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Essays on Banking and Finance

Preface

A safe and healthy banking system is a central pillar of financial stability and even political stability. Systemic banking crises lead to political instability and extremism while the same cannot be said of macroeconomic downturns unrelated to banking crises (see e.g. Funke, Schularick, and Trebesch, 2016). Several factors have been identified as potential culprits for the recent financial crisis, among them insufficient bank liquidity, insufficient bank capitalization, risk-taking incentives induced by compensation structures, and moral hazard emanating from government safety nets. My dissertation aims to enhance our understanding of two of these factors: capitalization and compensation, or put differently, (equity) capital and human capital.

The evidence from the recent financial crisis suggests that insufficient capitalization can hamper the survival of banks during crises (see e.g. Berger and Bouwman, 2013). Nonetheless, some practitioners in the financial industry demand a roll-back of the increases in bank capital requirements of the post-crisis era arguing that bank equity reduces banks' ability to lend (Berger and Bouwman, 2013). In papers 1 and 2, my co-authors and I find that, instead, capital helps banks to better fulfill one of their central roles in the financial system, namely providing credit to support productive activities of non-financial firms.

The role of human capital has been the subject of similar controversy. There is lively academic debate on whether short-term oriented incentives from variable compensation contributed to excessive-risk taking in the run-up to the financial crisis. (see e.g. Fahlenbrach and Stulz, 2011; Bebchuk, Cohen, and Spamann, 2010; Bai and Elyasiani, 2013; Efing, Hau, Kampkötter, and Steinbrecher, 2015) Just like equity capital, human capital in banks has been subject to a string of regulations in the post-crisis era that aim to increase compensation transparency and limit incentives for excessive risk-taking. As in the case of capitalization, there are voices demanding a roll-back of compensation regulation, e.g. in the UK in the context of Brexit.[1] It is therefore

[1] See https://www.theguardian.com/business/2017/nov/29/eu-rule-capping-bankers-bonuses-

important to make use of the increased availability of granular reporting data to improve our understanding of compensation in banks and how it is similar or dissimilar to compensation in non-financial firms. In two papers, one co-authored (paper 3) and one single-authored (paper 4), I take advantage of one of the regulations increasing compensation transparency and analyze a dataset collected from European banks' disclosure on compensation of key employees relevant for risk-taking decisions.

In paper 1, which is co-authored with Michael Koetter and Stefano Colonnello, we study the role of bank equity in affecting bank responses to covenant violations of US corporate borrowers over the period 1994 to 2012. There are theoretical reasons both for a negative and a positive effect of equity on monitoring intensity. On the one hand, equity increases banks' "skin in the game" (see e.g. Holmstrom and Tirole, 1997) and therefore enhances incentives to closely screen and monitor borrowers. On the other hand, a higher equity cushion reduces banks' incentives to intervene aggressively once a borrower gets into financial distress because they can easily absorb the increased capital requirements for loans associated with covenant violations.

Theoretical work by Gorton and Kahn (2000) and Berlin and Mester (1992) shows how loan covenants are a mechanism that institutionalizes constant monitoring of borrowers' actions and financial health. When firms break loan covenants, banks have the right to intervene in its management and frequently this involves restrictions on firm investment (see e.g. Chava and Roberts, 2008). We use the intensity of the reduction in firm investment in response to covenant violations as a measure of monitoring. We find that better capitalized banks restrict borrower investment less in response to covenant violations. Crucially, this relatively lower intensity in monitoring has positive effects on borrower performance. Firms with lower investment restrictions fare better in terms of return on assets. Thus an equity cushion seems to allow banks to be more lenient with borrowers that have violated a covenant and prevent potentially value-destroying interventions.

We subject our findings to an additional test in a quasi-experimental setting using the 2009 stress test by the Federal Reserve in the US as a laboratory. The stress test was unexpected and therefore induced an exogenous increase in bank capital. The higher the increase in bank capital in response to the stress test, the more lenient banks were with borrowers violating a covenant, which corroborates our evidence on the role of equity buffers in loan monitoring.

In paper 2, which is co-authored with Ralph Setzer, we study the role of bank

could-be-scrapped-after-brexit-says-bank-boss.

health, of which bank capital constitutes one of the central pillars, in determining the success of labor market reforms in the euro area. The financial crisis and the subsequent European sovereign debt crisis left several countries exhibiting structural rigidities in labor markets with severely weakened banking systems. This raises the question of reform sequencing, i.e. how to time necessary structural reforms and a clean-up of the banking system.

We try to shed light on the interaction of bank health and the success of labor market reforms by studying structural reforms in euro area countries over the period 1999 to 2013. Theoretical work from Wasmer and Weil (2004) shows how entrepreneurs can only benefit fully from the easing of structural rigidities if they get the necessary financing from banks. Thus, we differentiate between firms connected to strong and weak banks and compare their employment gains in response to labor market reforms. We determine whether a bank is weak based on several balance sheet characteristics, most prominently bank capitalization, to proxy the financing situation of the mostly small and medium-sized enterprises in our sample. We show that firms connected to strong banks at the time of reform implementation increase employment significantly more than firms connected to weak banks. Moreover, we show that this interaction is most important for firms that are more dependent on bank financing.

With the third paper, I turn to the role of human capital in banks and how certain compensation patterns from the literature on non-financial firms help to explain compensation patterns in banking. From the superstar literature pioneered by Rosen (1981) we know that the compensation of highly skilled workers greatly depends on the circumstances, where they are put to use. The literature on superstars in firms, namely the CEOs, shows how high-skilled managers tend to match with the largest firms, where their talent has the highest marginal impact (see e.g. Gabaix and Landier, 2008). The concept of marginal returns to talent and the resulting nexus between size and pay was extended to banking by Célérier and Vallée (2019) to explain why top employees in finance tend to earn more than in other industries. They argue the immaterial nature of capital and the high standardization in banking increases the impact of workers' talent on a company's profit, which leads highly skilled workers to match with high-paying firms in finance. In the study co-authored with Konstantin Wagner, we take the concept of marginal returns to talent and the size-pay nexus one step further and explain compensation differences within the banking industry rather than across industries.

We use a novel hand-collected database on the compensation of key employees at the executive and non-executive level, the so called material risk takers (MRTs), to study compensation in European banks over the period 2014 to 2018. We find that investment bankers' pay depends more on the size of the investment banking unit they are working in than the pay of retail bankers depends on the size of their respective busienss unit. We argue that the differences in the strength of the size-pay nexus is driven by higher marginal returns in investment banking than in retail-banking. A talented investment banker devising a clever trading strategy will have a much larger marginal impact than a talented retail banker whose business is relatively standardized and less dependent on workers' individual talent. Turning from the business-unit to the bank-level, we show that compensation of talented investment bankers is not only higher in larger investment banking units but also in banks with a higher relative focus on investment banking. Moreover, this compensation differential increases further if the investment banker works in banks with relatively lower oversight, proxied by the number of MRTs in overhead business units such as controlling or accounting.

In the fourth paper, I investigate the role of human capital in banks when they get into distress. Again, I am interested in how compensation patterns from the literature on non-financial firms can be used to explain bank compensation. The theoretical and empirical literature on non-financial firms has long established that firms usually adjust to distress by cutting employment and leaving compensation of the remaining workers unchanged (see e.g. Azariadis, 1975; Abbritti and Fahr, 2013). There are, however, theoretical reasons to expect banks to behave differently in distress. On the one hand, the prevalence of variable compensation could mean that banks rely heavily on cuts in variable compensation to bring down the overall wage bill in distress (Efing, Hau, Kampkötter, and Rochet, 2018). On the other hand, the existence of government safety nets might imply that banks even increase variable compensation in response to distress to engage in risk-shifting and gamble for resurrection (Hakenes and Schnabel, 2014; Ongena, Savaser, and Sisli Ciamarra, 2018). Few empirical papers observe how bankers' compensation is affected by bank distress and I use the data on MRTs, already analyzed in paper 3, to shed light on this question.

I classify banks as stressed based on whether they failed or passed the 2014 asset quality review (AQR) stress test by the ECB and document how compensation and employment of MRTs differs for these two groups of banks over the period 2014 to 2018. I document that, just as one would expect from non-financial firms, stressed banks reduce employment of MRTs while leaving compensation of the remaining MRTs largely unchanged. Crucially, there is no evidence of bonus cuts as the main margin of adjustment and only weak evidence of increases in bonus payments. While these results could be interpreted as a rejection of theories on the centrality of variable compensation adustment, the convergence of bank compensation patterns in distress with those commonly observed for firms could also be driven by post-crisis regulation. This explanation would be in line with the long-run evidence provided by Philippon and Reshef (2012) that compensation patterns in banking ebb and flow with waves of regulation and de-regulation.

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Paper 1:

Benign Neglect of Covenant Violations: Blissful Banking or Ignorant Monitoring?

Benign Neglect of Covenant Violations: Blissful Banking or Ignorant Monitoring?*†

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Abstract

Theoretically, bank's loan monitoring activity hinges critically on its capitalization. To proxy for monitoring intensity, we use changes in borrowers' investment following loan covenant violations, when creditors can intervene in the governance of the firm. Exploiting granular bank-firm relationships observed in the syndicated loan market, we document substantial heterogeneity in monitoring across banks and through time. Better capitalized banks are more lenient monitors that intervene less with covenant violators. Importantly, this hands-off approach is associated with improved borrowers' performance. Beyond enhancing financial resilience, regulation that requires banks to hold more capital may thus also mitigate the tightening of credit terms when firms experience shocks.

JEL Classification: G21, G32, G33, G34

Keywords: Bank Monitoring, Covenant Violations, Syndicated Loans, Business Cycle

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1 Introduction

Loan monitoring and screening qualify banks as information producers and informed lenders. Numerous empirical determinants of monitoring have been explored, ranging from loan characteristics to business cycle conditions (Cerqueiro, Ongena, and Roszbach, 2016; Gustafson, Ivanov, and Meisenzahl, 2020; Becker, Bos, and Roszbach, 2019). But bank funding received little attention despite being a potentially crucial supply-side driver of monitoring. We fill this void by studying empirically banks' monitoring activity conditional on their capital (and debt) structure.

The relationship between a bank's reliance on equity capital and monitoring activity over loans is ex ante ambiguous. Equity may induce more intense monitoring if it mitigates moral hazard problems that entail too little effort by banks to exert scrutiny due to limited liability and reliance on deposit funding (Allen, Carletti, and Marquez, 2011). Such a problem is mitigated by market discipline inducing banks to hold equity capital, which typically exceeds minimum regulatory requirements. Several other theoretical papers also predict a positive link between bank capitalization and monitoring intensity (Holmstrom and Tirole, 1997; Coval and Thakor, 2005; Mehran and Thakor, 2011; Jayaraman and Thakor, 2014). More generally, the "equity monitoring hypothesis" (Schwert, 2018) posits that bank capital alleviates the moral hazard problems inherent to the banking business by giving managers more "skin in the game" and thus motivating them to screen and monitor borrowers more diligently.

Alternatively, equity may reduce the bank's incentives to monitor and intervene in the governance of the borrowing firm. Less capitalized banks may face *binding* increased capital charges if borrowers become troubled and have thus an incentive to monitor them closely. By contrast, a well-capitalized bank may not need to restrict borrowers' action set through monitoring, because it has a sufficiently large equity cushion to absorb increased capital requirements. We are not aware of formal theories that formulate exactly this "equity buffer hypothesis", but a similar conjecture is put forward by Chava and Roberts (2008). This argument mirrors the one developed by Chodorow-Reich and Falato (2018): a tougher bank's stance may reflect not only borrowers' but also the bank's declining financial health. By them same token, a better capitalized lender will be more lenient during borrowers' distress.

We evaluate these two alternative hypotheses using the US syndicated loan market as a laboratory. Syndicated loans are a primary source of funding for US corporations, with a volume of $2.4 trillion in 2017 (Sufi, 2007).[1] Given pervasive reforms pertaining to capital and liquidity regulation (Hancock and Dewatripont, 2018), we focus on relating monitoring intensity to bank funding structure measures in general and the

[1] See https://www.reuters.com/article/us-uslending-records/u-s-syndicated-lending-topples-records.

role played by regulatory capital in particular.

Following Chava and Roberts (2008) we link syndicate banks to US corporations to measure bank monitoring between 1994 and 2012. They show that borrowing firms cut investment after covenant violations because creditors intervene with the management of borrowers. Covenant violations provide a useful setting to study bank monitoring because they trigger a transfer of control rights from shareholders to creditors.

We document substantial cross-sectional and time-series variation in bank monitoring. Risk-adjusted Tier 1 capital ratios exhibit a statistically significant and large relationship with our monitoring metric. Better capitalized banks adhere to a more lenient monitoring stance towards troubled borrowers, which is associated with improved borrower performance. Well-capitalized banks appear to permit borrowers the pursuit of value-increasing projects also when they violate a covenant. The result that better capitalized banks adhere to a "hands-off" approach after covenant violations contradicts the argument that equity favors monitoring by giving bankers more "skin in the game". Instead, larger equity buffers seem to permit banks to smooth negative shocks of borrowers and avoid to constrain corporate investment policy. Improved borrower performance points, in turn, to an efficiency-enhancing role of bank equity rather than to a lender distraction story. Whereas it is commonplace to considerable monitoring a desirable activity, it can also be too much of a good thing. Carletti (2004) shows theoretically under which circumstances banks monitor borrower too much. Hence, a lack of equity capital may induce banks to demand inefficient investment cuts, a form of excessive monitoring.

To support a causal interpretation of this result, we exploit a quasi-experiment that provides plausibly exogenous variation in bank equity capital. The Supervisory Capital Assessment Program (SCAP or stress test) of 2009 forced a number of US banks to issue equity immediately after the publication of results. We use this episode as a positive unanticipated shock to bank capitalization. The increase in equity induced banks to keep a looser monitoring stance in the years after the stress test. Thus, regulatory equity appears to "buffer" shocks and allows a benign treatment of covenant violators.

Another important facet of funding structure is the composition of its debt. Existing theories focus on the distinction between deposits and other forms of debt. Calomiris and Kahn (1991) and Diamond and Rajan (2001) argue that the threat of bank runs by depositors disciplines bankers. Therefore, banks relying heavily on deposit funding would have more incentives to monitor in our context (the "fragility monitoring hypothesis" in Schwert, 2018).[2] The same economic mechanism may be at work for banks highly exposed to rollover risk on the wholesale short-term funding market.

We do not find evidence that predicting larger exposures to creditor runs induces

[2]Acharya, Mehran, and Thakor (2016) consider both the bright (loan monitoring) and the dark side (risk-shifting) of debt for banks, concluding that this trade-off can lead to multiple equilibria.

bankers to exert more monitoring effort. Banks with a more fragile debt structure, i.e., characterized by a higher reliance on deposit or short-term funding, do not monitor their borrowers significantly more intensely after covenant violations.

We conclude that well-capitalized banks seem to be the more patient monitors that are less likely to impose inefficient investment cuts on borrowers. This result complements existing theories (e.g., Holmstrom and Tirole, 1997), which focus on bankruptcy rather than on covenant violations (i.e., technical defaults). In contrast to bankrupt firms, covenant violators appear to be sufficiently healthy to survive certain shocks. Heavy-handed creditor interventions after violations may therefore, in fact, destroy value.

This paper contributes to three strands of the literature. First, it relates to studies on the effect of covenant violations on corporate policies, such as investment (Chava and Roberts, 2008), financing (Roberts and Sufi, 2009), governance (Nini, Smith, and Sufi, 2012), employment (Falato and Liang, 2016), and board structure, see Ferreira, Ferreira, and Mariano (2018) for this last point and an overview of this literature. We study (bank) heterogeneity in creditor-induced investment reactions to covenant violations, which we use as a measure of bank monitoring intensity.[3] Moreover, we investigate how covenant-violation induced investment reactions relate to changes in performance around the same events. We believe that the *joint* analysis of reactions of corporate policies to violations as opposed to the investigation of single measures in isolation is an important avenue to better understand the role of creditors in the governance of borrowing firms.

Second, we relate to empirical studies linking heterogeneity in bank monitoring to syndicate structure (Sufi, 2007), collateral values (Cerqueiro et al., 2016), securitization (Wang and Xia, 2014), and business cycle conditions (Becker et al., 2019). Besides providing an overview of the literature, Gustafson et al. (2020) use confidential regulatory syndicated loan data from the Shared National Credit Program (SNC) to show that higher lead arranger shares, shorter loan maturities, private borrowers, and a smaller number of covenants lead to higher monitoring effort. By contrast, Plosser and Santos (2016) use expanded SNC data and find that a bank's role in the syndicate does not affect monitoring intensity. According to them, monitoring effort is determined by the economic exposure of a bank, i.e., the absolute value of a bank's individual loan share relative to a bank's size. We contribute to this literature by exploring the role of banks' funding structure for monitoring heterogeneity. Our findings clearly underpin that bank capitalization is a crucial supply-side determinant of monitoring compared to other bank traits, such as the bank's debt structure, business model, and efficiency.

[3] Roberts (2015) relates renegotiation outcomes after violations to *aggregate* banking sector leverage.

Third, our paper complements the literature that links observable financial health indicators of lenders to borrower actions. Murfin (2012) shows that better capitalized banks design looser covenants. Whereas he considers equity-induced bank heterogeneity in loan contracting, we investigate how capitalization influences bank heterogeneity in responses to covenant violations. The studies most closely related to ours are Chodorow-Reich and Falato (2018) and Acharya, Almeida, Ippolito, and Perez-Orive (2019). Both use changes in bank balance sheet characteristics during the financial crisis to explain heterogeneity in bank responses to covenant violations. Using SNC data, Chodorow-Reich and Falato (2018) show that during the financial crisis lenders used covenant violations as an opportunity to cut credit exposure that otherwise would have been hard to reduce given loans' high average maturity. Acharya et al. (2019) corroborate the findings of Chodorow-Reich and Falato (2018) using publicly available data on credit lines. These two studies examine one extreme of the whole spectrum of monitoring that we are considering. During a crisis, distressed banks may be less interested in intervening in the borrowing firms' management but rather want to implement lump-sum cuts in their loan book. Our study tests whether bank funding structure explains differences in monitoring looking over the entire business cycle, mitigating external validity concerns. Our results may thus provide guidance to policy-makers interested in designing regulation that brings banks closer to the optimal level of monitoring effort.

2 Empirical approach

We explain the economic intuition why and the empirical methods how we measure monitoring intensity in the context of covenant violations before relating it to bank traits.

2.1 Bank monitoring and covenant violations

The main goal of our analysis is to study how a bank's monitoring effort correlates with its characteristics, insulating their role from that of the borrowing firm's characteristics.

Bank monitoring activity is inherently elusive. Most studies therefore measure it indirectly, assuming that certain features of the bank-borrower relationship (e.g., closer geographical distance or loan concentration among syndicate members) are conducive to more intense monitoring (see, e.g., Sufi, 2007). Other, more recent studies take a different approach and look at observable monitoring activities.[4]

[4]Gustafson et al. (2020) look at banks' meetings with borrowers and on-site visits. Cerqueiro et al. (2016) and Becker et al. (2019) measure monitoring as the frequency of borrowers or collateral reviews. Plosser and Santos (2016) infer monitoring activity from changes to banks' internal borrower ratings.

These approaches focus either on specific loan characteristics linked to monitoring effort (e.g., the lead bank's share in syndicated loans) or on specific monitoring actions (e.g., collateral reviews). We follow a different route and reverse engineer banks' monitoring intensity starting from the effect of their actions on borrowing firms' policies. A main challenge is to impute changes in borrowing firms' policies to banks' monitoring actions. Our approach is to consider events when banks are likely to take monitoring actions. In line with Bird, Ertan, Karolyi, and Ruchti (2017), we use changes in borrowing firms' investment policy around violations of financial covenants contained in syndicated loan contracts as a proxy for banks' monitoring intensity.

Financial covenants set limits on accounting-based measures of financial health and performance (e.g., on net worth or current ratio) of borrowing firms. Loan covenants are commonly maintenance-based. Debtors must comply with the limits set in the loan contract at the end of each fiscal quarter (Nini et al., 2012). A covenant violation constitutes a technical default, after which the creditors can impose the immediate repayment (acceleration) or the termination of the loan. Creditors mostly use the threat of such actions to renegotiate the debt contract and extract concessions from borrowers (Roberts, 2015).

According to the theoretical work by Gorton and Kahn (2000) and Berlin and Mester (1992), monitoring entails renegotiating loan terms upon the arrival of new information about the firm's prospects. In their models, covenants and their violation are a mechanism to institutionalize regular renegotiations. After a violation, a lender can choose to liquidate certain projects of the borrower to prevent risk-taking. This is exactly what we are measuring in the form of restrictions on firm investment. More broadly, Nikolaev (2018) defines monitoring as both acquiring timely information about borrowers and acting upon that information to exert control on management. While monitoring measures such as loan reviews (Plosser and Santos, 2016), site visits, and borrower meetings (Gustafson et al., 2020) entail only the first part of that definition, our measure incorporates both parts since the lender has to acquire information to detect the violation.

Chava and Roberts (2008) and Nini et al. (2012) provide both anecdotal and large sample evidence consistent with increased monitoring following covenant violations (e.g., through increased frequency of required compliance reports). Whereas the change in investment policy linked to the resolution of the technical default can reflect a host of bank-side actions (typically changes in loan terms – interest rate, maturity, credit line availability, etc. – that make the borrower more financially constrained), it seems sensible to think that such actions capture also "pure" monitoring.

In sum, covenant violations provide a useful setting to study banks' monitoring activity for three reasons. First, they give a specific channel through which creditors can intervene in the governance of the borrowing firm, namely a formal transfer of control rights from shareholders to creditors. Second, covenant violations are widespread and involve also relatively healthy firms, thus providing a more complete picture of the role of creditors in borrowing firms (Nini et al., 2012). Third, the management of borrowing firms' only has limited ability (and incentives) to manipulate the firm's accounting ratios to avoid covenant violations (Roberts and Sufi, 2009). This feature and the discrete nature of covenant violation around the covenant threshold lend themselves to a regression discontinuity design (RDD), commonly used in the literature starting from Chava and Roberts (2008), which we discuss below more in detail.

2.2 Investment and covenant violations

As a preparatory analysis, we study the behavior of violating firms' investment around covenant violations without conditioning on the lender. The goal is to link our core analysis on observable differences in bank funding structure described below to the contraction in investment commonly observed in the literature (Chava and Roberts, 2008).

The borrowing firm's treatment status (violating vs. non-violating) exhibits a discontinuity with respect to the distance between the observed accounting ratio and the contractual covenant threshold. We exploit this discontinuity for identification purposes in a RDD at the firm-quarter level in the spirit of Chava and Roberts (2008) to isolate the effect of financing frictions on investment as follows:[5]

$$I_{f,q} = \alpha \cdot v_{f,q-1} + \boldsymbol{\eta}\mathbf{x}_{f,q-1} + \boldsymbol{\zeta}\mathbf{p}_{f,q-1} + \gamma_f + \gamma_q + \epsilon_{f,q}, \tag{1}$$

where f and q denote the borrowing firm and the (quarterly) period. $I_{f,q}$ is the firm's investment rate. The treatment variable is the firm-quarter-level covenant violation indicator $v_{f,q-1}$ defined as

$$v_{f,q-1} = \begin{cases} 1 & \text{if } z_{f,q-1} - z^0_{f,q-1} < 0 \text{ for any covenant in loans of firm } f \\ 0 & \text{otherwise,} \end{cases} \tag{2}$$

where $z_{f,q-1}$ is the observed value of the accounting measure restricted by the covenant and $z^0_{f,q-1}$ is the most binding covenant threshold contained in any of the firm's outstanding syndicated loan contracts. In this firm-quarter-level analysis, $v_{f,q-1}$ equals one if the firm violates any covenant in any of the outstanding loans. For a given accounting measure, the *relative* distance $(z_{f,q-1} - z^0_{f,q-1})/z^0_{f,q-1}$ is defined with respect to the tightest covenant threshold across the different outstanding loans at a

[5]This analysis is a sharp RDD because of the deterministic assignment rule into treatment and non-treatment. A caveat is that banks and firms can renegotiate the contract in anticipation of a violation. See Denis and Wang (2014) on firm policies after renegotiations outside of actual covenant violations.

given point in time. Thus, the assignment variable is the relative distance between the actual accounting measure and the threshold. Hence, a violation is not more severe simply because the level of the accounting measure and the corresponding threshold are relatively high to begin with.

We control for a vector of covariates $\mathbf{x}_{f,q-1}$ including Tobin's q, the contemporaneous cash flow, and the natural logarithm of total assets of the borrowing firm. We use a second-order polynomial of the relative distance of the different accounting measures from the tightest covenant threshold to specify a vector of smooth functions $\mathbf{p}_{f,q-1}$ (Gelman and Imbens, 2018). The inclusion of $\mathbf{p}_{f,q-1}$ improves the identification of the treatment effect α around the discontinuity and captures any information these distance measures may convey about the firm's growth prospects (Falato and Liang, 2016). Firm (γ_f) and time (γ_q) fixed effects absorb time-invariant differences in investment policy across borrowing firms and macroeconomic conditions. Error terms $\epsilon_{f,q}$ are clustered at the firm-level.

We repeat the analysis of investment around covenant violations, but treat each syndicated loan as a set of separate loans, one for each bank in the syndicate. The unit of observation is the loan-bank-firm-quarter, so that we can focus on the heterogeneity in investment responses depending on the bank from which the firm borrowed. We use this setting in our main analysis below and execute a RDD specified as follows:

$$I_{l,b,f,q} = \alpha \cdot v_{l,q-1} + \boldsymbol{\eta}\mathbf{x}_{f,q-1} + \boldsymbol{\zeta}\mathbf{p}_{l,q-1} + \gamma_{b,y} + \gamma_f + \gamma_q + \gamma_e + \epsilon_{l,b,f,q}, \tag{3}$$

where l, b, and y denote the syndicated loan deal, the lending bank, and the year, respectively. We add bank-year ($\gamma_{b,y}$) and fiscal quarter (γ_e) fixed effects to control for time-varying heterogeneity in investment across different banks' borrowers outside covenant violations and seasonality, respectively. The treatment variable is the loan-quarter-level covenant violation indicator $v_{l,q-1}$ defined as

$$v_{l,q-1} = \begin{cases} 1 & \text{if } z_{f,q-1} - z^0_{l,q-1} < 0 \text{ for any covenant in loan } l \\ 0 & \text{otherwise,} \end{cases} \tag{4}$$

where the difference relative to the firm-quarter-level indicator (2) lies in the covenant threshold $z^0_{l,q}$, which is now loan-specific.[6] In this setting, $v_{l,q-1}$ is equal to one if the firm violates any of the covenants contained in a given loan. Analogously to (1), we include a vector of smooth functions $\mathbf{p}_{l,q-1}$ of the relative distance between the different accounting measures and the loan-level covenant-threshold. As before, we only observe

[6]Thus, we do not need to focus on the tightest covenant. Time-subscripts indicate dynamic covenant thresholds. Current ratio thresholds might increase over time and net worth thresholds might increase with net income. As in Chava and Roberts (2008), we linearly interpolate initial and final covenant thresholds over the life of the loan.

borrowing firms' investment at the firm-quarter-level and the notation $I_{l,b,f,q}$ reflects the repetitive nature of our data structure. Because of this feature, we use two-way clustering by bank and time in the error term $\epsilon_{l,b,f,q}$ in line with Schwert (2018).[7]

In both specifications (1) and (3), the parameter α captures the treatment effect. The RDD allows us to identify the treatment effect as long the error terms ($\epsilon_{f,q}$ or $\epsilon_{l,b,f,q}$) do not exhibit the same discontinuity with respect to the threshold distance as the treatment variable (Falato and Liang, 2016).

We follow Chava and Roberts (2008) and estimate both specifications (1) and (3) without firms that never violate any covenant, but deviate slightly in the definition of the sample of violating firms and of the violation indicator ($v_{f,q-1}$ or $v_{l,q-1}$). First, we remove loans for which the firm is in violation in all quarters of their lifetime.[8] Second, we do not consider covenant violations as events that happen right at the beginning of a loan's lifetime. This approach allows us to improve comparability in terms of covenant design within our sample of loans by excluding those loans that are characterized by very strict covenants. Third, once a firm violates a covenant for the first time for a given loan, we require at least four quarters without a violation before we code another breach as a "new violation" in the same spirit as Nini et al. (2012). In this way, we aim to capture instances in which there is an actual transfer of control rights from shareholders to creditors. Unreported tests show the (in)sensitivity of the main results vis-à-vis monitoring coefficients obtained after accounting for covenant violations satisfying different combinations of these sample restrictions. Results are available upon request.

2.3 Heterogeneous effects of covenant violations across banks

The RDD specifications described so far do not capture heterogeneity across banks in borrowing firms' investment changes in the wake of covenant violations. We pursue a two-step approach to augment specification (3) to study bank heterogeneity in terms of capitalization, funding structure, and business models.

First, we use the variables defined as above to estimate the RDD specification:

$$\begin{aligned} I_{l,b,f,q} =& \alpha \cdot v_{l,q-1} + \sum_b \sum_y \beta_{b,y} \cdot v_{l,q-1} \times \gamma_{b,y} \\ &+ \boldsymbol{\eta} \mathbf{x}_{f,q-1} + \boldsymbol{\zeta} \mathbf{p}_{l,q-1} + \gamma_{b,y} + \gamma_f + \gamma_q + \gamma_e + \epsilon_{l,b,f,q}. \end{aligned} \tag{5}$$

Relative to equation (3), equation (5) interacts $v_{l,q-1}$ with bank-year fixed effects ($\gamma_{b,y}$).[9]

[7] We estimate specifications with rich sets of fixed effects by means of the Stata package REGHDFE, which implements the estimator proposed by Correia (2016).

[8] In our sample, 35.8% of all loans are violated at least once. Of these, roughly 18.5% (or 6.6% of our sample) are violated in all quarters of their lifetime.

[9] Ideally, we would interact $v_{l,q-1}$ with bank-quarter fixed effects rather than bank-year fixed

The parameters of interest are $\beta_{b,y}$, which gauge the time-varying component of bank-specific treatment effects of covenant violations on investment.

In the second step, we specify the estimated coefficients $\hat{\beta}_{b,y}$ as the dependent variables to study the relationship between $\hat{\beta}_{b,y}$ and bank funding structure, controlling for bank's business model traits. The bank-year panel specification to estimate is:

$$\hat{\beta}_{b,y} = \psi + \boldsymbol{\theta}\boldsymbol{\Gamma}_{b,y-1} + \upsilon_{b,y}, \tag{6}$$

where $\boldsymbol{\Gamma}_{b,y-1}$ is a vector of bank characteristics at annual frequency capturing funding structure through the level of equity capital (leverage ratio, risk-adjusted Tier 1 capital ratio) and debt composition (deposits and short-term funding), as well as the bank's business model through the scope of activities (non-interest income, trading activity, and bank size) and technology and efficiency (non-performing assets, net income, and cost-to-income ratio) of the bank. All variables in $\boldsymbol{\Gamma}_{b,y-1}$ are measured as of the last quarter of the year and lagged by one year. We first estimate univariate regressions for each of the bank characteristics contained in $\boldsymbol{\Gamma}_{b,y-1}$ and then a multivariate regression for the entire vector of covariates. In additional tests, we also interact $\boldsymbol{\Gamma}_{b,y-1}$ with measures of macroeconomic conditions to investigate how the role of different bank characteristics varies over the business cycle.

Whereas the first-step RDD estimates plausibly allow for causal inference on the (bank-time-specific) treatment effect of covenant violations on investment, the second step provides only correlations. As pointed out by Chodorow-Reich and Falato (2018) in a similar setting, to interpret $\boldsymbol{\Gamma}_{b,y-1}$ estimates causally, we would need to have "as good as random" matching between borrowers and banks. Unlike Chodorow-Reich and Falato (2018), we do not focus on the years around the Great Recession to achieve such a condition, thus we are left with arguably non-random matching (Schwert, 2018).

Our solution is to conduct two quasi-experiments within the second-step estimation. To test the implications of bank equity and funding fragility for monitoring intensity, we exploit plausibly exogenous shocks to (i) equity capital resulting from the US banks' assessment in the SCAP stress test of 2009 and (ii) exposure to bank runs following changes in the deposit insurance coverage around the world, respectively. These experiments scrutinize if the baseline correlation analysis between bank monitoring and funding structure supports a causal interpretation. We provide more details in Section 5.3.

Two caveats concerning the two-step approach remain. First, whereas we clus-

effects. Yet small banks experience only very few covenant violations in a specific quarter. This can lead to situations where all covenant violations on loans extended by a small bank in a given quarter are happening for loans that were syndicated together with other, larger banks in our sample. In those cases, it is problematic to disentangle the role of small banks from that of large players in the market. Therefore, we cannot estimate many bank-quarter-specific violation coefficients. To alleviate this issue, we interact $v_{l,q-1}$ with less granular fixed effects at the bank-year level.

ter standard errors by bank in equation (6), the dependent variable $\hat{\beta}_{b,y}$ is generated, which may require further corrections of standard errors because of measurement error (Gawande, 1997; Feenstra and Hanson, 1999; Dumont, Rayp, Thas, and Willemé, 2005). Assuming that the measurement error $(\hat{\beta}_{b,y} - \beta_{b,y})$ is uncorrelated with the error term $\upsilon_{b,y}$, the OLS estimator $\hat{\boldsymbol{\theta}}$ is consistent, but suffers from inflated standard errors, possibly leading to an under-rejection of the null hypothesis of non-significance (Roberts and Whited, 2013).[10]

Second, by construction the sample size in the second step is substantially smaller than in the second step, which limits statistical power and may entail an under-rejection of the null hypothesis of non-significance.

Appendix Section 5 presents a one-step approach addressing both caveats, which is less flexible though to study bank monitoring behavior. Therefore, we report in the remainder results from the two-step procedure.

3 Data

We describe data sources, sample selection, variable construction, and summary statistics.

3.1 Data and sample selection procedure

We use data on syndicated loans, borrowing firms, lending banks, and macroeconomic conditions. Syndicated loan data is from the Thomson-Reuters' Loan Pricing Corporation DealScan (Dealscan) database. We use quarterly accounting and stock price data about US public firms from the the Center for Research in Security Prices/Compustat merged (CCM) database, excluding financial institutions and utilities. We drop firm-quarters with missing information about sales, number of shares outstanding, stock price, and calendar date. We also drop firm-quarters for which net property, plant, and equipment (PPE) is below \$1M, for which leverage is zero, or for which the market (book) leverage lies outside of the unit interval. We match them to the syndicated loans using the link file provided by Michael Roberts, which builds on the sample of Chava and Roberts (2008).

We use bank quarterly balance sheet data from Compustat Banks, supplemented with Bankscope if information are missing for the 20 most active lenders. Syndicated loan and bank data are combined using the link file made available by Michael Schwert (2018). As a result, we focus on the 103 most active banks on the US syndicated loan market, of which 87 are covered by Compustat Banks. Unlike most of the literature, we sample all syndicate members and not only lead banks. Macroeconomic data are

[10] With a slight abuse of notation, we denote both the OLS estimator and the actual estimate as $\hat{\beta}_{b,y}$.

retrieved from the Federal Reserve Economic Data (FRED), St. Louis Federal Reserve Bank.

The sample starts in 1994, which is the first year when Dealscan provides sufficiently comprehensive information about covenants (Chava and Roberts, 2008). The sample runs until 2012, which is the last year covered by the Dealscan-CCM link file of Michael Roberts. We focus on Dealscan loans containing covenants on (tangible) net worth or the current ratio as in Chava and Roberts (2008) and build a matched quarterly panel of firms, which are assumed to be subject to a given covenant up to the maturity date of the corresponding loan. We identify covenant violations by testing if the observed (tangible) net worth or current ratio complies with the contractual threshold. This approach might result in some false positives, but enables us to measure the distance between the accounting quantity and the covenant threshold to enhance identification in the RDD.

We treat each syndicated loan as a number of separate loans to gauge heterogenous bank behavior, i.e., a loan deal of a given borrowing firm with n different banks enters as n separate bank-firm deals. As in Schwert (2018), deal-bank-firm triplets are the panel unit of analysis to study quarterly covenant violations as opposed to firm-quarter level violations in Chava and Roberts (2008).

3.2 Variable construction and summary statistics

In our analysis, we rely on borrowing firm-level and bank-level time-varying characteristics. Concerning borrowing firms' variables, investment is defined as capital expenditures over last quarter's PPE. Tobin's q is defined as total assets minus book equity plus market capitalization scaled by total assets. Cash flow is defined as income before extraordinary items plus depreciation and amortization over last quarter's PPE. We use the natural logarithm of total assets as a proxy for firm size. Return on assets (ROA) is defined as income before extraordinary items scaled by total assets.

To explain variation in monitoring intensity, we employ a host of bank characteristics contained in the vector $\mathbf{\Gamma}_{b,y-1}$ of the second-step specification (6).[11] The leverage ratio (common equity/assets) and the risk-adjusted Tier 1 capital ratio capture the bank's level of equity capital. Deposits-to-total assets and short-term funding-to-total assets speak to the composition of its debt. The natural logarithm of total assets (i.e., bank size), non-interest income over total revenue (i.e., the reliance on non-traditional banking services) and assets held for trading scaled by total assets (i.e., the involvement in trading activities) relate to the range of activities the bank operates in. To

[11] A caveat is the neglect of syndicate loan shares. Studies using publicly available datasets highlight the role of the lead arranger's loan share (see, e.g., Lee and Mullineaux, 2004; Sufi, 2007). But administrative data yields mixed evidence on whether the syndicate role (Gustafson et al., 2020) or rather participants' economic exposure (Plosser and Santos, 2016) are key to explaining monitoring intensity.

proxy for the monitoring technology and overall efficiency of the bank, we specify non-performing assets-to-total assets, net income-to-total assets, and the cost-to-income ratio. Appendix Table A.1 provides the list of 51 banks for which all of these variables are available for at least one year and can thus be included in the sample for the second-step estimation. These 51 banks still capture a large fraction of the market, namely 57.3% of all deals extended by our sample banks, calculated on the facility-level as in De Haas and Van Horen (2013) (64.7% of the total credit).

Finally, we measure US macroeconomic conditions by using an indicator variable for National Bureau of Economic Research (NBER) recessions, the National Financial Conditions Index (NFCI), and the Chicago Fed National Activity Index (CFNAI).

Table 1 shows summary statistics for firm variables in and outside covenant violations (Panel A and Panel B, respectively), bank characteristics (Panel C) and selected deal loan characteristics (Panel D). Covenant violating firms exhibit lower investment, cash flows, and ROA than other firms. They are also smaller and more levered. On average, the loan syndicates in our sample comprise 5.21 institutions, and 95% of deals include at least one revolver loan, arguably a monitoring intensive credit type. All firm and bank variables are winsorized at the 1st and 99th percentile. All monetary variables are expressed in millions of 2010 dollars. We provide detailed variable definitions in Appendix Table A.2.

4 Investment and covenant violations

As a building block for our subsequent tests on bank heterogeneity, it is important to verify that we obtain the well-known result of a reduction in investment due to covenant violations (Chava and Roberts, 2008; Nini et al., 2012).

The use of an RDD relies on the assumption that the running variable (i.e., the accounting ratio regulated by a covenant in our case) cannot be manipulated. This assumption is unlikely to be violated in our setting. As discussed extensively by Chava and Roberts (2008), lending relationships are valuable and firms are reluctant to risk their relationship and general reputation by manipulating their books. Nonetheless, in Appendix Figure A.1 we implement manipulation tests of the running variables based on the smooth local polynomial density estimator of Cattaneo, Jansson, and Ma (2019), who build on the approach of McCrary (2008). Reassuringly, we cannot reject the null hypothesis of no manipulation for any of the three accounting measures (net worth, tangible net worth, and current ratio). All figures clearly suggest that there is no discontinuity around the threshold (of zero relative distance).

Given this RDD validity check, Table 2 reports estimates of regression specifications studying the effect of covenant violations on borrowing firms' investment, without conditioning on the lending bank. In columns 1 and 2, we use the same firm-quarter data structure of Chava and Roberts (2008) and estimate equation (1). Reassuringly, we find a statistically significant reduction in investment linked to covenant violations. Column 1 focuses on the period 1994-2005 – the same used by Chava and Roberts (2008) – and the estimated magnitude of the change in investment of -0.8% is consistent (column 7 of their Table V (Panel A)). Column 2 extends the analysis to the entire sample period 1994-2012, yielding an effect that is only slightly smaller in magnitude.

In columns 3 and 4, we resort to our repetitive deal-bank-firm-quarter data structure and estimate equation (3). We still find a decline in investment following covenant violations, which is, however, statistically insignificant at conventional levels. The magnitude of the reduction over the deal-bank-firm-quarter data structure declines and ranges between -0.3% and -0.2%. This result is arguably a mechanic effect, which reflects that firms with multiple deals outstanding may be in violation of covenants for multiple deals at the same time. Consider, for example, a firm with two deals outstanding (deal 1 and deal 2), both containing a covenant on the current ratio (with thresholds at 175% and 150%, respectively). Assume that the firm's current ratio declines to 170% in period t, which violates deal 1's covenant. After t, the firm's current ratio declines further and reaches 145% in period $t + 2$, thus breaching also deal 2's covenant. The first transfer of control rights to creditors happens at time t, so that we are most likely to observe the sharpest reduction in investment between t and $t + 1$. The effect of the second violation between $t + 2$ and $t + 3$ is, in turn, arguably milder. In addition, columns 3 and 4 include bank-year fixed effects, which may also absorb part of the effect of covenant violations.

The estimated unconditional effect of covenant violations may mask important heterogeneity in the course of action followed by different lenders. We study next heterogeneous investment effects across banks and time, i.e., our proxy for bank monitoring intensity.

5 Heterogeneous effects of covenant violations across banks

The granular deal-bank-firm-quarter data structure allows us to scrutinize heterogeneity in monitoring and its relationship with bank funding structure and business cycle conditions. Our two-step approach consists of (i) a first step to isolate heterogeneous effects of covenant violations on investment across lending banks and time, and (ii) a second step to correlate these effects with bank funding structure and business cycle conditions (controlling for other bank time-varying characteristics).

5.1 First step

To tease out bank-induced heterogeneity in borrowers' investment response to violations through time, we estimate specification (5) in column 1 of Table 3. In this way, we obtain a vector of bank-year-specific coefficients that capture (heterogeneous) monitoring effects, namely $\hat{\beta}_{b,y}$. These coefficients measure the difference in the violation effect relative to the reference group, namely deals by Bank of America (BoA) in 2003.[12]

An F-test of joint significance rejects the null hypothesis that our monitoring effects $\hat{\beta}_{b,y}$ are equal to zero. In terms of economic significance, these effects exhibit an interquartile range of $0.0175 - (-0.0071) = 0.0246$, which is roughly $0.025/0.057 = 44\%$ of the mean investment rate in the regression sample. Thus, these simple tests suggest that bank heterogeneity in monitoring is both statistically and economically important.

Columns 2 and 3 show results where we specify ROA and Tobin's q as dependent variables to obtain bank-year-specific effects of covenant violations on borrowing firms' accounting performance and market value: $\hat{\beta}^{ROA}_{b,y}$ and $\hat{\beta}^{q}_{b,y}$. The F-tests corroborates the existence of an important degree of heterogeneity across bank-years. Below, we explore the correlation of $\hat{\beta}^{ROA}_{b,y}$ and $\hat{\beta}^{q}_{b,y}$ with our monitoring measure $\hat{\beta}_{b,y}$.

