-0.006 (0.004)	0.009*** (0.003)	0.001 (0.003)
median age	-0.007 (0.208)	-0.121 (0.182)	0.097 (0.188)
rural	-0.045 (0.045)	0.039 (0.043)	0.022 (0.047)
Constant	-2.617 (1.667)	-0.683 (1.551)	3.673** (1.661)
Regulator and Time FE	Yes	Yes	Yes
Pseudo-R2	0.130	0.197	0.147
Observations	44286	26985	30398

Table A.3.C Rare events logit model

This table reports the results of a non-linear regression of a dummy variable taking the value one in the presence of misconduct and zero otherwise on a social capital variable and control variables. I represent the marginal effect of the variable *social capital* in square brackets. I run a rare events logit model as suggested by King and Zang (2001). All independent variables are lagged one period. Fixed effects are specified on the table. Standard errors are robust to heteroscedasticity and clustered by bank. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1.

(1)	
Rare events	
Dependent var. :	P(Misconduct=1)
social capital	-0.116***[-0.03] (0.028)
equity ratio	-0.082*** (0.009)
allowance loan lease	0.293*** (0.025)
ROA	-0.532*** (0.020)
liquidity	-0.001 (0.004)
RWA	0.019*** (0.002)
deposit ratio	0.002 (0.004)
age	0.089*** (0.024)
size	-1.022*** (0.223)
size sq	0.051*** (0.009)
competition	0.256 (0.219)
BHC	-0.052 (0.057)
publicly held	-0.157** (0.079)
income pc	-0.272* (0.155)
employment	0.003 (0.002)
education	0.007* (0.004)
median age	-0.160 (0.244)
rural	0.031 (0.059)
Constant	2.261 (2.029)
Regulator FE	Yes
Time FE	Yes
Observations	101669

Table A.3.D Different regulators

This table reports the results of probit regressions of a dummy variable taking the value one in the presence of misconduct and zero otherwise on a social capital variable and control variables. I represent the marginal effects in square brackets. In Column 1, I run the baseline model on the sample of National banks (regulated by the OCC). In Column 2, I run the baseline model on the sample of State banks (regulated either by the FDIC or the Fed). In Column 3, I run the baseline model on the sample of National banks (regulated by the OCC) that are present in five or less states. In Column 4, I run the baseline model on the sample of State banks (regulated either by the FDIC or the Fed) that are present in five or less states. All independent variables are lagged one period. Fixed effects are specified on the table. Standard errors are robust to heteroscedasticity and clustered by bank. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1. Constant terms are included in the regression although they are not shown in the table for brevity.

	(1)	(2)	(3)	(4)
Sample :	Banks regulated by OCC	Banks regulated by FDIC or Fed	Banks in <=5 states regulated by OCC	Banks in <=5 states regulated by FDIC or Fed
Dependent var. :	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)
social capital	-0.036*[-0.002] (0.022)	-0.065***[-0.002] (0.015)	-0.041**[0.003] (0.021)	-0.067***[0.003] (0.014)
Controls	Yes	Yes	Yes	Yes
Regulator FE	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
Pseudo-R2	0.137	0.183	0.137	0.180
Observations	22164	79505	21892	79271