Given the large size of the vector $\hat{\beta}_{b,y}$ obtained from the specification shown in column 1 of Table 3, we provide a visual analysis in Appendix Figure A.2 rather than tabulating all the bank-year monitoring coefficients. In total, we estimate 640 coefficients and the left graph of Appendix Figure A.2 shows their distribution over time. Note that we do not obtain a balanced bank-year panel of monitoring coefficients for the second-step analysis. One reason is that several banks drop out of the sample early due to M&A activity, such as Bank One that was purchased by JPMorgan (JPM) in 2004. Other banks only exhibit covenant violations as of the late 1990s. The right graph of Appendix Figure A.2 shows the empirical density of the bank monitoring coefficients. Whereas the distribution peaks at 0%, we observe a substantial degree of heterogeneity.

To further explore bank heterogeneity, in Figure 1 we visualize the distribution of the monitoring coefficients year-by-year through box plots. Heterogeneity across banks is not just an artefact of changes in business cycle conditions over the sample period. The resulting variation in bank monitoring coefficients within each single year is substantial. Annual distributions reflect what we observe over the entire sample, i.e., a right-skewed distribution with a median slightly above zero. Nonetheless, time-series variation matters, as witnessed by fluctuations in both the central tendency (median) and dispersion (interquartile range) of our monitoring coefficients.

Overall, our first-step estimates point to a substantial degree of heterogeneity in banks' monitoring intensity following covenant violations.

[12]BoA is the reference bank, because it is most active in terms of number and volume of deals (Appendix Table A.1). Likewise, the reference year 2003 has most observations. Choosing the second-most active bank, JPM, leaves results intact. We do not report the coefficient estimate for the violation indicator in Table 3, because it provides only reaction information in the reference bank-year, which is devoid of interest per se.

5.2 Second step

Next, we link the heterogeneity in monitoring documented in the first step to banks' funding structure in general and capitalization in particular given that the latter plays a central role in many theoretical models of bank monitoring activities.

We implement the second step of the approach by estimating specification (6) and report coefficient estimates in Table 4. Columns 1 to 10 report univariate specifications for each of the bank characteristics contained in $\mathbf{\Gamma}_{b,y-1}$, whereas the model in column 11 includes the entire vector of bank characteristics. The model in column 12 features only bank traits that exhibit univariate significance (Tier 1, total assets, non-interest income, non-performing assets, and net income). Only for Tier 1, size, and non-performing assets we find a statistically significant relationship with $\hat{\beta}_{b,y}$ in each specification.

The positive link between $\hat{\beta}_{b,y}$ and Tier 1 capital brings further support to the equity buffer hypothesis, whereas it does not line up with the equity monitoring hypothesis. More capitalized banks – for which increased capital requirements stemming from violations are less likely to bind – appear to be more lenient towards violating firms, allowing them to invest more.[13] Also non-performing assets correlate positively with $\hat{\beta}_{b,y}$, which suggests that banks with a worse screening technology are less strict as monitors since a higher $\hat{\beta}_{b,y}$ corresponds to looser monitoring. In contrast, our estimates do not support theories emphasizing the fragility of banks' funding to explain monitoring efforts.

The positive and statistically significant coefficient of the Tier 1 ratio is robust to (i) controlling for overall bank quality by means of indicator variables reflecting poor accounting ratios exhibited by a bank (Appendix Table A.5), (ii) controlling for nonlinearities in the effect of regulatory ratios (Appendix Table A.6), and (iii) controlling for bank activity in the syndicated loan market (Appendix Table A.7).

Whether increased bank leniency – linked, for instance, to Tier 1 capital – is efficient or a symptom of distraction by bank monitors is an empirical question. We thus study how bank interventions captured by the coefficients in $\hat{\beta}_{b,y}$ correlate with the borrowing firms' performance around the same covenant violation events.

In Table 5, we examine the correlation between $\hat{\beta}_{b,y}$ and $\hat{\beta}^{ROA}_{b,y}$ ($\hat{\beta}^{q}_{b,y}$), the bank-year specific violation effect on ROA (Tobin's q) also obtained from the estimations in Table 3.[14] In column 1, we uncover a positive and significant relationship between $\hat{\beta}_{b,y}$ and $\hat{\beta}^{ROA}_{b,y}$. This result may seem at odds with the positive effect of covenant

[13] Appendix Table A.3 shows the bank-years without coefficient estimates from specification (5). Endogenous covenant design (Murfin, 2012) may, *inter alia*, determine a lack of observed violations for a given bank-year leading to a missing coefficient estimate. In Appendix Table A.4, we explore how the absence of such an estimate relates to bank observable characteristics. In line with the equity buffer argument, the association between the availability of $\hat{\beta}_{b,y}$ coefficient and Tier 1 capital is negative.

[14] Since $\hat{\beta}_{b,y}$ is a generated regressor, we adjust standard errors following Bertrand and Schoar (2003).

violations on ROA shown by Nini et al. (2012), but it can actually be reconciled with their findings. They document a negative (positive) effect of covenant violations on investment (performance), but they do not regress the violation-related adjustment in investment on the violation-related adjustment in performance.[15] To the best of our knowledge, we are the first to show that the positive effect of covenant violations on performance is driven by those instances in which the lending banks act in a more lenient fashion regarding their intervention behavior. This inference is corroborated by the positive – although insignificant – relationship between $\hat{\beta}_{b,y}$ and Tobin's q $\hat{\beta}^{ROA}_{b,y}$ in column 3. All in all, these results point to the efficiency of banks' leniency after covenant violations.

This result suggests that banks reacting strictly to violations pursue an inefficient solution, at least from the perspective of the borrowers. In light of the result on Tier 1, a possible explanation is that these banks are constrained in their choice set due to their relatively low capitalization. Rather than opting for the course of action maximizing borrowing firms' value, they chose to impose investment restrictions to protect their short-term claim on a borrower's cash flow. In other words, their action can be seen as an example of excessive monitoring.

The idea of excessive monitoring may seem counterintuitive at first sight. As noted by Pagano and Röell (1998), researchers in corporate finance usually think about settings in which principals provide too little monitoring due to free-riding. But from the viewpoint of firm owners, monitoring can be excessive. Specifically, Pagano and Röell (1998) and Burkart, Gromb, and Panunzi (1997) show how shareholders' overmonitoring can reduce firm value by disincentivizing managers from showing initiative and finding new investment projects. Specific to the case of monitoring by banks, Besanko and Kanatas (1993) and Carletti (2004) illustrate that in certain principal-agent settings banks monitor excessively and maximize utility at the expense of borrowers. Another strand of theoretical literature on inefficient bank interventions investigates financial contracting as a means to alleviate liquidation bias in distress (e.g., Gennaioli and Rossi, 2013).

Overall, the second-step results clearly support the equity buffer hypothesis. Better capitalized banks are more benign monitors of covenant violating firms. This monitoring style is associated with improved borrower performance, pointing to its efficiency rather than to distraction or shirking of managers and loan officers of well-capitalized banks.

Additional tests show that the bank monitoring measure does not correlate with the state of the business cycle (Appendix Figure A.3 and Appendix Table A.8). Likewise,

[15] In unreported results based on the the firm-quarter data structure of Chava and Roberts (2008), we also find a positive and significant effect of violations on the borrowing firms' ROA, which is perfectly in line with Nini et al. (2012).

the baseline results on the role of equity and debt structure for monitoring are robust to using a one-step procedure that does not suffer from the econometric issues discussed in Section 2.3 (Appendix Figure A.4 and Appendix Tables A.9, A.10, A.11).

5.3 Quasi-experimental evidence

We use the 2009 US SCAP stress test to draw causal inference on the equity monitoring hypothesis versus the equity buffer narrative. On May 7, 2009, the Federal Reserve Board (the Board) released the results of its first stress test after the financial crisis (the SCAP) for the 19 largest US banks. Ten banks were identified to have severe capital shortfalls, ranging from $0.6 billion to $33.9 billion. The results induced 14 banks to issue equity in the three month window around the publication of results. Importantly, as noted by Greenlaw, Kashyap, Schoenholtz, and Shin (2012) affected banks were not issuing capital in the three months before the publication. According to Morgan, Peristiani, and Savino (2014) the size of each bank's capital shortfall identified in the SCAP was not anticipated by market participants. Thus, we interpret this equity issuance as a plausibly exogenous increase in Tier 1 capital. We use issuance in the three months after the publication of the stress test scaled by 2008 total assets as our treatment intensity indicator.

Figure 2 shows that there was no clearly discernible difference in terms of Tier 1 capitalization as of the end of 2008 across treated banks (i.e., those that issued equity in the three months after the SCAP) and non-treated banks. The Board based its stress test on criteria that were not known ex ante and not tightly linked to Tier 1 capital, which arguably explains why markets did not anticipate the SCAP results. Reassuringly, the treated and non-treated group appear to be heterogeneous in terms of business model, both comprising a mix of global and more regional banks.

Table 6 shows the results of a difference-in-difference analysis. We interact the SCAP treatment measure indicator with year-indicators for the years 2010, 2011, and 2012 or a cumulative post-period indicator that is equal to one starting in 2010. We also control for bank-level total TARP equity injections scaled by 2007 total assets to account for selection into treatment, as well as for bank characteristics in $\mathbf{\Gamma}_{b,y-1}$. Across a range of specifications involving different sample restrictions and pre- and post-periods, we find a positive and significant effect of equity issuance activity linked to the SCAP on monitoring intensity. The positive effect of such equity shocks work in the same direction as Tier 1 capital in the baseline correlation analysis and corroborates the equity buffer narrative.

Finally, we exploit plausibly exogenous shocks to banks' exposure to runs, both on the deposit and on the wholesale funding market. Specifically, we specify in the vein of the SCAP analysis above three indicators of funding fragility: exposure to the reform of deposit insurance taken from Lambert, Noth, and Schüwer (2017), substantial co-syndication with Lehman Brothers, and large exposures to the subprime residential mortgage market. Appendix Tables A.12, A.13 and A.14 corroborate the absence of evidence that bank funding fragility matters for monitoring.

6 Limitations

This paper is one of the first attempts to empirically quantify how funding structure of banks impacts their monitoring activity. Although covenant violations provide a unique and useful setting to insulate the effect of bank actions on borrowing firms' governance, our empirical design suffers from some drawbacks on which we elaborate in this section.

First, our proxy for bank monitoring may entail non-trivial measurement errors. Besides the issues related to generated variables discussed in Section 2.3 and addressed in Appendix Section 5, covenant violations indeed trigger various bank reactions (such as changes to loan terms) together with enhanced monitoring. Although a dynamic loan renegotiation process is ineherent to covenant design and constitutes a form of monitoring by itself (Smith, 1993; Denis and Wang, 2014), changes in investment due to changes in loan terms should be ideally filtered out. But originations and renegotiations cannot be adequately distinguished in Dealscan (see Roberts, 2015), which makes such an exercise difficult. Thus, we have to assume that cross-firm differences in investment adjustment following violations are entirely ascribable to cross-bank differences in monitoring effort.

A second issue pertains to selection effects, which relate to contract design at origination as well as to renegotiations of covenants taking place before they are actually breached (Denis and Wang, 2014). Controlling for the borrowing firm's financial policies through a one-step procedure as in Appendix Table A.11 ameliorates this problem. But we cannot rule out that the sample is biased, for example towards those violations entailing smaller costs for borrowers. At the same time, Appendix Section 2 confirms that the availability of our monitoring measure, which depends on observing enough covenant violations for a given firm-bank-year triplet, depends on bank characteristics. The latter may also determine the type and the strictness of the covenants negotiated at origination.

Third, two important innovations in loan origination became established over the time span of our sample: nonbank lending and covenant-lite loans. Chernenko, Erel, and Prilmeier (2019) show that nonbank lenders rely less on financial covenants. Biswas, Ozkan, and Yin (2019) confirm this finding, but also document that nonbank lenders make extensive use of covenants restricting capital expenditures. By contrast, the rise in covenant-lite term loans did apparently not induce a major shift in covenant design, as banks continue to impose traditional covenants in loan packages through credit line facilities (Berlin, Nini, and Yu, 2020). Nonetheless, both nonbank lending and covenant lites are arguably important for our setting. Alas, Becker and Ivashina (2016) note that the reporting quality for the cov-lite indicator in Dealscan is poor and the differentiation between maintenance-based covenants (cov-heavy) and incurrence-based (cov-light) is hindered by several intermediate cases.

We believe that all three issues are of relevance. However, the robustness tests that can be conducted given the available data bear only limited indication that they are of first-order importance to the qualitative inference that better capitalized banks take a more lenient monitoring stance. At the same time, future research to scrutinize the sensitivity of this main result based on more detailed data in a more rigorous fashion seems warranted.

7 Conclusion

Loan monitoring is a key activity of banks as informed lenders. Several theories link the intensity and effectiveness of such an activity to bank funding structure as well as to the state of the business cycle.

This paper studies heterogeneity in monitoring across banks in the context of syndicated loans to US firms. Making use of a granular data structure linking lending banks to borrowing firms, we extract a bank-time specific measure of monitoring intensity. More specifically, we measure monitoring by analyzing banks' interventions in borrowers' management after covenant violations, which we approximate by firms' changes in investment policy.

This monitoring measure reveals the existence of substantial heterogeneity in monitoring both across banks and over time. The results clearly indicate that equity capital is an important determinant of bank monitoring incentives. Well-capitalized banks, which are better able to absorb negative shocks on their loan portfolio, keep a looser stance towards borrowing firms. This looser stance is linked to improved borrowers' performance instead of being distortive.

To move closer to causal inference, we investigate banks' monitoring responses towards exogenous shocks to their regulatory equity capital during the Supervisory Capital Assessment Program (SCAP) of 2009. This exercise confirms the inferences based on correlations quantified in the regression analysis.

Against the backdrop of ongoing regulatory changes that pertain to risk-adjusted capital requirements, leverage ratios, and liquidity buffers to insure banks against sudden re-financing stops, it is important to note that our results clearly corroborate the importance of risk-weighted capital buffers. Only larger Tier 1 capital buffers entail that banks pursue a more benign monitoring style, which in turn appears to enable financial intermediaries to better bolster shocks experienced by their borrowers that result in covenant violations.

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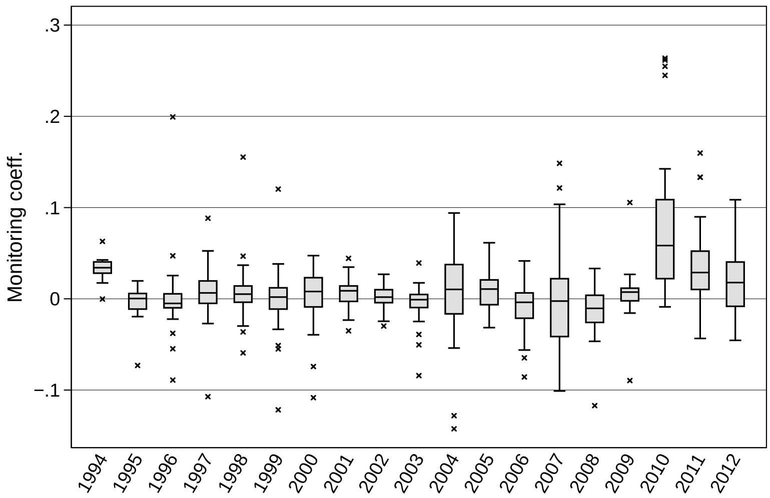

Figure 1: Distribution of bank monitoring through time
This figure visualizes the distribution of our bank monitoring measure $\hat{\beta}_{b,y}$ in each year of our 1994-2012 bank-year sample through box plots. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy.

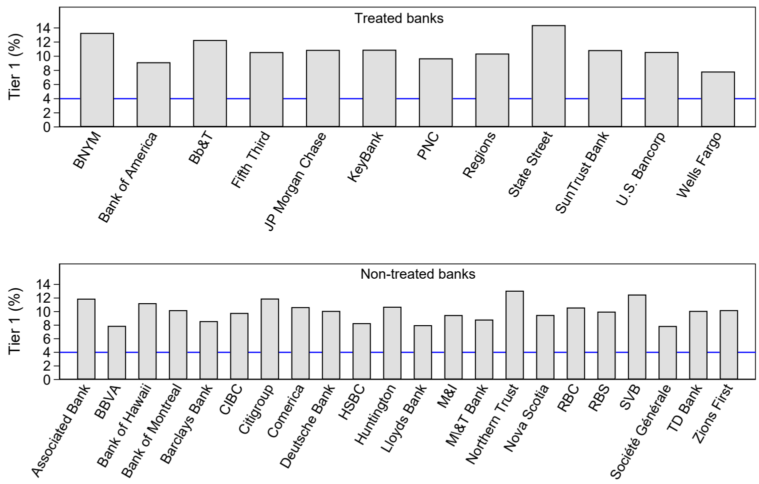

Figure 2: Risk-adjusted Tier 1 capital ratio before the SCAP stress test of 2009
This figure visualizes risk-adjusted Tier 1 capital ratio for treated (top graph) and non-treated (bottom graph) banks before the SCAP of 2009. The bar charts show the regulatory Tier 1 capital ratio at the end of 2008 together with its minimum threshold of 4% (horizontal blue line). Treated banks are those banks that issued equity in the three month-window around the SCAP stress test in May 2009.

Table 1: Summary statistics

This table shows summary statistics for our sample of US borrowing firms (from CCM), banks (from Compustat Banks and Bankscope) and syndicated loans (Dealscan) over the period 1994-2012. Panel A reports summary statistics for firm-quarters that are in covenant violation. Panel B reports summary statistics for firm-quarters that are not in covenant violation. Panel C reports summary statistics for the lending banks reported in Table A.1. Panel D reports summary statistics for syndicated loans. All monetary variables are expressed in millions of 2010 dollars. Refer to Appendix Table A.2 for variable definitions.

Panel A: Firm characteristics in covenant violation quarters

	N	Mean	SD	P25	Median	P75
Tobin's q	1,324	1.424	0.884	0.971	1.181	1.554
Cash flow	1,215	-0.178	0.641	-0.126	0.016	0.066
Investment	1,306	0.061	0.078	0.016	0.035	0.075
ROA	1,323	-0.038	0.078	-0.049	-0.009	0.008
ln(Assets)	1,324	5.532	1.453	4.465	5.431	6.451
Leverage	1,324	0.358	0.208	0.194	0.347	0.510
Current ratio	1,319	1.424	1.002	0.846	1.177	1.783
Net worth	1,324	220.138	512.525	20.659	61.768	189.398
Tangible net worth	1,319	220.573	513.415	20.596	61.738	189.486

Panel B: Firm characteristics outside covenant violation quarters

	N	Mean	SD	P25	Median	P75
Tobin's q	20,014	1.667	1.072	1.058	1.340	1.867
Cash flow	18,289	0.091	0.341	0.034	0.077	0.163
Investment	19,500	0.070	0.077	0.026	0.049	0.087
ROA	20,013	0.005	0.034	0.001	0.010	0.019
ln(Assets)	20,014	6.072	1.538	4.939	6.010	7.118
Leverage	20,014	0.257	0.174	0.116	0.245	0.370
Current ratio	19,933	2.381	1.706	1.434	1.985	2.785
Net worth	20,014	610.402	1591.436	68.696	185.826	529.535
Tangible net worth	19,930	605.627	1581.507	68.544	184.858	527.159

Panel C: Bank characteristics

	N	Mean	SD	P25	Median	P75
Leverage	2,626	0.076	0.023	0.062	0.079	0.092
Tier 1	2,565	0.097	0.021	0.080	0.092	0.110
Deposits	2,635	0.640	0.117	0.600	0.655	0.708
Short-term funding	2,438	0.047	0.053	0.005	0.029	0.075
ln(Assets)	2,644	11.699	1.494	10.586	11.510	12.815
Non-interest income	2,213	0.462	0.164	0.347	0.435	0.552
Trading	2,235	0.058	0.098	0.001	0.009	0.091
Non-performing assets	2,436	0.007	0.006	0.003	0.005	0.008
Net income	2,640	0.003	0.002	0.002	0.003	0.004
Cost-to-income	2,213	0.641	0.135	0.559	0.618	0.691

Panel D: Loan characteristics

	N	Mean	SD	P25	Median	P75
Facility amount	4,632	208.912	488.835	13.840	55.743	200.231
Deal amount	4,632	320.783	758.541	26.858	92.150	298.174
All-in-drawn spread (b.p.)	4,348	202.277	117.190	120.000	200.000	275.000
Syndicate size	4,624	5.207	6.574	1.000	2.000	7.000
Average maturity	4,629	43.572	22.385	25.600	38.000	60.000
At least one revolver	4,632	0.950	0.217	1.000	1.000	1.000
At least one secured	4,632	0.718	0.450	0.000	1.000	1.000
Corporate purpose	4,632	0.255	0.436	0.000	0.000	1.000
Working capital purpose	4,632	0.241	0.428	0.000	0.000	0.000
Debt repayment purpose	4,632	0.315	0.465	0.000	0.000	1.000
Takeover purpose	4,632	0.168	0.374	0.000	0.000	0.000

Table 2: Investment and covenant violations
This table reports estimates from RDD specifications for investment of borrowing firms around covenant violations. The sample in odd (even) columns covers the period 1994-2005 (1994-2012). The dependent variable is the borrowing firm's investment rate. The explanatory variables include the binary (0/1) covenant violation indicator, firm time-varying characteristics, and polynomials of distance measures from the covenant threshold. All independent variables are lagged by one quarter, except for *Cash flow (firm)*, which is contemporaneous with investment. Columns 1 and 2 report estimates of specification (1) over a firm-quarter data structure. Columns 3 and 4 report estimates of specification (3) over a deal-bank-firm-quarter data structure. Standard errors are clustered as indicated below. The *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	Investment			
	(1)	(2)	(3)	(4)
Violation (firm)	-0.008***	-0.007***		
	(-3.38)	(-3.15)		
Violation (deal)			-0.003	-0.002
			(-1.54)	(-1.02)
Tobin's q (firm)	0.022***	0.022***	0.019***	0.022***
	(5.86)	(6.81)	(7.70)	(8.72)
Cash flow (firm)	0.004	0.006**	0.010***	0.009***
	(1.03)	(2.00)	(2.73)	(2.77)
ln(Assets) (firm)	-0.007	-0.009**	-0.012***	-0.015***
	(-1.52)	(-2.46)	(-3.66)	(-4.06)
Default distance (NW)	-0.000	-0.000	0.001	0.001
	(-1.05)	(-0.97)	(1.15)	(1.08)
Default distance (CR)	0.009**	0.008***	0.016***	0.006
	(2.56)	(2.70)	(3.82)	(1.03)
Default distance (CR)^2	-0.001***	-0.000***	-0.003***	0.000
	(-3.21)	(-2.99)	(-3.21)	(0.13)
Default distance (NW)^2	0.000	0.000	-0.000	-0.000
	(1.01)	(0.93)	(-0.64)	(-0.54)
Firm FE	Yes	Yes	Yes	Yes
Bank-year FE	No	No	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Fiscal quarter FE	No	No	Yes	Yes
Observations	6,170	7,811	24,687	36,216
Adjusted R^2	0.381	0.364	0.461	0.416
Number of banks	-	-	87	91
Mean dep. var.	0.065	0.065	0.055	0.057
Unit of observation	Firm-quarter	Firm-quarter	Deal-bank-firm-quarter	Deal-bank-firm-quarter
Clustering	Firm	Firm	Bank-quarter	Bank-quarter
Sample selection	All banks	All banks	All banks	All banks
Sample period	1994-2005	1994-2012	1994-2005	1994-2012

Table 3: Investment, ROA, Tobin's q, and covenant violations

This table reports estimates from RDD specifications for investment, ROA and Tobin's q of borrowing firms around covenant violations. The sample covers the period 1994-2012 and has a deal-bank-firm-quarter structure. The explanatory variables include the binary (0/1) covenant violation indicator, firm time-varying characteristics, and polynomials of distance measures from the covenant threshold. All independent variables are lagged by one quarter, except for *Cash flow (firm)*, which is contemporaneous with the dependent variable. Column 1 reports estimates of the first-step specification (5) for borrowing firms' investment. Columns 2 and 3 are based on the same specification but using ROA and Tobin's q as dependent variable, respectively. In column 3, we remove Morgan Stanley from the estimation sample because it produces an outlier in the bank-year effect on Tobin's q, which reduces the bank sample size from 90 to 89. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	Investment	ROA	Tobin's q
	(1)	(2)	(3)
Violation × Bank-year FE	Yes	Yes	Yes
F-test (statistic)	4213.138***	3879.250***	544.110***
F-test (p-value)	0.000	0.000	0.000
Tobin's q (firm)	0.022***	0.005***	
	(7.59)	(2.75)	
Cash flow (firm)	0.009**	0.111***	0.153***
	(2.34)	(17.91)	(3.82)
ln(Assets) (firm)	-0.015***	-0.002	-0.189***
	(-3.50)	(-1.09)	(-4.99)
Default distance (NW)	0.000	-0.000	0.032**
	(0.38)	(-0.05)	(2.33)
Default distance (CR)	0.007	-0.003	0.037
	(1.02)	(-0.53)	(0.97)
Default distance (NW)^2	0.000	0.000	-0.001*
	(-0.10)	(0.95)	(-1.87)
Default distance (CR)^2	0.000	0.000	-0.000
	(0.01)	(0.20)	(-0.05)
Violation	Yes	Yes	Yes
Bank-year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
Fiscal quarter FE	Yes	Yes	Yes
Summary statistics:	$\hat{\beta}_{b,y}$	$\hat{\beta}^{ROA}_{b,y}$	$\hat{\beta}^{q}_{b,y}$
Mean	0.008	0.008	0.025
Standard deviation	0.040	0.152	0.262
Observations	36,195	36,390	36,206
Adjusted R^2	0.422	0.668	0.676
Number of banks	90	90	89
Mean dep. var.	0.057	0.001	1.450
Clustering	Bank-quarter	Bank-quarter	Bank-quarter
Sample selection	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012

Table 4: Monitoring and bank characteristics
This table reports estimates from the second-step OLS specification (6) over a 1994-2012 bank-year panel, where the dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The explanatory variables include bank time-varying characteristics. All independent variables are lagged by one year. Standard errors are clustered as indicated below. The *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Leverage	-0.030										-0.013	
	(-0.38)										(-0.11)	
Tier 1		0.443***									0.410***	0.375***
		(4.67)									(3.03)	(3.46)
Deposits			-0.011								-0.008	
			(-0.76)								(-0.20)	
Short-term funding				0.031							-0.014	
				(1.50)							(-0.30)	
ln(Assets)					0.002**						0.002	0.003*
					(2.04)						(1.15)	(1.88)
Non-interest income						-0.015*					-0.017	-0.012
						(-1.70)					(-1.48)	(-1.30)
Trading							0.002				0.003	
							(0.15)				(0.12)	
Non-performing assets								1.302***			0.702**	0.802**
								(6.18)			(2.52)	(2.57)
Net income									-1.259*		0.331	0.194
									(-1.69)		(0.30)	(0.28)
Cost-to-income										0.005	-0.001	
										(0.50)	(-0.07)	
Constant	0.010	-0.033***	0.015	0.006***	-0.017	0.015***	0.008***	-0.002	0.011***	0.004	-0.049	-0.061***
	(1.57)	(-3.88)	(1.53)	(3.25)	(-1.47)	(3.58)	(4.39)	(-0.60)	(4.53)	(0.67)	(-1.05)	(-2.91)
Observations	523	495	526	503	526	418	453	468	526	418	310	363
Adjusted R^2	-0.001	0.065	-0.000	0.001	0.006	0.003	-0.002	0.061	0.004	-0.002	0.094	0.109
Number of banks	66	64	67	63	67	56	66	62	67	56	51	52
Mean dep. var.	0.008	0.007	0.008	0.008	0.008	0.007	0.008	0.008	0.008	0.007	0.008	0.008
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012

Table 5: Bank monitoring over investment and performance of borrowing firms
This table reports estimates from a modified second-step OLS specification (6) over a 1994-2012 bank-year panel. The dependent variable is either $\hat{\beta}^{ROA}_{b,y}$ or $\hat{\beta}^{q}_{b,y}$. $\hat{\beta}^{ROA}_{b,y}$ ($\hat{\beta}^{q}_{b,y}$) is the estimated coefficient from a modified first-step specification (5) that captures the bank-time specific effect of covenant violations on the borrowing firm's ROA (Tobin's q) instead of the effect on its investment. The explanatory variables include bank time-varying characteristics and our monitoring measure, $\hat{\beta}_{b,y}$ from the original first-step specification (5). All independent variables are lagged by one year except for $\hat{\beta}_{b,y}$ which is contemporaneous with the dependent variables. Standard errors are clustered as indicated below are adjusted for the fact that $\hat{\beta}_{b,y}$ is a generated regressor following Bertrand and Schoar (2003). The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	$\hat{\beta}^{ROA}_{b,y}$	$\hat{\beta}^{q}_{b,y}$
	(1)	(2)
$\hat{\beta}_{b,y}$	0.097**	0.856
	(2.51)	(0.93)
Leverage	-0.004	0.286
	(-0.08)	(0.32)
Tier 1	0.075	0.625
	(1.48)	(0.90)
Deposits	-0.017	0.106
	(-1.41)	(0.51)
Short-term funding	-0.022	-0.250
	(-1.50)	(-0.86)
ln(Assets)	0.000	0.014
	(0.46)	(0.96)
Non-interest income	-0.007	0.064
	(-0.79)	(0.81)
Trading	-0.010	0.413*
	(-0.84)	(1.93)
Non-performing assets	-0.251	4.140
	(-1.50)	(1.60)
Net income	0.952	5.224
	(1.31)	(0.77)
Cost-to-income	0.010	0.156
	(1.23)	(1.38)
Constant	-0.003	-0.493
	(-0.17)	(-1.61)
Observations	310	310
Adjusted R^2	0.013	0.047
Number of banks	51	51
Mean dep. var.	0.0001	0.0526
Clustering	Bank	Bank
Sample selection	All banks	All banks
Sample period	1994-2012	1994-2012

Table 6: The SCAP quasi-experiment

This table reports estimates from the second-step OLS specification (6) augmented with a difference-in-differences exercise based on the publication of the SCAP stress test results on May 7, 2009. The dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. Explanatory variables include *SCAP* (defined as the bank-specific equity issuance after the publication of SCAP results scaled by 2008 total assets) and its interactions with year-specific or cumulative post-2009 indicators, *TARP* (defined as total TARP take-up scaled by 2007 total assets) and lagged time-varying bank characteristics $\mathbf{\Gamma}_{b,y-1}$. Specifications 1-6 include also non-US banks control for a US bank indicator and its interactions with post-2009 indicators. Information on the sample period/selection and standard error clustering is indicated below. The *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2010 × SCAP	4.110***						
	(2.79)						
2011 × SCAP	3.563**						
	(2.05)						
2012 × SCAP	2.586						
	(0.88)						
Post × SCAP		4.313***	3.718***	3.425**	3.256*	2.597*	2.593*
		(3.09)	(3.45)	(2.04)	(1.90)	(1.79)	(1.82)
TARP	-0.021	-0.075	-0.035	-0.042	-0.031	0.391	0.227
	(-0.13)	(-0.47)	(-0.22)	(-0.24)	(-0.20)	(1.38)	(0.89)
US × Post indicators	Yes	Yes	Yes	Yes	No	Yes	No
Main interaction terms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	310	269	292	310	236	130	78
Adjusted R^2	0.206	0.153	0.169	0.172	0.132	0.185	0.117
Number of banks	51	51	51	51	37	34	22
Mean dep. var.	0.008	0.005	0.007	0.008	0.006	0.019	0.018
Mean *SCAP*	0.003	0.003	0.003	0.003	0.004	0.003	0.004
Number of treated banks	12	12	12	12	12	11	11
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks	All banks	US banks	All banks	US banks
Sample period	1994-2012	1994-2010	1994-2011	1994-2012	1994-2012	2007-2012	2007-2012

Appendix to
"Benign Neglect of Covenant Violations: Blissful Banking or Ignorant Monitoring?"

1 Sample and variable definition

This section provides detailed information on the bank sample and the definitions of variables:

– List of banks included in the second-step estimation (Appendix Table A.1);

– Definition of variables (Appendix Table A.2).

2 Sample selection

In this section, we investigate those bank-years for which we are not able to estimate a coefficient by means of specification (5). Appendix Table A.3 lists those instances, which are clustered in the early sample years when Dealscan's coverage is more sparse.

The lack of a coefficient may signal statistical issues (e.g., for those banks with relatively few deals like Huntington National Bank and Bank of Hawaii, it is also relatively unlikely to observe a violation in a given firm-year that does not coincide with violations on larger banks' loans as well), but also deeper selection issues, especially concerning a bank's preferences in terms of covenant design (Murfin, 2012). Indeed, heterogeneity in banks' behavior in technical default may stem from heterogeneous monitoring incentives as well as from ex ante differences in the presence and tightness of covenants, which determine the likelihood of observing a technical default in first place.

Appendix Table A.4 shows coefficient estimates from a linear probability model analogous to equation (6), where the dependent variable is an indicator equal to one if $\hat{\beta}_{b,y}$ is non-missing for a given bank-year and zero otherwise. To keep the sample size constant, we code missing variables to zero and include a binary variable equal to one for each of them if the corresponding variable is missing and zero otherwise. In columns 1 to 10, we present estimates of univariate regressions on each of the bank characteristics in $\mathbf{\Gamma}_{b,y-1}$, which capture the bank's funding structure and business model. In column 11, the specification comprises the entire vector of bank covariates but it can only explain 10.6% of the variation in the dependent variable. In column 12, we include only those variables that are individually significant (Tier 1, deposits, total assets, total assets, trading activity, and cost-to-income ratio). Only Tier 1, total assets, and the cost-to-income ratio retain statistical significance across all specifications.

The negative association between the presence of a $\hat{\beta}_{b,y}$ coefficient and Tier 1 supports the equity buffer argument. Better capitalized banks can absorb larger shocks on risky loans and are thus potentially more prone to design loose covenants or to extend covenant-lite loans, which translates in a lower probability of observing a monitoring coefficient.[1] Interestingly, the effect of a plain leverage ratio is instead not statistically different from zero. This result is consistent with Dermine (2015), who shows theoretically that it is the uncertainty about the value of bank assets, and hence risk-adjusted capitalization, which might trigger bank runs. Our results support the notion that ample risk-adjusted capital provides banks with the ability to be patient with borrowers who violate covenants. The positive coefficient linked to bank size and cost-to-income ratio is consistent with increasing monitoring intensity at banks that are more likely to originate more complex credit products and that devote more resources per dollar of revenue on ensuring credit quality, respectively.

3 Visual evidence on the first-step estimation

This section presents:

- Manipulation tests of the running variables in the RDD around covenant violations (Appendix Figure A.1);
- The distribution of bank monitoring coefficients (Appendix Figure A.2).

4 Other potential determinants of bank monitoring

In this section, within the second step of our two-step procedure, we explore the role of other potential determinants of bank monitoring, ranging from measures of bank quality to bank distance from regulatory thresholds, bank activity in the syndicated loan market, and macroeconomic conditions.

First, the data used in the second step in Table 4 is coarse and the sample is relatively small. We reduce the dimension of the problem to capture overall bank quality and explain variation in monitoring across banks in Appendix Table A.5. Besides including Tier 1 capital – the only bank variable providing consistent results across different tests –, we define a "bad" bank in columns 1, 2, and 3 if its mean non-performing assets, non-interest income, and cost-to-income ratio is in the top quartile of the distribution of mean bank values, respectively. None of these "bad bank" measures exhibits a significant correlation with our bank monitoring coefficients. Tier 1 ratios, in turn, retain a positive and statistically significant coefficient.

Second, it is possible that the baseline linear specification (6) neglects richer patterns in the relationship between regulatory capital and bank monitoring. Banks that are close to (or below) regulatory thresholds for the risk-adjusted Tier 1 capital ratio or the leverage ratio may behave very differently from healthy institutions. Alternatively, the relation between Tier 1 capital and monitoring may become weaker as the bank becomes more capitalized. Table A.6 tackles these points by replacing the leverage ratio and the risk-adjusted Tier 1 capital ratio with the absolute value of their distance from

[1] This result corroborates the finding of Murfin (2012) that lower equity induces banks to design tighter covenants.

the respective US (pre-Basel III) and Basel II regulatory thresholds of 4% (column 1), by adding a quadratic term in the Tier 1 capital (column 2), and by controlling both for the leverage ratio distance from the US regulatory threshold and the quadratic term in the Tier 1 capital ratio (column 3). Either specification confirms the positive and statistically significant relation between Tier 1 capital and $\hat{\beta}_{b,y}$.[2]

Third, in Table A.7 we augment the baseline second-step regression (6) with measures of the bank's activity in the syndicated loan market. We control for the bank's share of outstanding syndicated loans within our sample in a given year (column 1), the bank's share of syndicated loans within our sample originated in a given year (column 2), the bank's share of all outstanding syndicated loans with (tangible) net worth and current ratio covenants to US borrowers in a given year (column 3), how frequently the bank acts as lead arranger among outstanding loans within our sample in a given year (column 4), and how frequently the bank acts as lead arranger among loans within our sample originated in a given year (column 5). The positive relationship between between Tier 1 capital and $\hat{\beta}_{b,y}$ is again unscathed.

Finally, in the next section we explore how bank monitoring intensity relates to the state of the business cycle.

4.1 The role of the business cycle

The theoretical literature proposes several channels as to why there may be a link with macroeconomic conditions. Ruckes (2004) argues that banks have less incentives to screen borrowers in upturns because the pool of loan applications is of high quality. The reverse argument holds in downturns. Mariathasan and Zhuk (2018) develop a similar argument in a rational inattention framework where loan officers' time to spend on each loan is limited.[3] Martinez-Miera and Repullo (2017) show how monitoring incentives differ between booms and busts due to fluctuations in real interest rates and the aggregate supply of savings. The state of the business cycle, besides being important per se, can also interact with the bank's funding structure in shaping monitoring incentives. For instance, a bank may take advantage of its equity capital buffer exactly in recessions and be able to exert effective monitoring even during those periods.

We thus verify empirically whether and how bank monitoring is affected by the business cycle. Appendix Figure A.3 visualizes the dynamics of monitoring coefficients $\hat{\beta}_{b,y}$ alongside recession periods and CFNAI. The left (right) graph plots the mean (standard deviation) of the monitoring coefficients. The non-cyclical behavior of the average monitoring intensity (except for the spike in 2010-11) – as witnessed by its insignificant correlation of 26.57% with CFNAI – does not support theories predicting countercyclical patterns in monitoring incentives because of the procyclical nature of loan quality applications (Ruckes, 2004) or because of rational inattention in expansions (Mariathasan and Zhuk, 2018). The dispersion of monitoring intensity is also non-cyclical and not significantly correlated with CFNAI. Interestingly, such a standard deviation appears to go through cycles, which are however non-synchronous (or even unrelated) with the cycle of the economy. This finding is hard to reconcile with existing theories.

To further explore the business cycle properties of bank monitoring, we augment specification (6) with interactions between the bank variables in $\mathbf{\Gamma}_{b,y-1}$ and macroe-

[2] For columns 2 and 3, we refer to the average marginal effect (AME) of Tier 1 capital.

[3] Both studies focus on loan screening, but the argument extends naturally to monitoring.

conomic indicators (NBER recessions, NFCI, CFNAI) in Appendix Table A.8. Given that we use annual data, the indicator for NBER recessions (column 1) is equal to one if the first month of the year is in recession, and zero otherwise. NFCI (column 2) measures conditions on US capital markets and the banking system. It has an average of zero and positive (negative) values indicate tighter (looser) financial conditions. CFNAI (column 3) measures aggregate economic activity in the US. It is on average equal to zero. Positive (negative) values indicate growth above (below) trend. None of the bank characteristics in $\mathbf{\Gamma}_{b,y-1}$ interacts meaningfully with the business cycle. Also the significantly positive relationship with Tier 1 capital does not vary over the business cycle. This finding confirms that bank monitoring is non-cyclical and inconsistent with theories predicting an important role for the business cycle.

5 One-step approach

To address the shortcomings of the two-step approach described in Section 2.3, we also implement a one-step procedure which (i) does not suffer from the issues linked to generated variables, (ii) relies on the entire sample of observations. In particular, we estimate this RDD specification:

$$I_{l,b,f,q} = \alpha \cdot v_{l,q-1} + \boldsymbol{\theta} \cdot v_{l,q-1} \times \mathbf{\Gamma}_{b,q-1} + \boldsymbol{\eta}\mathbf{x}_{f,q-1} + \boldsymbol{\zeta}\mathbf{p}_{l,q-1} + \gamma_{b,q} + \gamma_f + \gamma_e + \epsilon_{l,b,f,q}, \quad \text{(I)}$$

where $\mathbf{\Gamma}_{b,q-1}$ is a vector of bank time-varying traits defined as in equation (6), but measured at quarterly frequency and $\gamma_{b,q}$ are bank-by-quarter fixed effects. We cluster standard errors by bank and time and are interested in the vector of coefficients $\boldsymbol{\theta}$.

The main disadvantage of this approach relative to the two-step procedure is that it directly assumes the same relationship between bank actions after technical defaults and $\mathbf{\Gamma}_{b,q-1}$ for all banks and periods in the sample. By contrast, in the two-step procedure we make this assumption only in the second step, whereas the first step allows us to capture also that part of bank heterogeneity in technical default that is not explained by the vector of bank characteristics $\mathbf{\Gamma}_{b,q-1}$. Moreover, only the two-step procedure allows us to investigate whether bank interventions after covenant violations lead to higher or lower firm performance. The one-stage procedure only allows us to assess the direct impact of covenant violations on firm performance without identifying those covenant violations that actually lead to an investment restriction.

5.1 Results

The main analysis of the paper (based on the two-step approach) does not support the fragility monitoring hypothesis hypothesis. However, such a lack of support should be interpreted with caution. The second-step estimates may suffer from (i) measurement error in the (generated) dependent variable and (ii) limited statistical power. Both forces generate a bias against finding statistically significant correlations. We address these concerns through the one-step approach.

Appendix Table A.9 shows coefficient estimates from the one-step specification (I) for investment over the granular deal-bank-firm-quarter data structure. In column 1, we use all banks in our sample. In column 2, we focus on the banks used in column 11 of Table 4. We then define a "discontinuity sample" as those deal-bank-quarter observations for which the absolute value of the relative distance between (tangible) net worth or current ratio and the corresponding covenant threshold is less than 0.2

as in Chava and Roberts (2008).[4] Column 3 replicates column 1 over the discontinuity sample. The most consistent result is the positive and significant interaction between risk-adjusted Tier 1 ratio and covenant violations, corroborating the equity buffer argument. Appendix Figure A.4 provides visual evidence that indeed investment of borrowers declines less after covenant violations for loans made by highly-capitalized banks.

The proxy for banks' exposure to bank runs – short-term funding – remains insignificant. As such, the absence of evidence supporting the funding fragility hypotheses is not primarily driven by econometric concerns associated with the two-stage baseline approach. We also find a negative and significant correlation with non-interest income, which contradicts the intuition that more diversified banks may pay less attention to troubled borrowers.