Table A.3.E Headquarter relocations

This table reports the results of a linear probability model. I run a regression of a dummy variable taking the value one in the presence of misconduct and zero otherwise on different independent variables. In Column 1, I report a cross-sectional linear probability regression of a dummy variable representing misconduct on a dummy (*SCincreasing*) taking the value one if a bank is relocating its headquarters to a county that has a higher level of social capital and zero otherwise, *post* that takes the value one the third, fourth and fifth year after the headquarter relocation and zero otherwise, and interaction term of the two and the rest of bank and county-level controls that I use in the baseline model. I run this regression on a sample of banks that relocate their headquarters once during the sample period (292 banks are affected). I remove from the sample banks that relocate more than once as in Hasan et al. (2017b). I follow Parsons et al. (2018) that indicate that the median time of misconduct detection is 3 years. Consequently I remove the three first years after the relocation, and run the regression on the three years previous to the relocation (t-3, t-2 and t-1) and the third, fourth and fifth year (t+3, t+4 and t+5) after the relocation. I only use three years pre- and post-event periods to avoid to the extent possible the influence of other events taking place as I get further away from the relocation event. I restrict the sample to relocation events taking place between 2002 and 2012 to have a sufficient number of pre- and post-event years. There are 141 relocation events that are related to social capital increasing relocations and 151 events related to social capital decreasing relocations. I use time and regulator dummies. Standard errors are robust to heteroscedasticity. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1.

	(1) Relocations
Dependent var. :	Misconduct
SCincreasing	0.017 (0.013)
post	0.040** (0.019)
SCincreasing *post	-0.037* (0.022)
equity ratio	-0.001 (0.002)
allowance loan lease	0.029*** (0.011)
ROA	-0.036*** (0.006)
liquidity	-0.001 (0.001)
RWA	0.001 (0.000)
deposit ratio	0.001 (0.001)
age	0.004 (0.008)
size	-0.070 (0.052)
size sq	0.003 (0.002)
competition	0.086 (0.061)
BHC	-0.028 (0.020)
publicly held	0.011 (0.021)
income pc	0.047 (0.037)
employment	-0.001 (0.001)
education	-0.000 (0.001)
median age	-0.093 (0.076)
rural	-0.008 (0.016)
Constant	0.013 (0.507)
Regulator and time dummies	Yes
Adj-R2	0.095
Observations	1419

Table A.3.F Different geographical areas

This table reports the results of probit regressions of a dummy variable taking the value one in the presence of misconduct and zero otherwise on a social capital variable and control variables. I represent the marginal effects in square brackets. In Column 1, I run the baseline model on the sample of banks located in the northern area of the US. In Column 2, I run the baseline model on the sample of banks located in the southern area of the US. In Column 3, I run the baseline model on the sample of banks located in the eastern area of the US. In Column 4, I run the baseline model on the sample of banks located in the western area of the US. Panel A shows the results of the regressions. Panel B shows the classification of the states into North, South, East or West. All independent variables are lagged one period. Fixed effects are specified on the table. Standard errors are robust to heteroscedasticity and clustered by bank. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1. Constant terms are included in the regression although they are not shown in the table for brevity.

Panel A	(1)	(2)	(3)	(4)
Sample :	North	South	East	West
Dependent var. :	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)
social capital	-0.063***[-0.003] (0.019)	-0.050**[-0.002] (0.020)	-0.053***[-0.002] (0.015)	-0.079***[-0.003] (0.021)
Controls	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Pseudo-R2	0.165	0.171	0.173	0.168
Observations	53973	47696	68378	33291

Panel B		
State	Columns (1) and (2)	Columns (3) and (4)
Alabama	south	east
Arizona	south	west
Arkansas	south	east
California	south	west
Colorado	south	west
Connecticut	north	east
Delaware	north	east
District of Columbia	north	east
Florida	south	east
Georgia	south	east
Idaho	north	west
Illinois	north	east
Indiana	north	east
Iowa	north	east
Kansas	south	west
Kentucky	south	east
Louisiana	south	east
Maine	north	east
Maryland	south	east
Massachusetts	north	east
Michigan	north	east
Minnesota	north	east
Mississippi	south	east
Missouri	north	east
Montana	north	west
Nebraska	north	west
Nevada	south	west
New Hampshire	north	east
New Jersey	north	east
New Mexico	south	west
New York	north	east
North Carolina	south	east
North Dakota	north	west
Ohio	north	east
Oklahoma	south	west
Oregon	north	west
Pennsylvania	north	east
Rhode Island	north	east
South Carolina	south	east
South Dakota	north	west
Tennessee	south	east
Texas	south	west
Utah	south	west
Vermont	north	east
Virginia	south	east
Washington	north	west
West Virginia	south	east
Wisconsin	north	east
Wyoming	north	west