We scrutinize the results from Table A.9 with regards to a broader discontinuity sample and to using more parsimonious as well as richer specifications. In column 1 of of Appendix Table A.10, we obtain similar results with a bandwidth of 0.4 as Ferreira, Ferreira, and Mariano (2018). In columns 2 to 4, we verify that the findings are robust to excluding firm control variables, which could themselves be affected by covenant violations leading to the problem of "bad controls" (Angrist and Pischke, 2008).

Moreover, it is worth noting that covenant violations substantially affect borrowing firms' financial policy: the effect of lenders' actions on borrowers' investment will hinge on the ability of the latter to find alternative sources of capital. At the same time, one can expect lenders' leniency following violations to depend on the purpose of the deal. In Appendix Table A.11, we confirm our finding on Tier 1 capital using specifications that include additional interactions of the covenant violation indicator with variables related to the borrower's financial policy and to the purpose of the deal: borrower's rating status (column 1), borrower's leverage ratio (column 2), deal purpose indicators (column 3), and all the previous variables (column 4). Interestingly, investment cuts after violations appear to be significantly milder for deals with working capital and debt repayment purposes, arguably because covenant design is contingent on the purpose of the loan (Paik, Hamilton, Lee, and Yoon, 2019).

In sum, results from the one-step approach support the inference drawn on the basis of the baseline specification. Larger equity buffer mitigate banks' monitoring responses to covenant violations, and we find no evidence in support of the funding fragility story.

6 Quasi-experimental evidence on the funding fragility hypothesis

In this section, we provide quasi-experimental evidence on the funding fragility hypothesis. We resort to plausibly exogenous shocks to the probability of runs by retail depositors (deposit insurance reforms) and to the probability of runs on the wholesale funding market (Lehman Brothers bankruptcy and the collapse of mortgage-backed securities – MBS – market during the Great Recession).

6.1 Deposit insurance coverage reforms

We first turn to reforms changing deposit insurance coverage to obtain plausibly exogenous variation in banks' exposure to runs. An increased insurance coverage translates

[4]The optimal bandwidth criterion by Imbens and Kalyanaraman (2012) suggests almost the same bandwidth: 0.203.

into a lower probability of depositor runs and allows us to conduct causal inference on the fragility monitoring hypothesis, which postulates that bankers should monitor less intensely in such circumstances.

We combine information from Demirgüç-Kunt, Kane, and Laeven (2008), Demirgüç-Kunt, Kane, and Laeven (2014), and Schich (2009) on reforms increasing deposit insurance coverage for the country-years in our second-step sample. In total, we rely on 10 single-country reforms and the 2011 EU-wide increase in deposit insurance coverage. We construct a recursive reform index showing the running sum of deposit insurance coverage reforms that is similar to the employment protection reform index by Simintzi, Vig, and Volpin (2014). In column 1 of Appendix Table A.12, we employ this variable in a regression with our baseline bank characteristics and country indicators. We do not find any statistically significant effect of coverage reforms on monitoring intensity.

To use bank-level variation in funding fragility, we then focus on US banks and take a closer look at the 2008 Emergency Economic Stabilization Act (EESA), which increased deposit insurance coverage from \$100,000 to \$250,000 per depositor. In line with Lambert, Noth, and Schüwer (2017), we assign banks to treatment and control groups based on bank-level changes in insured deposits around the EESA.[5]

Column 2 of Appendix Table A.12 interacts the treatment indicator with a post-EESA indicator equal to one from 2009 onwards. The positive effect supports the funding fragility story. But in column 3, where we use a more narrow time window, the effect is insignificant.[6] In column 4, we obtain a similar result implementing the EESA experiment over the entire sample of banks and controlling for the country-level reform indicator.

6.2 Wholesale funding market freeze

Whereas deposits are an important source of funding for banks, large institutions have access to many alternatives to refinance themselves. To ascertain that the lack of evidence about the funding fragility hypothesis in Appendix Table A.12 is not the byproduct of banks' ability to find alternative sources of finance, we follow Chodorow-Reich (2014) and exploit two different shocks to banks' rollover risk amidst the Great Recession: the bankruptcy of Lehman Brothers in September 2008 and the prolonged freeze of the US MBS market.[7] According to the funding fragility hypothesis, banks that were more exposed to these shocks should increase monitoring, making them less lenient after covenant violations.

Appendix Table A.13 looks at bank exposure to the Lehman Brothers shock as measured by the fraction of each bank's pre-crisis syndicate lending portfolio where Lehman Brothers had a lead role. We classify a bank as "affected" by the shock if such a measure is above the sample median. The funding fragility hypothesis suggests that affected banks would exhibit lower $\hat{\beta}_{b,y}$ immediately after the shock, namely in 2008-2009, when the tensions on the wholesale funding market were at their highest. We examine the year-by-year effect of Lehman default (column 1) as well as its cumulative

[5] Because our sample of US banks is considerably smaller than that of Lambert et al. (2017), we rely on a different definition of treatment and control group. We assign a bank to the treatment group if its change in insured deposits is above the 75th percentile, and to the the control group otherwise.

[6] Unreported results for year indicators show that the positive effect is entirely due to the years 2011 and 2012, which suggests a spurious correlation unrelated to the 2008 increase in deposit insurance.

[7] We use measures of bank exposure to the two shocks available on Gabriel Chodorow-Reich's website.

effect over different time windows (columns 2-7). Although we obtain some significant results, they have neither the sign nor the timing implied by the funding fragility hypothesis.

Appendix Table A.14 focuses on bank pre-crisis exposure to the US mortgage market, which was a prominent driver of bank funding stress throughout the Great Recession. To capture this exposure, we use the bank's pre-crisis stock return sensitivity to the ABX AAA 2006-H1 index and classify a bank as "affected" by the shock if such a measure is above the sample median. Again, column 1 sheds light on the year-specific effects of the shock, whereas columns 2-7 examine its cumulative effect over different time windows. We do not find evidence that banks more exposed to the MBS market were less lenient after covenant violations.

Overall, the quasi-experimental setting lends no support to the funding fragility hypothesis, reinforcing the result from the baseline analysis.

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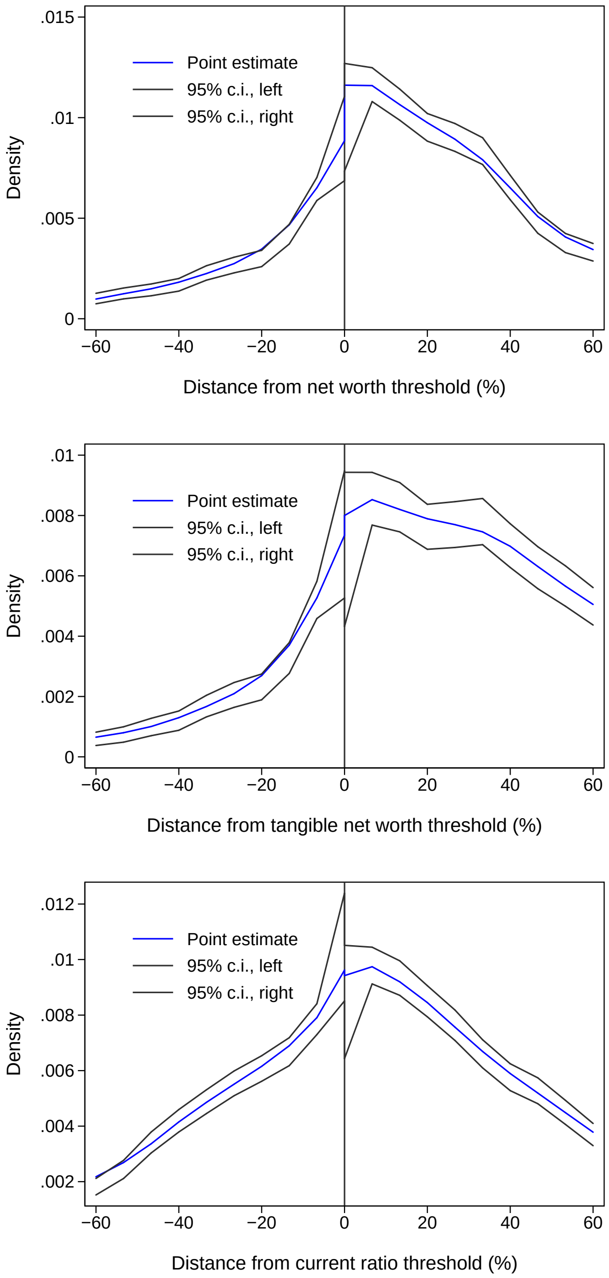

Figure A.1: Manipulation tests
This figure shows a density plot of the relative distance of a firm's accounting variable in a given quarter to the respective covenant threshold in the loans in our sample. The top graph shows the density plot for net worth covenants. The middle graph shows tangible net worth covenants. The bottom graph shows current ratio covenants. The point estimate and the confidence intervals are based on the smooth local polynomial density estimator by Cattaneo, Jansson, and Ma (2019) and a bandwidth of 0.2.

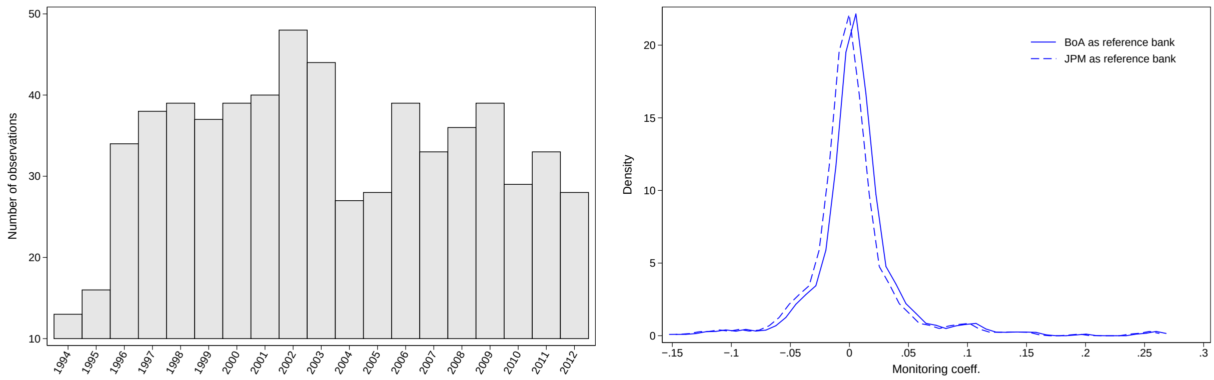

Figure A.2: Distribution of bank monitoring
This figure visualizes the distribution of our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The left graph shows the number of available observations in each year between 1994 and 2012. The right graph plots the density of $\hat{\beta}_{b,y}$ using BoA (solid line) and JPM (dashed line) as the reference bank.

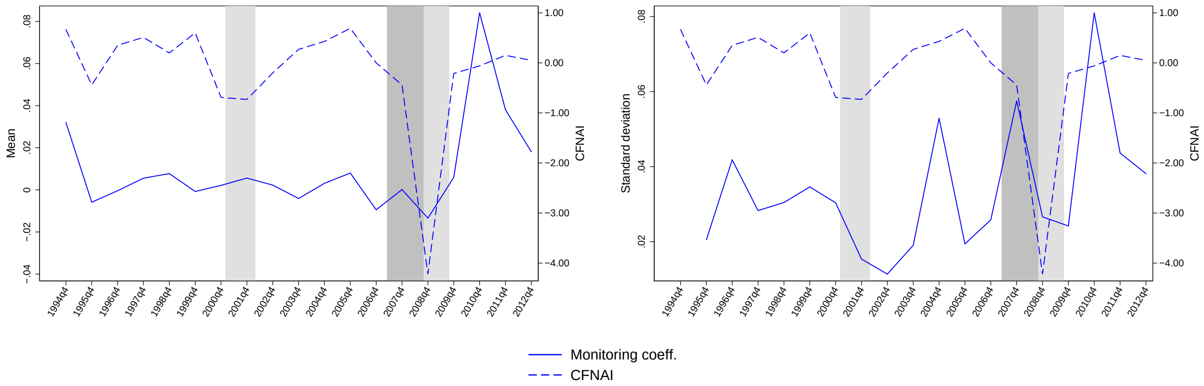

Figure A.3: Bank monitoring through the business cycle
This figure visualizes the mean (left graph) and the standard deviation (right graph) of our bank monitoring measure $\hat{\beta}_{b,y}$ in each quarter between 1994 and 2012. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. Business cycle is measured by means of CFNAI (dashed line), NBER recessions (shaded in light grey), and the early phase of the Great Recession before Lehman Brothers' bankruptcy as defined by (shaded in dark grey, defined as in Kahle and Stulz, 2013).

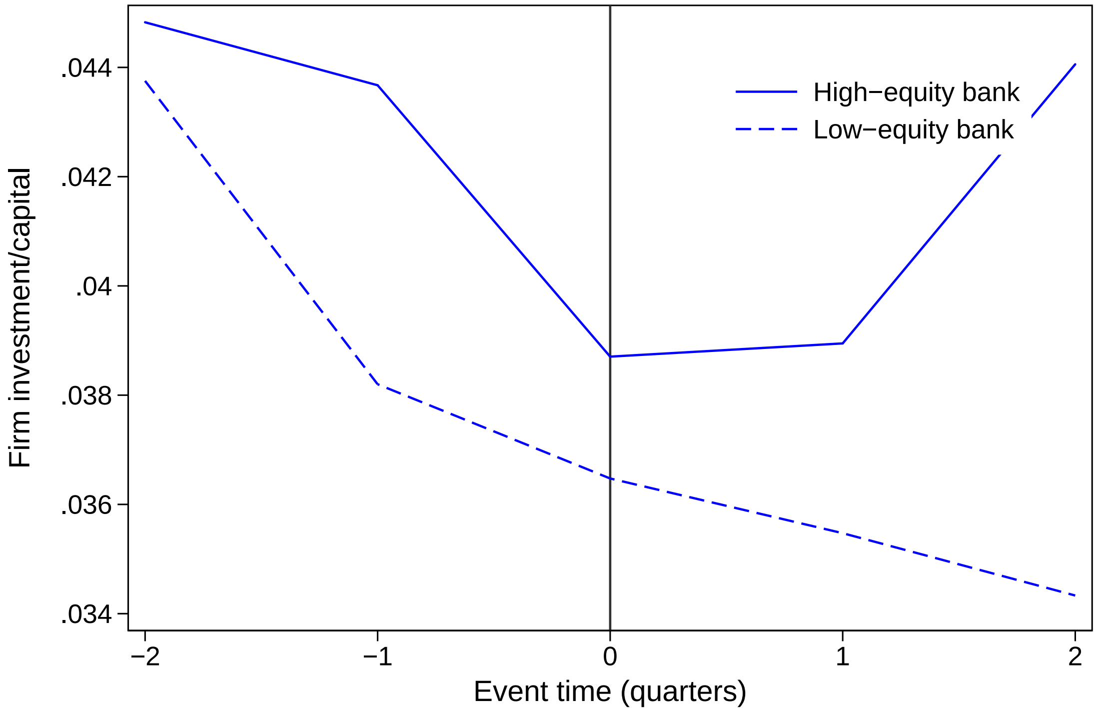

Figure A.4: Investment around covenant violations conditional on bank capitalization
This figure shows the dynamics of borrowing firms' investment around first-time covenant violations. The dashed line shows investment for firms receiving loans from high-equity banks, i.e., those with an above-median Tier 1 capital. The solid line shows investment for firms receiving loans from low-equity banks, i.e., those with a below-median Tier 1 capital.

Table A.1: Bank sample
This table shows the syndicated loan market share of the 51 banks in our second-step sample, i.e., those with all bank variables contained in $\mathbf{\Gamma}_{b,y-1}$ from equation (6) available in at least one year, which can thus be used to estimate such a specification.

	Deals		Volume	
Bank name	Number	Share (%)	$B	Share (%)
Bank of America	1,174	6.045	72.487	7.216
JP Morgan Chase	873	4.495	69.693	6.938
Wells Fargo	662	3.409	28.338	2.821
Wachovia (active until 2008)	593	3.053	32.402	3.226
Bank One Corp (active until 2004)	562	2.894	27.498	2.737
ABN Amro Bank (active until 2007)	428	2.204	25.748	2.563
U.S. Bancorp	411	2.116	21.073	2.098
Fleet Bank, later Fleet Boston (active until 2004)	389	2.003	19.976	1.989
Comerica	379	1.951	18.489	1.841
BNP Paribas	376	1.936	26.043	2.592
SunTrust Bank	368	1.895	19.048	1.896
PNC	347	1.787	15.764	1.569
BNYM	340	1.751	22.498	2.240
Bank of Montreal	338	1.740	19.176	1.909
Citigroup	323	1.663	31.200	3.106
KeyBank	277	1.426	13.743	1.368
Deutsche Bank	263	1.354	26.019	2.590
National City (active until 2008)	249	1.282	9.650	0.961
Bank of Nova Scotia	243	1.251	11.122	1.107
Mellon Bank (active until 2007)	222	1.143	13.021	1.296
Royal Bank of Scotland	205	1.056	13.731	1.367
Wachovia (old, active until 2000)	159	0.819	11.557	1.150
Société Générale	150	0.772	11.584	1.153
Royal Bank of Canada	148	0.762	8.365	0.833
Northern Trust	138	0.711	6.320	0.629
Barclays Bank	132	0.680	12.962	1.290
Fifth Third Bancorp	129	0.664	4.864	0.484
SVB	127	0.654	1.447	0.144
JP Morgan (active until 2000)	119	0.613	11.564	1.151
HSBC	117	0.602	10.716	1.067
BBVA	104	0.536	4.542	0.452
TD Bank	102	0.525	3.074	0.306
Compass Bank	75	0.386	3.012	0.300
Hibernia National Bank	64	0.330	2.801	0.279
Regions	56	0.288	2.313	0.230
CIBC	52	0.268	1.625	0.162
State Street	50	0.257	2.048	0.204
AmSouth Bank	45	0.232	1.785	0.178
Huntington National Bank	44	0.227	1.173	0.117
M&T Bank	42	0.216	1.796	0.179
Bb&T Bank	37	0.191	1.224	0.122
Zions First National	34	0.175	1.509	0.150
Bank of Hawaii	31	0.160	1.518	0.151
Provident Bank (active until 2004)	29	0.149	0.656	0.065
Commerce Bank (active until 2008)	27	0.139	0.894	0.089
SouthTrust Bank (active until 2004)	23	0.118	0.732	0.073
M&I Bank (active until 2011)	21	0.108	0.897	0.089
Lloyds Bank	18	0.093	1.124	0.112
Bank of the West	16	0.082	0.634	0.063
Associated Bank	14	0.072	0.584	0.058
First Merit Bank	4	0.021	0.078	0.008
Total (all 51 lenders)	11,129	57.304	650.113	64.717

Table A.2: Definition of variables

Variable	Databases	Definition
Borrowing firm variables:		
Tobin's q (firm)	CCM	Market value of equity plus book value of debt over book value of assets.
Cash flow (firm)	CCM	Income before extraordinary items plus depreciation and amortization over last quarter's property plant and equipment.
Investment (firm)	CCM	Capital expenditures over last quarter's property, plant, and equipment.
ROA (firm)	CCM	Income before extraordinary items over total assets.
ln(Assets) (firm)	CCM	Natural logarithm of the firm's total assets. Total assets are expressed in millions of 2010 dollars.
Leverage (firm)	CCM	Debt in current liabilities plus long-term debt over total assets.
Current ratio (firm)	CCM	Current assets over current liabilities.
Net worth (firm)	CCM	Total assets minus total liabilities (current liabilities plus long-term debt plus deferred taxes and investment tax credit plus other liabilities).
Tangible worth (firm)	CCM	Current assets plus other assets plus property plant and equipment minus total liabilities.
Loan-related variables:		
Violation (deal)	CCM and Dealscan	Indicator equal to one if a firm violates at least one of the covenants specified in the deal. We consider current ratio, net worth, and tangible net worth covenants.
Violation (firm)	CCM and Dealscan	Indicator equal to one if a firm violates at least one of the covenants specified in any deal in a certain firm-quarter. The deal-level violations are defined as in the case of *Violation (deal)*.
Default distance (CR)	CCM and Dealscan	The relative distance of a firm's current ratio from the threshold specified in a certain deal. This distance is defined as $(z_{f,q} - z^0_{l,q})/z^0_{l,q}$, where $z_{f,q}$ is the observed value of the accounting measure restricted by the covenant and $z^0_{l,q}$ is the covenant threshold contained in the syndicated loan contract for a specific quarter. The distance is set to zero if the deal is not bound by a current ratio covenant.
Default distance (NW)	CCM and Dealscan	The relative distance of a firm's net worth or tangible net worth from the respective threshold specified in a certain deal. The distance is set to zero if the deal is not bound by a net worth covenant.
Facility amount	Dealscan	The total volume of a certain facility within a loan deal expressed in millions of 2010 dollars.
Deal amount	Dealscan	The total volume of a certain loan deal (package) expressed in millions of 2010 dollars.
All-in-drawn spread	Dealscan	The amount in basis points a borrower pays over the LIBOR for every dollar drawn.
Syndicate size	Dealscan	The number of lenders per loan.
Average maturity	Dealscan	The average maturity of all term loan facilities within a loan deal weighted by facility amounts. For loan deals without any term loan facilities, the average is calculated over all facility types. The definition of facility loan types is based on Berg, Saunders, and Steffen (2016).
At least one revolver	Dealscan	Indicator equal to one if at least one of the facilities within a loan deal is classified as a revolver based on the definition from Berg et al. (2016).
At least one secured	Dealscan	Indicator equal to one if at least one of the facilities within a loan deal is secured.
Corporate purpose	Dealscan	Indicator equal to one if the deal purpose is classified as "corporate" according to the definition by Beyhaghi, Nguyen, and Wald (2019).
Working capital purpose	Dealscan	Indicator equal to one if the deal purpose is classified as "working capital" according to the definition by Beyhaghi et al. (2019).
Debt repayment purpose	Dealscan	Indicator equal to one if the deal purpose is classified as "debt repayment" according to the definition by Beyhaghi et al. (2019).

(Continued)

Table A.2: – *Continued*

Takeover purpose	Dealscan	Indicator equal to one if the deal purpose is classified as "takeover" according to the definition by Beyhaghi et al. (2019).
Lending bank variables:		
Leverage	Compustat Banks and Bankscope	Common equity over total assets.
Tier 1	Compustat Banks and Bankscope	Risk-adjusted Tier 1 capital ratio.
Deposits	Compustat Banks and Bankscope	Total deposits over total assets.
Short-term funding	Compustat Banks and Bankscope	Other short-term borrowings, securities sold under repurchase agreements, and commmercial paper over total assets. (Bankscope: Deposits from banks, repos and cash collaterals, and commercial paper over total assets.)
ln(Assets)	Compustat Banks and Bankscope	Natural logarithm of the bank's total assets. Total assets are expressed in millions of 2010 dollars.
Non-interest income	Compustat Banks and Bankscope	Total non-interest income over the sum of net-interest-income and total non-interest income.
Trading	Compustat Banks and Bankscope	Trading/dealing account securities over total assets. (Bankscope: All securities and assets held for trading, excluding derivatives, over total assets.)
Non-performing assets	Compustat Banks and Bankscope	Total non-performing assets (impaired loans for Bankscope) over total assets.
Net income	Compustat Banks and Bankscope	Net income over total assets.
Cost-to-income	Compustat Banks and Bankscope	Total non-interest expense over the sum of net interest income and total non-interest income as defined by Bankscope and FRED's aggregate cost-to-income series for US banks.
SCAP	Greenlaw, Kashyap, Schoenholtz, and Shin (2012)	Equity issuance in the three months after the publication of the SCAP results on May 7, 2009 (scaled by 2008 total assets).
TARP	US Treasury	Total forced capital injections as part of the TARP program (scaled by 2007 total assets).
Leverage (distance)	Compustat Banks and Bankscope	The absolute value of the distance of the ratio of common equity to total assets from the US regulatory threshold of 4%.
Tier 1 (distance)	Compustat Banks and Bankscope	The absolute value of the distance of the ratio of the risk-adjusted Tier 1 capital ratio to the Basel II threshold of 4%.
Loan share (outst.)	Dealscan	Average yearly loan share among all outstanding loans within our sample in a given year.
Loan share (orig.)	Dealscan	Average yearly loan share among all deals originated within our sample in a given year.
Loan share (aggreg.)	Dealscan	Average yearly loan share calculated by relating the total loan share volume of a given bank to the total volume of all (tangible) net worth and current ratio loans to US borrowers originated in a given year.
Lead credit (outst.)	Dealscan	Average yearly frequency of lead arranger credit among all outstanding loans within our sample in a given year.
Lead credit (orig.)	Dealscan	Average yearly frequency of lead arranger credit among all deals originated within our sample in a given year.

(Continued)

Table A.2: – *Continued*

Deposit insurance reform	Demirgüç-Kunt et al. (2008), Demirgüç-Kunt et al. (2014) and Schich (2009)	Country-year indicator that starts as 0 in the first year of the sample and increases by 1 for each reform increasing deposit insurance coverage.
Affected (EESA)	Lambert et al. (2017)	Indicator variable equal to 1 if the bank's change in the ratio of insured deposits to total assets induced by the EESA is above the 75th percentile, and zero otherwise.
Affected (Lehman)	Chodorow-Reich (2014)	Indicator equal to one if a bank's fraction of pre-crisis lending portfolio where Lehman Brothers had a lead role is above the sample median.
Affected (ABX)	Chodorow-Reich (2014)	Indicator equal to one if a bank's pre-crisis stock return loading on the ABX AAA 2006-H1 index is above the sample median.

Table A.3: Missing bank-years
This table lists bank-years for which $\hat{\beta}_{b,y}$ cannot be estimated in the first-step specification (5) for the banks from the estimation sample of column 11 of Table 4 (also listed in Table A.1) – excluding the year 2003 for Bank of America, which is our reference bank-year.

Bank name	Missing years
Bank of America	–
JP Morgan Chase	–
Wells Fargo	1995
Wachovia (active until 2008)	–
Bank One Corp (active until 2004)	1994
ABN Amro Bank (active until 2007)	1994, 1995
U.S. Bancorp	1994, 1995, 1996
Fleet Bank, later Fleet Boston (active until 2004)	1994, 1995
Comerica	1994, 1995, 1996
BNP Paribas	–
SunTrust Bank	1996
PNC	–
BNYM	2010, 2011, 2012
Bank of Montreal	–
Citigroup	1998
KeyBank	1994, 1995, 1996
Deutsche Bank	1994, 1995, 1997
National City (active until 2008)	1994, 1995, 2005
Bank of Nova Scotia	2004
Mellon Bank (active until 2007)	2004, 2005
Royal Bank of Scotland	1998, 1999, 2002, 2003, 2004, 2006
Wachovia (old, active until 2000)	1997
Société Générale	–
Royal Bank of Canada	2004
Northern Trust	1995, 1996, 2004, 2010, 2012
Barclays Bank	1994, 1995, 1996, 1999, 2003, 2004, 2005
Fifth Third Bancorp	1995, 1997, 1998, 1999
SVB	1998, 1999, 2001, 2006, 2011, 2012
JP Morgan (active until 2000)	1994, 1995
HSBC	–
BBVA	–
TD Bank	2004, 2009, 2010, 2012
Compass Bank	1994, 1996, 2000
Hibernia National Bank	1995, 1996, 1997
Regions	1999, 2000, 2001, 2007, 2008, 2010, 2011
CIBC	2004, 2005, 2007, 2008, 2010, 2011, 2012
State Street	1995, 1996, 1997, 2001, 2004, 2005, 2007, 2008, 2010, 2011, 2012
AmSouth Bank	1995, 2001, 2003
Huntington National Bank	1994, 1995, 1997, 1998, 2001, 2004, 2005, 2006, 2007, 2008, 2009, 2010, 2011
M&T Bank	1996, 1997, 2000, 2002, 2004, 2005, 2006, 2007, 2008, 2010, 2011, 2012
Bb&T Bank	2002, 2004, 2006, 2007, 2009, 2010, 2012
Zions First National	1997, 1998, 2000, 2001, 2003, 2004, 2005
Bank of Hawaii	1996, 1997, 2000, 2002, 2003, 2004, 2005, 2006, 2007, 2008, 2010, 2011
Provident Bank (active until 2004)	1994, 1995, 1998
Commerce Bank (active until 2008)	1996, 1997, 1998, 1999, 2003
SouthTrust Bank (active until 2004)	1999, 2000, 2001
M&I Bank (active until 2011)	1999, 2001, 2002
Lloyds Bank	2002, 2003, 2004, 2005, 2008, 2009
Bank of the West	1995, 1996, 1997, 1998, 2000
Associated Bank	2002, 2003, 2004, 2005, 2006, 2007, 2008, 2010, 2012
First Merit Bank	1999, 2000

Table A.4: Availability of a monitoring estimate and bank characteristics
This table reports estimates from linear probability models over a 1994-2012 bank-year panel, where the dependent variable is an indicator variable equal to one if $\hat{\beta}_{b,y}$ is non-missing for bank b in year y, and zero otherwise. $\hat{\beta}_{b,y}$, our bank monitoring measure, is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The list of bank-years for which $\hat{\beta}_{b,y}$ is missing is provided in Appendix Table A.3. The explanatory variables include bank time-varying characteristics. For each of these variables, (i) we set missing observations to zero, and (ii) add to the specification a binary variable equal to one if the corresponding variable is missing and zero otherwise (denoted as *Missing variable FE*). All independent variables are lagged by one year. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	Non-missing $\hat{\beta}_{b,y}$											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Leverage	-1.604										1.628	
	(-1.41)										(1.12)	
Tier 1		-3.428***									-3.192**	-3.036**
		(-2.63)									(-2.35)	(-2.35)
Deposits			-0.751***								-0.134	-0.223
			(-4.29)								(-0.53)	(-1.01)
Short-term funding				0.780							0.035	
				(1.63)							(0.09)	
ln(Assets)					0.081***						0.089***	0.068***
					(5.34)						(3.61)	(2.75)
Non-interest income						0.314					0.095	
						(1.61)					(0.65)	
Trading							0.678**				-0.138	-0.144
							(2.58)				(-0.49)	(-0.50)
Non-performing assets								-0.633			-4.320	
								(-0.16)			(-1.08)	
Net income									-9.497		-5.119	
									(-0.84)		(-0.40)	
Cost-to-income										0.291**	0.276*	0.235**
										(2.02)	(1.81)	(1.99)
Constant	0.843***	1.048***	1.201***	0.690***	-0.247	0.606***	0.683***	0.730***	0.752***	0.559***	-0.217	0.223
	(10.97)	(8.31)	(11.05)	(17.64)	(-1.30)	(5.76)	(17.02)	(17.28)	(16.58)	(4.84)	(-0.44)	(0.52)
Missing variable FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	908	908	908	908	908	908	908	908	908	908	908	908
Adjusted R^2	0.035	0.035	0.061	0.028	0.086	0.037	0.033	0.015	0.029	0.035	0.106	0.100
Number of banks	91	91	91	91	91	91	91	91	91	91	91	91
Mean dep. var.	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687	0.687
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012

Table A.5: Monitoring and bank quality
This table reports estimates from OLS regressions over a 1994-2012 bank-year panel, where the dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The explanatory variables include Tier 1 and "bad bank" indicators. A bank is considered as "bad" if its mean non-performing assets, non-interest income, and cost-to-income ratio is in the top quartile of the distribution of mean bank values in columns 1, 2, and 3, respectively. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$		
	(1)	(2)	(3)
Tier 1	0.430***	0.471***	0.475***
	(4.26)	(4.74)	(4.84)
Bad bank	0.003	-0.001	0.001
	(0.69)	(-0.39)	(0.37)
Constant	-0.033***	-0.036***	-0.037***
	(-3.68)	(-4.04)	(-4.14)
Bad bank measure	Non-performing assets > Q3	Non-interest income > Q3	Cost-to-income > Q3
Observations	477	435	426
Adjusted R^2	0.059	0.076	0.077
Number of banks	64	63	63
Mean dep. var.	0.007	0.007	0.008
Clustering	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012

Table A.6: Monitoring, bank characteristics, and nonlinearities in regulatory ratios
This table reports estimates from the second-step OLS specification (6) augmented with nonlinearities in leverage ratio and risk-adjusted Tier 1 capital ratio over a 1994-2012 bank-year panel, where the dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The explanatory variables include baseline bank time-varying characteristics as well as nonlinear terms in bank regulatory ratios. All independent variables are lagged by one year. AMEs for the risk-adjusted Tier 1 capital ratio are reported below. Standard errors are clustered as indicated below. The *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:		$\hat{\beta}_{b,y}$	
	(1)	(2)	(3)
Leverage (distance)	-0.131		-0.128
	(-1.31)		(-1.33)
Leverage		-0.012	
		(-0.11)	
Tier 1 (distance)	0.412***		
	(3.14)		
Tier 1		0.900	0.861
		(0.90)	(0.89)
Tier 1 (squared)		-2.353	-2.156
		(-0.48)	(-0.46)
Deposits	-0.009	-0.010	-0.010
	(-0.21)	(-0.25)	(-0.25)
Short-term funding	-0.023	-0.013	-0.023
	(-0.53)	(-0.30)	(-0.53)
ln(Assets)	0.002	0.002	0.002
	(1.11)	(1.17)	(1.13)
Non-interest income	-0.020*	-0.017	-0.020*
	(-1.73)	(-1.50)	(-1.75)
Trading	-0.004	-0.000	-0.007
	(-0.16)	(-0.01)	(-0.29)
Non-performing assets	0.734***	0.693**	0.726**
	(2.68)	(2.49)	(2.64)
Net income	0.663	0.329	0.658
	(0.57)	(0.30)	(0.57)
Cost-to-income	0.001	-0.001	0.001
	(0.08)	(-0.09)	(0.06)
Constant	-0.027	-0.072	-0.065
	(-0.60)	(-1.02)	(-0.95)
Tier 1 (AME)		0.446***	0.445***
		(3.21)	(3.30)
Observations	310	310	310
Adjusted R^2	0.097	0.091	0.095
Number of banks	51	51	51
Mean dep. var.	0.008	0.008	0.008
Clustering	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012

Table A.7: Monitoring, bank characteristics, and bank activity on the syndicated loan market
This table reports estimates from the second-step OLS specification (6) augmented with controls for loan shares and lead arranger frequency, where the dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The explanatory variables include baseline bank time-varying characteristics as well as measures of bank activity on the syndicated loan market. All independent variables are lagged by one year. Standard errors are clustered as indicated below. The *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$				
	(1)	(2)	(3)	(4)	(5)
Leverage	-0.013	-0.045	-0.071	0.011	-0.019
	(-0.12)	(-0.39)	(-0.71)	(0.10)	(-0.17)
Tier 1	0.409***	0.432***	0.448***	0.416***	0.433***
	(3.19)	(3.38)	(3.26)	(3.14)	(3.32)
Deposits	-0.012	-0.007	-0.007	-0.012	-0.006
	(-0.29)	(-0.18)	(-0.16)	(-0.28)	(-0.15)
Short-term funding	-0.017	-0.021	-0.021	-0.013	-0.017
	(-0.39)	(-0.47)	(-0.46)	(-0.31)	(-0.39)
ln(Assets)	0.002	0.002	0.002	0.002	0.003
	(0.79)	(1.16)	(0.73)	(1.12)	(1.58)
Non-interest income	-0.021*	-0.020*	-0.015	-0.019	-0.018
	(-1.87)	(-1.74)	(-1.31)	(-1.63)	(-1.50)
Trading	0.004	-0.001	-0.004	0.009	0.005
	(0.15)	(-0.04)	(-0.14)	(0.37)	(0.20)
Non-performing assets	0.651**	0.626**	0.685**	0.654**	0.643**
	(2.28)	(2.23)	(2.38)	(2.20)	(2.24)
Net income	0.374	0.601	0.569	0.329	0.549
	(0.35)	(0.57)	(0.51)	(0.30)	(0.51)
Cost-to-income	-0.001	0.001	0.001	-0.001	0.000
	(-0.11)	(0.06)	(0.09)	(-0.11)	(0.02)
Loan share (outst.)	-0.017				
	(-1.09)				
Loan share (orig.)		-0.017			
		(-1.55)			
Loan share (aggreg.)			0.098		
			(0.75)		
Lead credit (outst.)				-0.010	
				(-0.88)	
Lead credit (orig.)					-0.010
					(-1.08)
Constant	-0.032	-0.045	-0.049	-0.045	-0.059
	(-0.63)	(-0.91)	(-1.10)	(-0.95)	(-1.26)
Observations	310	303	303	310	303
Adjusted R^2	0.094	0.106	0.102	0.093	0.104
Number of banks	51	49	49	51	49
Mean dep. var.	0.008	0.008	0.008	0.008	0.008
Clustering	Bank	Bank	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012	1994-2012	1994-2012

Table A.8: Monitoring, bank characteristics, and business cycle conditions

This table reports estimates from the second-step OLS specification (6) augmented with interactions with business cycle measures over a 1994-2012 bank-year panel, where the dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification (5) and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. The explanatory variables include bank time-varying characteristics and their interactions with business cycle measures. Each column uses a different measure of business cycle as indicated below. All independent variables are lagged by one year. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$		
	(1)	(2)	(3)
Leverage	0.074	-0.031	-0.032
	(0.56)	(-0.25)	(-0.24)
Tier 1	0.258*	0.437***	0.449***
	(2.00)	(3.22)	(2.91)
Deposits	0.013	-0.010	-0.016
	(0.28)	(-0.24)	(-0.35)
Short-term funding	0.025	-0.017	-0.012
	(0.51)	(-0.38)	(-0.23)
ln(Assets)	0.000	0.003	0.003
	(0.19)	(1.54)	(1.20)
Non-interest income	-0.010	-0.025*	-0.023
	(-0.69)	(-1.77)	(-1.49)
Trading	0.022	-0.009	-0.013
	(0.51)	(-0.30)	(-0.39)
Non-performing assets	1.007**	0.590	0.570
	(2.37)	(1.66)	(1.46)
Net income	-0.215	-0.201	-0.311
	(-0.18)	(-0.16)	(-0.22)
Cost-to-income	0.008	0.009	0.009
	(0.51)	(0.61)	(0.56)
Business cycle	-0.145	-0.049	-0.001
	(-1.63)	(-0.96)	(-0.06)
Business cycle × Leverage	-0.360	0.069	-0.029
	(-1.46)	(0.72)	(-0.57)
Business cycle × Tier 1	0.802**	-0.001	0.104
	(2.66)	(-0.01)	(1.36)
Business cycle × Deposits	-0.022	0.027	-0.009
	(-0.35)	(0.89)	(-0.67)
Business cycle × Short-term funding	-0.014	-0.016	-0.000
	(-0.15)	(-0.44)	(-0.02)
Business cycle × ln(Assets)	0.008*	0.002	0.000
	(1.82)	(0.96)	(0.14)
Business cycle × Non-interest income	-0.018	0.005	-0.002
	(-0.61)	(0.50)	(-0.36)
Business cycle × Trading	-0.008	0.010	-0.012
	(-0.11)	(0.51)	(-1.14)
Business cycle × Non-performing assets	0.068	0.101	0.097
	(0.10)	(0.29)	(0.53)
Business cycle × Net income	5.432**	-0.112	-0.516
	(2.24)	(-0.10)	(-0.90)
Business cycle × Cost-to-income	0.007	-0.010	-0.000
	(0.30)	(-1.18)	(-0.01)
Constant	-0.046	-0.060	-0.051
	(-0.89)	(-1.27)	(-0.95)
Business cycle measure	NBER recession	NFCI	CFNAI
Observations	310	310	310
Adjusted R^2	0.119	0.077	0.076
Number of banks	51	51	51
Mean dep. var.	0.008	0.008	0.008
Clustering	Bank	Bank	Bank
Sample selection	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012

Table A.9: Monitoring and bank characteristics (alternative approach)
This table reports estimates from the one-step RDD specification (I) for investment of borrowing firms around covenant violations. The sample covers the period 1994-2012 and has a deal-bank-firm-quarter structure. The dependent variable is the borrowing firm's investment rate. The explanatory variables include the binary (0/1) covenant violation indicator, its interaction with bank time-varying characteristics, firm time-varying characteristics, and polynomials of distance measures from the covenant threshold. All independent variables are lagged by one quarter, except for *Cash flow (firm)*, which is contemporaneous with investment. In column 1, the sample includes all banks in our dataset. In column 2, the sample of banks includes the banks from the estimation sample of column 11 of Table 4 (also listed in Table A.1). In column 3, the (discontinuity sample) includes those firm-quarters with an absolute distance from the (tangible) net worth or current ratio covenant threshold below 0.2. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	Investment		
	(1)	(2)	(3)
Violation	-0.024	-0.026	-0.046
	(-0.77)	(-0.81)	(-1.29)
Viol. × Leverage	-0.078	-0.061	0.011
	(-1.23)	(-0.97)	(0.14)
Viol. × Tier 1	0.324***	0.331***	0.812***
	(2.91)	(3.01)	(4.46)
Viol. × Deposits	0.001	0.001	-0.021
	(0.04)	(0.02)	(-1.24)
Viol. × Short-term funding	0.002	0.004	-0.000
	(0.09)	(0.15)	(-0.01)
Viol. × ln(Assets)	0.001	0.001	-0.001
	(0.38)	(0.48)	(-0.64)
Viol. × Non-interest income	-0.018*	-0.019*	-0.028**
	(-1.92)	(-1.97)	(-2.42)
Viol. × Trading	0.006	0.007	-0.008
	(0.25)	(0.30)	(-0.36)
Viol. × Non-performing assets	0.312	0.307	0.571
	(1.10)	(1.02)	(1.32)
Viol. × Net income	0.560	0.596	0.969
	(0.76)	(0.78)	(0.77)
Viol. × Cost-to-income	-0.004	-0.005	0.023
	(-0.45)	(-0.54)	(1.48)
Tobin's q (firm)	0.025***	0.025***	0.013**
	(6.94)	(6.98)	(2.20)
Cash flow (firm)	0.010**	0.009**	0.013*
	(2.56)	(2.52)	(1.86)
ln(Assets) (firm)	-0.011***	-0.011***	-0.000
	(-2.77)	(-2.73)	(-0.04)
Default distance (NW)	0.000	0.000	0.005
	(0.31)	(0.31)	(0.85)
Default distance (CR)	0.008	0.008	0.019
	(1.23)	(1.22)	(0.78)
Default distance (NW)^2	0.000	0.000	-0.000
	(0.42)	(0.47)	(-0.49)
Default distance (CR)^2	-0.000	-0.000	-0.016
	(-0.12)	(-0.10)	(-1.12)
Main interaction terms	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Bank × Quarter FE	Yes	Yes	Yes
Fiscal quarter FE	Yes	Yes	Yes
Observations	18,881	18,419	4,137
Adjusted R^2	0.415	0.415	0.558
Number of banks	63	50	52
Mean dep. var.	0.056	0.056	0.051
Clustering	Bank-quarter	Bank-quarter	Bank-quarter
Sample selection	All banks	Table A.1's banks	Discontinuity (< 0.2)
Sample period	1994-2012	1994-2012	1994-2012

Table A.10: Monitoring and bank characteristics (alternative approach) – Discontinuity sample and no control variables
This table reports estimates from the one-step RDD specification (I) for investment of borrowing firms around covenant violations. The sample covers the period 1994-2012 and has a deal-bank-firm-quarter structure. The dependent variable is the borrowing firm's investment rate. The explanatory variables include the binary (0/1) covenant violation indicator, its interaction with bank time-varying characteristics, firm time-varying characteristics (only in column 1), and polynomials of distance measures from the covenant threshold. All independent variables are lagged by one quarter, except for *Cash flow (firm)*, which is contemporaneous with investment. The discontinuity sample in column 1 includes those firm-quarters with an absolute distance from the (tangible) net worth or current ratio covenant threshold below 0.4. The sample in columns 2-4 is defined as in columns 1-3 of Table A.9. Standard errors are clustered as indicated below. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	Investment			
	(1)	(2)	(3)	(4)
Violation	-0.085**	-0.058*	-0.058*	-0.042
	(-2.18)	(-1.77)	(-1.71)	(-1.24)
Viol. × Leverage	0.051	-0.085	-0.070	0.032
	(0.91)	(-1.14)	(-0.95)	(0.43)
Viol. × Tier 1	0.626***	0.378***	0.383***	0.759***
	(4.26)	(3.33)	(3.45)	(4.29)
Viol. × Deposits	0.033	0.008	0.007	-0.020
	(1.41)	(0.33)	(0.29)	(-1.13)
Viol. × Short-term funding	-0.010	-0.003	-0.000	-0.001
	(-0.42)	(-0.10)	(-0.00)	(-0.05)
Viol. × ln(Assets)	0.001	0.002	0.002	-0.001
	(0.64)	(1.51)	(1.52)	(-0.63)
Viol. × Non-interest income	-0.028**	-0.011	-0.012	-0.032***
	(-2.07)	(-1.02)	(-1.10)	(-2.77)
Viol. × Trading	0.039	0.003	0.004	-0.000
	(1.57)	(0.11)	(0.19)	(-0.00)
Viol. × Non-performing assets	-0.257	0.316	0.330	0.517
	(-0.66)	(1.05)	(1.04)	(1.21)
Viol. × Net income	0.333	0.402	0.405	0.892
	(0.26)	(0.51)	(0.50)	(0.72)
Viol. × Cost-to-income	0.014	-0.002	-0.003	0.019
	(0.99)	(-0.20)	(-0.30)	(1.42)
Main interaction terms	Yes	Yes	Yes	Yes
Firm control variables	Yes	No	No	No
Polynomials	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Bank × Quarter FE	Yes	Yes	Yes	Yes
Fiscal quarter FE	Yes	Yes	Yes	Yes
Observations	8,352	20,124	19,615	4,469
Adjusted R^2	0.505	0.385	0.384	0.561
Number of banks	59	64	50	52
Mean dep. var.	0.053	0.056	0.056	0.050
Clustering	Bank-quarter	Bank-quarter	Bank-quarter	Bank-quarter
Sample selection	Discontinuity (< 0.4)	All banks	Table A.1's banks	Discontinuity (< 0.2)
Sample period	1994-2012	1994-2012	1994-2012	1994-2012

Table A.11: Monitoring and bank characteristics (alternative approach) – Additional control variables
This table reports estimates from the one-step RDD specification (I) for investment of borrowing firms around covenant violations (augmented for additional control variables). The sample covers the period 1994-2012 and has a deal-bank-firm-quarter structure. The dependent variable is the borrowing firm's investment rate. The explanatory variables include the binary (0/1) covenant violation indicator, its interaction with bank time-varying characteristics, firm time-varying characteristics, deal purpose indicators, polynomials of distance measures from the covenant threshold, and the interactions of borrower rating measures, borrower leverage, and deal purpose indicators with the covenant violation indicator. All independent variables are lagged by one quarter, except for *Cash flow (firm)*, which is contemporaneous with investment. Standard errors are clustered as indicated below. The *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	Investment			
	(1)	(2)	(3)	(4)
Viol. × Tier 1	0.320***	0.290***	0.272***	0.239**
	(2.83)	(2.72)	(2.85)	(2.49)
Viol. × Rated (firm)	0.004			0.006
	(0.66)			(1.06)
Viol. × Investment grade (firm)	0.005			0.001
	(0.72)			(0.11)
Viol. × Leverage (firm)		-0.025		-0.025
		(-1.49)		(-1.51)
Viol. × Corporate purp.			0.015	0.014
			(1.49)	(1.25)
Viol. × Working cap. purp.			0.019**	0.018**
			(2.36)	(2.05)
Viol. × Debt repayment purp.			0.016**	0.014*
			(2.26)	(1.99)
Viol. × Takeover purp.			0.009	0.010
			(1.01)	(1.14)
Main interaction terms	Yes	Yes	Yes	Yes
Interactions of *Viol.* with main bank char.	Yes	Yes	Yes	Yes
Firm control variables	Yes	Yes	Yes	Yes
Polynomials	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Bank × Quarter FE	Yes	Yes	Yes	Yes
Fiscal quarter FE	Yes	Yes	Yes	Yes
Observations	18,881	18,881	18,881	18,881
Adjusted R^2	0.417	0.417	0.416	0.419
Number of banks	63	63	63	63
Mean dep. var.	0.056	0.056	0.056	0.056
Clustering	Bank-quarter	Bank-quarter	Bank-quarter	Bank-quarter
Sample selection	All banks	All banks	All banks	All banks
Sample period	1994-2012	1994-2012	1994-2012	1994-2012

Table A.12: The deposit insurance quasi-experiment

This table reports estimates from the second-step OLS specification (6) augmented with the quasi-experimental deposit insurance exercise. The dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. *Deposit insurance reform* is defined as the running sum of deposit insurance coverage reforms, starting with 0 in the first year for all countries and adding 1 for each increase in coverage. *Affected (EESA)* is an indicator equal to one if a bank's change in insured deposits over total assets induced by the 2008 EESA reform is above the 75th percentile among US banks, and zero otherwise. Column 1 reports estimates obtained over the entire sample using *Deposit insurance reform.* Column 2 interacts *Affected (EESA)* with *Post* focusing on US banks from Lambert et al. (2017, LNS17), where *Post* is an indicator equal to one for the period 2009-2012. Column 3 considers a time window of two years around EESA, where the pre- and post-period are defined as 2007-2008 and 2009-2010, respectively. Column 4 combines the specifications of columns 1 and 2. All specifications include country fixed effects (the reference country is the US) and lagged time-varying bank characteristics $\mathbf{\Gamma}_{b,y-1}$. The t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$			
	(1)	(2)	(3)	(4)
Deposit insurance reform	0.002			0.001
	(0.61)			(0.26)
Affected (EESA)		-0.014	-0.002	-0.007
		(-1.58)	(-0.12)	(-1.17)
Post		0.008	-0.018	0.005
		(1.18)	(-0.79)	(0.62)
Post × Affected (EESA)		0.039**	0.005	-0.016
		(2.44)	(0.23)	(-1.20)
Country FE	Yes	Yes	Yes	Yes
Bank characteristics	Yes	Yes	Yes	Yes
Observations	310	170	46	310
Adjusted R^2	0.162	0.144	-0.047	0.164
Number of banks	51	22	17	51
Mean dep. var.	0.008	0.006	0.011	0.008
Mean *Affected (EESA)*	0.755	0.235	0.283	.
Number of treated banks	36	5	4	37
Clustering	Bank	Bank	Bank	Bank
Sample selection	All banks	LNS17	LNS17	All banks
Sample period	1994-2012	1994-2012	2007-2010	1994-2012

Table A.13: The Lehman Brothers quasi-experiment

This table reports estimates from the second-step OLS specification augmented with a difference-in-differences exercise based on pre-crisis co-syndication activity with Lehman Brothers up to its bankruptcy in 2008. The dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. Explanatory variables include *Affected (Lehman)* (an indicator variable equal to one if the fraction of a bank's pre-2008 syndicated lending portfolio where Lehman had a lead role is above the sample median) and its interactions with year-specific or cumulative post-2007 indicators, *TARP* (total TARP take-up scaled by 2007 total assets), *SCAP* (bank-specific equity issuance after the publication of SCAP results scaled by 2008 total assets), and lagged time-varying bank characteristics $\mathbf{\Gamma}_{b,y-1}$. All specifications also control for a US bank indicator and its interactions with post-2007 indicators. To be included in the sample, a bank must have information on pre-2008 co-syndication activity with Lehman Brothers based on data by Chodorow-Reich (2014, CR14). Information on the sample period and standard error clustering is indicated below. The *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2008 × Affected (Lehman)	0.019						
	(1.09)						
2009 × Affected (Lehman)	0.009						
	(0.82)						
2010 × Affected (Lehman)	0.039*						
	(2.00)						
2011 × Affected (Lehman)	-0.008						
	(-0.52)						
2012 × Affected (Lehman)	-0.030*						
	(-1.83)						
Post × Affected (Lehman)		0.024	0.019	0.025*	0.015	0.005	0.024
		(1.37)	(1.61)	(2.03)	(1.34)	(0.41)	(1.39)
TARP	-0.093	0.040	0.029	-0.230	-0.236	-0.257	-0.137
	(-0.44)	(0.12)	(0.11)	(-0.78)	(-0.89)	(-1.08)	(-0.28)
SCAP	-0.136	-1.838	-1.608	-0.995	-0.669	-0.261	1.763
	(-0.14)	(-1.11)	(-1.28)	(-0.78)	(-0.60)	(-0.30)	(1.40)
US × Post indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main interaction terms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	221	149	170	187	206	221	110
Adjusted R^2	0.225	0.015	-0.004	0.043	0.072	0.073	0.160
Number of banks	28	26	28	28	28	28	26
Mean dep. var.	0.010	0.001	0.001	0.006	0.009	0.010	0.009
Mean *Affected (Lehman)*	0.471	0.483	0.476	0.476	0.476	0.471	0.491
Number of treated banks	13	13	13	13	13	13	13
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Sample selection	CR14	CR14	CR14	CR14	CR14	CR14	CR14
Sample period	1994-2012	1994-2008	1994-2009	1994-2010	1994-2011	1994-2012	2005-2010

Table A.14: The ABX quasi-experiment

This table reports estimates from the second-step OLS specification augmented with a difference-in-differences exercise based on pre-crisis exposure to MBSs. The dependent variable is our bank monitoring measure $\hat{\beta}_{b,y}$. $\hat{\beta}_{b,y}$ is the estimated coefficient from the first-step specification and captures the bank-time specific effect of covenant violations on the borrowing firm's investment policy. Explanatory variables include *Affected (ABX)* (an indicator variable equal to one if a bank's pre-crisis stock return loading on the ABX AAA 2006-H1 index is above the sample median) and its interactions with year-specific or cumulative post-2007 indicators, *TARP* (total TARP take-up scaled by 2007 total assets), *SCAP* (bank-specific equity issuance after the publication of SCAP results scaled by 2008 total assets), and lagged time-varying bank characteristics $\mathbf{\Gamma}_{b,y-1}$. All specifications also control for a US bank indicator and its interactions with post-2007 indicators. To be included in the sample, a bank must have information on its pre-crisis exposure to MBSs based on data by Chodorow-Reich (2014, CR14). Information on the sample period and standard error clustering is indicated below. The *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.2 for variable definitions.

Dependent variable:	$\hat{\beta}_{b,y}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2008 × Affected (ABX)	0.002						
	(0.11)						
2009 × Affected (ABX)	-0.001						
	(-0.13)						
2010 × Affected (ABX)	-0.003						
	(-0.13)						
2011 × Affected (ABX)	0.023						
	(1.67)						
2012 × Affected (ABX)	0.009						
	(0.55)						
Post × Affected (ABX)		0.001	-0.002	-0.006	-0.000	0.001	-0.003
		(0.09)	(-0.19)	(-0.50)	(-0.03)	(0.12)	(-0.20)
TARP	-0.081	0.083	0.014	-0.321	-0.280	-0.281	-0.534
	(-0.35)	(0.24)	(0.05)	(-0.90)	(-0.95)	(-1.03)	(-1.09)
SCAP	0.373	-0.749	-0.552	0.122	0.216	0.411	2.938**
	(0.36)	(-0.48)	(-0.43)	(0.08)	(0.17)	(0.37)	(2.38)
US × Post indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main interaction terms	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	221	149	170	187	206	221	110
Adjusted R^2	0.189	-0.031	-0.041	0.022	0.058	0.064	0.177
Number of banks	28	26	28	28	28	28	26
Mean dep. var.	0.010	0.001	0.001	0.006	0.009	0.010	0.009
Mean *Affected (ABX)*	0.407	0.409	0.406	0.406	0.408	0.407	0.418
Number of treated banks	11	11	11	11	11	11	10
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Sample selection	CR2014	CR2014	CR2014	CR2014	CR2014	CR2014	CR2014
Sample period	1994-2012	1994-2008	1994-2009	1994-2010	1994-2011	1994-2012	2005-2010

Paper 2:

Firm-Level Employment, Labour Market Reforms, and Bank Distress

Firm-Level Employment, Labour Market Reforms, and Bank Distress*†

Moritz Stieglitz‡ Ralph Setzer§

Abstract

We explore the impact of financial frictions on the employment effect of labour market reforms. Our study combines a new cross-country reform database on labour market reforms with matched firm-bank data for nine euro area countries over the period 1999 to 2013. While we find that labour market reforms are overall effective in increasing employment, restricted access to bank credit can undo up to half of medium to long-term employment gains at the firm-level. Entrepreneurs without sufficient access to credit cannot reap the full benefits of more flexible employment regulation.

JEL Classification: G21, J21, J60, K31

Keywords: Labour Market Reforms, Bank Stress, Employment Protection, Unemployment Insurance

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1 Introduction

We study how financial frictions impact the effectiveness of labour market reforms. Entrepreneurs need to have ample financing to fully take advantage of labour market deregulation. Being connected to a stressed relationship bank might prevent entrepreneurs from obtaining sufficient funding to finance the employees and machines needed for the new projects induced by a reform.

A number of studies have shown that labour market rigidities are a significant impediment to labour market clearing, resulting in high and persistent unemployment.[1] Several euro area countries feature both a high level of labour market rigidity and weaknesses in their banking sector. Moreover, the typical European firm is small and heavily dependent on bank financing. If financial frictions matter for labour market reform outcomes, the presence of weak banks might modify the employment gains from labour market liberalisation in these countries. This raises the issue of the sequencing of reforms. Our results suggest that countries can gain from complementing labour market reforms with a comprehensive clean-up of the banking sector.

Our research question also touches a deeper economic issue, namely the way financial market and factor market frictions interact in shaping firm policies. We analyse how the benefits of removing frictions in one market (labour) can be undermined by existing frictions in another market (financial). Our hypothesis is based on the theoretical framework on the interaction of labour and credit market frictions by Wasmer and Weil (2004). Their study suggests that entrepreneurs need bank financing to take full advantage of labour market deregulation. The standard argument for labour market reforms is that the resulting decrease in the user cost of labour will induce entrepreneurs to undertake the marginal project, which they were hesitating to implement before. However, insufficient access to funding can make entrepreneurs unable to finance the employees and machines needed for this marginal project. Hence, the new project might not be implemented at all or only on a smaller scale. The same holds for complementary follow-up projects, with potential long-term negative consequences for firm-level and economy-wide growth.[2]

[1]For an extensive overview of the literature on structural reforms and their interdependencies with other policies see Masuch et al. (2018).

[2]One can think of entrepreneurs connected to weak banks as being trapped in a bad equilibrium which lowers the long-term employment gains from the reform. Thus, cleaning up the banking sector in combination with a labour market reform might help to push more firms towards the good equilibrium. For an exposition on how complementary projects can lead to a “big push” in a multiple equilibria setting see the study by Murphy et al. (1989).

We test our hypothesis empirically by studying the interaction of labour market reforms and bank financing conditions in a matched bank-firm sample with annual data from nine euro area countries over the period 1999 to 2013. We find that the gains in firm-level employment induced by labour market reforms are significantly reduced if a firm is connected to a weak bank after the implementation of a reform. We also find that the negative link between bank distress and reform success is strongest among firms that are more bank dependent and have less capacity for internal financing. Specifically, we find that the negative link is stronger for firms that are small, sell goods of high durability, or operate with a production technology inducing high external financial dependence. Moreover, the link is stronger during firm-years characterised by recessions. At the same time, our analysis of recessions also shows that bank distress reduces firm-level employment gains from reforms even during normal times.

We study three labour market reform types of major interest for policymakers in advanced economies: i) those that liberalise or tighten employment protection for regular workers, ii) those that reduce or increase the generosity of unemployment benefit schemes in terms of size and/or duration, and iii) those that liberalise or tighten the regulation of temporary employment.[3] Our empirical setup extends the difference-in-differences approach employed in Simintzi et al. (2015) to compare firms in countries that implement one of our three reform types with similar firms that are not implementing the same reform at the same point in time. To explore the interdependency of reform success of banking financing, we then compare firms connected to either weak or strong banks within the same country after a reform has been implemented.

Among our three reform types, the negative relationship between bank weakness and reform success is most pronounced for unemployment benefit reforms. While a reform reducing the generosity of unemployment benefit schemes can induce a medium to long-term shift in firm-level employment of up to 8.5%, the same effect is only 4.8% for firms connected to a weak bank. We estimate this differential effect of bank financing conditions after controlling for country-level factors with a rich fixed effects structure. Disentangling the effect of bank financing conditions from general macroeconomic developments like the business cycle is only possible with firm-level micro data. Note, however, that according to Gal and Hijzen (2016) unweighted firm-level regressions only capture the response of the typical incumbent firm. This warrants caution in

[3]In the following, we will refer to liberalising reforms simply as "reforms" while calling tightening reforms "counter-reforms". Note that our dataset provides too few tightening labour policy measures to allow us to provide a separate estimate for counter-reforms, which is why we do not discuss them separately.

deriving conclusions about the aggregate effect of reforms.[4] We are focusing on the differential employment impact of reforms on financially constrained firms, which means that we are also abstracting from the effect of reforms on productivity and sectoral reallocation. Moreover, our focus on employment effects in the long-run means that we are abstracting from potential short-term costs and distributional implications of reforms.[5]

Firms and banks match in an unobservable process, which might drive our results. We address this issue by excluding firms whose relationship bank is relatively weak already at the time these firms enter the sample. This set of firms is more likely to have matched with weak banks purposefully (see Schwert (2018)). The sample restriction thus leaves us with a set of particularly bank-dependent firms that start out with a strong bank which subsequently becomes weak. This restriction increases the power of our model to detect the negative interaction between bank weakness and reforms. While our baseline aproach only allows us to find a negative relationship between bank financing and unemployment benefit reforms, we can now find this relationship for all three reform types.

Another confounding factor could be reverse causality between bank and firm health. Large firms might be able to drag down their relationship bank once their own health deteriorates. To address this concern, we re-estimate our model using only small- and micro firms. Banks are better able to diversify a portfolio of exposures to small- and micro firms and it is less likely that a deterioration in their health turns a strong bank into a weak one. Small- and micro firms also are a set of firms that are particularly bank-dependent. We document that our main result that bank financing limits the employment gains of unemployment benefit reforms holds and even grows in magnitude.

Our paper contributes to three strands of literature. It relates to the literature focusing on the macroeconomic impact of structural reforms. A host of recent studies look at product and labour market reforms and their interdependence with macroeconomic policies and conditions.[6] Duval et al. (2017), Adhikari et al. (2018), and Duval and Furceri (2018) focus on labour market reforms in particular and find that their

[4]In Appendix Section D we discuss the specific assumptions required for an aggregate interpretation of our effects and provide an illustrative back-of-the-envelope calculation.

[5]Bassanini and Cingano (2019) show that labour market reforms can lead to transitory employment losses and Krebs and Scheffel (2013) show how labour market reforms can create winners and losers with unevenly distributed welfare losses.

[6]See Bouis et al. (2016), Banerji et al. (2017), Duval et al. (2017), Adhikari et al. (2018), and Duval and Furceri (2018).

short-term impact on employment is only positive during upturns. We contribute by showing that bank financing conditions have a similar but independent impact on reform success by using our micro-data setting to control for time-varying developments at the sector-and country-level.

Several studies explore the impact of labour market reforms using cross-country firm-level data. Simintzi et al. (2015) find that reforms increasing employment protection decrease firm leverage. They argue that an increase in labour market protection constitutes an increase in operating leverage and thus crowds out financial leverage.[7] We provide a new angle by studying firm policies when there is a decrease in operating leverage through a liberalising reform but firm leverage cannot rise due to financial frictions. Some recent studies also focus on the interaction of labour market reforms and firm-level financing conditions. Using data on syndicated loans to large public companies, Alimov (2015) shows that higher employment protection decreases firms' access to credit in terms of higher loan spreads and less favorable non-pricing terms. Using survey data, Moro et al. (2017) show that lower employment protection increases firms' access to credit. Cingano et al. (2010) and Calcagnini et al. (2009, 2014, 2015) find that financial constraints exacerbate the negative impact of increases in employment protection on firm investment. In these studies, financial constraints are proxied with different measures of firms' internal liquidity. Antoun de Almeida and Balasundharam (2018) proxy financial constraints with firm leverage and find no significant interaction between firm leverage and the employment outcomes of labour market reforms. We contribute by showing that firm-level employment outcomes of labour market reforms depend on the differential impact of banks' ability to grant credit. More specifically, the effects of labour market policies depend on firm-specific bank financing conditions, which are arguably independent of firms' own growth and financing decisions at different stages of their life cycle.[8] This is important from a policy-making perspective because regulators can more easily target bank health than wide-spread firm balance sheet weaknesses. Thus, a comprehensive clean-up of the banking sector might ensure successful reform outcomes even when firms have few internal funds based on observable liquidity measures. Furthermore, we contribute relative to the studies mentioned above by differentiating between three different types of labour market reforms classified in

[7]Serfling (2016) obtains a similar result using data on US state-level legislation and Karpuz et al. (2018) find that the increase in operating leverage also induces firms to increase cash holdings.

[8]Farre-Mensa and Ljungqvist (2016) test different firm-level measures of financial constraints commonly used in the literature and conclude that they mostly reflect differences in firms' own policies.

a novel reform database.[9]

Finally, our study contributes to the literature on firm financing and employment protection. Cingano et al. (2016) show how the introduction of employment protection for previously exempt small Italian firms leads to higher investment, especially among firms endowed with higher internal liquidity. Claessens and Ueda (2018) find that the staggered introduction of wrongful-discharge laws in US states increased growth in knowledge intensive industries, especially in states with simultaneous bank branch deregulation.[10] In contrast, Bai et al. (2019) and Lee and Shin (2018) find that wrongful-discharge laws in the US lead to lower firm-level investment, especially among financially constrained firms. Laeven et al. (2018) compare the performance of firms subject to different employment protection rules during the financial crisis in Spain. While they study the effect of financial shocks at given levels of labour market frictions, we study the effect of changes in labour market frictions given different levels of financial frictions.

2 Theoretical background

2.1 Labour market reforms and employment

We consider three types of labour market reforms. We look at reforms that decrease employment protection for regular workers. We analyse reforms that reduce the generosity and/or duration of unemployment benefit schemes and we look at reforms that decrease the regulation of temporary employment.

Lazear (1990) shows how reforms that liberalise employment protection for regular workers can increase firm-level employment by decreasing dismissal costs and encouraging new hiring. However, he points out that in the short run these types of reforms can have a countervailing effect on employment by making it easier to fire existing workers. In the short-run, the countervailing effect might dominate since laying off incumbent employees can be more swiftly implemented than searching for and hiring

[9] The database by Duval et al. (2018b) has several advantages relative to other databases commonly used in the literature. Rather than providing a long list of reforms as the European Commission's LABREF database, it focuses on major reforms, which are identified by textual analysis of OECD country reports. While changes in widely used OECD labour market indicators also feed into the mechanism identifying major reforms, the database by Duval et al. (2018b) has the additional advantage of providing the exact implementation dates of the underlying policies.

[10] According to theoretical work by Janiak and Wasmer (2014), increasing employment protection from very low levels can lead to higher capital-labour ratios due to the complementarity between physical and firm-specific human capital.

adequate new ones. The positive long-term effects of reforms on employment remain, however, unquestioned.

There are several mechanisms how reforms that reduce the generosity of unemployment benefit schemes can lead to an increase in firm-level employment. The search and matching literature suggests that making benefits less generous reduces reservation wages and thereby increases workers' job-search incentives (see e.g. Pissarides (2001)), which leads to increases in labour supply and employment and decreases in equilibrium wages (see e.g. Krebs and Scheffel (2013)). In contrast, Krause and Uhlig (2012) focus on the effect of unemployment benefit reforms on the cost of vacancy creation. In their model, a decrease in unemployment benefits lowers the relative cost of posting a vacancy by increasing the probability that a given vacancy will be filled. Here, unemployment benefit reforms lead to higher demand for labour through lower entry cost of employers into the matching market.

Temporary employment reforms can encourage hiring by decreasing dismissal costs for fixed-term employees or by lifting restrictions on hiring on a temporary basis rather than on an open-ended basis.[11] In addition, our reform dataset also counts reforms that ease restrictions on the use of temporary agency workers as temporary employment reforms. Given our focus on within-firm changes, this particular manifestation of a temporary employment reform could actually lead to a negative effect on regular firm-level employment.[12] These countervailing effects decrease our power to determine the effects of temporary employment reforms on firm-level employment and obscure the role of financing in moderating the size of employment effects.

2.2 Labour market reforms and financing

After having discussed how labour market reforms can increase employment, we now examine how financing can limit the magnitude of this increase. The most relevant study in this respect is the paper by Wasmer and Weil (2004), which combines credit

[11] An example for such a reform would be the extension of the continuous time period during which workers can be employed on the basis of renewed fixed-term contracts as opposed to regular open-ended contracts.

[12] The actual size of this negative effect depends on country-specific reporting rules on whether temporary agency workers are counted on the payroll of the firm employing agency workers or the temporary work agency itself. Note that while this measurement uncertainty diminishes our ability to estimate effects for temporary employment reforms in particular, it is unlikely to introduce much bias into the empirical results for the other reform types. This is because among the countries in our sample, temporary agency workers only make up between 0.3% (Greece) and 3.5% (Netherlands) of total employment (OECD, 2013).

market and labour market frictions in a DGSE model where employers need to obtain financing from a bank before they can post a vacancy. They show that even when labour markets move towards a frictionless state, financing will still be a limiting factor for job creation. Other theoretical studies that suggest a complementarity between access to credit and labour market deregulation are Koskela and Stenbacka (2004) and Rendon (2013). Recent models combining credit and labour market frictions focus more on the financial crisis and try to explore how their interaction amplifies financial shocks. They shed more light on the interaction between employment and credit frictions and identify small, young and highly-leveraged firms as the ones most likely to be affected by financial frictions in their employment decisions (Boeri et al., 2013; Buera et al., 2015).

2.3 Graphical analysis

We visualise the main theoretical mechanism of our study by using a simplified diagram, which is based on the stylised right-to-manage model from Ciminelli et al. (2018). We assume a standard constant elasticity of substitution (CES) production function with constant returns to scale:

$$\begin{aligned} Y &= F(K, AL) \\ &= (\alpha(K)^{\varepsilon} + (1-\alpha)(AL)^{\varepsilon})^{1/\varepsilon} \end{aligned} \tag{1}$$

where K denotes capital, L denotes labour, and A denotes labour-augmenting technical change. ε relates to the elasticity of substitution between labour and capital according to $\sigma = 1/(1-\varepsilon)$. Capital and labour are complements if $\varepsilon < 0 \rightarrow \sigma < 1$ and substitutes if $\varepsilon > 0 \rightarrow \sigma > 1$.

Labour market frictions take the form of bargaining between employers and workers. After the wage is determined, employers take the wage as given and set employment.[13] The model specifically refers to employment protection reforms that decrease the bargaining power of workers and thereby lower the wage. The key insights of the model for employment can, however, also be applied to unemployment benefit reforms where the lower bargained wage stems from a decrease in the reservation wage rather than

[13] In a right-to-manage model, the manager retains the "right" to unilaterally set employment levels. This contrasts with the efficient bargaining model where employers and employees bargain over both wages and employment. According to Ciminelli et al. (2018), the right-to-manage describes rather well the actual functioning of labour markets in Europe. Nevertheless, employees might also have some power over employment levels so that the actual bargaining process likely also contains elements from an efficient bargaining model.

a decrease in workers' bargaining power. In both cases, the lower wage increases firm labour demand.[14]

Figure 1 depicts how the decrease in wages lowers the user cost of labour, shifts the employers' isocost curve outwards and thereby increases firm demand for labour and capital. At this point, the interaction between labour market reforms and financing comes into play. Financing constraints, which are visualised by the red crosses inhibiting the movement of K, L, and the jump of the production isoquant, can prevent firms from increasing labour and capital to the new optimum. Note that we are assuming that $\varepsilon < 0 \rightarrow \sigma < 1$, which implies that labour and capital are complements so that the income effect dominates the substitution effect.[15] The underlying narrative is that employers have to get financing before they can hire new workers and buy the new machines needed to put them to productive use.[16]

3 Data

3.1 Data and sample selection procedure

Our sample contains firms from nine out of a total of nineteen euro area countries, namely Austria, Germany, Spain, France, Greece, Ireland, Luxembourg, the Netherlands, and Portugal. For this subset of countries, we have extensive data on firm and bank characteristics, firm-bank links, and structural reforms.

We obtain information on labour market reforms for 13 euro area countries during the period of 1970-2013 from a newly released IMF database.[17] The database is

[14]Note that in the case of unemployment benefit reforms, we do not need to rely on the assumption that wages changes are the main mechanism how reforms increase employment. The mechanism set out in Krause and Uhlig (2012) relies on a decrease in the cost of posting a vacancy. Here, we would also see an increase in firms' labour demand, whose magnitude is dependent on access to financing. In contrast, the effects of the temporary employment reforms considered in our study and their connection to financing conditions are theoretically ambiguous and run through separate channels (see our discussion in Section 2.1). Therefore, temporary employment reforms cannot easily be integrated into our simplified model analysis.

[15]In theory, in a case where capital and labour are highly substitutable, the decrease in the user cost of labour might even lead firms to demand less capital. However, according to several empirical studies most firms in Europe and the US operate with technologies that exhibit some degree of complementarity (Ciminelli et al., 2018; Laeven et al., 2018; Antras, 2004; Oberfield and Raval, 2014; Lawrence, 2015). In a robustness check in Section B of the appendix, we explicitly look at firms where labour and capital are substitutes.

[16]According to the Ajello (2016)'s analysis of US firms' cash flow statements, roughly a third of the need for external financing emanates from working capital needs, which includes financing the wages of new workers, while the remaining part emanates from the accumulation of fixed capital.

[17]The database relies on the analysis of textual OECD data and OECD indicators, which is why

described in Duval et al. (2018b) and follows a narrative approach, which relies on text-search of OECD country reports and a set of rules and cross-checks to identify "major" reforms that have a significant impact on labour (and product) markets. Table 1 shows the distribution of reforms across countries. Among the reforms in our sample, roughly 20% are tightening reforms. There is substantial heterogeneity in the sense that some countries enact a range of different reforms for each of our three reform types while, for example, Luxembourg does not enact a single reform that qualifies as "major" according to the IMF classification during our sample period. In the following section, we will describe how these reforms map into the recursive reform indicator used in our empirical analysis.

Our firm-level data is from Bureau van Dijk's ORBIS database and includes all euro area countries from 1999-2015. We remove firms with inconsistent balance sheets by dropping observations with zero, missing or negative total assets and observations in which the sum of total equity, current-, and non-current liabilities is below 98% or above 102% of total assets.[18] We use unconsolidated annual accounts to avoid double-counting when both the consolidated account of the parent (with all its subsidiaries) and the unconsolidated account of the parent (without subsidiaries) are available. Our analysis concentrates on the non-financial business economy, which excludes, for example, the government sector. For details on our sample composition and a discussion of representativeness of the database, we refer the reader to Section A of the appendix.

ORBIS also provides data on bank-firm linkages for 15 out of 19 euro area countries. The countries without data on bank-firm linkages in ORBIS are Belgium, Finland, Italy and Slovakia and are therefore excluded from our study. Note that the bank-firm link is taken from the 2017 ORBIS extract and is not time-varying. Thus, we are relying on the assumption put forward by Kalemli-Ozcan et al. (2015) and commonly used in the literature that bank-firm relationships are sticky.[19] We assume that a firm's reported

euro area countries that are not part of the OECD, or have only joined the OECD very recently, are not covered. The countries with missing reform data are Cyprus, Estonia, Lithuania, Latvia, Malta, and Slovenia. Also note that the IMF reform database assigns a given reform to the year t if it was implemented within the first half of the year t while it assigns it to the year t+1 if it was implemented in the second half of the year t.

[18]Note that in order to decrease survivorship bias, we do not drop all inactive firms. Our results are robust to excluding inactive firms as defined in Kalemli-Ozcan et al. (2015). This encompasses companies with a status that reads either "inactive", "dissolved", "in liquidation" or "bankruptcy". Regressions excluding inactive firms are available upon request.

[19]Giannetti and Ongena (2012) explicitly compare the 2005 and 2013 versions of the ORBIS relationship database and Kalemli-Ozcan et al. (2019) do the same for the 2013 and 2015 versions. Both confirm that bank-firm relationships are sticky. This seems also to be the case in the US as discussed in Chodorow-Reich (2014). Other recent studies that use ORBIS and rely on the assumption of sticky

relationship banks also reflect its borrowing relationships. We manually match the name of the firm's relationship banks with unconsolidated bank data from Bureau van Dijk's Bankscope database. Over 95% of bank names from the relationship database can be matched to a bank-identifier in Bankscope. For firms that report more than one bank relationship, we assign the largest domestic bank among the reported banks, in terms of total assets in the year 2000, as the company's main relationship bank. Only if there is no domestic bank available, we take the largest foreign bank as the firm's main relationship bank.[20]

3.2 Variable construction

In our analysis, we use variables at the firm-, bank-firm-, country- and sector level. The dependent variable is the natural logarithm of the total number of employees. To capture variation in a firm's indebtedness we control for leverage, defined as financial debt relative to total assets.[21] We proxy a firm's liquidity by dividing cash holdings by total assets and a firm's access to marketable collateral by controlling for tangibility, defined as tangible fixed assets over total assets. To capture changes in a firm's profitability, we calculate the return on assets by relating net income to total assets. Each firm-level explanatory variable is winsorised at the 1st and 99th percentile.[22]

We use the bank stress measure proposed in Storz et al. (2017) to gauge bank health based on balance sheet data from Bankscope. We use principal component analysis to construct an indicator consisting of bank characteristics commonly associated with bank stress. The indicator is defined as the first principal component of a bank's capitalisation, NPL ratio, return on assets, z-score, and liquidity.[23] Capitalisation is measured as bank equity over total assets. The NPL ratio is defined as nonperforming loans over total loans. Return on assets is defined analogously to our firm control variable. Bank z-score is measured as equity and net income over the standard deviation of the return on assets.[24] Bank liquidity is defined as the difference between liquid

relationships are Storz et al. (2017), Andrews and Petroulakis (2019) and Duval et al. (2019).

[20]Only 1.69% of firms have a foreign bank as their main relationship bank.

[21]Financial debt includes, e.g., loans and bonds but excludes non-financial debt like deferred tax liabilities.

[22]Note that we do not winsorise the dependent variable since we do not want to treat firms with very few or very many employees as outliers.

[23]The bank stress indicator loads positively on the NPL ratio with an eigenvector of 0.26 and negatively on capitalisation (-0.66), return on assets (-0.66), z-score (-0.02) and liquidity (-0.24).

[24]According to Laeven and Levine (2009), a bank's z-score reflects the inverse of the probability of insolvency.

assets and runnable short-term liabilities normalised by total assets, where runnable short-term liabilities are the sum of deposits and short-term funding.[25]

The advantage of our bank stress measure relative to market-based measures, such as CDS spreads, is that it is available also for small and non-listed banks. For the small subsample of banks in the ORBIS firm-bank link database for which CDS data is available, Storz et al. (2017) compare their bank stress measure with CDS spreads and find that the correlation between both variables is 68%. Principal component analysis has by now become a standard tool in economics and finance.[26] Other recent papers using principal component analysis to gauge bank stress are Chodorow-Reich and Falato (2018) for US banks and Andrews and Petroulakis (2019) for European banks. While Andrews and Petroulakis (2019) use a slightly different set of bank variables, the variation of their measure across countries and over time is similar to the variation displayed in our Appendix Table A.3.

We dichotomise our continuous bank stress measure by defining a weak bank indicator equal to one if bank stress is higher than the 75h percentile over all observations within the regression sample.[27] In all regressions we also control for bank size, which we define as the natural logarithm of inflation-adjusted total assets.

Our labour market reform variables are modelled after the recursive reform indicators from Simintzi et al. (2015). For each reform type we use a recursive reform indicator that starts out with zero at the beginning of the sample and increases by one for a liberalising reform while decreasing by one for a tightening reform. Thus, a liberalising reform shifts the reform indicator upwards until the end of the sample period (unless there is a counter-reform or a second reform). This permanent shift in the indicator aims to capture the medium to long-term effect of reforms on firm-level employment. Appendix Tables A.4, A.5, and A.6 show the evolution of each of the three reform indicators over time.[28]

[25] Our measure is related to the concept of illiquidity risk described in Morris and Shin (2016) and the liquidity coverage ratio defined in the Basel III framework. Note that we define liquid assets as the sum of securities, derivatives and loans and advances to banks.

[26] See Aït-Sahalia and Xiu (2018) for a discussion of principal component analysis and an overview over different applications in the economic literature.

[27] Thus, the weak bank indicator takes into account the entire variation across firms and over time to determine whether a given firm-year observation is characterised by easy or tight access to credit.

[28] Due to the limited number of reforms in our sample, we do not distinguish between unemployment benefit reforms that affect the level and those that affect the duration of benefit schemes. In practice, both types of reforms are often combined. While according to Duval et al. (2018b) the German reform of 2005 mostly changed the duration of unemployment benefits, the re-organisation of different benefit schemes and more stringent means testing likely also affected the amount of benefits paid out in practice. In a similar vein, the Portuguese reform of 2012 affected both the duration and the level

Whenever our fixed effects structure does not absorb variation at the country-sector-year level, we also control for GDP growth and sector growth, which are calculated based on AMECO and Eurostat data, respectively.

A detailed description of all variables can be found in Appendix Table A.1.

4 Empirical approach

We estimate the following cross-country panel regression using data at the firm-year level:

$$\begin{aligned} ln(employees)_{jsct} = {} & \beta_1 WeakBank_{jsct-1} \\ & + \beta_2 Regular_{ct-2} + \beta_3 Regular_{ct-2} * WeakBank_{jsct-1} \\ & + \beta_4 Unempl_{ct-2} + \beta_5 Unempl_{ct-2} * WeakBank_{jsct-1} \\ & + \beta_6 Tempor_{ct-2} + \beta_7 Tempor_{ct-2} * WeakBank_{jsct-1} \\ & + ReformInteractions \\ & + \beta Controls + FEs + \varepsilon_{jsct}, \end{aligned} \tag{2}$$

where j denotes firms, s denotes sectors, c denotes countries and t denotes years. $Regular$ denotes reforms of employment protection for regular workers, $Unempl$ denotes reforms of unemployment benefit schemes, $Tempor$ denotes reforms of the regulation of temporary employment, and $WeakBank$ denotes our binary bank stress indicator. The vector of control variables contains our firm-level covariates, bank size, sector growth and GDP growth. Moreover, we include all possible double interactions and the triple interaction between all three reform types. We cluster standard errors at the firm-level.[29] We lag the reform indicators by two years as we are interested in the medium to long-term effects of reforms. Our empirical results therefore describe the (average) reform impact after two years and beyond.[30] All other variables are lagged by one year. This means that we assume that changes in the health of a firm's main bank have a more immediate impact than legislative changes at the country-level.[31] Our

of benefits. In contrast, the Dutch reform of 2007 only impacted the duration of benefits and the Irish reform of 2011 only concerned the level of benefits.

[29]In a robustness check in Section C of the appendix, we verify that our main result is robust to clustering at the country-level.

[30]There are two main reasons why we do not look at short-run effects. Firstly, our data is at a low, annual, frequency which reduces our power to find short-run effects. Secondly, in the short-run employment deregulations are likely to increase layoffs and reductions in unemployment benefit schemes can lead to negative short-run aggregate demand effects. These two negative short-run mechanisms do not have clear link to bank financing conditions, as opposed to the long-run mechanism increasing employment, which we have described in Section 2.

[31]We verify that our main result is robust to lagging also reforms by one year only.

main coefficients of interest are β_3, β_5, and β_7 and we expect them to have negative values reflecting the negative interaction between bank weakness and reforms. If the data did not support our hypothesis, this would suggest that European firms can easily finance new workers out of internal internal cash-flows or non-bank sources of external finance. It would also suggest that capital and labour are relatively substitutable so that firms do not need to purchase costly new machines to increase employment in response to reforms. One could even imagine a positive interaction effect if firms that are both operating with high-substitutability technologies and are connected to weak banks use labour market reforms as an opportunity to reduce their dependence on their relationship bank by making production less capital intensive.[32]

We estimate equation (2) with two different levels of fixed effects. In the less restrictive case, we use sector-year fixed effects and firm fixed effects. Sector-year fixed effects control for time-varying industry trends, such as the growing importance of the ICT sector. Firm-fixed effects control for time-invariant differences among the firms in our sample, including time-invariant differences at the country-level, such as different legal systems. Moreover, since in our setting each firm is connected to exactly one bank, the firm fixed effects encompass bank fixed effects. The interaction terms between each reform type and the weak bank indicator capture the difference across firms within the treatment group along our weak vs. strong bank dichotomy: In other words, we compare firms that were connected to weak banks after the implementation of a reform with firms that were connected to strong banks at the same point in time.

We can sharpen our analysis by substituting our sector-year fixed effects with country-sector-year fixed effects. While this does no longer allow us to identify the effect of reforms per se, we are able to compare treated firms connected to either weak or strong banks within the same country, industry and year. This way we can also control for very specific but relevant confounding factors like country-sector-year specific wage bargaining agreements.

We do not necessarily claim that the empirical relationships we uncover are causal. The difference in reform outcomes between firms connected to weak and firms connected to strong banks is policy-relevant even if it merely constitutes a conditional correlation. The following two assumptions are helpful for reducing the potential for spurious correlations.

Firstly, we assume that labour market reforms are orthogonal to firm-level decisions. The biggest threat to our identification would come from firm- or sector-level lobbying

[32]We specifically analyse the role of the elasticity of substitution in Section B of the appendix.

increasing the probability of reforms. However, there is no empirical or theoretical support for the lobbying explanation of reforms. Instead, both theory and empirics point to a host of country-level determinants that affect the probability of the implementation of labour market reforms. The theoretical literature points to recessions (Fernandez and Rodrik, 1991), a country's degree of unionisation (Saint-Paul, 2002), and voting systems (Pagano and Volpin, 2005) as the most important determinants of labour market regulation. On the empirical side, Simintzi et al. (2015) find that the degree of employment protection is linked to a country's degree of unionisation and income equality. According to Botero et al. (2004), a country's legal origin is the most important determinant of the stringency of employment protection. Duval et al. (2018a) and Dias da Silva et al. (2017) look at structural reforms in general and identify crises, high unemployment rates and political factors like EU directives or the EU accession process as the most important country-level determinants. We automatically control for all of these time-varying and time-invariant country-level factors in our most restrictive specification featuring firm- and country-sector-year fixed effects.