Table A.3.G. Other county-level variables

This table reports the results of probit regressions of a dummy taking the value one in the presence of misconduct and zero otherwise on a subset of variables. In Column 1, I include the social capital variable, the same control variables that I use in Table 4.3 and a subset of county-level controls (population growth, wage growth, natural log of total population, percentage of people voting for the republican party, natural logarithm of the number of banks, level of trust and percentage of religious adherents). In Columns 2 to 7 I add each of the additional county-level variables that I use in Column 1 separately (except *religion*). I represent the marginal effect of the variable *social capital* in square brackets. Fixed effects and the cluster level of standard errors are specified in the table. Standard errors are robust to heteroscedasticity. All independent variables are lagged one period. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1. Constant terms are included in the regression although they are not shown in the table for brevity.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Pop gr	Wage gr	Pop	Pop Density	Republican	Nb of banks
Dependent var. :	P(Misconduct=1)						
social capital	-0.036** [-0.002] (0.015)	-0.051*** [-0.002] (0.012)	-0.056*** [-0.003] (0.012)	-0.039*** [-0.002] (0.014)	-0.038*** [-0.002] (0.013)	-0.055*** [-0.003] (0.012)	-0.054*** [-0.002] (0.012)
population gr.	0.112 (0.107)	0.036 (0.098)					
wage gr.	0.000 (0.004)		-0.000 (0.004)				
population (ln)	0.011 (0.020)			0.027** (0.011)			
pop density (ln)	0.021 (0.016)				0.031*** (0.010)		
republican	-0.001 (0.001)					-0.002** (0.001)	
numb of banks (ln)	-0.013 (0.017)						0.010 (0.013)
religion	0.000 (0.001)						
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo-R2	0.169	0.168	0.168	0.168	0.168	0.168	0.168
Observations	98843	99780	101669	101669	101669	101669	101669

Table A.3.H. Board variables and distance from the regulator

This table reports the results of regressions of a dummy taking the value one in the presence of misconduct and zero otherwise on a subset of variables. In Column 1 I run a linear probability model including *social capital* the standardized variable *distance to regulator*, an interaction term of this variable with *social capital* and the rest of control variables we use in the baseline tests. In Column 2, I run the baseline test on the sample of publicly held banks for which variables of the board compositions are available. We add *board size* and *board independence* to the regression. In Column 3, we run the baseline probit regression including an additional control variable to capture the effect of bank complexity. I represent the marginal effect of the variable *social capital* in square brackets. Fixed effects and the cluster level of standard errors are specified in the table. Standard errors are robust to heteroscedasticity. All independent variables are lagged one period. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1. Constant terms are included in the regression although they are not shown in the table for brevity.

	(1)	(2)	(3)	(4)
	Distance to regulator	Board variables	Proxy for complexity	Proxy for complexity
Sample :	Less diversified state banks (<=5 states)	Less diversified banks (<=5 states)	Less diversified banks (<=5 states)	Less diversified banks (<=5 states)
Regression method:	OLS	Probit	Probit	Probit
Dependent variable :	Misconduct	P(Misconduct=1)	P(Misconduct=1)	P(Misconduct=1)
social capital	-0.003*** (0.001)	-0.103*[-0.008] (0.061)	-0.060***[-0.003] (0.012)	-0.063***[-0.003] (0.012)
distance to regulator (std)	0.000 (0.001)			
social capital* distance to regulator (std)	0.000 (0.001)			
board size		-0.184 (0.283)		
board independence		0.340 (0.254)		
HQ deposits			-0.034 (0.050)	-0.029 (0.046)
Ln(1+ avg distance to HQ)			0.013 (0.009)	
Ln(1+avg SC distance)				0.144*** (0.052)
Controls	Yes	Yes	Yes	Yes
Regulator FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Adj-R2	0.067			
Pseudo-R2		0.106	0.165	0.167
Observations	79036	5802	100000	95808