Our second identification assumption is that bank financing is orthogonal to both reforms and firm-level employment. It is unlikely that variations in the health of a single bank drive a reform at the country-level. A more serious threat would be reverse causality emanating from large firms that are able to drag down their creditors as their own health worsens. We address this concern by excluding large firms from our sample. Moreover, it is possible that only a certain type of firm matches with weak banks. Then it might be that the firm-level employment patterns we observe are driven by some unobservable factor driving both bank health and firm-level employment. We address the issue of endogenous bank-firm matching by excluding a group of firms likely to have matched with weak banks purposefully.

Note that the positive credit supply effects found by Alimov (2015) and Moro et al. (2017) do not threaten our identification. According to the these studies, labour market deregulation reduces banks' perception of borrower credit risk because it increases firms' flexibility to react to financial distress. As long as weak banks do not perceive the change in borrower risk differently from strong banks, this positive credit supply effect of labour market reforms will bias our analysis against finding a negative differential effect of bank distress on employment gains at the firm-level.

In Table 2, we show summary statistics for the firm-year observations in our regression sample.[33] We show descriptive statistics for firm-level variables (Panel A),

[33]Note that due to the lag structure of equation (2), our regression sample effectively uses reform

variables describing the balance sheet of each firm's main bank (Panel B), country-level variables (Panel C) and sector-level variables (Panel D). Table 3 shows the distribution of banks and firms by country within our final regression sample. We refer the reader to Section C of the appendix for robustness checks addressing concerns about coverage heterogeneity.

5 Main results

5.1 Baseline results

In Table 4, we show the results of our baseline regression based on equation (2). Column 1 shows the results for the specification using the less restrictive set of fixed effects, which allows us to gauge the effect of labour reforms per se. Here we capture variation in employment within firms and across countries. A firm located in a country implementing a reform will belong to the treatment group while a firm located in a country that is not implementing a reform will belong to the control group. The results in column 1 suggest that labour market reforms have a positive medium to long-term effect on firm-level employment. Recall, again, that we are using unweighted regressions focusing on the employment response of the average firm. It might well be that the aggregate employment impact of the reforms we study is significantly different from the effects we are measuring.

Among the three reform types, unemployment benefit schemes has the most positive effect amounting to a permanent shift of 8.5% in employment for firms connected to strong banks.[34] The same effect is only 4.8% for firms connected to weak banks after the implementation of the reform.[35] This confirms our hypothesis of a negative interaction between bank weakness and labour market reforms. The reforms induce a shift in firm-level employment but the size of this shift is moderated by bank financing conditions.

data from 1999-2013, bank-and firm explanatory variables from 2000 to 2013 and firm-level employee data from 2001 to 2014.

[34]This is calculated based on the $(exp(\beta) - 1) * 100$ formula needed in log-level specifications. Also note that, strictly speaking, this number only describes the effect of implementing an unemployment benefit reform when a country has not yet implemented any other reform type, i.e. at a point in time where all three reform indicators are still equal to zero. Furthermore, note that our results only refer to the intensive margin since we are capturing within firm variation and because newly created firms might not immediately start reporting balance sheet information to chambers of commerce or other data providers.

[35]This is calculated using $(exp(\beta_4 + \beta_5) - 1) * 100$. Note that we dichotomise the weak bank indicator based on the 75h percentile of bank stress in the regression sample containing the 2,075,151 observations from column 1 and do not re-define it for subsequent regressions.

If firms do not have sufficient access to financing in the aftermath of the reform, some part of the potential employment boost is permanently lost.

We also find a positive effect of regular employment reforms on firm-level employment and a negative interaction between regular employment reforms and bank weakness. However, the coefficients are much smaller in magnitude than the coefficients for the case of unemployment benefit schemes. The fact that our results are less strong for regular employment reforms can be explained by the potentially very negative short-term effects discussed in Section 2.1. Even though we are capturing medium to long-term shifts in employment, a very large negative short-term effect will reduce the size of the overall estimate.

We find a positive interaction effect between temporary employment reforms and bank weakness. This result could be driven by country-specific peculiarities in the way labour markets react to bank stress. Caggese and Cuñat (2008) analyze the dualism of Italy's labour market where fixed-term employees absorb most of the firm-level employment volatility. They find a substitutability between access to credit and fixed-term employment. Firms with insufficient access to credit try to regain flexibility by hiring mostly on a temporary basis. It might be that firms in the European periphery, where dual labour markets are common, use temporary employment reforms as an opportunity to further intensify this strategy.[36]

In column 2, we compare treated firms connected to strong banks with treated firms connected to weak banks within the same country-sector-year cell. The advantage of this framework is that we are able to control for a range of confounding factors at the country-sector-year level. For example, the effect of country-specific business cycle conditions, which might drive both firm-level employment and the decision to implement reforms, and events like a country- and sector-specific wage-bargaining agreement are controlled for. The significantly negative interaction between unemployment benefit reforms and bank weakness is preserved and only slightly decreases in magnitude. The coefficient on the interaction between temporary employment reforms and bank weakness becomes insignificant and even turns around in terms of sign. The country-sector-year fixed effects have absorbed the cross-country differences in labour market

[36]Recall, however, that our estimates concerning temporary employment reforms are subject to some measurement error induced by country-specific counting rules for temporary agency workers. This measurement error will be especially large in the specification of column 1, where the less restrictive fixed effects structure allows for cross-country variation. For these reasons temporary employment reforms and their interaction with bank weakness is not the focus of this study and we refrain from showing the respective estimates outside of Table 4. We do, however, consider them to be important control variables and they are therefore included in all of our estimations.

dualism which were likely driving the positive interaction in column (2). The negative interaction between regular employment reforms and bank weakness is preserved but loses statistical significance.[37]

5.2 Channels

To explore the channels behind our main result, we use a triple difference-in-differences framework. We build on the specification with firm- and country-sector-year fixed effects and introduce a third factor, denoted as η, that arguably influences the link between the success of labour market reforms and bank financing conditions. We want to explore whether we are indeed identifying the mechanism we have in mind: A bank-dependent firm will benefit less from labour market deregulation if it cannot get a loan to fund the hiring of new employees and the machines needed to provide a productive work environment. We look into three different measures of η: i) a measure of sector-specific external financial dependence, ii) a measure of the durability of the goods produced by each sector, and iii) an binary indicator for negative country-level GDP growth.

In column 1 of Table 5, we let η denote each sector's reliability on external financial dependence, which is calculated in a study by Duygan-Bump et al. (2015). The authors construct a sector-specific measure of external financial dependence by calculating the percentage of capital expenditure financed with external funds. Their measure reflects technological reasons that constrain a firm financially. For example, certain industries, such as pipelines or metal mining, require higher fixed costs and feature longer time periods until a given investment pays off relative to less constrained sectors like the tobacco industry. The identification approach in this triple interaction regression is inspired by the argumentation of Rajan and Zingales (1998). Since the external financial dependence measure is constructed based on US data, we get a source of variation in the tightness of the link between the health of a firm's main bank and firm-level employment that is orthogonal to the variation within our dataset.[38]

[37] Another factor that could lead us to underestimate the effects or regular employment reforms could be that employment protection might not be binding to the same degree across firms and industries. However, in Section C of the appendix we find that the interaction of regular employment reforms and bank weakness remains insignificant even in a subsample of firms, where employment protection is likely to bind.

[38] Moreover, Rajan and Zingales (1998) argue that the US is the perfect economy to obtain an estimate of external financial dependence that is capturing actual variation in sector-level technologies rather than country-specific variation since it is the economy with the most developed financial system.

We find a significantly negative coefficient on the triple interaction between unemployment benefit reforms, bank weakness and the external financial dependence of a firm's sector-level technology. This implies that the negative interaction between bank weakness and labour market reforms is especially high for firms in sectors using technology characterised by high external financial dependence. In contrast, firms operating in sectors with low fixed costs and abundant cash flows will not be as affected if their main bank becomes troubled.

Another factor relevant for a firm's financial vulnerability is its products durability. In recessions, consumers cut spending more quickly on durable goods than on non-durable goods. Thus, durable-good producers are more financially vulnerable in recessions and less able to make up reduced access to credit from their main bank by using own funds. In column 2 of Table 5, η captures the durability of each sector's output taken from Bils et al. (2013). We find that our negative interaction between reforms and bank weakness is especially high for firms operating in high-durability sectors. This is in line with economic intuition because durable goods producers are especially vulnerable in business cycle downturns. However, the triple interaction using the measure of durability is less sizable and significant than the triple interaction which uses our measure of external financial dependence.[39]

In line with the previous result, column 3 shows that the negative interaction between unemployment benefit reforms and bank weakness is especially strong during recessions. Here, η is a time-varying indicator that is equal to one if a country's GDP growth turns negative. The coefficient on the constituent term for the interaction of bank financing and recessions is significantly negative. Being connected to a weak bank has a particularly detrimental effect on employment during recessions when firms themselves have weaker balance sheets.[40] The constituent term on unemployment benefit reforms and bank weakness shows that the interaction between bank financing and labour market reforms is also significant outside of recessions. Crises exacerbate but do not fully drive the link between reform success and bank financing conditions.

According to theoretical work by Buera et al. (2015), another important factor that tightens the link between bank financing and firm employment is a firm's size, since small firms are usually more reliant on credit to finance their production. When examining the role of firm size, we refrain from using a triple interaction model since

[39]Note that Bils et al. (2013) are able to calculate their durability measure only for a subset of sectors, which reduces the sample size and the power of our model.

[40]See the discussion in Giroud and Mueller (2017) on how weak household-, firm- and bank balance sheets reinforced each other exacerbating the impact of the Great Recession in the US.

our results would be entirely driven by the few firms that switch the size class during the sample period. Instead, we run subsample regressions and compare the coefficients among the different subsamples. We use the European Commission's definition of micro, small, medium, and large firms described in European Commission (2015) and run our baseline regression in the subsamples of i) micro- and small firms, ii) medium-sized firms, and iii) large firms. The results in column 1 of Table 6 are even stronger than the results from column 2 of Table 4, which suggest that the link between bank financing conditions and reforms indeed is strongest for smaller firms, which is in line with economic intuition. Smaller firms are more dependent on bank financing because they do not have easy access to alternative sources of funding. Thus, they will be especially inhibited in their ability to benefit from labour market reforms if their main bank is not in good health. We do not find any significant effects in columns 2 and 3 representing the subsamples of medium and large firms, respectively, which can, however, be due to small sample sizes.[41]

6 Robustness checks

Table 7 deals with the concern that our results might driven by endogenous bank-firm matching. Ultimately, we cannot fully rule out that the ex-ante process of relationship building between banks and firms is driving our results without actually observing this process. However, we can use the variation at hand to identify a subset of firms for which endogenous bank-firm matching is especially relevant. Specifically, we exclude firms that are already connected to a weak bank in the year they enter the sample. These could be firms that have picked relatively weak banks on purpose because they were enticed by better loan conditions or were unable to build a relationship with stronger banks. By excluding this set of firms, we are only left with variation that comes from firms starting out with a link to a bank in good health. Then a firm's relationship bank can either remain in good health throughout the sample period or become stressed (i.e. become a weak bank) over time.

In this specification, our main result of the negative interaction between bank weakness and unemployment benefit reforms remains intact. Even the negative coefficients on the interaction term for regular employment reforms turns significant. Thus, the

[41] Given that the results are strongest for small-and micro firms, we find it useful to provide a small back-of-the-envelope calculation in Section D of the appendix using employment-weighted averages of bank weakness at the country-level to get a better sense of the aggregate importance of our estimates at the country level.

exclusion of firms for which endogenous bank-firm matching is especially relevant has actually increased the power of our model. This suggests that, if anything, endogenous bank-firm matching will make us underestimate the negative interaction effect between bank weakness and labour market reforms. According to Schwert (2018), bank-dependent firms are more likely to match with healthy banks while less dependent firms purposefully match with banks that are less well capitalised. This would explain why excluding firms more likely to match with weak banks actually increases our ability to find a negative interaction between labour market reforms and bank weakness.

Another concern is that banks might become stressed due to the deteriorating health of their borrowers. This argument is most relevant for large firms that are able to drag down their creditors once they become distressed. This could happen for example by outright default or by missed interest payments by a firm whose size makes it a significant contributor to its main bank's overall loan portfolio. The regression in column 1 of Table 6 already addresses this concern by estimating our model only in the subsample of micro and small firms, which automatically excludes firms that do not belong to the group of small and medium enterprises (SMEs).[42]

In Section C of the appendix, we show a series of further robustness checks in order to verify that our results are neither driven by specific subsamples nor overly sensitive to specification choices. We show that our results are robust to excluding certain countries, looking at different firm-age subsamples, controlling for the weighted average cost of capital (WACC), excluding all control variables, clustering at the country level, lagging reforms only by one year, using continuous or three-category measures of bank stress, excluding firms connected to more than one bank, taking into account the degree to which employment protection is binding, and excluding certain reforms.

7 Conclusion

Economic theory suggests that labour market deregulation can increase employment by decreasing the user cost of labour and thus encouraging firms to implement new projects. However, entrepreneurs can only take advantage of labour market deregulation if they can get sufficient financing to be able to pay the new employees and purchase the new machines required for the marginal project. We find that bank financing

[42] In an unreported regression we only exclude large firms, rather than excluding medium and large firms. Thus we are left with a sample of only SME-firms. The results are very similar and available upon request.

is an important moderator of the impact of labour market deregulation on firm-level employment. We look at reforms of employment protection for regular workers, unemployment benefit reforms and reforms of the regulation of temporary employment. The negative interaction effect between labour market reforms and bank weakness is most robust in the case of unemployment benefit reforms. Specifically, we find that firms that were connected to a weak bank after the implementation of an unemployment benefit reform increase employment by only half as much as similar firms connected to stronger banks.

Moreover, we find that the negative effect of weak banks on firm-level employment gains after reforms is especially strong for firms operating in sectors with a high dependence on external financing, for durable goods producers, for small firms, and during recessions.

In a series of robustness checks, we address concerns about endogenous bank-firm matching, and reverse causality. Moreover, we verify that our results are robust to different subsamples and do not depend on specification choices.

Our results highlight the importance of policies complementing labour market reforms with a comprehensive strengthening of bank balance sheets. Ensuring that banks are adequately capitalised, unburdened by high levels of non-performing loans, and sufficiently liquid will help entrepreneurs to realise the full potential of employment gains after the implementation of labour market reforms. Our findings are particularly relevant for countries in the euro area periphery where high levels of labour market regulation go together with bank weaknesses.

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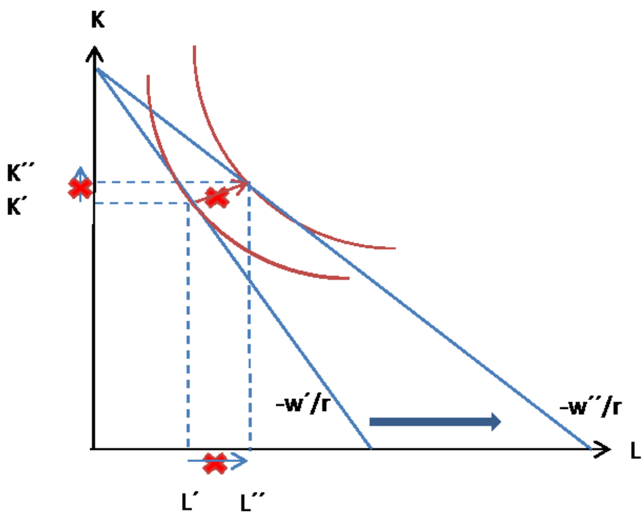

Figure 1: CES production function with intermediate σ
This figure visualises a CES production function of the form $Y = F(K, AL) = (\alpha(K)^{\varepsilon} + (1-\alpha)(AL)^{\varepsilon})^{1/\varepsilon}$. The elasticity of substituion, σ, is assumed to take on an intermediate value between zero and one. The labour reform shock lowers the user cost of labour shifting the isocost curve outwards. Financing, visualised with red crosses prevents the firm from increasing K and L as much as it would desire thereby inhibiting the jump to a higher profit curve.

Table 1: Labour market reforms across countries
This table shows the incidence of reforms concerning employment protection of regular workers, the replacement rate and duration of unemployment benefit schemes and reforms of the regulation of temporary employment within our sample. The incidence numbers relate the years in which reforms were enacted to the total number of country-years over 1999-2013. The total number of country-years excludes Luxembourg due to its small size and the reform incidence excludes an unemployment benefit reform enacted in Portugal in 1999 because this country-year is not in our sample. Please see Appendix Tables A.4, A.5, and A.6 for the evolution of the recursive reform indicator built by using the implementation dates of the reforms below.

Country	Regular	Unempl.	Tempor.
Austria	1	0	0
Germany	1	1	3
Spain	3	0	3
France	2	0	0
Greece	2	0	2
Ireland	2	1	1
Luxembourg	0	0	0
Netherlands	0	1	1
Portugal	2	1	3
Total	13	4	13
Incidence	11%	3%	11%

Table 2: Summary statistics

This table shows summary statistics for the observations in our regression sample. Panel A reports summary statistics for our dependent variable, the log of employment and several firm-level control variables. Panel B reports summary statistics on the variables describing the health of each firm's main bank. Panel C reports summary statistics on our country-level control variables. Panel D reports summary statistics for several time-invariant sector-level measures that are meant to characterize the technology used in each sector. Refer to Appendix Table A.1 for variable definitions.

	N	Mean	SD	P25	Median	P75
Panel A: Firm-level variables						
Ln(Employment)	2,075,151	2.277	1.392	1.386	2.197	3.091
Leverage	2,075,151	0.190	0.224	0.003	0.106	0.303
Cash	2,075,151	0.142	0.174	0.019	0.072	0.201
Tangibility	2,075,151	0.214	0.221	0.042	0.136	0.322
Return on assets	2,075,151	0.019	0.123	0.000	0.019	0.062
WACC	1,747,585	0.015	0.016	0.003	0.009	0.021
Panel B: Firm-bank-level variables						
Bank size	2,075,151	11.402	1.591	10.629	11.525	12.858
Bank stress	2,075,151	-0.028	0.309	-0.200	-0.056	0.106
Weak bank (bank stress >= p75)	2,075,151	0.250	0.433	0.000	0.000	1.000
Panel C: Country-level variables						
Real GDP growth	2,075,151	-0.001	0.025	-0.018	0.002	0.019
Sector growth	2,075,151	-0.006	0.074	-0.033	0.001	0.027
Recession	2,075,151	0.372	0.483	0.000	0.000	1.000
Panel D: Sector-level variables						
EFD	1,953,760	0.184	0.283	-0.050	0.160	0.380
Elasticity of substitution (Ciminelli)	2,075,151	0.535	0.257	0.390	0.470	0.530
Elasticity of substitution (Laeven)	2,054,937	0.979	0.475	0.720	0.780	0.920
Durability	1,841,488	1.391	1.364	0.100	0.960	2.270

Table 3: Firms and banks by countries
This table shows the distribution of firms and banks within the regression sample across countries. Note that banks can be present in multiple countries.

Country	Firms	Banks
AT	3,105	21
DE	10,631	791
ES	151,844	86
FR	115,512	159
GR	16,996	17
IE	4,572	23
LU	311	5
NL	1,412	16
PT	83,539	69

Table 4: Labour market reforms, weak banks and employment
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. Note that each firm is connected to one main bank so that firm fixed effects implicitly encompass bank fixed effects. In addition, firm fixed effects also encompass country fixed effects. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank, country-level time-varying information and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)	
	(1)	(2)
Regular empl. reform	0.004***	
	(2.71)	
Regular empl. ref. × weak bank	-0.012***	-0.001
	(-10.92)	(-0.58)
Unemployment benefit reform	0.082**	
	(2.36)	
Unempl. benefit ref. × weak bank	-0.035***	-0.023***
	(-5.80)	(-3.92)
Temporary empl. reform	0.007**	
	(2.44)	
Temporary empl. ref. × weak bank	0.049***	-0.002
	(18.93)	(-1.06)
Regular empl. ref. × temp. ref.	-0.023***	
	(-15.97)	
Regular empl. ref. × unemployment ref.	-0.028**	
	(-2.39)	
Temporary empl. ref. × unemployment ref.	-0.070**	
	(-2.40)	
Reg. × temp. × unempl.	0.025	
	(1.23)	
Weak bank	-0.011***	-0.001
	(-6.72)	(-0.62)
Sector growth	0.033***	
	(5.03)	
Real GDP growth	0.837***	
	(18.26)	
Leverage	-0.017***	-0.013***
	(-4.30)	(-3.24)
Cash	-0.034***	-0.047***
	(-6.90)	(-9.79)
Tangibility	0.061***	0.061***
	(8.58)	(8.62)
Return on assets	0.228***	0.218***
	(50.92)	(49.28)
Bank size	0.004***	-0.004***
	(3.83)	(-3.51)
Firm FE	Yes	Yes
Sector-year FE	Yes	No
Country-sector-year FE	No	Yes
Observations	2,075,151	2,070,262
Adjusted R^2	0.934	0.935
Number of firms	387,922	387,113
Number of banks	1,121	1,116
Mean dep. var.	2.277	2.272
Clustering	Firm	Firm
Sample period	2001-2014	2001-2014

Table 5: Channels: External financial dependence, durability, and recessions
This table reports estimates from a triple interaction framework complementing the double interaction model from equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank, time-invariant sector-level measures and the three recursive reform indicators. In column 1, η relates to external financial dependence and in column 2 it relates to the durability of goods produced, both at a time-invariant sector level. In column 3, η reflects a binary country-year indicator equal to one in years of negative GDP growth. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)		
	(1)	(2)	(3)
Regular empl. ref. × weak bank	-0.002	-0.002	-0.001
	(-1.06)	(-1.40)	(-0.74)
Regular empl. ref. × weak bank × η	0.004	0.001	0.001
	(0.96)	(1.45)	(0.29)
Unempl. benefit ref. × weak bank	-0.013*	-0.010	-0.026***
	(-1.87)	(-1.12)	(-3.16)
Unempl. benefit ref. × weak bank × η	-0.053**	-0.009*	-0.427*
	(-2.50)	(-1.70)	(-1.65)
Weak bank× η	-0.005	-0.001	-0.005**
	(-0.83)	(-0.93)	(-2.18)
Weak bank	-0.001	-0.000	-0.000
	(-0.49)	(-0.04)	(-0.01)
Temporary empl. ref. × weak bank	Yes	Yes	Yes
Temporary empl. ref. × weak bank × η	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes
Bank size	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Sector-year FE	No	No	No
Country-sector-year FE	Yes	Yes	Yes
Observations	1,870,553	1,764,339	2,070,262
Adjusted R^2	0.939	0.939	0.935
Number of firms	354,845	335,332	387,113
Number of banks	1,106	1,099	1,116
Mean dep. var.	2.246	2.221	2.272
Clustering	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014

Table 6: Small and micro, medium, and large firms
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. In column 1, we only include micro firms and small firms according to the definition of the European Commission (European Commission, 2015) while in columns 2 and 3 we only include medium and large firms, respectively. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)		
	(1)	(2)	(3)
	Small and micro	Medium	Large
Regular empl. ref. × weak bank	0.001	-0.002	0.012
	(1.18)	(-0.60)	(1.31)
Unempl. benefit ref. × weak bank	-0.025***	-0.009	0.013
	(-4.29)	(-0.60)	(0.19)
Weak bank	-0.002	0.000	0.003
	(-1.15)	(0.01)	(0.24)
Temporary empl. ref. × weak bank	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes
Bank size	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Sector-year FE	No	No	No
Country-sector-year FE	Yes	Yes	Yes
Observations	1,793,468	199,037	50,299
Adjusted R^2	0.898	0.892	0.935
Number of firms	344,437	42,426	10,345
Number of banks	1,021	778	385
Mean dep. var.	1.912	4.318	5.978
Clustering	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014

Table 7: Robustness: Firms that are less likely to match with weak banks purposefully
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. We exclude firms that are already connected to a weak bank in the moment they enter the sample. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
	Excl. firms with weak bank in t_0
Regular empl. ref. × weak bank	-0.003*
	(-1.86)
Unempl. benefit ref. × weak bank	-0.021***
	(-3.62)
Weak bank	0.002
	(0.95)
Temporary empl. ref. × weak bank	Yes
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	1,810,791
Adjusted R^2	0.935
Number of firms	339,273
Number of banks	1,055
Mean dep. var.	2.298
Clustering	Firm
Sample period	2001-2014

Online Appendix for "Firm-Level Employment, Labour Market Reforms, and Bank Distress"

A Firm sample composition

We exclude firms operating in special sectors, such as agriculture and mining, or firms from the financial sector, given their specific leverage characteristics. Thus, we focus on eleven NACE sections (sections C-N, excluding K) with most firms operating in manufacturing, wholesale and retail trade, real estate and construction services, as well as food and accommodation services. The advantage of the ORBIS database relative to databases containing only public companies is that it also includes SMEs, which are an important part of the euro area economy. See Kalemli-Ozcan et al. (2015) for a thorough discussion of the representativeness of ORBIS relative to Eurostat data. Most relevant to our study is the representativeness in terms of employment. In Table A.2 we show the total number of employees in our cleaned firm-level dataset from the nine countries relevant for our study and relate it to Eurostat data. Coverage is very heterogeneous across time and countries because ORBIS collects data from different national providers that in turn collect data based on public disclosure rules subject to changes over time.[1] In Section C, we address concerns on coverage heterogeneity of ORBIS.

B Elasticity of substitution between labour and capital

Our analysis in Section 2 relies on the assumption that labour and capital are to some extent complementary. In the few sectors where capital and labour are substitutes, i.e. where the elasticity of substitution is above one, the interaction between bank financing conditions and labour market reforms is driven by two countervailing forces. On the one hand, the need for working capital financing still generates a negative interaction effect between bank weakness and employment gains induced by labour market reforms. On the other hand, a firm operating with a technology characterised by substitutability could respond to the reform by increasing its use of labour at the expense of capital. If such a firm is also connected to a weak bank it might be especially prone to sell some of its capital stock to finance new employees and to make its production process less

[1] The data providers and the disclosure rules determining coverage are described in detail in Table A.1 in Kalemli-Ozcan et al. (2015)

dependent on external financing by its weak relationship bank. Depending on which of the the countervailing forces is stronger, the overall interaction effect between bank financing and labour market reforms might even be positive.[2]

We want to investigate the role of the elasticity of substitution by splitting our sample above and below the economically relevant threshold of an elasticity of one. Unfortunately, the estimation of sector-specific elasticities of substitution relies on a myriad of assumptions and is therefore highly method dependent. As a case in point, the construction sector has the lowest estimated elasticity of substitution among all sectors in the study by Ciminelli et al. (2018), whereas it has the highest elasticity in the study by Laeven et al. (2018). We opt to use both measures in two separate sample splits while excluding the construction sector to make both sets of estimates more comparable.

In Table A.7, we compare firms in sectors with an elasticity of substitution below and above one. In columns 1 and 2, we use the estimates by Ciminelli et al. (2018), while we use the estimates by Laeven et al. (2018) in columns 3 and 4. Columns 1 and 3 show our baseline results for a sample of sectors with an elasticity below one. Here the theoretical prediction is unambiguous and in fact our baseline results remains intact. In columns 2 and 4, we look at firms operating in sectors with an elasticity above one. We do not find any evidence for a positive interaction effect between bank financing and labour market reforms.[3] Notably, in column 2 we are able to find a negative interaction effect between regular employment reforms and bank weakness which is not statistically significant in the corresponding regression for firms operating with complementarity in column 1. This might be driven by different magnitudes of the short-term effect of regular employment reforms. As we discussed in Section 2, laying off employees can be implemented faster than searching for and hiring new ones. This short-term negative effect might be especially strong for firms that are operating with substitutability and are connected to weak banks. The labour market reforms might allow them to lay off employees without fearing unduly disruption in their production. In contrast, firms with technologies characterised by complementarity might be more cautious in implementing layoffs, given the risk that machines might become idle and

[2]Laeven et al. (2018) look at a financial shock instead of a labour market shock but their study suggests a mechanism similar to the one described above. They find that Spanish firms that are both subject to an elasticity of substitution greater than one and face lower levels of financial frictions because they fall below a size threshold react to the financial crisis by increasing employment.

[3]Our inability to find a positive effect could, of course, also be due to the severely diminished sample size in these two subsample regressions.

cannot be put to good use without adequate numbers of employees.

C Further sensitivity checks

As discussed in Section 10, coverage in ORBIS can be uneven across countries and over time. Two things stand out in Table A.2, which relates ORBIS employment numbers to the corresponding Eurostat figures. Firstly, our ORBIS sample over-represents employment for Luxembourg, the Netherlands, and Ireland. The disadvantage of using unconsolidated accounting data is that corporate profit shifting motives might especially affect balance sheet data of large multinational companies. According to Tørsløv et al. (2018), the three countries mentioned above feature corporate tax systems particularly attractive for multinationals, which is why profit shifting is especially relevant in their case. Column 1 of Table A.8 verifies that our main result is robust to excluding all three countries. Secondly, due to missing data on employees, the coverage for Portuguese companies from 2002 to 2005 is almost non-existent. Column 2 verifies that our results are also robust to excluding data from Portuguese companies prior to 2006.

Firm age is another relevant factor for our purposes since it moderates the need for financing. While young firms might still be struggling to establish themselves on the market and build relationships with creditors, older firms are more likely to have established stable business- and bank relationships. In columns 3 and 4 of Table A.8, we show subsample regressions for young and old firms using the 25-years threshold from Gal and Hijzen (2016). Our main result of a negative interaction between bank weakness and unemployment benefit reforms can be reproduced in both subsamples.

In our baseline regression, we focus on the general relationship between access to credit, proxied by the health of a firm's main bank, and firm-level employment dynamics. Ideally, we would also have very detailed information on the interest payments on specific loans provided by a firm's main bank that we can then compare to an appropriate benchmark interest rate for each firm's peer group. Due to data availability, we can only construct a relatively crude measure of a firm's weighted average cost of capital (WACC). To estimate the WACC, we use a measure that lumps together dividends and interest paid on debt and divide this measure by total assets. Table A.9 shows that controlling for this measure leaves our main result qualitatively unchanged.

In Table A.10, we want to assure the reader that our results are not driven by our choice of control variables. We re-estimate our baseline regression without any firm-level controls and without controlling for bank size. Omitting all control variables decreases the explanatory power of our model and lowers the coefficient for our main result, the negative interaction between unemployment benefit reforms and the binary weak bank indicator. At the same time, we also gain some explanatory power relative to our baseline by including a larger set of firms in our analysis. This allows us to find a slightly significant negative interaction effect between weak banks and regular employment reforms, while this effect is insignificant in our baseline regression.

In our baseline regression, we cluster standard errors on the firm-level to account for firm-specific unobserved shocks that induce correlation between all within-firm observations. Clustering at the country-level is a sensible alternative to clustering at the firm-level since the reforms we are studying are legislated at the country-level (see the discussion in Bertrand et al. (2004)). In Table A.11, we cluster standard errors at the country- instead of at the firm-level. The significance of our main results is preserved. Note, however, that this regression has to be taken with a grain of salt due to the low number of clusters, given that our sample only includes nine countries.

Recall that in our baseline regression we lag reforms by two years while all other explanatory variables are lagged by one year in order to allow for a recognition lag among entrepreneurs. In Table A.12, we lag all explanatory variables by one year and find that our main result remains intact.

In our baseline model, we dichotomise bank stress based on the 75th percentile within the regression sample in order to ease interpretability of the interaction effects. In Table A.13, we use the continuous version of the bank stress variable instead of the binary weak bank indicator. We can reproduce our main result of a negative interaction between unemployment benefit reforms and bank weakness. Note that in this regression we are sacrificing ease of interpretability for the sake of more detailed variation in bank stress. Here, we can only conclude that the employment effect of unemployment benefit reforms diminishes with *increasing* bank stress. In Table A.14, we divide bank stress into three terciles instead of dichotomising it. Here we find that the interaction of unemployment benefit reforms with the highest tercile of bank stress features an even higher coefficient than the one in our baseline regression (where the weak bank indicator captures the highest quartile).[4]

[4]Note that the positive coefficient on the constituent term of medium and high bank stress is not a cause for concern. Due to the interactive structure of our model, these coefficients only measure the relative effect during those years in which a country has not implemented a single one of the three reform types in our sample. Thus it captures the difference between firms connected to banks with low stress relative to firms connected with higher levels of stress during limited and relatively early country-year subsamples.

Table A.15 features a robustness check that excludes firms having reported more than one bank relationship in the bank-firm linkage part of the ORBIS dataset.[5] As noted by Kalemli-Ozcan et al. (2019), multi-bank firms are common in exactly those countries that feature relatively stressed banking sectors, i.e. Spain, Portugal, and Greece. Thus, dropping multi-bank firms amounts to a rather extreme change in our sample composition. We therefore opt to re-define the binary weak bank indicator on the basis of the new subsample instead of defining it based on our original baseline regression subsample. Column 1 shows the result of using this newly defined binary weak bank measure. With this estimation framework we are unable to recover our main result in the new subsample. Given the considerable reduction in the variation of bank stress that is available in this subsample, we think it is a sensible choice to use the continuous version of our bank stress indicator rather than the binary measure. When using the continuous bank stress variable (also used in Table A.13), we are indeed able to reproduce our main result also within the subsample of firms that have reported only one relationship bank.

As discussed in Section 5.1 of the manuscript, one reason for the relatively weak results for regular employment reforms in our baseline approach might be that we both consider sectors where employment protection might be strongly binding and sectors where it might be non-binding or only weakly binding. As laid in e.g. Bassanini et al. (2009) and Ciminelli et al. (2018), employment protection should have a larger effect on firms, which regularly adjust their workforce in comparison to firms whose workforce is rather fixed over time and therefore are little affected by dismissal protection rules. Ciminelli et al. (2018) construct a measure that uses the percentage of laid-off workers across US industries to proxy a firm's technology-driven propensity to adjust the workforce. In Table A.16, we use their estimates to differentiate between firms operating in sectors with a propensity below the 75h percentile within the regression sample (column 1) and firms operating in sectors above the 75th percentile (column 2). The results for regular employment reforms remain insignificant in both specifications, while our main result on the interaction of unemployment benefit reforms and bank weakness holds across both subsamples.

Given that our baseline results are strongest for unemployment benefit reforms, we offer a robustness check in Table A.17, where we exclude each of the four unemployment benefit reforms in our sample using both the less and the more restrictive fixed effects specification from our baseline regression table (Table 4 in the manuscript). We

[5] Recall that in these cases we keep the bank with the highest total assets in the year 2000.

exclude German firms from 2005 onward in columns 1 and 2, Dutch firms from 2007 onward in columns 3 and 4, Irish firms from 2011 onward in columns 6 and 7, and Portuguese firms from 2012 onwards in columns 7 and 8. With the exception of column 8, our main result of a negative interaction between unemployment benefit reforms and bank weakness holds across all reform exclusion and fixed effects combinations. The absence of significant results in column 8 is not surprising given the extraordinarily good coverage of Portuguese firms in ORBIS (see Table A.2) and the relatively high bank stress among banks connected to Portuguese firms (see Table A.3).[6] These two features imply that Portugal contributes a lot of observations and within-country variation, which is especially relevant in the specification with country-sector-year fixed effects, where we compare firms within the same country-sector at the same point in time. In the less restrictive fixed effects specification in column 7, we still find a (weakly) significant negative interaction effect between unemployment benefit reforms and bank weakness even after excluding the Portuguese unemployment benefit reform.

D Back-of-the-envelope-calculation

As we discussed in Section 1 of the manuscript, our estimates cannot strictly be interpreted in a macroeconomic sense. Nonetheless, it is illustrative to consider the implications of our estimates for emplomyent at the country level. As discussed e.g. in Chodorow-Reich (2014) such an aggregation exercise based on bank-firm data relies on two, admittedly rather strong, assumptions: First, the general equilibrium effect is simply the sum of the partial equilibrium effects at the firm-level. Second firms connected to banks that we do not classify as weak are unconstrained, i.e. would not create more employment if they were connected to an even stronger bank.

To gain a sense of the aggregate implications of our estimates we start by calculating the weighted average share of firms connected to weak banks for each sector (at the Nace 64-sector level) in each country, using firm-level employment from our dataset as weights. We then average this measure across all sectors within a given country-year, using aggregate sector-level employment from Eurostat as weights. In the next step, we take the average of this country-year level measure of weak bank prevalence for the post-reform period.[7] This provides us with a proxy for the share of employment in firms

[6] Also note that the 90% confidence interval of the effect in column 8 of Table A.17 includes the baseline estimate from column 2 of Table 4 of the manuscript.

[7] To keep in line with the timing of our regression model, where we lag the weak bank indicator by one year and the reforms themselves by two years, the calculation of the average takes into account

connected to weak banks in the years after reform implementation. This allows us to compare the increase in employment implied by the baseline regression estimates with the hypothetical scenario of employment growth in the case all firms were connected to a strong bank.

While the point estimates of our paper imply a level shift 8.5% for firms connected to strong banks and a level shift of only 4.8% for firms connected to weak banks, we prefer to use the estimates implied by the confidence intervals to obtain a range estimate that adequately reflects the uncertainty surrounding our estimates.[8] To obtain the employment creation effect of the baseline estimate, we multiply our country-level proxy for the share of firms connected to weak banks with the corresponding estimate for the level shift in employment and multiply one minus this share the corresponding estimate for firms connected to strong banks. The difference to the hypothetical scenario without any weak bank connections equals the employment growth that was lost due to weak banks.[9] The resulting ranges are 0.26 to 0.59 percentage points for the German unemployment benefit reform of 2004, 0.89 to 2.05 percentage points for the Dutch reform of 2007, 1.93 to 4.44 percentage points for the Irish reform of 2011, and 2.18 to 5.02 percentage points for the Portuguese reform of 2012.

If we relate the above results to the total number of country-level employees in the year of reform implementation, we obtain the following ranges for employment that could have been created if all firms had been connected to strong banks: i) (approximately) 57,000 to 132,000 employees for the German unemployment benefit reform of 2004, ii) 42,000 to 97,000 employees for the Dutch reform of 2007, iii) 17,000 to 40,000 employees for the Irish reform of 2011, and iv) 53,000 to 122,000 employees for the Portuguese reform of 2012.[10]

the period ranging from the first year after reform implementation to the end of our sample period.

[8] The corresponding numbers implied by the confidence intervals are 1.4% vs. -0.9% for the lower bounds (i.t.o. absolute value) of the confidence intervals and 16.1% vs. 10.8% for the upper bounds.

[9] Precisely stated i.t.o. our regression model, it is the percentage point employment growth lost over the period from the year t+2 after the reform to the end of our sample period.

[10] For total employment, we only take into account the sectors included in our studies (for further details see Section). Naturally, our estimates also cannot be applied to the population of self-employed persons.

Table A.1: Definition of variables

Variable	Source	Definition
Firm-related variables:		
Ln(employees)	ORBIS	The natural logarithm of the total number of employees.
Leverage	ORBIS	Financial debt (e.g. loans and bonds) over total assets.
Cash	ORBIS	Cash over total assets.
Tangibility	ORBIS	Tangible fixed assets over total assets.
Return on assets	ORBIS	Net income over total assets.
WACC	ORBIS	Dividends and interests paid on debt over total assets.
Firm-bank variables:		
Bank size	Bankscope	Size of a given firm's main relationship bank. Size is based on the natural logarithm of a bank's total assets in m EUR of 2015.
Bank stress	Bankscope	Financial constraints faced by a given firm proxied by the health of its main relationship bank. The indicator is based on the principal component derived from the following bank-level variables:
Bank capitalization	Bankscope	Equity over total assets.
Bank non-performing loans	Bankscope	Non-performing loans over total loans.
Bank return on assets	Bankscope	Net income over total assets.
Bank Z-score	Bankscope	Equity and net income over SD(return on assets).
Bank liquidity	Bankscope	Liquid assets (securities, derivatives and loans and advances to banks) less deposits and short-term funding over total assets.
Weak bank	Bankscope	The bank stress indicator for a given firm-year is above the 75th percentile of the distribution of bank stress within the regression sample.
Country level variables:		
Real GDP growth	AMECO	Change in annual real GDP relative to previous year.
Recession	AMECO	Binary indicator equal to one if a country's GDP growth is negative in a certain year.
Sector growth	Eurostat	Change in annual real value added relative to previous year (for each sector in each country).
Regular employment reform	Duval et al. (2018)	Recursive indicator reflecting major reforms that change employment protection for regular workers. The indicator starts out with 0 and increases by one for liberalizing reforms while decreasing by one for tightening reforms.
Unemployment benefit reform	Duval et al. (2018)	Recursive indicator reflecting major reforms that change unemployment benefit replacement rates or the duration of unemployment benefits. The indicator starts out with 0 and increases by one for liberalizing reforms while decreasing by one for tightening reforms.
Temporary employment reform	Duval et al. (2018)	Recursive indicator reflecting major reforms that change the regulation of temporary employment. The indicator starts out with 0 and increases by one for liberalizing reforms while decreasing by one for tightening reforms.
Sector-related variables:		
EOS (Ciminelli)	Ciminelli et al. (2018)	Technological elasticity of substitution between labour and capital estimated through a production function approach. Ciminelli et al. (2018) estimate the elasticity of substitution through a production function approach and use industry-level data on output, inputs and productivity from the 2017 EU KLEMS database for 13 EU countries.

(Continued)

Table A.1: – *Continued*

EOS (Laeven)	Laeven et al. (2018)	Technological elasticity of substitution between labour and capital estimated through a production function approach. Laeven et al. (2018) et al use industry-level data on output, inputs and productivity from the US KLEMS database from 1947-2007.
EFD	Duygan-Bump et al. (2015)	External financial dependence (EFD). Duygan-Bump et al. (2015) calculate the EDF as the proportion of capital expenditure financed with external funds for mature Compustat firms from 1980-1996.
Durability	Bils et al. (2013)	Bils et al. (2013) create a measure of durability for the products sold by firms in each US industry based on life expectancy tables from insurance companies and estimates from the US Bureau of Economic Analysis.
Workforce adjustment prop.	Ciminelli et al. (2018)	Technology-driven propensity to adjust the workforce estimated based on US data from 2011-2013. Ciminelli et al. (2018) compute their measure using individual-level data from the US Displaced Workers Survey relating the number of laid-off workers in a particular industry to total wage and salary employment.

Table A.2: Orbis coverage

This table shows the employment coverage (in %) of our ORBIS dataset relative to Eurostat. Employment numbers from ORBIS are based on the total number of employees reported by the firms in our sample while Eurostat numbers are based on national account data on total employees in the sectors included in our sample.

Country	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
AT	12	14	14	14	17	11	12	13	18	35	54	55	60	66
DE	23	20	21	22	29	38	39	40	43	44	51	59	62	62
ES	62	68	68	68	69	68	65	65	67	67	66	67	68	68
FR	84	52	59	57	52	49	49	42	41	46	43	38	46	51
GR	33	41	42	43	43	42	41	40	45	45	46	45	45	48
IE	11	25	39	77	94	140	154	159	171	171	179	193	195	276
LU	32	139	95	81	104	113	199	190	205	204	207	187	199	192
NL	73	83	88	99	101	102	87	79	111	110	120	123	122	120
PT	14	6	1	1	1	83	87	89	89	87	88	89	89	89

Table A.3: Bank stress

This table shows the evolution of our bank stress indicator. It reflects the financial constraints faced by the firms in our sample as proxied by the health of their main relationship bank. The bank stress indicator captures the principal component of a bank's capitalization, non-performing loans, return on assets, Z-score and liquidity. Note that this table shows the variation of the continuous bank stress measure rather than the dichotomized weak bank indicator. Refer to Appendix Table A.1 for variable definitions.