Table A.3.I. Consequences of bank misconduct and the role of social capital: IV treatment effects model

This table reports in Panel A the results of the regressions of a variable that measures the annual percentage change of the market share of deposits in each county on a misconduct variable, a standardized measure of social capital, the interaction of both and a subset of bank level and county-level control variables and in Panel B the summary statistics of the instrumental variables used in the regression. I use a post-crisis period sample (2011-2015). In Column 1, I run a treatment-effects instrumental variable model in two steps. I only use banks regulated by the FDIC and OCC because the instrument *avg salary resid* is only available for those banks. All independent variables are lagged one period. The control variables included in the first and second stage regressions are those that I use in Table 4.10, including *Z Social Capital Mkt* in the first stage. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Definitions for all variables are provided in Appendix 4.1. Constant terms are included in the regression although they are not shown in the table for brevity. This regression is computed using the stata command *itreatreg* with the option *two step*. Standard errors are not robust to heteroscedasticity and are not clustered because the two step method does not allow for it.

(1)	
Panel A	Endogeneity : Two step
Dependent var. :	%ΔMarket share of deposits
Sample :	Post-crisis : Only OCC and FDIC banks
	Second stage
misconduct	-0.727 (0.765)
Z social capital mkt	-0.451*** (0.164)
misconduct * Z social capital mkt	-0.774*** (0.262)
Controls	Yes
lambda	0.312 (0.445)
Regulator FE	Yes
Time FE	Yes
State FE	Yes
Cluster	Bank-county
Wald-test	8205.159
Observations	95737
	First stage
Dependent var. :	misconduct
avg salary resid	0.790*** (0.040)
distance to reg (ln)	-0.146*** (0.002)
Controls	Yes
Wald-test	18892.02
Observations	95709

Panel B Variables	mean	sd	p25	p50	p75
Instrumental variables					
avg salary resid	0.004	0.097	-0.043	-0.012	0.021
distance to reg (ln)	4.193	1.236	3.703	4.485	5.018

5. General Conclusion

The research in banking has considerably studied the effects of regulations, supervision and other formal mechanisms on the behavior of banks. Traditionally, empirical research has focused on capital requirements, restrictions to entry and activities, deposit insurance and supervisory actions, among others. Following the 2007-2009 financial crisis, the research in banking has shifted its attention to the causes of banks vulnerability to shocks, factors that make banks more resilient and business conduct. One of the main aspects highlighted by this literature is that regulation and other formal mechanisms have proved to be relatively inefficient in (i) containing risk-taking (e.g., Hellwig 2010), (ii) ensuring that banks have enough capital buffers for them to withstand shocks (e.g., Thakor 2014) or (iii) improving bank business conduct (e.g., Zingales 2015). One reasonable conclusion of these findings is that regulatory policies might have a limited outreach. Therefore, studying the context in which banks operate may provide additional explanations about other potential non-regulatory mechanisms that can work as a complement to regulations and supervision.

Another important issue is that bank regulation may work in undesirable directions. For instance, even if one of the regulatory goals is to reduce risk-taking incentives, research has found that banks may adjust their capital levels and at the same time increase their risks: Low-capitalized banks exhibit a negative relationship between capital and risk adjustments, whereas better capitalized banks maintain capital buffer levels by increasing risk when capital increases (Jokipii and Milne 2011, Kim and Santomero 1988). Furthermore, increasing capital requirements may also lead to a decrease in lending with adverse consequences for the real economy (Gropp et al. 2018). Again, given this evidence on the undesired effects of

regulations, it is economically important to study how other non-regulatory mechanisms could be complementary to formal regulatory tools.

With these aspects in mind, in this dissertation we have aimed at studying how non-regulatory incentives can address aspects that are of first-order importance for financial stability, and as a consequence, financial regulation. We have focused on three aspects: (i) how the stock-market influences determine the performance of banks across crises, (ii) how tax policy can incentivize higher capital ratios, and (iii) how social norms, specifically social capital, shape bank business conduct and customer relationships.

In the first chapter, we focus on the vulnerability of banks to crises. In particular, we study how stock-market forces determine the persistence of performance across crises. In this analysis, we observe that the persistence of business models that make banks more vulnerable across crises is not a specificity of publicly held banks but also applies to privately held institutions. However, we also show that among privately held banks there is a group of banks that perform well across crises. To deepen this analysis, we look at banks that make a private-to-public transition between crises. We observe that top performer banks in one crisis do not perform well during the next crisis after becoming publicly held, mainly if they are more subject to greater short-termist influences from the stock-market.

In the second chapter, we shift the focus to bank capital as a factor that makes banks more resilient to economic shocks. Particularly, we focus on the extent to which the tax system may provide incentives that lead to better capitalized financial institutions. We exploit a tax change that reduces significantly the unequal tax treatment between equity and debt with respect to interests and cost of equity deductibility and show that, when provided with a tax incentive on new equity increases, banks increase their equity ratios. We also show that the increase in capital ratios does not survive the removal of the tax incentive. When the debt-equity tax bias

is reestablished, banks reduce their equity ratios downwards. These findings document that tax incentives are an important determinant of bank capital structure.

In the third chapter, we focus on bank misconduct. In this chapter, we study how social capital can shape the behavior of banks. In particular, we document that social capital (defined as strength of civic norms and density of social networks) helps to cultivate a social/cultural environment surrounding bank headquarters that contributes to a reduction in the probability that a bank is involved in a misconduct case. Moreover, social capital exerts some discipline on wrongdoers. Once misconduct is revealed, banks tend to lose more deposits market-share in areas characterized by higher social capital levels.

The three chapters of this dissertation provide insights that are important for a better understanding of bank behavior. Our findings lead to some important contributions for the literature and inform the policy debate:

- Our findings in the first chapter provide an important contribution to the literature studying the effects of the stock-market on bank behavior. A first view of the literature is that the stock-market imposes market discipline by providing banks with incentives to conduct their business safely and soundly. A second view, mostly developed in the corporate finance literature, is that stock-market listing can induce short-termist influences that bias managers' decisions towards the short-run. The results of our paper are in line with the second view. We show that stock-market forces induce changes on banks that reduce their resilience to shocks. This finding contributes to the academic debate about the effects of stock-market influence by highlighting a negative consequence of stock-market listing on banks. From a policy perspective, the results of this chapter have implications for financial supervision. As performance and resilience to shocks are important issues for bank

regulation and supervision, our results provide information on how stock-market driven short-termism may induce changes in banks that affect their performance during crises.

- Our results in Chapter 2 contribute to the debate on how to tax banks properly (Roe and Troege 2017). Our findings document that the design of the corporate income tax system matters for banks as they change their capital ratios following changes in the design of the tax system. Even though taxes may be less important for larger financial institutions, we show that a different design of the corporate income tax (one that reduces the unequal treatment between equity and debt financing), can lead to a better capitalized banking sector. This chapter provides an interesting additional contribution on how tax policy can be used to boost capital buffers (Thakor 2014).
- Finally, our results in Chapter 3 provide interesting implications about how social capital helps to cultivate an environment surrounding bank headquarters that deter undesirable behaviors such as bank misconduct. This study contributes to the nascent body of literature that finds that cultural attributes and social norms influence managerial behavior. The literature in finance has traditionally focused on within bank factors that may affect misconduct. However, we provide additional evidence showing that external social norms (culture) influence bank business conduct. This chapter also shows how social capital, a factor that boosts trust in business relationships, becomes a disciplinary force when banks breach their customers' trust. This result provides novel evidence on how social norms can shape bank-customer relationships.

6. References

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