Country	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
AT	-0.06	-0.05	-0.03	-0.10	-0.11	-0.23	-0.16	-0.09	0.02	0.05	0.04	-0.02	0.03	-0.05
DE	-0.43	-0.39	-0.34	-0.28	-0.20	-0.26	-0.24	-0.25	-0.21	-0.19	-0.00	-0.02	-0.02	-0.03
ES	-0.01	-0.02	-0.04	-0.19	0.10	0.04	-0.01	0.03	-0.27	-0.23	-0.17	0.03	0.08	-0.02
FR	0.13	0.15	0.13	0.12	0.03	-0.01	0.01	-0.08	-0.11	-0.10	-0.09	-0.10	-0.10	-0.12
GR	-0.07	-0.00	-0.02	-0.10	-0.03	-0.15	-0.18	-0.03	-0.00	0.13	1.13	0.92	0.58	0.83
IE	-0.16	-0.13	-0.13	-0.21	-0.29	-0.35	-0.26	-0.10	0.03	0.32	0.49	0.64	0.84	0.51
LU	NA	NA	NA	-0.41	-0.44	-0.48	-0.29	-0.32	-0.37	-0.42	0.14	-0.17	-0.18	-0.20
NL	-0.15	-0.07	-0.10	-0.11	-0.20	-0.25	-0.22	-0.11	-0.24	-0.17	0.06	0.21	0.36	0.33
PT	-0.01	-0.01	-0.03	-0.04	-0.08	-0.07	-0.09	-0.05	-0.17	-0.14	-0.04	0.16	0.36	0.47

Table A.4: Regular employment protection reforms

This table shows the evolution of the recursive reform indicator for regular employment protection reforms within our sample. The indicator jumps by one for (liberalizing) reforms and decreases by one for counter-reforms. (Note that this indicator only reflects changes but not the level of employment protection for regular workers.)

Country	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
AT	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1
DE	0	0	0	0	0	1	1	1	1	1	1	1	1	1	1
ES	0	0	0	1	1	1	1	1	1	1	1	1	2	2	3
FR	0	0	0	0	-1	-1	-1	-1	-1	-1	0	0	0	0	0
GR	0	0	0	0	0	0	0	0	0	0	0	0	1	2	2
IE	0	0	0	0	0	0	0	1	1	1	1	1	1	0	0
LU	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
NL	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
PT	NA	0	0	0	0	1	1	1	1	1	1	2	2	2	2

Table A.5: Unemployment benefit scheme reforms
This table shows the evolution of the recursive reform indicator for reforms of unemployment benefit schemes within our sample. The indicator jumps by one for (liberalizing) reforms and decreases by one for counter-reforms. (Note that this indicator only reflects changes and not level of the generosity of unemployment benefit schemes.)

Country	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
AT	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
DE	0	0	0	0	0	0	1	1	1	1	1	1	1	1	1
ES	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
FR	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
GR	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
IE	0	0	0	0	0	0	0	0	0	0	0	0	1	1	1
LU	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
NL	0	0	0	0	0	0	0	0	1	1	1	1	1	1	1
PT	NA	0	0	0	0	0	0	0	0	0	0	0	0	1	1

Table A.6: Temporary employment reforms

This table shows the evolution of the recursive reform indicator for temporary employment reforms within our sample. The indicator jumps by one for (liberalizing) reforms and decreases by one for counter-reforms. (Note that this indicator only reflects changes but not the level of temporary employment regulation.)

Country	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
AT	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
DE	0	0	0	1	2	2	2	2	2	2	2	2	2	1	1
ES	0	0	0	0	0	0	0	0	0	0	0	0	1	0	1
FR	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
GR	0	0	0	0	0	1	1	1	1	1	1	1	2	2	2
IE	0	0	0	0	0	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
LU	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
NL	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
PT	NA	0	0	-1	-1	0	0	0	0	1	1	1	1	1	1

Table A.7: Robustness: Sectors where labour and capital are substitutes
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank, time-invariant sector-level measures and the three recursive reform indicators. In columns 1 and 2, EOS relates to the elasticity of substitution between labour and capital estimated by Ciminelli et al. (2018). In columns 3 and 4 we use the alternative estimates by Laeven et al. (2018). Columns 1 and 3 exclude and columns 2 and 4 only include firms from sectors with an EOS above one. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)			
	(1)	(2)	(3)	(4)
	$EOS_{Cim} < 1$	$EOS_{Cim} \geq 1$	$EOS_{Laev} < 1$	$EOS_{Laev} \geq 1$
Regular empl. ref. × weak bank	-0.001	-0.009*	-0.002	0.000
	(-0.68)	(-1.87)	(-1.38)	(0.03)
Unempl. benefit ref. × weak bank	-0.016***	-0.033	-0.016***	-0.028
	(-2.58)	(-1.49)	(-2.65)	(-1.24)
Weak bank	0.001	-0.002	0.001	-0.003
	(0.26)	(-0.22)	(0.24)	(-0.49)
Temporary empl. ref. × weak bank	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Bank size	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Sector-year FE	No	No	No	No
Country-sector-year FE	Yes	Yes	Yes	Yes
Observations	1,590,016	172,217	1,621,997	140,236
Adjusted R^2	0.940	0.929	0.938	0.942
Number of firms	292,430	33,738	300,502	25,666
Number of banks	1,034	671	1,059	585
Mean dep. var.	2.322	2.092	2.260	2.757
Clustering	Firm	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014	2001-2014

Table A.8: Robustness: Countries with over-coverage or uneven coverage; young vs. old firms
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. In column 1 we exclude Luxembourg, Ireland and the Netherlands and in column 2 we exclude employment data from Portugal prior to 2006. In column 3 we only include firms less than 25 years old while we only look at firms older than 25 years in column 4. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)			
	(1)	(2)	(3)	(4)
	Excl. LU,IE,NL	Excl. PT pre-2006	Age<25	Age>=25
Regular empl. ref. × weak bank	-0.001	-0.001	-0.003*	-0.000
	(-0.56)	(-0.59)	(-1.94)	(-0.24)
Unempl. benefit ref. × weak bank	-0.023***	-0.023***	-0.017**	-0.017*
	(-4.14)	(-3.93)	(-2.27)	(-1.89)
Weak bank	-0.001	-0.001	0.002	-0.002
	(-0.63)	(-0.56)	(0.91)	(-0.56)
Temporary empl. ref. × weak bank	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Bank size	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Sector-year FE	No	No	No	No
Country-sector-year FE	Yes	Yes	Yes	Yes
Observations	2,044,823	2,069,640	1,481,856	556,692
Adjusted R^2	0.934	0.935	0.927	0.956
Number of firms	381,108	387,104	300,933	114,284
Number of banks	1,096	1,116	1,037	806
Mean dep. var.	2.258	2.272	2.103	2.709
Clustering	Firm	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014	2001-2014

Table A.9: Robustness: Weighted average cost of capital

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × weak bank	-0.001
	(-0.72)
Unempl. benefit ref. × weak bank	-0.026***
	(-3.75)
Weak bank	-0.001
	(-0.73)
WACC	-0.007
	(-0.14)
Temporary empl. ref. × weak bank	Yes
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	1,772,610
Adjusted R^2	0.934
Number of firms	344,486
Number of banks	1,097
Mean dep. var.	2.403
Clustering	Firm
Sample period	2001-2014

Table A.10: Robustness: Excluding control variables
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables only include the weak bank indicator reflecting the health of a firm's main bank and its interaction with the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × weak bank	-0.002*
	(-1.85)
Unempl. benefit ref. × weak bank	-0.012***
	(-2.67)
Weak bank	-0.001
	(-0.59)
Temporary empl. ref. × weak bank	Yes
Firm controls	No
Bank size	No
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	2,419,003
Adjusted R^2	0.934
Number of firms	503,155
Number of banks	1,351
Mean dep. var.	2.213
Clustering	Firm
Sample period	2001-2014

Table A.11: Robustness: Country-level clustering

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of a firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × weak bank	-0.001
	(-0.44)
Unempl. benefit ref. × weak bank	-0.023***
	(-7.16)
Weak bank	-0.001
	(-0.36)
Temporary empl. ref. × weak bank	Yes
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	2,070,262
Adjusted R^2	0.935
Number of firms	387,113
Number of banks	1,116
Mean dep. var.	2.272
Clustering	Country
Sample period	2001-2014

Table A.12: Robustness: Lagging reforms by one year only
This table reports estimates of equation 2. The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year including, in this robustness check, the reform indicators. Standard errors are clustered as indicated below. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × weak bank	-0.000
	(-0.20)
Unempl. benefit ref. × weak bank	-0.022***
	(-5.19)
Weak bank	-0.003
	(-1.56)
Temporary empl. ref. × weak bank	Yes
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	2,310,709
Adjusted R^2	0.930
Number of firms	410,796
Number of banks	1,132
Mean dep. var.	2.253
Clustering	Firm
Sample period	2001-2014

Table A.13: Robustness: Continuous bank stress

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × bank stress	0.000
	(0.15)
Unempl. benefit ref. × bank stress	-0.050***
	(-6.34)
Bank stress	-0.002
	(-0.53)
Temporary empl. ref. × bank stress	Yes
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	2,070,262
Adjusted R^2	0.935
Number of firms	387,113
Number of banks	1,116
Mean dep. var.	2.272
Clustering	Firm
Sample period	2001-2014

Table A.14: Robustness: Three categories of bank stress
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheets of each firm's main bank and the three recursive reform indicators. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)
	(1)
Regular empl. ref. × medium stress	-0.002
	(-1.13)
Regular empl. ref. × high stress	-0.002
	(-0.96)
Unempl. benefit ref. × medium stress	-0.022
	(-1.19)
Unempl. benefit ref. × high stress	-0.048**
	(-2.51)
Medium stress	0.009***
	(3.61)
High stress	0.011***
	(4.12)
Temporary empl. ref. × medium stress	Yes
Temporary empl. ref. × high stress	Yes
Firm controls	Yes
Bank size	Yes
Firm FE	Yes
Sector-year FE	No
Country-sector-year FE	Yes
Observations	2,070,262
Adjusted R^2	0.935
Number of firms	387,113
Number of banks	1,116
Mean dep. var.	2.272
Clustering	Firm
Sample period	2001-2014

Table A.15: Robustness: Excluding multi-bank firms
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. In column 1, we use a binary weak bank indicator that is re-defined based on the new regression sample using only single-bank firms. In column 2, we use the same subsample but the continuous bank stress indicator that is also used in Appendix Table A.13. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. t-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)	
	(1)	(2)
	Excl. multi-bank firms	
Regular empl. ref. × weak bank (new)	0.001	
	(0.59)	
Regular empl. ref. × bank stress		0.004
		(1.37)
Unempl. benefit ref. × weak bank (new)	0.002	
	(0.44)	
Unempl. benefit ref. × bank stress		-0.027***
		(-2.93)
Weak bank (new)	-0.001	
	(-0.31)	
Bank stress		-0.003
		(-0.81)
Temporary empl. ref. × weak bank (new)	Yes	
Temporary empl. ref. × bank stress		Yes
Firm controls	Yes	Yes
Bank size	Yes	Yes
Firm FE	Yes	Yes
Sector-year FE	No	No
Country-sector-year FE	Yes	Yes
Observations	1,239,891	1,239,891
Adjusted R^2	0.943	0.943
Number of firms	249,345	249,345
Number of banks	1,086	1,086
Mean dep. var.	2.083	2.083
Clustering	Firm	Firm
Sample period	2001-2014	2001-2014

Table A.16: Robustness: Propensity to adjust the workforce
This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. In column 1, we include firms with a time-invariant, sector-specific propensity to adjust the workforce below the 75th percentile within the regression sample. In column 2, we include those firms with sector-specific values above the 75th percentile. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)	
	(1)	(2)
	Low propensity	High propensity
Regular empl. ref. × weak bank	-0.001	0.001
	(-1.03)	(0.35)
Unempl. benefit ref. × weak bank	-0.018***	-0.034***
	(-2.77)	(-2.84)
Weak bank	-0.001	-0.001
	(-0.48)	(-0.30)
Temporary empl. ref. × weak bank	Yes	Yes
Firm controls	Yes	Yes
Bank size	Yes	Yes
Firm FE	Yes	Yes
Sector-year FE	No	No
Country-sector-year FE	Yes	Yes
Observations	1,395,125	675,137
Adjusted R^2	0.940	0.927
Number of firms	257,303	129,810
Number of banks	1,033	893
Mean dep. var.	2.256	2.306
Clustering	Firm	Firm
Sample period	2001-2014	2001-2014

Table A.17: Robustness: Excluding each of the four unemployment benefit reforms

This table reports estimates of equation (2). The sample covers the period 2001-2014 and has a firm-year structure. The dependent variable is the log of employment. The explanatory variables include firm-level balance sheet information, time-varying information on the balance sheet of each firm's main bank and the three recursive reform indicators. In columns 1 and 2 we exclude observations from Germany from 2005 onwards. In columns 3 and 4 we exclude observations from the Netherlands from 2007 onwards. In columns 5 and 6 we exclude observations from Ireland from 2011 onwards and in columns 7 and 8 we exclude observations from Portugal from 2012 onwards. All independent variables are lagged by one year except for the reform indicators which are lagged by two years. Standard errors are clustered as indicated below. *t*-statistics are reported in parentheses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Ln(employment)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Regular empl. reform	0.004***		0.004***		0.004***		-0.002	
	(3.11)		(2.71)		(3.11)		(-1.52)	
Regular empl. ref. × weak bank	-0.013***	-0.001	-0.012***	-0.001	-0.013***	-0.001	-0.004***	0.000
	(-11.08)	(-0.43)	(-10.99)	(-0.59)	(-11.27)	(-0.55)	(-3.60)	(0.31)
Unemployment benefit reform	0.111		0.076***		0.022		0.104	
	(0.56)		(3.67)		(0.11)		(0.52)	
Unempl. benefit ref. × weak bank	-0.035***	-0.024***	-0.034***	-0.023***	-0.040***	-0.024***	-0.032*	-0.006
	(-5.68)	(-3.99)	(-5.80)	(-3.91)	(-6.76)	(-4.16)	(-1.67)	(-0.29)
Weak bank	-0.011***	-0.001	-0.011***	-0.001	-0.013***	-0.001	-0.007***	-0.000
	(-6.71)	(-0.61)	(-6.73)	(-0.62)	(-7.49)	(-0.62)	(-4.40)	(-0.05)
Temporary empl. reform	Yes		Yes		Yes		Yes	
Temporary empl. ref. × weak bank	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector and GDP growth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reform interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank size	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year FE	Yes	No	Yes	No	Yes	No	Yes	No
Country-sector-year FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2,050,914	2,047,007	2,074,689	2,070,006	2,064,055	2,059,421	1,887,504	1,882,666
Adjusted R^2	0.934	0.935	0.934	0.935	0.934	0.935	0.936	0.937
Number of firms	377,386	376,804	387,808	387,041	386,618	385,803	384,167	383,359
Number of banks	357	353	1,114	1,111	1,119	1,114	1,081	1,076
Mean dep. var.	2.264	2.261	2.276	2.272	2.272	2.267	2.321	2.317
Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Sample period	2001-2014	2001-2014	2001-2014	2001-2014	2001-2014	2001-2014	2001-2014	2001-2014

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Paper 3:

Marginal Returns to Talent for Material Risk Takers in Banking

Marginal Returns to Talent for Material Risk Takers in Banking*†

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Abstract

Economies of scale explain compensation differentials over time, across firms of different size, different hierarchy-levels, and different industries. Consequently, the most talented individuals match with the largest firms in industries where marginal returns to talent are greatest. We explore a new dimension of this size-pay nexus by showing that marginal returns also differ across activities within firms and industries. Using hand-collected compensation data on European banks, we find that the size-pay nexus is strongest for investment bankers and for banks with market-based business models. Thus, managerial compensation is most sensitive to size increases for activities that are easily scaled up.

JEL Classification: G21, G24, G34

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1 Introduction

Economies of scale are a central concept in economics. Rosen (1981) coined the term superstar economics to capture how two similarly high-skilled individuals earn vastly different fortunes, depending on the circumstances under which they put their talent to use. For a very long time, the finance literature has focused on a specific group of superstars, namely top-managers and CEOs of corporations. Intuitively, the impact of talented top managers will increase with the resources at hand. For instance, a smart financing strategy that allows for a decrease in capital costs has a larger absolute effect when implemented in a larger corporation.

Economies of scale can explain CEO compensation differentials across firms and over time. More recent evidence supports that scalability of talent also relates to cross-sector and cross-hierarchy differences in pay. The central contribution of our paper is to document that even within a sector with high returns to talent, the nature of tasks can explain compensation differences within firms and across business models.

The group of firms we chose as a laboratory for this endeavor are European banks. This group is of special interest to policymakers and scholars alike. First, it is a sector from which we know that high excess returns to talent can be attained (Philippon and Reshef, 2012). Second, several scholars have pointed out how excessive compensation could have lead to excessive risk-taking in the run-up to the financial crisis. Consequently, understanding compensation of bankers has been the focus of numerous studies (e.g. Bhagat and Bolton, 2014; Efing, Hau, Kampkötter, and Steinbrecher, 2015). We show that the compensation of material risk takers (MRTs), which is a group of managers in European banks that is much broader than just the group of executives, depends on the activities of the business unit they are located in. To that end, we use hand-collected data on MRTs' compensation across bank business units. We collected this data from reports mandated by CRD IV disclosure rules, which were implemented in 2014.

We find that total remuneration of MRTs in investment banking business units is much more sensitive to the size of the business unit than in retail banking and business units with supportive functions. On average, we find that for each percentage point increase in relative business unit size, investment bankers earn 1% more. We argue that the underlying factor explaining these differences is heterogeneity across business units in marginal returns to talent. According to Gabaix and Landier (2008), marginal returns to talent capture how strongly the effect of talent on project size translates into increasing firm profits. We hypothesize that retail banking exhibits relatively low marginal returns to talent relative to investment banking. Even the most talented retail banker has limited impact when giving out a loan and will mostly rely on standardized credit scoring models when deciding on whether or not to grant the loan. In contrast, the occasional failures of single traders causing huge losses are an example of the tremendous impact individual investment bankers can have on their banks' performance. More generally, an exceptionally talented investment banker can easily scale up the proceeds from her ingenious asset allocation, successful trading strategy, or savvy in closing M&A deals by tailoring her approach to the needs of the specific customer and the circumstances of the specific transaction.

We go on to show that these differences in marginal returns to talent across business units also matter for the prevalence of performance pay. Célérier and Vallée (2019) argue that marginal returns to talent should determine both total compensation and the degree of variable pay. We document that the ratio of variable-to-fixed compensation exhibits the same dynamics as total compensation regarding the relationship between size and pay in different business units. More specifically, we document that for each percentage point increase in relative business unit size, the ratio of variable-to-fixed compensation of investment bankers increases by 0.5% .

Our second contribution is to show that differences in marginal returns to talent do not only matter across different bank business units, but also across banks with different business models. We understand a bank's business model as the specific mix of activities a bank engages in. Our central business model measure compares the distribution of MRTs across the two opposite poles of a bank's range of activities, namely retail banking and investment banking, which represent traditional and non-traditional banking, respectively. We classify banks as market-focused if the ratio of MRTs in investment banking to retail banking is in the top quartile of the distribution. Using this approach, we show that MRTs in investment banking earn significantly more if they work in a market-focused bank. Investment bankers on average earn one third more in terms of total pay when their bank is market-focused, while the variable-to-fixed ratio is about 12% higher in such banks.

While the focus of our business model analysis is on the mix of activities, and here, especially on the specialization of banks, we also capture heterogeneity in the inner workings of a bank. To that end, we sum up all the MRTs in overhead, i.e., supportive functions, and relate them to the number of MRTs in the bank's profit centers, i.e., retail and investment banking. We classify a bank as low overhead if this ratio is below the sample median. We hypothesize that banks with low overhead tend to err on the side of growth in the trade-off between growth and safety, which is the central dichotomy in the model by Song and Thakor (2019) of bank culture. In the following, we use bank business model and bank culture synonymously since we regard them as two sides of the same coin. Indeed, we find that investment bankers earn even more in a market-focused bank if it is also characterized by low overhead. In the sense that low overhead can be regarded as a low degree of oversight and low bureaucratization, this result can be reconciled with a view of marginal returns to talent being higher in a setting, where talented bankers are less constrained in the scope of their actions.

Our third contribution can be regarded as a distilled version of the previous two tests. Presumably, marginal returns to talent play the greatest role among the high earners in a bank. The disclosure rules of CRD IV define high earners as those employees that earn more than EUR 1 mln. a year. If the type of activities are as important as we deem them to be, we expect to be able to explain variation in the number of income millionaires and their compensation with our business model classification. Indeed, we find that even after controlling for bank size, a bank's focus on market-based finance is a significant determinant of high earner compensation.

Our paper contributes to two different strands of the literature. First, it relates to the literature using economies of scale to solve two distinct but related puzzles in the literature on managerial compensation. The first puzzle is the marked increase in executive pay since the mid-1980s. The second one is why this increase has been especially pronounced in the finance industry. Building on the idea of concept of superstar economics by Rosen (1981), Gabaix and Landier (2008) point to the increase in firm size and the tight relationship between size and compensation as the central explanatory factor for the increase in CEO pay. They show how the marginal returns to talent for skilled CEOs are higher in larger firms, which leads to the most talented CEOs matching with the largest firms. This size-pay nexus can also be used to explain compensation differentials within firms, namely between employees at different hierarchy levels (Mueller, Ouimet, and Simintzi, 2017). Marginal returns to talent have also been employed to explain why top managers seem to earn a premium in the finance industry. Philippon and Reshef (2012) find that this premium has emerged only after the wave of deregulation in the mid 1980s. In the decades before, tight regulation had inhibited managers' scope of action and thus rendered differences in talent largely irrelevant. Célérier and Vallée (2019) argue that in addition to regulation, the immaterial nature of banks' input differentiates marginal returns to talent in finance from industries, where operations cannot be scaled up as easily. Our contribution is to document that marginal returns to talent do not only differ across firms, time, hierarchies, and industries but also across different types of activities as proxied by different bank business units.

Our analysis also relates to the literature on bank business models and in particular to the literature connecting business models and compensation. Song and Thakor (2019) devise a theoretical model of bank culture and show that manager incentive contracting serves to match managers and banks with similar preferences regarding the trade-off between safety and growth. Barth and Mansouri (2018) and Hagendorff, Saunders, Steffen, and Vallascas (2018) show empirically how differences in risk taking and incentive compensation can be explained via bank culture and idiosyncratic manager effects, respectively. Beyond the papers explicitly taking into account compensation, a host of papers uses a combination of various observables to cluster banks into distinct business models: funding and trading activity (Roengpitya, Tarashev, and Tsatsaronis, 2014), sources of income, funding, and activities (ECB, 2016), retail-focus and degree of diversification (Mergaerts and Vander Vennet, 2016), balance sheet composition and performance (Farnè and Vouldis, 2017), size, complexity, activities, geographic reach, funding, and ownership structure (Lucas, Schaumburg, and Schwaab, 2019). We contribute to this literature by using a new business model characterization based on the number of MRTs employed in different business units. This way we can explain variation in managerial compensation practices below the CEO-level, likely emanating from different marginal returns to talent for different types of activities.

2 Institutional setting

Bank compensation has been under intense regulatory scrutiny in the post-crisis years, which has resulted in a stream of regulations. Implementing the recommendations of the Financial Stability Board (FSB), the EU introduced the European Capital Requirements Directive (CRD) III in 2010. It regulates, among others, the minimum deferral of variable pay of bankers to better align risk-taking incentives with long-term performance.[1] The new directive was supposed to regulate the pay of all *staff whose professional activities have a material impact on the risk profile of credit institutions*, commonly referred to as *identified staff* or *material risk takers* (MRTs).

In 2013, the EU complemented the CRD III with a new directive, the CRD IV,

[1]Directive 2010/76/EU came into effect in 2011.

and an accompanying regulation, the Capital Requirements Regulation (CRR).[2] In the CRD IV, the EU introduced the so called bonus-cap which limited the ratio of allowed variable to fixed compensation for all MRTs (Colonnello, Koetter, and Wagner, 2020). Importantly for our purposes, the new set of regulations also required banks to disclose the number of MRTs and their total, fixed, and variable compensation at the aggregate level, split by business areas. In addition, banks have to disclose the number of high earners, i.e., employees earning above EUR 1 million, by payment bands of EUR 500,000. Banks have to identify MRTs based on qualitative criteria such as an employee's position (e.g. as a member of the management body or as the head of a material business unit) or the size of the loan portfolio under management by the employee and based on quantitative criteria such as the employee's total remuneration.[3]

3 Marginal returns to talent

The impact of managerial skills increases with the resources available in the situation where skills are put to use. Consequently, more skilled CEOs match with larger firms where they earn more as their marginal returns to talent are higher (Gabaix and Landier, 2008).

To structure our discussion on how the size-pay nexus varies across different activities within the finance industry, we follow the formalization of the mechanics of the size-pay nexus as presented by Célérier and Vallée (2019). Here, the firm's target function is described as:

$$T \times S^{\alpha} - S - w(T) \ , \tag{1}$$

where S is project size and $w(T)$ is the wage for a worker of talent T. The parameter α determines marginal returns to a manager's talent. Under the assumption of perfect competition at the labor demand side, firms compete for talented workers and workers have full bargaining power. Optimizing over project size S, the resulting wage takes

[2]Directive 2013/36/EU and Regulation 575/2013 both came into effect in 2014 and are commonly referred to collectively. Henceforth, we will adopt the common practice and refer to both regulations as the CRD IV.

[3]These criteria were specified in the Commission Delegated Regulation 604/2014, which in turn implemented recommendations from a technical document by the European Banking Authority (EBA), the EBA Regulatory Technical Standards 2013/11.

the form

$$w(T) = T \times S_T^{*\alpha} - S_T^* \ ,$$
$$\text{or} \quad w(T) = T^{\frac{1}{1-\alpha}} \alpha^{\frac{1}{1-\alpha}} (1-\alpha) \ . \tag{2}$$

From equation (2) we can see that marginal returns to talent are positive. Consequently, more skilled workers, i.e., those with higher values of T, earn higher wages. In line with Gabaix and Landier (2008), more skilled individuals match with occupations related to larger projects, i.e., larger values of S. The match between talent and size can ultimately be traced back to scale returns to talent, i.e., more skilled individuals will match with occupations with higher values of α.

Célérier and Vallée (2019) go on to assume that α varies across industries and that it is higher in the finance industry than in non-finance industries. Consequently, working in finance is rewarded with a premium based on higher returns to talent. We hypothesize that α does not only differ across industries but also within one industry across different activities. Thus, companies will value talent more when hiring workers in business units exhibiting higher returns to talent. At the same time, we conjecture that more skilled workers will select into business units with higher returns to talent.

In the context of the industry in our focus, i.e. the banking industry, we expect marginal returns to talent to MRTs to be higher in investment banking than in retail banking or overhead functions. Retail-banking is a low-margin activity generating fixed income streams. Profits are generated not from scaling up the activities of very talented individual retail bankers but rather by scaling up low-margin products like debit cards on a national or even international level. In contrast, individual talent plays a much larger role in the deal-oriented investment banking business. Here, a small number of very talented individuals can generate much higher returns to talent. For example, the same effort by a team of very talented investment bankers in M&A can generate vastly higher profits than a less talented team because the most talented M&A advisors attract clients with larger deal volumes, i.e., higher values of S. Hence, we expect more talented investment bankers to match with banks, where the investment banking business is more important, compared to other business units. Empirically, we would expect compensation to rise more strongly with increasing relative business unit size for material risk takers in investment banking units compared to other business units. This reflects higher marginal returns to talent, i.e., higher values of α in investment banking.

4 Business models

In the previous section, we laid out why the relationship between business unit size and material risk takers' pay should be stronger in investment banking across all banks. Still, the size-pay nexus for investment banking will not be the same across all banks. We expect that marginal returns to talent for investment bankers in banks with a particular focus on investment-banking should be even higher than in a bank with a similarly sized investment banking business unit but with a business model focused more on traditional banking such as retail banking. Grouping banks into different business models will thus help us to refine our analysis of heterogeneity in the strength of the size-pay nexus across banks and business units.

We define business models along the dimension of a bank's market focus. The two opposite poles regarding a bank's activities are investment banking, i.e., capital market-focused activities, and traditional retail banking (Gorton and Metrick, 2012). We determine a bank's market focus by relating the number of material risk takers in the investment banking business unit to the number of MRTs in retail banking. We consider banks in the middle of the domain, i.e, those with a less pronounced focus on either market-based or retail-based finance, as universal banks.[4]

While a bank's activities represent an outside view on its business model, we also want to use the inside view for our business model classification. To that end, we summarize all business units that are not the actual profit centers of a bank into an aggregate overhead business unit and compare the number of MRTs in overhead to the number of MRTs in the profit centers, i.e., retail- and investment banking. We assume that the relative weight of overhead functions like compliance, HR, and risk control reflects how much a bank relies on bureaucratization and control to rein in risk takers in profit centers and thus sheds light on a bank's self-positioning in the trade-off between safety and growth as described in Song and Thakor (2019). While we think that this is a reasonable assumption, we acknowledge that the weight of overhead functions could also to some degree reflect bank complexity, e.g. the complexity of a bank's corporate structure.

[4]Note that in our empirical analysis, we concentrate on either market- or retail focused banks. We do not estimate separate coefficients for universal banks as they constitute the reference group.

5 Empirical approach

5.1 Size-pay nexus across banks

In the first step of our analysis we investigate the relation between bank-size and the pay level of MRTs. In contrast to Gabaix and Landier (2008) who only look at CEOs, we analyze the compensation of below-CEO level employees, namely the MRTs. We implement this analysis running regressions of the following form:

$$y_{ijt} = \beta_1 s_{it} + \beta_2 s_{ijt} + \beta_3 n_{ijt} + \mathbf{1}\boldsymbol{f}_{it} + \epsilon_{ijt}, \tag{3}$$

where i, j, and t denote the bank, business unit, and year, respectively. Our MRT-level compensation measure, y_{ijt}, is the logarithm of the sum of total annual pay of all MRTs in a given business unit.[5] Our main independent variables are the size measures, s_{it} and s_{ijt}. We use the logarithm of a bank's total assets s_{it} to capture firm size. We complement the aggregate bank-level measure of firm size with a new measure of relative business unit size, s_{ijt}, which relates the number of MRTs in a given business unit to the total number of MRTs in the entire bank. By incorporating this measure into the analysis, we point out that it is not just the total size of a bank that determines pay-levels of employees, but also the relative importance of a business unit within a bank in which employees work. Like this, we prepare the ground for the second step of our analysis, which entails the analysis of heterogeneity in the size-pay nexus across different types of business units.

We argue that our MRT-based relative size indicator offers several advantages relative to measures based on bank financials or simple headcounts of all employees in a business unit. Our measure does not depend on the subjective process of identifying the accounting-based measure that most adequately reflects a business unit's size and it abstracts from non-essential employees, which do not necessarily inform on the relative importance of a business unit within a bank.

Since we are using the sum of total pay as a dependent variable, it is important to control for the (logarithm of) the absolute number of MRTs in a given business unit, n_{ijt}. Furthermore, we add different sets of fixed effects, f_{it}, which include time fixed effects, bank fixed effects, and business unit fixed effects. While bank fixed effects

[5] For cases where a bank does not report any information for one or more of the eight EBA business units, we assume that this business unit does in fact not exist in the given bank. When a given business unit comprises two EBA categories we split compensation and number of MRTs evenly across relevant EBA categories.

control for a bank's culture and business model, business unit fixed effects control for business-unit-specific compensation culture, e.g., general pay differences among MRTs in investment banking relative to MRTs in retail banking. Note that bank fixed effects encompass country fixed effects and thus control for unobserved time-invariant differences in bank compensation and reporting standards across countries.

5.2 Size-pay nexus across business units

We now turn to the heterogeneity of the size-pay nexus across business units. For this analysis, we aggregate the eight EBA business units to three business units to sharpen our analysis and to avoid overfitting. As we focus on key personnel below the management board, we exclude the EBA categories *management body in its supervisory function* and *management body in its management function*. These two categories do not constitute business units in the actual sense and their compensation is not comparable to the remaining business units.[6] Moreover, we exclude the business unit *asset management* due to the low number of banks within our sample, which have an asset management unit. Lastly, we summarize the business units *corporate functions*, *independent control function*, and the residual category *all other* in a new business unit, which we call overhead. As discussed in chapter 4, these business units do not represent a profit center but rather perform support and control functions. Thus, it is a natural choice to use the overhead business unit as the reference category in our regressions looking into heterogeneity across business units. We run regressions of the following form:

$$y_{ijt} = \boldsymbol{\beta}_1 \boldsymbol{b}_j + \beta_2 s_{ijt} + \boldsymbol{\beta}_3 \boldsymbol{b}_j s_{ijt} + n_{ijt} + \boldsymbol{\lambda} \boldsymbol{c}_{it} + \mathbf{1} \boldsymbol{f}_{it} + \epsilon_{ijt}, \quad (4)$$

where i, j, and t denote the bank, business unit, and year, respectively. In addition to the dependent variable from Equation (3), the logarithm of the total pay of all MRTs in a given business unit, we now also look at a measure of variable pay, namely the aggregate ratio of variable to fixed compensation for all MRTs in a business unit. The vector b_j comprises indicator variables for the three business units retail banking, investment banking, and overhead. Our main variable of interest is the interaction of the business unit indicators with our business-unit-specific size measure, s_{ijt}, which is defined by the ratio of MRTs in a business unit over the total number of MRTs in a

[6]For example, in some banks and jurisdictions MRTs in the *management body in it supervisory function* only receive attendance fees for supervisory meetings and no variable pay.

bank as described further above. The coefficients in $\boldsymbol{\beta}_3$ capture the heterogeneity in the size-pay nexus across business units. The strength of each coefficient provides a measure for the marginal returns to talent, γ, prevalent in the respective business unit. We hypothesize that γ will be largest for the investment banking business unit, where we except the highest marginal returns to talent as laid out in Section 3. We also expect marginal returns to talent to increase the degree of performance pay. Thus, β_3 should be also highest for investment banking when using the variable-to-fixed compensation ratio as the dependent variable.

The bank-specific size measure (the logarithm of total assets), s_{it}, from Equation (3) has been relegated to the vector of bank-control variables, $\boldsymbol{c}_{it}$, which also comprises the return on average assets and the cost-to-income ratio as measures of profitability and efficiency, respectively. Moreover, we keep on controlling for the logarithm of the number of MRTs in a each business unit, n_{ijt}, to prevent that our effects are driven by simple mechanical correlations.

5.3 Size-pay nexus across business models

We further investigate if heterogeneity in marginal returns to talent also emanates from bank business models. The degree to which a bank resorts to non-traditional banking is captured by our market focus indicator, which relates the number of MRTs in the investment banking business unit to the number of MRTs in the retail banking unit. We divide the indicator into three categories so that bank-years in the upper quartile and bank-years in the lower quartile represent a high and low degree of market focus, respectively. Banks that fall into the middle category can be thought of as universal banks, which have a more even distribution of MRTs across business units, reflecting a business model balanced between traditional and non-traditional banking.

While the market focus indicator captures the banks profit centers, we also want to analyze how a high degree of overhead affects the size-pay nexus. To that end, we relate the number of MRTs in the aggregate overhead business unit to the number of MRTs in investment banking and retail banking. We dichotomize our indicator by setting it equal to one if the overhead share is below the median within our sample. A low overhead share would reflect a low degree of bureaucratization and overhead and thus a bank that tends to prefer safety over growth. By controlling for the cost-to-income ratio we make sure that a low overhead share does not simply reflect a high degree of efficiency.

In our analysis of business models we exclude the business-unit specific size measures, s_{ijt}, to prevent collinearity with the bank-year specific business model indicators. Apart from that, we employ the control variables and fixed effects structure from Equation (4), which leads to the following regression equation:

$$y_{ijt} = \boldsymbol{\beta}_1 \boldsymbol{b}_j + \beta_2 bm_{it} + \boldsymbol{\beta}_3 \boldsymbol{b}_j bm_{it} + n_{ijt} + \boldsymbol{\lambda c}_{it} + \mathbf{1}\boldsymbol{f}_{it} + \epsilon_{ijt}, \tag{5}$$

where the bank-year level business model indicator is denoted bm_{it}. First, we run regressions with only one of two business model measures interacted with the business unit indicators and then we run combined regression, where the main variable of interest is the triple interaction of market focus, low overhead, and the respective business unit indicator, i.e., retail banking or investment banking.[7] We hypothesize that total and variable compensation is highest for banks with a high market-focus and low overhead corresponding to a situation, where marginal risk takers in the business unit with the highest marginal returns to talent, i.e., investment banking, are least restrained by bureaucracy and oversight.

5.4 Size-pay nexus and high earners

We now turn away from MRTs to the analysis of high earners, which are defined as income millionaires. While the data that is publicly available is at the bank-level and therefore does not allow us an analysis of heterogeneity across business units, the high earners provide an ideal testing ground for the relationship between the size-pay nexus and a bank's business model. We would expect that the most important determinant for the number of high earners is the degree of a bank's market focus. We therefore run regressions of the form:

$$y_{it} = \beta_1 bm_{it} + \beta_2 s_{it} + \boldsymbol{\lambda c}_{it} + \mathbf{1}\boldsymbol{f}_{it} + \epsilon_{it}, \tag{6}$$

where i and t denote bank and year, respectively. Our dependent variable is either the number of high earners or the total pay of all high earners within a bank. Given that there is less heterogeneity and a lower number of observations in a bank-level setting, we favor power over the ease of interpretation and use a continuous version of the categorical market focus indicator from the previous chapter. Our business model measure, bm_{it}, is thus simply the ratio of the number MRTs in investment banking over

[7] Recall that the aggregate overhead business unit serves as the reference category

the number of MRTs in retail banking. Our coefficient of interest is the strength of the connection between a bank's market focus and the number and pay of high earners, captured by β_1.

Note that in specification (6), we explicitly report coefficient estimates of bank size s_{it}. This allows us to directly relate the nexus between business model and pay to the size-pay nexus. We would expect that bank size has a positive impact on the number of high earners, i.e. a positive and significant coefficient estimate β_2. If the impact of a bank's business model is also meaningful for its pay policies, we would also expect a positive coefficient estimate for bank business model, i.e. a positive and significant coefficient estimate β_1.

6 Data and summary statistics

We hand-collect data on MRTs and high earners in European banks over the period 2014 to 2018. As discussed in Section 2, the beginning of our sample period is defined by the implementation of regulatory publication requirements on MRT pay in the CRR. We restrict our data collection effort to the sample of 124 banks that took part in the 2014 EBA stress test.[8]

According to EBA guidelines, banks have to split up the information on their MRTs by eight business areas: i) *the management body in its supervisory function*, ii) *the management body in its management function*, iii) *investment banking*, iv) *retail banking*, v) *asset management*, vi) *corporate functions* (such as HR and IT), vii) *independent control functions* (such as risk management, compliance and internal audit), and the residual category viii) *all others*.[9] Moreover, the EBA guidelines require banks to disclose the number of high earners according to bins of 500,000 EUR.

We find information on MRTs and high earners in a wide variety of report types, predominantly in annual reports, special reports on compensation practices, and CRR reports. Most institutions base their disclosure on MRTs and high earners on the EBA templates, as discussed in Section 2. Figure A.1 and Figure A.2 show an example of a table for disclosure on MRTs and a table for disclosure on high earners, respectively. In those cases, where the categories in the MRT-table do not perfectly match the official EBA nomenclature of the eight business units listed in Section 2, we hand-match them

[8]See https://eba.europa.eu/risk-analysis-and-data/eu-wide-stress-testing/2014 for the list of institutions included. Among this group of banks, we find at least some information on MRTs and high earners for 95 institutions.

[9]EBA guidelines EBA/GL/2014/08

to the closest EBA category.

Table 1 depicts summary statistics for a collapsed version of our main dataset, i.e., a bank-year panel. Here, each bank-year observation carries all the information of the associated business units. Balance sheet variables and MRT variables are winsorized at the 1st and 99th percentiles. Refer to Appendix Table A.1 for variable definitions. In Table 2, we split the sample of banks based on our business model measure capturing the degree of market focus. The univariate evidence points in the direction of the hypothesis developed in Section 5, i.e., banks with a high market focus exhibit higher average pay of MRTs in all business units but especially in investment banking. Moreover, we observe higher numbers of high earners in banks with a stronger market focus. However, the stark differences in total assets highlight the need for the multivariate regressions featured in the following section.

In addition to the non-parametric evidence on the role of bank business models, we provide visual evidence on the size-pay nexus across banks and business units. Figure 1 exhibits the cross-sectional size-pay nexus. Depending on the size of the bank, MRTs in all business units tend to earn more, which arguably reflects higher marginal returns to talent in larger banks in line with Gabaix and Landier (2008). Figure 2 provides visual evidence regarding our main hypothesis from Section 2. The relationship between the size of the business unit, as gauged by our MRT-based size measure, and compensation of MRTs is strongest for investment banking. Again, this arguably reflects relatively higher marginal returns to talent in business units related to investment banking.

7 Results

7.1 Size-pay nexus across banks and business units

We examine the well-established size-pay nexus by first looking at the classical measure of size, namely bank total assets. In columns 1-3 of Table 3, we document that MRTs in larger banks command a significantly higher total salary. Since our dependent variable is measured at the level of MRTs in a business unit rather than simply looking at CEO pay, our results also corroborate Mueller et al. (2017)'s result that differences in marginal returns to talent also determine compensation differences within a bank. In columns 4-6, we show that our MRT-based size measure captures variation in the size-pay nexus above and beyond total assets. For each percentage point increase in the relative size of a business unit, we find a roughly 0.6% increase in total compensation. In all columns, we control for the number of MRTs in each business unit to make sure that our results are not simply driven by the mechanical relationship between the number of MRTs and the total aggregate pay of MRTs in the respective business unit. Note that our results hold across different sets of fixed effects that either control for time-invariant compensation culture in business units, banks or for the combination of both.

Next, we turn to the analysis of heterogeneity across business units. To that end, we interact our MRT-specific size measure with business unit indicators for investment banking, retail banking, and the aggregate overhead business unit. In columns 1-3 of Table 4, we again look at total pay of MRTs in each business unit and find evidence for our central hypothesis regarding the importance of marginal returns to talent. MRTs in investment banking earn significantly more than MRTs in the reference category (overhead) across three specifications controlling for time-varying factors at the bank-level, the number of MRTs in a business unit, and time-invariant compensation cultures at the business-unit and bank-level. The coefficient in column 3 suggests that for each percentage point increase in the relative size of the investment banking unit, we find a roughly 1.5% increase in total compensation, while the same effect is only 0.5% for MRTs in the overhead business units (the reference category).

At the same time, we do not find an effect for retail banking, which arguably reflects lower marginal returns to talent associated with the activities conducted in that business unit. In columns 4-6 of Table 4, we look at the ratio of variable to fixed pay of MRTs in each business unit. While we do not find an effect in the specification with business unit fixed effects only, in the remaining two specifications we find a positive compensation differential for MRTs in investment banking and only a weak positive effect for MRTs in retail banking. The results in Table 4 suggest that indeed marginal returns to talent, or γ in the terminology of Equation 1, are highest in investment banking, which leads to positive compensation differentials of MRTs in investment banking business units regarding both total and variable-to-fixed compensation.

7.2 Size-pay nexus across business models

Now we turn to the analysis of bank business models and test to what extent compensation is not only determined by heterogeneity in activities across business units but also by differences in the specialization in activities and the positioning in the trade-off between growth and safety across banks.

In Table 5, we interact our first business model measure, which captures the degree of a bank's market focus by relating MRTs in investment banking to MRTs in retail banking, with the business unit indicators. In columns 1-3, we find that banks with a market focus in the top quartile of the distribution exhibit significantly higher pay for investment bankers relative to MRTs in overhead, while we do not find a similar effect for retail banking. When looking at variable-to fixed compensation, the picture becomes even starker. Here, we find a significant positive effect for MRTs in investment banking if they work in a bank with a high market focus, while the variable-to-fixed compensation ratio is significantly lower for MRTs in retail banking.

While in the previous analysis we took the outside view at a bank's specialization in activities, we now examine the inside view of a bank's business model. We compare banks with different degrees of bureaucracy and oversight, proxied by the ratio of MRTs in overhead business units to MRTs investment and retail banking. In Table 6, we show that MRTs in investment banking in banks with below median bureaucracy and oversight command higher pay. However, the results only hold for the case of total pay and in the specification with business unit fixed effects. Apparently, the inside view alone does not give us enough power to find compensation differentials.

This is why in Table 7, we combine the inside and the outside view on a bank's business model in a triple interaction regression. In columns 1-3, we find that retail bankers and to an even larger degree investment bankers earn more in terms of total pay in banks with low overhead. The effect is magnified in banks whose business model is both characterized by low overhead and a high degree of market focus. The additional effect only exists for investment bankers. This confirms our hypothesis that MRTs engaging in activities with high marginal returns to talent command even higher pay when they are less constrained by bureaucracy and oversight. This result does not extend to the case of variable-to-fixed pay in columns 3-6. We do, however, find that MRTs in retail banking earn less variable pay when a bank is market-focused. This suggests that the degree of bureaucracy and oversight does not play a large role for bonus payouts relative to the specialization of a bank.

7.3 Size-pay nexus and high earners

Our analysis of high earners can be regarded as a distillation of the tests we have conducted so far. Income millionaires are a natural choice for an examination of the relationship between marginal returns to talent and compensation. We hypothesize that the specialization in activities a bank engages in is the key factor in determining the distribution of income millionaires across banks. Specifically, we want to test whether our business model indicator capturing the degree of a bank's market focus is able to predict the number and compensation of income millionaires even after controlling for bank size. In Table 8, we use the continuous version of our market focus indicator and compare its effect on the number of high-earners and their total pay with the effect of bank size. In panel A, we look at total pay and find that our business-model indicator trumps the influence of bank size as soon as we control for both bank and time fixed effects. We find the same dynamics when looking at the total number of high earners in panel B.

8 Conclusion

Economies of scale determine compensation across firms of different size, across different hierarchy levels, and across different industries. We explore a new dimension of the interplay between marginal returns to talent, scale, and managers' compensation by documenting heterogeneity in returns to talent in one sector, i.e., the European banking industry, along the specific types of activities in which institutions engage. More specifically, we investigate if pay structure patterns are compatible with differences in marginal returns to talent across different business units and across different business models.

We make use of hand-collect data on compensation of material risk takers, which is available due to post-crisis disclosure requirements. These data comprise information on pay of managers not limited to top management, and are split by business units.

We document that within larger business units, employees receive higher pay. This effect is especially pronounced for investment bankers. Talented retail bankers have little leeway to scale up talent, as their business is highly standardized. In contrast, investment bankers regularly work in small teams handling specific investment products, trading strategies, or M&A deals. Here, a talented banker can have a much larger impact on outcomes. Consequently, the impact of a talented investment banker on a specific project is scaled up relatively more with increasing project size.

We go on to show that compensation also depends on the specialization of a bank. We classify banks into business models along two dimensions. On the one hand, we look at the degree of market focus of a bank. On the other hand, we consider the importance of supportive and controlling overhead functions. We find that investment bankers earn more in market-focused banks. Pay for investment bankers is even higher at market-focused banks when the importance of overhead functions is low and investment bankers are less restricted in their freedom of action. Furthermore, the degree of market focus is also the central determinant of the number of high earners, i.e, those with annual income of more than EUR 1 mln., at the bank-level. In summary, we show that differences in marginal returns to talent associated with different activities within the banking industry are an important driver of compensation patterns for managers below the CEO level.

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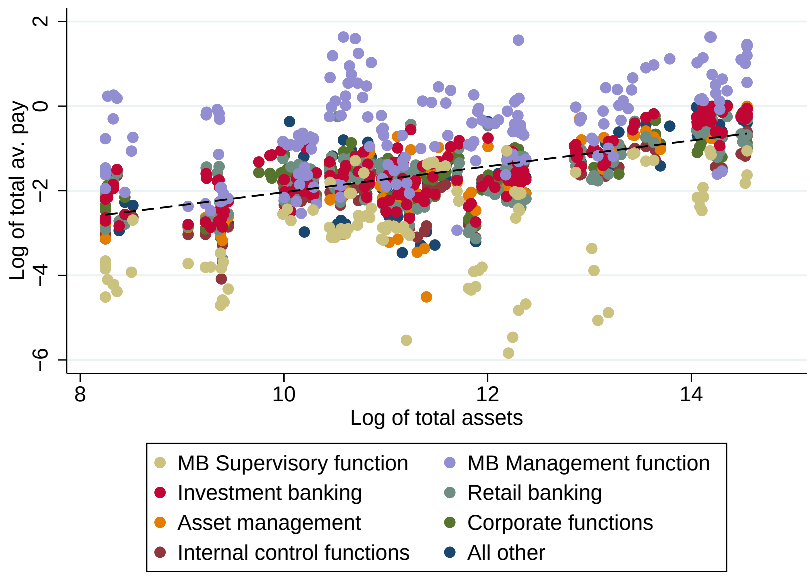

Figure 1: Size-pay nexus and bank size This figure visualizes the relationship between firm size, measured by the logarithm of total assets, and average compensation of MRTs in European banks over the period 2014 to 2018. Each dot represents the logarithm of total average pay of MRTs in a particular bank-year in one of the eight business units specified by the EBA. The black dashed line is a fitted regression line.

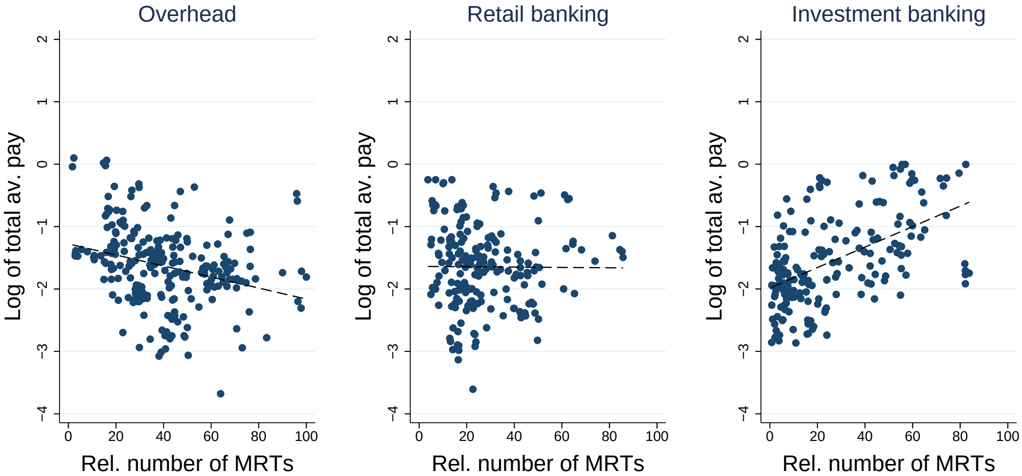

Figure 2: Size-pay nexus and business unit size This figure visualizes the relationship between business unit size and average compensation of MRTs in European banks over the period 2014 to 2018 in three different business units. Business unit size is proxied by the number of MRTs in each business unit relative to the total number of MRTs in the respective bank-year. Each dot represents the logarithm of total average pay of MRTs in a particular bank-year for the business units *overhead*, *retail banking*, and *investment banking*, respectively. The *overhead* business unit is an aggregate cateogry summarizing the business units *corporate functions*, *independent control function*, and the residual category *All Other*. The black dashed lines are fitted regression lines.

Table 1: Summary statistics

This table shows summary statistics for our European sample banks over the period 2014-2018. Both bank characteristics and business unit characteristics are reported at the bank-level. Refer to Appendix Table A.1 for variable definitions.

	N	Average	S.E.	p25	Median	p75
Bank characteristics						
Total assets (mln. EUR)	181	344,409.345	555,646.069	34,424.242	70,634.766	381,295.000
ROA (in %)	181	0.228	0.860	0.100	0.330	0.590
Cost-to-income ratio	181	0.649	0.197	0.553	0.622	0.716
Market-to-retail ratio	181	1.468	2.898	0.111	0.375	1.182
Overhead-to-profit-center ratio	181	0.358	0.177	0.229	0.332	0.467
MRT characteristics:						
Number of MRTs	181	436.729	609.121	74.000	158.000	534.000
Rel. BU size (overhead)	181	0.358	0.177	0.229	0.332	0.467
Rel. BU size (retail)	181	0.279	0.171	0.165	0.239	0.366
Rel. BU size (inv. banking)	181	0.188	0.208	0.030	0.098	0.283
Average pay of MRTs in inv. banking (mln. EUR)	181	0.248	0.253	0.083	0.167	0.305
Average pay of MRTs in retail (mln. EUR)	181	0.247	0.168	0.127	0.205	0.296
Average pay of MRTs in overhead (mln. EUR)	181	0.254	0.191	0.142	0.209	0.290
Total number of high earners	146	45.404	121.875	0.000	2.000	12.000
Total pay of high earners	146	83.176	230.153	0.000	3.750	21.500

Table 2: Summary statistics

This table shows summary statistics for our European sample banks over the period 2014-2018. Both bank characteristics and business unit characteristics are reported at the bank-level. Banks with market focus exhibit a market-to-retail ratio in the top quartile. Banks with retail focus exhibit a market-to-retail ratio in the bottom quartile. Refer to Appendix Table A.1 for variable definitions.

	Banks with market foucs				Banks with retail focus			
	N	Average	S.E.	Median	*N*	Average	S.E.	Median
Bank characteristics								
Total assets (mln. EUR)	46	911,091.430	723,478.530	545,479.000	46	73,338.307	123,253.520	37,525.342
ROA (in %)	46	0.153	0.401	0.260	46	0.228	0.706	0.360
Cost-to-income ratio	46	0.698	0.166	0.674	46	0.664	0.157	0.639
Market-to-retail ratio	46	4.857	4.187	3.315	46	0.023	0.037	0.000
Overhead-to-profit-center ratio	46	0.274	0.128	0.286	46	0.386	0.185	0.367
MRT characteristics:								
Number of MRTs	46	908.191	850.638	612.500	46	141.459	149.262	76.500
Rel. BU size (overhead)	46	0.274	0.128	0.286	46	0.386	0.185	0.367
Rel. BU size (retail)	46	0.146	0.071	0.138	46	0.377	0.224	0.295
Rel. BU size (inv. banking)	46	0.493	0.150	0.479	46	0.008	0.013	0.000
Average pay of MRTs in inv. banking (mln. EUR)	46	0.480	0.307	0.413	46	0.052	0.092	0.000
Average pay of MRTs in retail (mln. EUR)	46	0.359	0.190	0.329	46	0.194	0.078	0.192
Average pay of MRTs in overhead (mln. EUR)	46	0.351	0.224	0.275	46	0.196	0.086	0.203
Total number of high earners	43	126.349	196.723	20.000	33	2.242	3.437	0.000
Total pay of high earners (mln. EUR)	43	225.372	373.743	29.500	33	4.235	7.060	0.000

Table 3: Size-pay nexus for banks and business units
This table reports estimates from regressions of total pay of material risk takers (MRTs) on characteristics of banks and business units. The sample covers all business units for EU banks between 2014 and 2018 and has a bank-business unit-year structure. The independent variables are log(*BU size)*), which is the logarithm of the total number of MRTs by business unit, log(*Total assets*), which is the logarithm of total assets of a bank, and *Rel. BU size* (columns 4 to 6), which is the number of MRTs within a business unit over the total number of MRTs by bank. All columns include time fixed effects, columns 1, 3, 4, and 6 include business unit fixed effects and columns 3 and 6 include bank fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	log(Total pay)					
	(1)	(2)	(3)	(4)	(5)	(6)
log(BU size))	0.824***	0.850***	0.850***	0.764***	0.714***	0.771***
	(0.043)	(0.023)	(0.023)	(0.057)	(0.055)	(0.034)
log(Total assets)	0.356***	0.259	0.259	0.380***	0.250*	0.279*
	(0.047)	(0.157)	(0.157)	(0.053)	(0.136)	(0.147)
Rel. BU size (in %)				0.007**	0.007*	0.006***
				(0.003)	(0.004)	(0.002)
Time FE	X	X	X	X	X	X
Business unit FE	X		X	X		X
Bank FE		X	X		X	X
Mean(y)	1.592	1.592	1.592	1.592	1.592	1.592
S.D.(y)	1.732	1.732	1.732	1.732	1.732	1.732
R^2	0.872	0.936	0.936	0.876	0.849	0.938
N	1,086	1,086	1,086	1,086	1,086	1,086

Table 4: Size-pay nexus for retail vs. investment banking
This table reports estimates from regressions of total pay of material risk takers (MRTs) and the ratio of variable pay to fixed pay of MRTs on characteristics of banks and business units. The sample covers the business units overhead, retail banking, and investment banking for EU banks between 2014 and 2018 and has a bank-business unit-year structure. The independent variables are *Rel. BU size*, which is the number of MRTs within a business unit over the total number of MRTs by bank, *RB*, which is a dummy variable that takes the value of one if a business unit is related to retail banking, *IB*, which is a dummy variable that takes the value of one if a business unit is related to investment banking, and interactions of *Rel. BU size* and business unit indicators *RB* and *IB*. In all columns, we use log(*BU size*)), which is the logarithm of total number of MRTs by business unit, log(*Total assets*), which is the logarithm of total assets of a bank, *ROA* and *Cost-to-income ratio* as control variables. All columns include time fixed effects, columns 1, 3, 4, and 6 include business unit fixed effects and columns 3 and 6 include bank fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	log(Total pay)			log(Variable-to-fixed)		
	(1)	(2)	(3)	(4)	(5)	(6)
Rel. BU size	0.003	0.005***	0.005***	0.003	-0.000	-0.000
	(0.003)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
RB × Rel. BU size (in %)	0.007	-0.000	-0.000	-0.001	0.002*	0.002*
	(0.005)	(0.003)	(0.003)	(0.004)	(0.001)	(0.001)
IB × Rel. BU size (in %)	0.016***	0.010***	0.010***	0.004	0.005***	0.005***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)
Time FE	X	X	X	X	X	X
Business unit FE	X		X	X		X
Bank FE		X	X		X	X
Controls	X	X	X	X	X	X
Mean(y)	2.256	2.253	2.253	0.288	0.288	0.288
S.D.(y)	1.787	1.789	1.789	0.343	0.343	0.343
R^2	0.956	0.987	0.987	0.448	0.811	0.811
N	498	496	496	498	496	496

Table 5: Size-pay nexus for high vs. low market focus

This table reports estimates from regressions of total pay of material risk takers (MRTs) and the ratio of variable pay to fixed pay of MRTs on characteristics of banks and business units. The sample covers the business units overhead, retail banking, and investment banking for EU banks between 2014 and 2018 and has a bank-business unit-year structure. The independent variables are *Market-focus*, which is an indicator variable that takes the value of minus one if a bank's market-to-retail ratio is in the bottom quartile within our sample, one if a bank's market-to-retail ratio is in the top quartile within our sample and zero otherwise, *RB*, which is a dummy variable that takes the value of one if a business unit is related to retail banking, *IB*, which is a dummy variable that takes the value of one if a business unit is related to investment banking, and interactions of *Market-focus* and business unit indicators *RB* and *IB*. In all columns, we use log(*BU size*)), which is the logarithm of the total number of MRTs by business unit, log(*Total assets*), which is the logarithm of total assets of a bank, *ROA*, and *Cost-to-income ratio* as control variables. All columns include time fixed effects, columns 1, 3, 4, and 6 include business unit fixed effects and columns 3 and 6 include bank fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	log(Total pay)			log(Variable-to-fixed)		
	(1)	(2)	(3)	(4)	(5)	(6)
Market-focus	-0.106	-0.101*	-0.101*	-0.017	0.019	0.019
	(0.089)	(0.054)	(0.054)	(0.045)	(0.058)	(0.058)
RB		-0.041			0.005	
		(0.034)			(0.010)	
IB		-0.018			0.069***	
		(0.054)			(0.025)	
Market-focus × RB	-0.031	-0.024	-0.024	-0.015	-0.031**	-0.031**
	(0.051)	(0.041)	(0.041)	(0.018)	(0.014)	(0.014)
Market-focus × IB	0.610***	0.331***	0.331***	0.203***	0.118***	0.118***
	(0.128)	(0.069)	(0.069)	(0.056)	(0.034)	(0.034)
Time FE	X	X	X	X	X	X
Business unit FE	X		X	X		X
Bank FE		X	X		X	X
Controls	X	X	X	X	X	X
Mean(y)	2.234	2.234	2.234	0.281	0.281	0.281
S.D.(y)	1.830	1.830	1.830	0.337	0.337	0.337
R^2	0.952	0.986	0.986	0.474	0.792	0.792
N	442	442	442	442	442	442

Table 6: Size-pay nexus for low vs high overhead

This table reports estimates from regressions of total pay of material risk takers (MRTs) and the ratio of variable pay to fixed pay of MRTs on characteristics of banks and business units. The sample covers the business units overhead, retail banking, and investment banking for EU banks between 2014 and 2018 and has a bank-business unit-year structure. The independent variables are *Low overhead*, which is a dummy variable taking the value of one if a bank's overhead-to-profit-center ratio is below the median within our sample, *RB*, which is a dummy variable that takes the value of one if a business unit is related to retail banking, *IB*, which is a dummy variable that takes the value of one if a business unit is related to investment banking, and interactions of *Low overhead* and business unit indicators *RB* and *IB*. In all columns, we use log(*BU size)*), which is the logarithm of total number of MRTs by business unit, log(*Total assets*), which is the logarithm of total assets of a bank, *ROA*, and *Cost-to-income ratio* as control variables. All columns include time fixed effects, columns 1, 3, 4, and 6 include business unit fixed effects and columns 3 and 6 include bank fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	log(Total pay)			log(Variable-to-fixed)		
	(1)	(2)	(3)	(4)	(5)	(6)
Low overhead	0.123	-0.031	-0.031	0.026	0.050	0.050
	(0.122)	(0.083)	(0.083)	(0.059)	(0.037)	(0.037)
RB		-0.026			0.023	
		(0.047)			(0.014)	
IB		0.019			0.108***	
		(0.056)			(0.038)	
Low overhead × RB	0.088	0.031	0.031	-0.006	-0.010	-0.010
	(0.062)	(0.052)	(0.052)	(0.026)	(0.021)	(0.021)
Low overhead × IB	0.353**	0.149	0.149	0.048	0.010	0.010
	(0.138)	(0.099)	(0.099)	(0.060)	(0.047)	(0.047)
Time FE	X	X	X	X	X	X
Business unit FE	X		X	X		X
Bank FE		X	X		X	X
Controls	X	X	X	X	X	X
Mean(y)	2.219	2.219	2.219	0.286	0.286	0.286
S.D.(y)	1.789	1.789	1.789	0.330	0.330	0.330
R^2	0.947	0.984	0.984	0.426	0.787	0.787
N	478	478	478	478	478	478

Table 7: Size-pay nexus along market-focus and overhead dimensions
This table reports estimates from regressions of total pay of material risk takers (MRTs) and the ratio of variable pay to fixed pay of MRTs on characteristics of banks and business units. The sample covers the business units overhead, retail banking, and investment banking for EU banks between 2014 and 2018 and has a bank-business unit-year structure. from 2014 to 2018. The independent variables are *Market-focus*, which is and indicator variable that takes the value minus one if a bank's market-to-retail ratio is in the bottom quartile within our sample, one if a bank's market-to-retail ratio is in the top quartile within our sample, and zero otherwise, *Low overhead*, which is a dummy variable taking the value of one if a bank's overhead-to-profit-center ratio is below the median within our sample, *RB*, which is a dummy variable that takes the value of one if a business unit is related to retail banking, *IB*, which is a dummy variable that takes the value of one if a business unit is related to investment banking, and interactions of *Market-focus*, *Low overhead*, and business unit indicators *RB* and *IB*. In all columns, we use log(*BU size)*), which is the logarithm of total number of MRTs by business unit, log(*Total assets*), which is the logarithm of total assets of a bank, *ROA*, and *Cost-to-income ratio* as control variables. All columns include time fixed effects, columns 1, 3, 4, and 6 include business unit fixed effects and columns 3 and 6 include bank fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	log(Total pay)			log(Variable-to-fixed)		
	(1)	(2)	(3)	(4)	(5)	(6)
Low overhead	0.089	-0.110	-0.110	0.026	0.046	0.046
	(0.129)	(0.079)	(0.079)	(0.065)	(0.038)	(0.038)
RB		-0.147***			-0.019	
		(0.052)			(0.020)	
IB		-0.190**			0.041	
		(0.072)			(0.045)	
Low overhead × Market focus	0.084	-0.060	-0.060	0.121	-0.083	-0.083
	(0.145)	(0.070)	(0.070)	(0.076)	(0.064)	(0.064)
RB × Low overhead	0.245***	0.168***	0.168***	0.037	0.040	0.040
	(0.076)	(0.058)	(0.058)	(0.026)	(0.030)	(0.030)
IB × Low overhead	0.282**	0.214**	0.214**	-0.016	0.025	0.025
	(0.108)	(0.084)	(0.084)	(0.056)	(0.050)	(0.050)
Market focus	-0.126	-0.052	-0.052	-0.074	0.075	0.075
	(0.107)	(0.067)	(0.067)	(0.044)	(0.083)	(0.083)
RB × Market-focus	-0.126*	-0.104	-0.104	-0.041***	-0.044***	-0.044***
	(0.075)	(0.064)	(0.064)	(0.014)	(0.015)	(0.015)
IB × Market-focus	0.394***	0.172**	0.172**	0.100	0.079*	0.079*
	(0.121)	(0.072)	(0.072)	(0.061)	(0.042)	(0.042)
RB × Market-focus × Low overhead	0.048	0.063	0.063	0.027	0.004	0.004
	(0.078)	(0.067)	(0.067)	(0.021)	(0.020)	(0.020)
IB × Market-focus × Low overhead	0.244*	0.274***	0.274***	0.119	0.074	0.074
	(0.136)	(0.094)	(0.094)	(0.079)	(0.045)	(0.045)
Time FE	X	X	X	X	X	X
Business unit FE	X		X	X		X
Bank FE		X	X		X	X
Controls	X	X	X	X	X	X
Mean(y)	2.234	2.234	2.234	0.281	0.281	0.281
S.D.(y)	1.830	1.830	1.830	0.337	0.337	0.337
R^2	0.958	0.987	0.987	0.500	0.796	0.796
N	442	442	442	442	442	442

Table 8: High-earners and relative importance of investment banking
This table reports estimates from regressions of outcomes at the level of high earners on firm size and business model characteristics. The sample covers all EU banks between 2014 and 2018 and has a bank-year structure. High earners is defined by regulation as staff earning more than one mln. EUR a year. In Panel A, the dependent variable is *Total pay of high earners* and in Panel B the dependent variable is *Total number of high earners*. The independent variables are log(*Total assets*), which is the logarithm of total assets of a bank, and *Market-to-retail ratio*, which is the ratio of material risk takers (MRTs) in investment banking over MRTs in retail banking. In all columns we use *ROA*, and *Cost-to-income ratio* as control variables. All columns include time fixed effects and all even columns include bank fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Panel A: Total pay of high earners

Dependent variable:	Total pay of high-earners					
	(1)	(2)	(3)	(4)	(5)	(6)
log(Total assets)	66.518**	-12.603	52.076***	-65.748		
	(24.940)	(36.767)	(17.959)	(93.542)		
Market-to-retail ratio			21.769	11.868***	30.935*	11.197***
			(14.968)	(3.181)	(17.488)	(2.307)
Time FE	X	X	X	X	X	X
Bank FE		X		X		X
Controls	X	X	X	X	X	X
Mean(y)	79.626	84.450	79.626	84.450	79.626	84.450
S.D.(y)	225.383	231.504	225.383	231.504	225.383	231.504
R^2	0.305	0.948	0.395	0.953	0.259	0.952
N	153	144	153	144	153	144

Panel B: Total number of high earners

Dependent variable:	Total number of high earners					
	(1)	(2)	(3)	(4)	(5)	(6)
log(Total assets)	36.466***	-9.481	28.631***	-32.940		
	(13.043)	(20.172)	(9.383)	(45.399)		
Market-to-retail ratio			11.810	5.239***	16.849*	4.903***
			(7.509)	(1.434)	(8.937)	(1.008)
Time FE	X	X	X	X	X	X
Bank FE		X		X		X
Controls	X	X	X	X	X	X
Mean(y)	43.497	46.132	43.497	46.132	43.497	46.132
S.D.(y)	119.358	122.571	119.358	122.571	119.358	122.571
R^2	0.326	0.958	0.420	0.962	0.273	0.961
N	153	144	153	144	153	144

Appendix for "Marginal Returns to Talent for Material Risk Takers in Banking"

	Managers and Board of Directors	Investment banking	Retail banking	Asset management	Support functions	Control function	Others	Total
Number of personnel identified	*23*	*328*	*232*	*23*	*92*	*80*	*4*	*782*
Of which number of personnel identified and deferred	*2*	*265*	*67*	*12*	*36*	*18*	*4*	*404*
Total remuneration	4.9	173.4	69.8	11.5	27.6	18.5	2.2	308.0
Of which fixed amount	3.2	87.6	45.4	6.0	18.1	12.7	1.2	174.1
Of which variable amount	1.8	85.9	24.5	5.5	9.5	5.8	1.0	133.9

Figure A.1: MRT-table from remuneration report, Crédit Agricole 2018 This figure shows an exemplary excerpt from a remuneration report complying with CRD IV disclosure rules on MRT-level compensation. Banks are required to report fixed and variable compensation and the total number of MRTs across different business units at yearly frequency.

Total remuneration	France	Europe (excluding France)	Rest of the world
Between €1,000,000 and €1,500,000	5	6	4
Between €1,500,000 and €2,000,000	1	1	1
Between €2,000,000 and €2,500,000	1	1	1
Between €2,500,000 and €3,000,000	1	-	1

Figure A.2: High-earners-table from remuneration report, Crédit Agricole 2018 This figure shows an exemplary excerpt from a remuneration report complying with CRD IV disclosure rules on the number of income millionaires or *high earners*. Banks are required to report the number of income millionaires within bins of 500,000 EUR.

Table A.1: Definition of variables

Variable	Databases	Definition
Bank-level		
Cost-to-income ratio	Bankscope and Bankfocus	Non-interest expenses over the sum of net interest income and other operating income.
Low overhead	Hand-collected	Indicator equal to one if a bank's overhead-to-profit-center ratio is below the median within our sample.
Market focus	Hand-collected	Indicator equal to minus one if a banks' market-to-retail ratio is in the bottom quartile in our sample, equal to one if a bank's market-to-retail ratio is in the top quartile and zero otherwise.
Market-to-retail ratio	Hand-collected	Ratio of a bank's total number of MRTs related to investment banking over total number of MRTs related to retail banking.
Overhead-to-profit-center ratio	Hand-collected	Ratio of a bank's overhead staff over total number of MRTs from investment banking and retail banking.
ROA	Bankscope and Bankfocus	Return on average assets.
Total assets	Bankscope and Bankfocus	Total assets.
Total number of high earners	Hand-collected	Total number of high earners, which are defined as staff that earning at least EUR 1 mln. a year.
Total pay of high earners	Hand-collected	Total pay of all high earners within a bank, which are defined as staff that earns at least EUR 1 mln. a year.
Business unit-level		
BU size	Hand-collected	Total number of MRTs in a business unit.
IB	Hand-collected	Indicator equal to one if a business unit is related to investment banking.
Rel. BU size	Hand-collected	Total number of MRTs in a business unit over total number of MRTs in a bank.
RB	Hand-collected	Indicator equal to one if a business unit is related to retail banking.
Total pay	Hand-collected	Total pay of material risk takers within a business unit.
Variable-to-fixed	Hand-collected	Ratio of total variable pay over total fixed pay within a business unit.

Paper 4:

Managerial Compensation in Stressed Banks: Evidence from the ECB's AQR Stress Test

Managerial Compensation in Stressed Banks: Evidence from the ECB's AQR Stress Test*

Moritz Stieglitz†

Abstract

Bank compensation in distress is often regarded as special due to the prevalence of high bonus payments. I study European banks over the period 2014 to 2018 to determine how banks adjust compensation during distress in the post-crisis era. I use a novel hand-collected database to analyze compensation and employment of managerial employees at both the executive and non-executive level. Banks are classified as (un-)stressed based on the results of the ECB's 2014 stress test. As commonly observed for non-financial firms, distressed banks reduce overall wage expenses by reducing employment rather than cutting fixed or variable compensation. The convergence of bank behavior with that of non-financial firms might be be due to post-crisis compensation regulation and bail-in rules.

JEL Classification: G21, G33, G34

Keywords: Banks, compensation, bank distress

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1 Introduction

Firms usually adjust to distress by reducing employment while leaving wages of the remaining workers unchanged (Azariadis, 1975). The banking literature, however, has long argued that compensation in banks is special, in particular for senior employees involved in risk-taking decisions. Banks operate in a high-risk environment with volatile returns and therefore try to tie wages to market conditions by heavily relying on bonus payments, which reduces wage rigidity and operating leverage. This could allow banks to adjust to distress mainly by cutting bonus payments rather than laying off workers (see e.g. Oyer, 2004; Efing, Hau, Kampkötter, and Rochet, 2018). Another mechanism that could induce banks to behave differently is the existence of government safety nets and the possibility to gamble for resurrection close to bankruptcy. In fact, given a sufficiently weak regulatory framework the resulting moral hazard could lead banks to even increase variable compensation in distress (Hakenes and Schnabel, 2014). There are few empirical studies documenting how banks endogenously change the compensation policy for key employees once they become distressed and this study tries to close this gap.

I study a sample of managerial employees in European banks over the period 2014 to 2018 and differentiate between stressed and non-stressed banks based on failing or passing the ECB's 2014 Asset Quality Review (AQR) stress test. My empirical results suggest that bank compensation in distress does not seem to be so special relative to non-financial firms. Stressed banks cut employment while leaving fixed and variable compensation of the remaining bankers unchanged, which is in line with the downward nominal wage rigidity widely observed in aggregate (Abbritti and Fahr, 2013) and micro data (Messina, Duarte, Izquierdo, Du Caju, and Hansen, 2010; Du Caju, Kosma, Lawless, Messina, and Rõõm, 2015) from non-financial sectors. One reason for my findings could be that banks simply do not behave as theorized in the banking literature. One alternative explanation is that post-crisis regulation has been so successful that bonus payments are simply too low to serve as an important adjustment tool and that the bail-in clause has eradicated gambling for resurrection and concurrent bonus increases in distress. In fact, according to Philippon and Reshef (2012) bank compensation has been changing in tandem with regulatory and de-regulatory waves for decades. In addition to regulation, industry trends such as technological change might have changed both banks' business models and the way the compensate their employees. While my setting does not allow me to pin down the specific mechanism or regulation underlying my results, they constitute important stylized facts for policymakers and future research alike.

Hitherto, the literature has mostly focused on CEO compensation due to data limitations. However, CEOs are not the only type of employees taking crucial decisions relevant for a bank's overall risk stance. In each bank there are several mid-level managers, traders, and loan officers with large portfolios whose decisions can significantly affect bank-level performance and who are highly compensated. I cover both this group of so called "material risk takers" (MRTs) and executive managers and examine their headcounts, fixed, and variable compensation. Thus, the data thus allows me not only to analyze whether the adjustment is mostly price- or quantity based, i.e. whether it is mostly based on wage adjustments or on layoffs, but also which group is affected the most.

Specifically, I document that stressed banks decrease the number of MRTs outside of the management board (MB) by up to 14.8% in the two years after the stress test. For those non-MB MRTs that evade layoffs, I fail to find evidence of a decrease in fixed or variable compensation. Instead, there is weak evidence of a small increase in average compensation and the share of variable compensation. Thus, the adjustment is mostly quantity-based, which is in line with standard downward nominal wage rigidity widely observed in non-financial firms. MRTs inside the MB appear to be shielded from both a price- and a quantity-based adjustment. This is suggestive of executive power and entrenchment, which are also phenomenons widely discussed in the literature on executive compensation in non-financial firms (see e.g. Bebchuk and Fried (2003)).

Furthermore, I examine the heterogeneity of my main result by adding an additional layer of differentiation. Stressed banks that were better capitalized before the AQR stress test exhibit a less pronounced decrease in non-MRT headcounts. Thus, equity seems to shield banks from harsher human capital adjustments. However, this result only holds for a simple leverage ratio and not for the risk-adjusted tier 1 ratio.

While I do not claim that my treatment, i.e. failing the AQR stress test, is exogenous, my results depend on a comparison of stressed and non-stressed banks. In a robustness check, I test whether my results are driven by pre-treatment trends in size, performance and capitalization. I improve the comparability of the two groups by excluding stressed banks with a very high ex-ante propensity to fail the stress test and non-stressed banks with a very high propensity of passing the stress test. My results remain qualitatively unchanged, which suggests that they are not driven by pre-treatment trends. In a further robustness check, I test whether there is evidence of a human capital response at the aggregate, bank-wide level. Indeed, I document that banks also decrease the total number of employees, albeit with some delay relative to the human capital adjustment at the MRT-level.

My study relates to both the general literature on executive compensation in distressed firms and the literature on compensation in distressed banks in particular. The literature on executive compensation is mostly focused on CEOs and finds mixed evidence on the consequences of firm distress. For example, Gilson and Vetsuypens (1993), Henderson (2007) and Kang and Mitnik (2008) all look at CEO compensation in US firms subject to a bankruptcy procedure in the pre-financial crisis era and find contradicting results. While Henderson (2007) document that CEO compensation is not significantly changed during distress, both Gilson and Vetsuypens (1993) and Kang and Mitnik (2008) provide evidence of an increase in CEO turnover and a decrease in CEO compensation. In a more recent study, Carter, Hotchkiss, and Mohseni (2020) find that CEO contracts are significantly altered when firms approach bankruptcy. They highlight that there is a substantial difference in executive compensation between a firm that is simply performing poorly and one that is approaching bankruptcy. Distressed firms become focused on preserving liquidity and adjust compensation contracts to align the incentives of executives in that direction. Goyal and Wang (2017) focus on a different dimension of compensation in distress, namely that executives voluntarily leaving during distress is a particularly disruptive type of turnover. They document that bankrupt US firms use so-called key-employee retention plans that involve bonus payments to dissuade executives from leaving. My study adds to the literature by significantly increasing the scope of employees analyzed. I not only look compensation of executives but at key employees in positions relevant for risk-taking all across the bank hierarchy.

Many papers analyze how bankers' compensation affect bank risk-taking and default risk. For example, there are several papers that study how short-term oriented performance pay might have contributed to excessive risk-taking in the run-up to the financial crisis (see e.g. Fahlenbrach and Stulz (2011), Bebchuk, Cohen, and Spamann (2010), Bai and Elyasiani (2013), and Efing, Hau, Kampkötter, and Steinbrecher (2015)). In contrast, there are very few papers that study how bankers' compensation changes in response to distress. In this regard, my study is most closely related to Efing et al. (2018) who find a contraction in bonus pay for non-executive employees of banks in German-speaking countries during the financial crisis. My study complements their paper by looking at the post-crisis era and by including executive employees and banks from other European countries. The fact that the bonus cuts observed among distressed banks during the financial crisis do not seem be relevant any more for distressed banks in the European Banking Union is very notable and could possibly be related to post-crisis regulatory changes.

2 Institutional setting

2.1 Disclosure requirements on material risk takers

In response to the financial crisis and the belief among policymakers and scholars that excessive compensation was one of its causes, EU member states implemented a host of regulations regarding bankers' compensation. These regulations were aimed to reduce incentives for short-term risk taking and to increase transparency on compensation structures. In 2011, the EU implemented the European Capital Requirements Directive, or CRD III, which among other things introduced the concept of material risk takers.[1] The Capital Requirements Directive IV and the Capital Requirement Regulation (CRR), implemented in 2014 and jointly referred to as "CRD IV", then introduced disclosure requirements for this particular group of employees. The regulation requires banks to disclose data on total, fixed, and variable compensation and the number of MRTs split up by business units. Moreover, the Commission Delegated Regulation 604/2014 implemented a recommendation of the European Banking Authority and provided a concrete procedure how to identify MRTs among bank employees based on quantitative criteria such as total remuneration or the size of the loan portfolio under management and qualitative criteria such as an elevated position like department head.

2.2 The ECB's AQR stress test

Before the start of the Single Supervisory Mechanism (SSM) in November 2014, the ECB engaged in a comprehensive assessment of European banks' balance sheet to identify and eradicate balance sheet weaknesses before the mutualization of supervision and resolution in the European Banking Union. The ECB announced the AQR and the accompanying stress test on October 23, 2013. Both the timing of the AQR and the strictness of the modalities of the asset review and stress test took markets by surprise (Abbassi, Peydro, Iyer, and Soto, 2020). The asset quality review was an

[1]The original description used in the regulation is "staff whose professional activities have a material impact on the risk profile of credit institutions. In addition to the term material risk takers, another common shorthand to refer to this group of bank employees is "identified staff".

assessment of the balance sheet as of December 31, 2013 and some banks used the time between the announcement in October and the cutoff date in December to change the risk composition of their balance sheet only to (partially) reload on risky assets later on (Abbassi et al., 2020). The asset quality review was then followed by a stress test exercise based on the newly reviewed balance sheet figures, which was conducted in July 2014. The results of the stress test were released in October 2014. Among the 130 euro area banks reviewed, 25 banks failed the stress test resulting in a total capital shortfall of EUR 24.6 billion. Among those 25 banks, 12 make it into my final dataset and are marked by bold font in Table 1.

3 Theory

There are theoretical reasons why banks might rely on employment adjustment while insulating wages during distress and why banks might instead rely on wage cuts while insulating employment.

The implicit contracting literature tries to explain why firms usually adjust to adverse shocks by lowering employment and leaving wages for the remaining employees unchanged. One common explanation are different risk preferences of firms and workers. Since firms are usually assumed to be risk neutral while workers are risk averse, the optimal contract involves some form of wage insurance against adverse shocks at the firm-level as long as the firm is far enough away from bankruptcy (see e.g. Azariadis (1975) and Harris and Holmstrom (1982)).

When I look at wages for top employees in particular, competitive job markets for managerial talent are another reason for firms to avoid wage cuts. This might be especially relevant in the banking industry where recent changes in business models have made skills less location- and firm-specific and thus increased managerial labor mobility (Acharya, Pagano, and Volpin, 2016). The risk of the most skilled managerial employees leaving the bank in response to wage cuts becomes even more relevant during distress, given that their turnover could prove to be particularly disruptive (Goyal and Wang, 2017).

While the preceding arguments explain why firms usually try avoid wage cuts during distress, the banking literature highlights a channel that might even explain increases in wages, specifically regarding variable compensation. The possibility of bail-outs could lead banks to engage in risk shifting and use performance pay to incentivize the pursuit of high-risk, high-return projects (Hakenes and Schnabel, 2014; Ongena, Savaser, and

Sisli Ciamarra, 2018).

Theories explaining downward adjustments of wages usually rely on financial frictions as the main factor preventing banks from insulating the compensation of their top employees against adverse shocks. Berk, Stanton, and Zechner (2010) study human capital adjustments in highly levered firms and show how it can be optimal for firms and workers to agree to temporary pay cuts during distress. Efing et al. (2018) look at banks and build a model with financial frictions, where across-the-board bonus cuts are used to preserve cash during distress. Moreover, they show that this strategy is especially relied upon by banks with risky business models. In Oyer (2004), the theoretical mechanism inducing firms to rely on variable compensation adjustment during downturns is that tying wages to market conditions allows them to guarantee employee's participation constraint regardless of the availability of outside offers.

Taken together, the theoretical literature predicts cuts in employment rather than wage cuts as the preferred means of adjustment for non-financial firms. However, theories with a focus on highly levered firms (such as banks) offer explanations for temporary wage cuts in total compensation, cuts in variable compensation and even increases in variable compensation. Ultimately, the human capital response in distressed banks is an empirical question.

4 Empirical approach

I try to capture all relevant dimensions of human capital adjustment in stressed banks by looking at a host of different outcome variables both at the level of material risk-takers within the MB and outside the MB. I estimate the following specification:

$$ln(y_{ict}) = \boldsymbol{\beta}_1 s_i \times p_t + \boldsymbol{\gamma} \boldsymbol{c}_{ict} + \mathbf{1} \boldsymbol{f}_{ict} + \epsilon_{ict}, \tag{1}$$

where i, c, and t denote bank, country, and year, respectively. For the dependent variable, y_{ict}, I use the number of MRTs differentiated by being inside or outside the MB and several measures of their total and variable compensation.

My main independent variable is the continuous stress indicator, s_i, which is equal to the ratio of banks' equity shortfall during the 2014 AQR stress test to banks' 2013 total assets. The indicator is equal to zero for banks that did not exhibit any capital shortfall during the stress test. I interact this indicator with year-dummies, denoted by p_t, to be able to better describe the evolution of the dependent variables over time.[2] Note that my setting does not constitute a difference-in-difference analysis since I am lacking MRT data for the period before the stress test.

I prefer to exclude balance sheet measures from the vector of control variables, $\boldsymbol{c}_{ict}$, since they might themselves be outcomes of the treatment (Angrist and Pischke, 2008). The vector of fixed effects, $\boldsymbol{f}_{ict}$, contains bank fixed effects that control for time-invariant differences across banks and year fixed effects that control for bank-invariant time-varying factors like the business cycle. In my preferred and most restrictive specification, I substitute the year fixed effects with interacted country-group times year fixed effects.[3] I differentiate between three groups: i) countries at the euro area periphery to control for the after-effects of the European sovereign debt crisis, ii) euro area core countries, and iii) non-euro-area countries to take into account that they did not participate in the stress test.[4] With this set of fixed effects I capture country-specific business cycles and country-specific developments in the banking sector.

My analysis of distressed banks relies on a comparison of their adjustment efforts to the developments among non-stressed banks. Since the selection into the group of stressed and non-stressed banks is determined by bank health, pre-AQR trends determining performance during the stress test could confound my analysis of post-stress-test adjustments. I therefore provide a robustness check, where I improve the comparability of stressed and non-stressed groups by excluding banks with high propensities to become part of either group based on several pre-treatment characteristics. I specify which propensities I consider as exceptionally high based on a visual inspection of the distribution of propensity scores, as discussed in Section 6.1. In a further robustness check, I extend the analysis to non-MRT employees by re-estimating my preferred specification using headcounts and compensation of all bank employees as dependent variables.

[2] In some specifications, I use a post-2014 interaction that is equal to one for the years 2015-2018 and zero for 2014 instead of the year dummies. The pooled time-period-by-stress interactions allow me to use a triple interaction framework in an analysis focusing on the the heterogeneity of my results regarding ex-ante balance sheet characteristics.

[3] The sample size is too small to allow me to specify country-year fixed effects.

[4] The first group contains Cyprus, Spain, Greece, Ireland and Italy. The second group contains Austria, Belgium, Germany, France, Malta, Netherlands, and Slovenia and the third group contains Denmark, Great Britain, Hungary, Poland, and Sweden. In a robustness check, I confirm that my results are robust to excluding banks from the third group.

5 Data and summary statistics

This paper builds upon the database first introduced in Stieglitz and Wagner (2020). Banks fulfill their CRD IV disclosure requirements by reporting the number of material risk takers and their variable, fixed, and total compensation split up by eight business units classified by an EBA template.[5] In this study, I only differentiate between MRTs in- and outside of the MB and thus summarize MRTs in the remaining business units as "non-MB" MRTs. Moreover, I exclude MRTs from the MB in supervisory function because their compensation includes several non-standard components such as attendance fees for board meetings and is therefore not comparable to that of the other business units.

Given that the CRD IV was only implemented in 2014, my sample period starts in 2014 and ends in 2018. The hand-collection effort is restricted to the 124 European banks from in- and outside the euro area that participated in the 2014 EBA stress test. Since I combine MRT data with balance sheet data from Bankscope and Bankfocus, I restrict my final sample to banks with non-missing total assets pre-AQR, i.e. in 2013. Because several banks do not report any information on MRTs or only report aggregate compensation without headcounts, I further restrict my sample to banks that report at least twice on the yearly number of MRTs by business unit.[6] This procedure leaves me with 57 banks. The number of banks varies across specification depending on the variables included.

In Table 2, I show summary statistics for all variables from the MRT dataset and variables relating to banks' balances sheet or all employees within a bank. All variables are winsorized at 1% and 99% except for the AQR stress variable, i.e. the continuous equity shortfall determined in the AQR stress test. I do not winsorize the stress variable because extreme values of stress, i.e. non-linearities in performance rather than just poor performance, is precisely what I am interested in. Refer to Appendix Table A.1 for variable definitions.

Figure 1 shows the evolution of several aggregate bank-level characteristics for stressed (solid line) vs. non-stressed (dashed line) banks from 2013-2018. Stressed

[5]The template was introduced in EBA guideline EBA/GL/2014/08. See the appendix for further details.

[6]The vast majority of banks excluded by this culling procedure are banks that do not report at all or only report compensation without headcounts. However, there are nine banks that do report on headcounts but only once, mostly towards the end of the sample period. Only in one case, this is clearly due to sample attrition, namely in the case of Banco Popolare di Vicenza, which reports on MRTs in 2016 but goes bankrupt and is then acquired by Intesa in the following year.

banks are smaller in terms of total assets, total labor expenses, and the total number of employees, rely more on deposit funding and exhibit higher impaired loans, lower return on assets (ROA), and slightly higher cost-to-income ratios.[7] Over time, stressed banks improve their ROA, decrease the total number of employees, and increase their capitalization in terms of both the risk-adjusted tier 1 ratio and equity over total assets. The evolution of capital for stressed banks is determined by the legal obligation to improve capitalization in response to failing the stress test. Furthermore, both types of banks increase their reliance on deposits. Note that while I include the year 2013 in Figure 1 to be able to better compare the pre- and post AQR periods, I exclude 2013 in all of my regressions since the MRT dataset only starts in 2014.

Figures 2 and 3 show the evolution of several outcomes at the MRT level. Stressed banks employ a lower number of MRTs outside of the MB and grant both groups of MRTs lower total (Figure 2) and variable compensation (Figure 3), which is likely due to their smaller size. Over time, the variable compensation share for non-MB MRTs in stressed banks seems to increase slightly, while it decreases for non-stressed banks. Other than that, there is no clearly discernible pattern in the immediate aftermath of the AQR stress test also given that the confidence intervals for MRT variables are relatively large. My visual inspection thus highlights the need to go beyond non-parametric comparisons and remove time-invariant differences and common trends within a regression framework. Moreover, the simple distinction between banks that fail and those that do not fail the stress test does not seem to suffice to detect diverging trends after the AQR, which warrants the use of a measure of a continuous measure of distress.

6 Results

First, I examine the number of MRTs and their fixed and variable compensation to identify the main margin of adjustment to distress. I then subject my results to several sensitivity and robustness checks.

In Table 3, I analyze the effect of bank stress on the number of MRTs in- and outside of the MB and two different measures of their total compensation. Average pay increases for non-MB MRTs (column 1) but not for MRTs inside the MB (column 2) in stressed banks in the two years after the stress test.[8] Headcounts decrease both for

[7] Note that in tables and figures I use ROA expressed in terms of percentages, while I use ROA expressed in terms of decimals when I employ it as a regression control variable.

[8] In the following, I will talk of "stressed banks" vs. "non-stressed banks" for the sake of brevity. Strictly speaking, I would need to say "more stressed banks relative to less stressed banks and banks

MRTs inside and outside the MB (columns 3 and 4), while the effect is stronger for non-MB MRTs. In unreported regressions, I document that the result for MB-MRTs (column 4) loses significance once I use my most restrictive specification that includes country-group-by-year fixed effects. In columns 5 and 6, I show that after controlling for the number of MRTs there remains no statistically significant effect of bank stress on average pay. Given that average pay and headcounts are mechanically related, the increase in average pay for non-MB MRTs I found in column 1 was thus likely driven by the decrease in headcounts documented in column 3.

Table 4 reports the results of my baseline regression for average variable pay, the share of variable over fixed compensation, and total variable pay for MRTs in- and outside the MB. I use the natural logarithm of one plus compensation when I employ average variable pay and total variable pay as dependent variables to account for banks with zero variable compensation. In the case of the variable share, I use the dependent variable in levels. The only significant result is a small increase in the variable share for non-MB MRTs in stressed banks (column 3). Thus, Table 4 does not provide evidence for theories such as Oyer (2004) and Efing et al. (2018) predicting reductions in bonus payments as the main margin of adjustment in stressed banks. At the same time, Table 4 also does not provide sufficient evidence for risk-shifting in the sense of Hakenes and Schnabel (2014) and Ongena et al. (2018), given that the increase in the variable pay share is weakest right after the stress test and given that all other specifications of Table 4 are insignificant. Instead, the result in column 3 is likely driven either by factors unrelated to the AQR or mechanically driven by the recovery in ROA experienced by stressed banks after the AQR (see Figure 1). The results from unreported regressions support this interpretation. When I use my most restrictive specification, where I control for time-varying factors unrelated to the AQR and for ROA and other balance sheet measures, the result for the variable share of non-MB MRTs loses significance.

Overall, Tables 3 and 4 suggests that the reduction of headcounts for non-MB MRTs is the main margin of adjustment. The effect is not only highly statistically but also economically significant. For example, the coefficient for the 15-by-stress interaction in column 3 of Table 3 suggests that for each percentage point increase in the ratio of equity shortfall to 2013 total assets there is a 14.8% decrease in headcounts for non-MB MRTs.[9] One percentage point is roughly the difference between the smallest relative

without any equity shortfall" given the continuous nature of my stress indicator.

[9] This number is based on the formula $exp(\beta) - 1) * 100$ for a precise interpretation of coefficients

equity shortfall relative to the median equity shortfall within my sample. This result confirms theoretical predictions from the implicit contracting literature that firms in distress try to avoid wage cuts and rely on reductions in employment as the main margin of adjustment. I also do not detect any evidence of temporary pay cuts as predicted by Berk et al. (2010). The fact that my results are mostly insignificant for MRTs inside the MB is suggestive of management power and entrenchment as discussed e.g. in Bebchuk and Fried (2003).

I go on by scrutinizing my main result in Table 5. In column 1 of Table 5, I check how sensitive my results are to including balance sheet controls. To capture changes in bank size, I include the natural logarithm of total assets. I include impaired loans over total assets and return on assets (ROA) to capture changes in bank health and performance. To capture changes in banks' funding structure and business model I control for the ratio of deposits over total assets and the ratio of equity over total assets. Moreover, I control for the cost-to-income ratio, which captures both changes in the business model and the pressure to cut costs, which could be an alternative driver for human capital adjustment. In addition to lagged ROA, I also add contemporaneous ROA to be able to capture immediate, short-term pressure for human capital adjustment emanating from bank performance. Due to the missing values in balance sheet characteristics, the sample size in column 1 of Table 5 is somewhat smaller than the sample size in column 3 of Table 3. Nonetheless, the results remain qualitatively unchanged.

In column 2 of Table 5, I use my most restrictive and preferred specification by combining bank fixed effects with interacted country-group-by-year fixed effects, which control for country-specific business cycles or developments in the banking sector.[10] The negative effect on the number of non-MB MRTs in stressed banks in 2015 and 2016 is only slightly smaller than in column 3 of Table 3 and remains highly significant. This regression constitutes my baseline regression.

In column 3, I modify my baseline regression by pooling the year-by-stress interactions into a single post-2014-by-stress interaction, where post-2014 is equal to one starting in 2015. Even though the coefficient mixes the negative effect from 2015 and 2016 with the null effects from 2017 and 2018, the result is still significant, albeit only at a 10% significance level.

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in log-level regression models.

[10]In addition, the country-group-by-year fixed effects control for the fact that banks from outside of the euro area are non-stressed by construction because they did not participate in the ECB's AQR stress test. In Appendix Table A.2, I confirm that my results remain unchanged if I drop non-euro area banks altogether.

In Table 6, I combine the post-2014-by-stress interaction from the previous analysis with indicators describing heterogeneity in business models. These indicators are based on pre-AQR characteristics to limit concerns about endogenous responses in the modifying variable. To capture heterogeneity in business models emanating from the funding structure, I classify banks as highly capitalized if they are above the 75th percentile of capitalization among sample banks in 2013. I measure capitalization either with the regulatory risk-adjusted tier 1 capital ratio (column 1) or the ratio of equity over total assets (column 2). The coefficient on the triple interaction with risk-adjusted capitalization (column 1) suggests that stressed banks decrease headcounts more if they were better capitalized ex-ante. In contrast, the coefficient on the triple interaction with equity over total assets (column 2) suggests that stressed banks decrease headcounts less if they were better capitalized ex-ante. The results in column 2 are more statistically significant and more in line with economic intuition than the results in column 1, which is why I interpret my findings as evidence that an equity cushion allows stressed banks to engage in a less aggressive human capital adjustment.[11]

In columns 3 and 4, I investigate the heterogeneity of my main result regarding two additional measures reflecting differences in banks' business model. In column 3, I classify banks according to their market orientation. I use the business model measure from Stieglitz and Wagner (2020), which relates the number of MRTs in investment banking to the number of MRTs in retail banking. Retail banking and investment banking can be regarded as opposite poles in the continuum between traditional and modern market-focused banking (Gorton and Metrick, 2012). I therefore classify those banks as market-focused that exhibit a ratio above the median among sample banks in 2014.[12] The results suggest that there do not seem to be significant differences in human capital adjustment among stressed banks along the market-focus dimension.

I try to capture a different dimension of heterogeneity in business models by looking at stark differences in the maturity of pre-AQR funding structure. I classify banks as highly dependent on short-term funding if their ratio of short-term over long-term funding is above the 75th percentile within the sample in 2013. Long-term funding consists of senior debt maturing after one year, subordinated borrowing, other funding,

[11]Another reason leading me to prefer the results in column 2 is that several banks used the two months between the announcement and the cutoff date of the AQR to temporarily shed risky assets, making risk-adjusted ratios less reflective of capitalization (Abbassi et al., 2020).

[12]Given that most stressed banks in my sample tend to be smaller and less-market focused, no stressed bank exhibits a market-focus above the 75th percentile and only Banca Popolare di Milano exhibits a market-focus above the median.

and shares and hybrid capital accounted for as debt. To capture the reliance on short-term funding I follow Buch, Buchholz, and Tonzer (2015) and use the sum of deposits from banks, repos and cash collateral, and other deposits and short-term borrowings. In column 4, I find that there does not seem to be a difference in human capital adjustment among stressed banks with more or less reliance on short-term funding.

In column 5, I look at both bank capitalization and net income as of 2013 and relate it to the standard deviation of ROA over 2012-2013 to capture a bank's stance regarding the tradeoff between risk and return. This measure is commonly referred to as *z-score* and reflects the inverse of the probability of insolvency (Laeven and Levine, 2009).[13] Column 5 suggests that banks that fared better in terms of risk-adjusted return had to resort to a less aggressive human capital adjustment once they got into distress.

6.1 Robustness checks

My analysis relies on the comparison of stressed and non-stressed banks and therefore on the assumption that these two groups are comparable and subject to broadly similar pre-treatment trends. To investigate whether my results might be driven by extreme differences between the two groups, I estimate the propensity to become stressed based on pre-AQR, 2013 growth rates. I pick the three variables that are arguably most important to determine heterogeneity in pre-AQR bank structure, namely total assets to capture size, ROA to capture performance and equity over total assets to capture capitalization. Figure 4 shows the propensity scores based on the three aforementioned variables and on the dichotomous bank stress measure, where all banks with a non-zero equity shortfall in the AQR stress test are classified as stressed. As is evident from the graph, there are indeed some banks whose pre-AQR trends suggest a very high probability of either failing or not failing the AQR stress test, which threatens the comparability of these two groups. I therefore re-estimate my main analysis while excluding the banks represented by the leftmost bar and the two rightmost bars in Figure 4. Technically speaking, I am excluding banks from the control group with a very high ex-ante probability of being untreated and banks from the treatment group with a very high ex-ante probability of being treated.[14] In column 1 of Table 7, I re-estimate my baseline regression (see column 2 of Table 5) and document that excluding

[13] Only Banca Popolare dell'Emilia Romagna falls above the (median-) threshold.

[14] The two stressed banks that I am excluding are Piraeus and Carige and the two non-stressed banks are Barclays and HSH Nordbank.

the aforementioned groups of banks does not change my results in a qualitative sense. Stressed banks still exhibit a decrease in headcounts of non-MB MRTs in 2015 and 2016 relative to 2014.

In my baseline regression, I employ a continuous measure of bank stress. In column 2 of Table 7, I use the binary measure of banks stress also used in Figures 1, 2 and 3, which only differentiates between banks that have failed and those that have not failed the stress test. All coefficients in column 2 are insignificant, which confirms the insight gained from the descriptive evidence that a simple binary differentiation does not suffice to detect an effect of bank stress on human capital adjustment. These results are not surprising given that Table 1 shows that there were several banks that were very close to clearing the bar of non-zero equity shortfall. Using a binary model thus represents a fundamentally different approach because it shuts down the differentiation between different degrees of failing and treats all of these stressed banks the same.[15]

In columns 3 and 4, I investigate the human capital response at a more aggregate level by using my baseline approach for two measures that encompass all employees in a bank. In column 3, I analyze total labor expenses and document a decline of roughly 3% for each percentage point increase in relative equity shortfall. The total number of bank employees decreases by about 5% starting in 2016 (column 4). While it is hard to directly relate these results to the results from the MRT-level, the decrease in non-MB MRT headcounts might be driving the decrease in aggregate labor expenses in 2015 (column 3). The MRT employees that were cut from the banks' payroll were all highly paid and their departure might be enough to dent labor expenses at an aggregate level. In contrast, regarding only headcounts, the number of MRTs is too small for a reduction in their numbers to be detected in the aggregate. Thus, it is not surprising that there is no significant response in 2015 in column 4. The decrease in headcounts starting in 2016 is likely driven by non-MRT layoffs.

7 Limitations

[15]Note that the choice between binary and continuous bank stress also affects the results for some of my other dependent variables. In unreported results, I re-run my baseline specification from column 2 of Tables 5 with binary bank stress and the dependent variables from Tables 3 and 4. The increase in the variable pay share in 2015 for non-MB MRTs that I also found in column 3 of Table 4 becomes slightly stronger and is now accompanied by small increases in average and total variable pay concentrated in 2017. This is also reflected by a slightly significant increase in total overall pay for non-MB MRTs in 2017.

While the empirical exercise I conduct is informative about the endogenous human capital adjustment in response to distress, it suffers from some limitations.

First, the factors that made certain banks fail the stress test likely also influence their post-failure behavior. However, I am agnostic about the causes of individual distress since I am instead interested in banks' endogenous decisions once they have gotten into distress. Moreover, the robustness check where I exclude banks with a very high or low ex-ante propensity to fail the stress test alleviates concerns that my results are driven by banks that are not comparable.

Second, as mentioned in Section 2.2, some banks changed the risk composition of their balance sheet in the two months between the announcement and cutoff date for the AQR only to (partially) reload on risky assets later on (Abbassi et al., 2020). Depending on how successful banks were in this endeavor, less banks might have failed the stress test than required by the underlying fundamentals, which might introduce considerable bias in my comparison. Note, however, that if I assume that the window-dressing banks that wrongfully ended up in the control group are not fundamentally different in terms of their compensation practices, the resulting bias would only lead me to underestimate the intensity of the human capital adjustment of stressed banks.

Third, the MRT dataset is subject to survivorship bias. Banco Popolare and Banca Popolare di Milano merged in 2017, which led to the creation of a new bank and therefore the discontinuation of MRT disclosure by the two predecessors. While in this case there is a clearly observable reason for non-disclosure, in many other cases the failure to disclose MRT data cannot be pinned down on observables. If the tendency to comply with EBA reporting requirements were correlated with bank characteristics relevant for bank health and compensation practices, my empirical design would suffer from sample selection bias.

Fourth, I cannot ascertain to what extent my main result, namely the decrease in non-MB MRT employment is driven by actual lay-offs. It could be that the decrease comes from (early) retirement combined with a pause in new hires. This would, however, not threaten the overall validity of my story of human capital adjustment. Another possibility is that stressed banks re-classify certain MRT employees as non-MRTs. This could be either because their portfolio of tasks has been changed or, more concerning for my purposes, because banks want to decrease compensation transparency and regulators leave them too much leeway in the process of MRT identification.

8 Conclusion

When banks get into distress, the prevalence of variable compensation and the existence of government safety nets might lead to a human capital response that is very different from that of non-financial firms. Non-banks usually adjust to distress by cutting employment and leaving compensation unchanged. In contrast, there are theories suggesting that banks should rely mostly on variable pay adjustment in reaction to distress. My aim is to empirically determine the actual path of human capital adjustment in stressed banks based on a far wider sample of employees than is common in the literature.

I use a novel hand-collected database, which covers both executive and non-executive employees that are taking decisions relevant for bank risk-taking. I use the 2014 AQR stress test to define banks either as stressed or non-stressed during my 2014-2018 sample period. The banks in my sample adjust to distress by reducing MRT headcounts while keeping compensation for the remaining MRTs unchanged. However, this result only applies to MRTs outside the MB with stressed banks shielding both compensation and employment of executive MRTs, which could be suggestive of executive power and entrenchment. Moreover, I provide evidence that banks that were better capitalized ex-ante had to resort to a relatively less stringent decrease in headcounts after failing the stress test. To test whether my results might be driven by pre-treatment trends, I estimate the propensity to fail the stress test based on pre-AQR balance sheet trends. Excluding banks that are less comparable to banks from the opposite treatment group does not significantly alter my main result. In addition, I document that the decrease in headcounts evident at the MRT-level also takes place at the wider bank-level, albeit with a certain lag suggesting that MRTs are the most immediate lever of human capital adjustment in stressed banks.

In summary, my results suggest that in the post-crisis period banks' human capital adjustment to distress is in line with the response commonly observed for non-financial firms. This contrasts with evidence provided in related studies observing sweeping bonus cuts in banks during the financial crisis. One possible reason for the observed change in bank behavior could be that post-crisis regulation has curbed variable compensation and incentives for risk-shifting.

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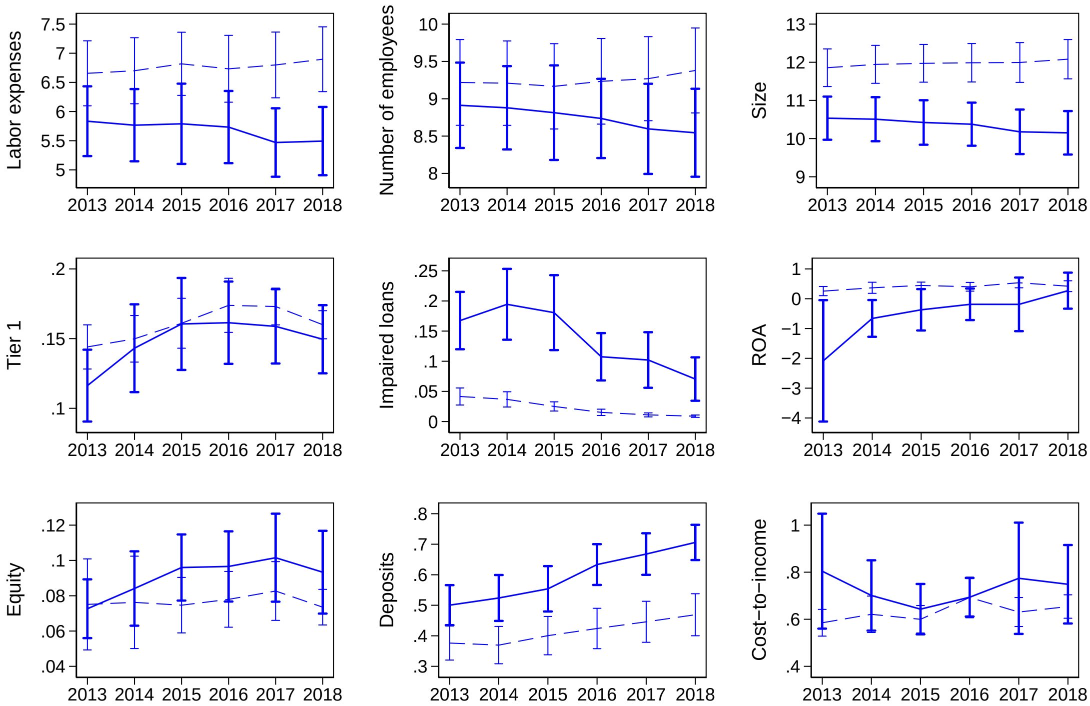

Figure 1: Bank-wide employees and balance sheet characteristics This figure shows the evolution and the respective 95%-confidence intervals for several bank balance sheet characteristics and the number of bank-wide employees over the period 2014 to 2018 for stressed (sold line) vs. non-stressed banks (dashed line). Stressed banks are those with a non-zero equity shortfall in the ECB's 2014 AQR stress test. Refer to Appendix Table A.1 for variable definitions.

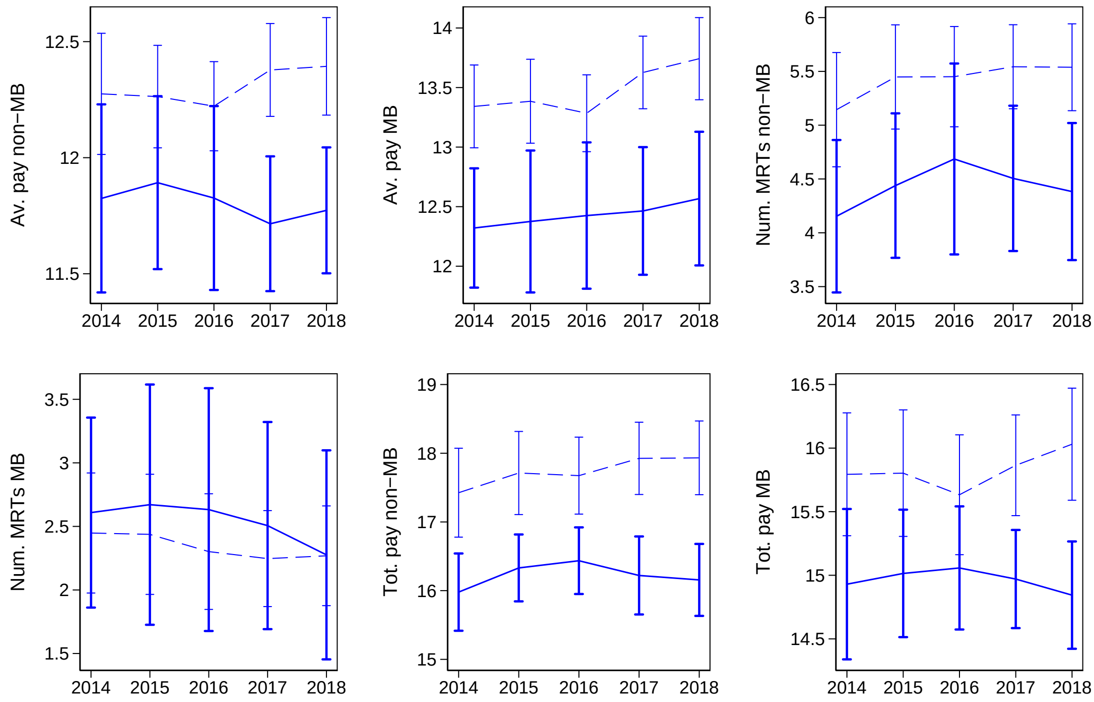

Figure 2: MRT headcounts and total compensation This figure shows the evolution and the respective 95%-confidence intervals of the number of MRTs outside and inside of the management board ("non-MB" vs. "MB") and several measures of their compensation over the period 2014 to 2018 for stressed (solid line) vs. non-stressed banks (dashed line). Stressed banks are those with a non-zero equity shortfall in the ECB's 2014 AQR stress test. Refer to Appendix Table A.1 for variable definitions.

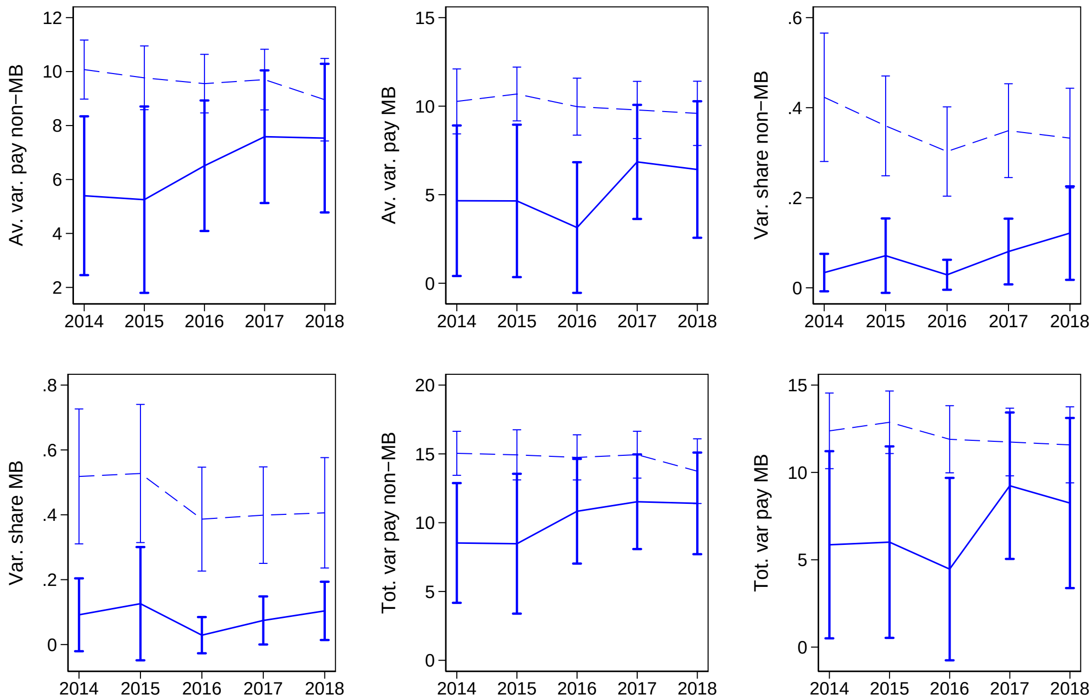

Figure 3: MRT variable compensation This figure shows the evolution and the respective 95%-confidence intervals of several measures of variable compensation for MRTs outside and inside of the management board ("non-MB" vs. "MB") over the period 2014 to 2018 for stressed (solid line) vs. non-stressed banks (dashed line). Stressed banks are those with a non-zero equity shortfall in the ECB's 2014 AQR stress test. Refer to Appendix Table A.1 for variable definitions.

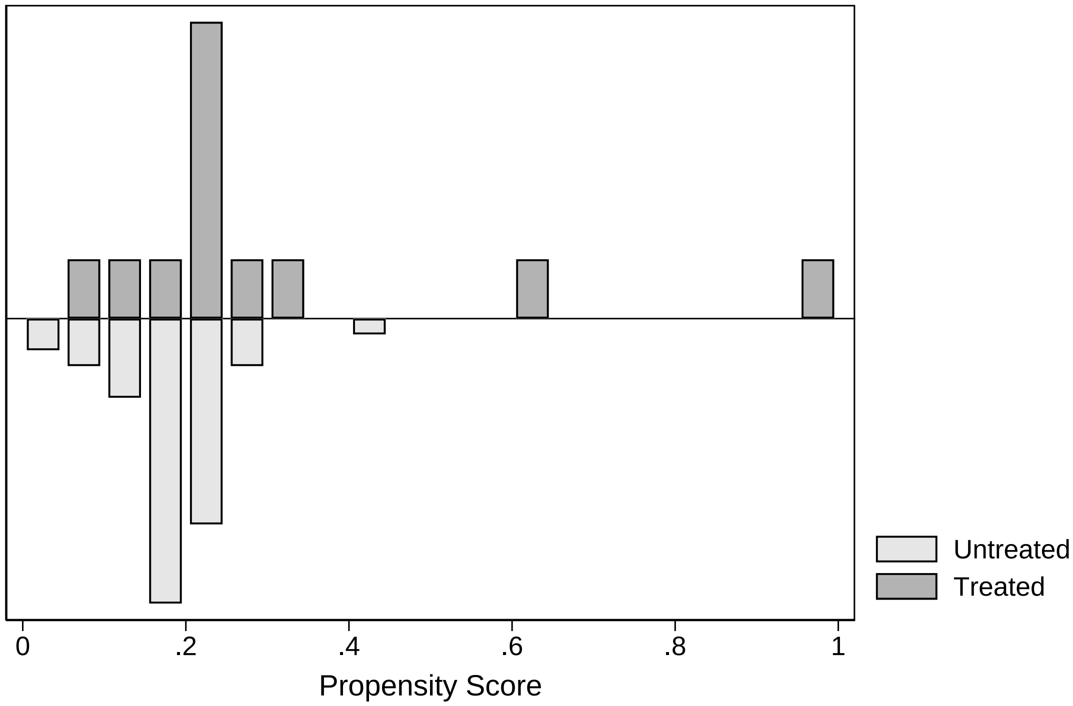

Figure 4: Propensity to fail the stress test This figure shows the estimated propensity to fail the ECB's 2014 stress test based on pre-treatment trends in size, performance and capitalization. The distribution of propensity scores of banks that have failed the stress test ("treated" banks) are represented by the dark shaded bars while that of non-failed banks ("untreated" banks) are represented by lighter shaded bars.

Table 1: Stressed banks according to the ECB's 2014 AQR stress test
This table lists the names and domiciles of the 25 banks that failed the ECB's 2014 AQR stress test and their equity shortfall expressed in billion euros. The shortfall is based on the comparison of solvency ratios in two hypothetical adverse scenarios with pre-defined minimum capitalization thresholds. See ECB (2014) for further details.

Bank name	Shortfall (bil.)	Country
Eurobank	4.63	Greece
Monte dei Paschi di Siena	4.25	Italy
National Bank of Greece	3.43	Greece
Banca Carige	1.83	Italy
Cooperative Central Bank	1,17	Cyprus
Banco Comercial Portugues	1.14	Portugal
Bank of Cyprus	0.92	Cyprus
Östereichischer Volksbanken-Verb.	0.86	Austria
Permanent TSB	0.85	Ireland
Veneto Banca	0.71	Italy
Banco Popolare	0.69	Italy
Banca Popolare di Milano	0.68	Italy
Banca Populare di Vicenza	0.68	Italy
Piraeus Bank	0.66	Greece
Credito Valtellinese	0.38	Italy
Dexia	0.34	Belgium
Banca Popolare di Sondrio	0.32	Italy
Hellenic Bank	0.28	Cyprus
Münchener Hypothekenbank	0.23	Germany
AXA Bank Europe	0.2	Belgium
C.R.H.	0.25	France
Banco Popolare Dell' Emilia Romagna	0.13	Italy
Nova Ljublyanska	0.03	Slovenia
Liberbank	0.03	Spain
Nova Kreditna	0.03	Slovenia

Table 2: Summary statistics
This table shows summary statistics for my European sample banks for variables related to material risk takers over the period 2014-2018 and for variables related to the bank as a whole over the period 2012 to 2018. Refer to Appendix Table A.1 for variable definitions.

	N	Average	S.E.	p25	Median	p75
Material risk taker variables						
Av. pay non-MB	236	12.205	0.645	11.841	12.142	12.488
Av. pay MB	222	13.261	1.027	12.444	13.258	14.021
Num. MRTs non-MB	236	5.226	1.336	4.143	4.928	6.479
Num. MRTs MB	222	2.375	1.211	1.609	2.079	3.135
Tot. pay non-MB	236	17.431	1.673	16.238	17.029	18.667
Tot. pay MB	223	15.641	1.247	14.809	15.482	16.349
Av. var. pay non-MB	203	8.887	3.690	7.936	10.191	11.156
Av. var. pay MB	206	8.997	5.117	7.685	11.038	12.948
Var. share non-MB	203	0.288	0.299	0.026	0.217	0.420
Var. share MB	206	0.366	0.465	0.011	0.192	0.579
Tot. var. pay non-MB	203	13.648	5.510	12.476	15.184	16.999
Tot. var. pay MB	206	10.935	6.127	10.017	13.408	15.144
Bank-wide variables						
Stress	399	0.373	1.080	0.000	0.000	0.000
Labor expenses	371	6.531	1.686	5.379	6.219	7.889
Number of employees	358	9.149	1.656	7.919	8.841	10.491
Size	376	11.631	1.608	10.521	11.252	12.919
Tier 1	334	0.152	0.049	0.120	0.142	0.174
Impaired loans	356	0.050	0.067	0.009	0.021	0.063
ROA	376	0.103	1.126	0.049	0.277	0.559
Equity	376	0.076	0.055	0.049	0.064	0.084
Deposits	376	0.439	0.203	0.297	0.454	0.596
Cost-to-income	374	0.652	0.233	0.530	0.620	0.722
Market focus	154	1.767	2.987	0.162	0.495	1.800
Short- ov. long-term fund.	365	4.277	9.535	0.821	1.581	3.089
Z-score	376	131.425	649.880	9.840	31.236	49.408

Table 3: Bank stress and the number and compensation of material risk takers in- and outside of the management board

This table reports estimates from regressions of the number and compensation of material risk takers (MRTs) on a measure of bank distress. The sample covers European banks between 2014 and 2018 and has a bank-year structure. The dependent variables is either the average compensation of MRTs outside (column 1) and inside (column 2) the management board or the number of MRTs outside (column 3) or inside (column 4) the management board or their respective total compensation (columns 5 and 6). The main independent variable is a bank's relative equity shortfall in the ECB's 2014 AQR stress test interacted with year indicators. Columns 5 and 6 include controls for the number of MRTs in the specific sub-group considered. All columns include bank and year fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Av. pay non-MB	Av. pay MB	Num. MRTs non-MB	Num. MRTs MB	Tot. pay non-MB	Tot. pay MB
	(1)	(2)	(3)	(4)	(5)	(6)
15 × Stress	0.057**	0.033	-0.160***	-0.059*	-0.004	0.005
	(0.022)	(0.034)	(0.040)	(0.034)	(0.016)	(0.026)
16 × Stress	0.072***	0.036	-0.149***	-0.053**	0.016	0.007
	(0.018)	(0.049)	(0.044)	(0.026)	(0.012)	(0.051)
17 × Stress	0.014	0.009	-0.044	-0.042	-0.003	-0.012
	(0.040)	(0.048)	(0.102)	(0.042)	(0.012)	(0.051)
18 × Stress	0.021	0.024	-0.089	-0.091**	-0.013	-0.032
	(0.045)	(0.049)	(0.105)	(0.045)	(0.019)	(0.054)
$\text{MRTs}_t^{\text{non-MB}}$					0.606***	
					(0.026)	
$\text{MRTs}_t^{\text{MB}}$						0.430***
						(0.093)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
C-group-year FE	No	No	No	No	No	No
Observations	236	222	236	222	236	222
Adjusted R^2	0.922	0.856	0.923	0.847	0.995	0.936
Number of banks	57	55	57	55	57	55
Number of stressed banks	12	12	12	12	12	12
Mean dep. var.	12.205	13.261	5.226	2.375	17.431	15.632
Clustering	Bank	Bank	Bank	Bank	Bank	Bank
Sample period	2014-2018	2014-2018	2014-2018	2014-2018	2014-2018	2014-2018

Table 4: Bank stress and variable compensation of material risk takers in- and outside of the management board

This table reports estimates from regressions of the variable compensation of material risk takers (MRTs) on a measure of bank distress. The sample covers European banks between 2014 and 2018 and has a bank-year structure. The dependent variables is either the average variable compensation of MRTs outside (column 1) and inside (column 2) the management board or the ratio of variable to fixed compensation of MRTs outside (column 3) or inside (column 4) the management board or their respective total variable compensation (columns 5 and 6). The main independent variable is a bank's relative equity shortfall in the ECB's 2014 AQR stress test interacted with year indicators. Columns 5 and 6 include controls for the number of MRTs in the specific sub-group considered. All columns include bank and year fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Av. var. pay non-MB	Av. var. pay MB	Var. share non-MB	Var. share MB	Tot. var. pay non-MB	Tot. var. pay MB
	(1)	(2)	(3)	(4)	(5)	(6)
15 × Stress	-0.283	-0.163	0.013*	-0.020	-0.524	-0.217
	(0.361)	(0.162)	(0.007)	(0.020)	(0.526)	(0.198)
16 × Stress	0.396	-0.002	0.024**	-0.011	0.270	0.033
	(0.245)	(1.213)	(0.012)	(0.026)	(0.302)	(1.695)
17 × Stress	0.533**	0.483	0.025**	-0.002	0.447	0.803
	(0.236)	(0.500)	(0.010)	(0.019)	(0.274)	(0.744)
18 × Stress	0.427	0.183	0.025**	-0.006	0.400	0.223
	(0.262)	(1.169)	(0.011)	(0.026)	(0.312)	(1.636)
$\text{MRTs}_t^{\text{non-MB}}$					0.325	
					(0.957)	
$\text{MRTs}_t^{\text{MB}}$						-0.013
						(1.075)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
C-group-year FE	No	No	No	No	No	No
Observations	203	206	203	206	203	206
Adjusted R^2	0.710	0.624	0.832	0.823	0.748	0.602
Number of banks	49	52	49	52	49	52
Number of stressed banks	11	11	11	11	11	11
Mean dep. var.	8.887	8.997	0.288	0.366	13.648	10.935
Clustering	Bank	Bank	Bank	Bank	Bank	Bank
Sample period	2014-2018	2014-2018	2014-2018	2014-2018	2014-2018	2014-2018

Table 5: Bank stress and the number of material risk takers outside of the management board
This table reports estimates from regressions of the number of material risk taker (MRTs) outside the management board on a measure of bank distress. The sample covers European banks between 2014 and 2018 and has a bank-year structure. The main independent variable is a bank's relative equity shortfall in the ECB's 2014 AQR stress test interacted with year indicators. In column 1, I use bank and year fixed effects and add several bank balance sheet controls. In columns 2 and 3, I substitute year fixed effects with interacted country-group times year fixed effects. In column 3, I interact a bank's relative equity shortfall with a pooled indicator that is equal to one starting in 2015 instead of using year indicators. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Num. MRTs non-MB		
	(1)	(2)	(3)
15 × Stress	-0.136***	-0.160***	
	(0.041)	(0.043)	
16 × Stress	-0.098**	-0.124***	
	(0.043)	(0.047)	
17 × Stress	-0.087*	-0.059	
	(0.045)	(0.100)	
18 × Stress	-0.155***	-0.127	
	(0.046)	(0.101)	
Post-14 × Stress			-0.118*
			(0.066)
Size	-0.386		
	(0.459)		
Tier 1	1.417		
	(2.156)		
Imp. Loans	-0.844		
	(1.118)		
ROA	-2.923		
	(4.201)		
ROA_t	-3.487		
	(7.858)		
Equity	-6.457		
	(5.014)		
Deposits	0.875		
	(1.051)		
Cost-to-inc.	0.175		
	(0.194)		
Bank FE	Yes	Yes	Yes
Year FE	Yes	No	No
C-group-year FE	No	Yes	Yes
Observations	187	236	236
Adjusted R^2	0.941	0.923	0.923
Number of banks	50	57	57
Number of stressed banks	11	12	12
Mean dep. var.	5.429	5.226	5.226
Clustering	Bank	Bank	Bank
Sample period	2014-2018	2014-2018	2014-2018

Table 6: Bank stress, the number of material risk takers, and bank business models
This table reports estimates from regressions of the number of material risk taker (MRTs) outside the management board on a measure of bank distress. The sample covers European banks between 2014 and 2018 and has a bank-year structure. The main independent variable is a bank's relative equity shortfall in the ECB's 2014 AQR stress test interacted with an indicator equal to one starting in 2015 and binary indicators for pre-stress test characteristics. In column 1, high tier-1 represents banks with a risk-adjusted tier 1 capital ratio above the 75th percentile in 2013. In column 2, high-equity represents banks with a ratio of equity over total assets above the 75th percentile in 2013. In column 3, high market focus represents banks with a ratio of investment banking MRTs over retail banking MRTs above the 2014-median. In column 4, high short-term over long-term represents banks with a ratio of short-term to long-term funding above the 75th percentile in 2013. In column 5, high z-score represents banks with a z-score above the 2013-median. All columns include bank fixed effects and interacted country-group times year fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Num. MRTs non-MB				
	(1)	(2)	(3)	(4)	(5)
Post-14 × Stress	-0.093	-0.178***	-0.122	-0.085	-0.127*
	(0.083)	(0.047)	(0.080)	(0.097)	(0.063)
Post-14 × High tier-1	-0.338				
	(0.264)				
Post-14 × High equity		-0.350*			
		(0.202)			
Post-14 × High market focus			0.105		
			(0.344)		
Post-14 × High st ov. lt				-0.101	
				(0.237)	
Post-14 × High z-score					-0.207
					(0.246)
Post-14 × Stress × High tier-1	-0.862**				
	(0.345)				
Post-14 × Stress × High equity		0.454***			
		(0.075)			
Post-14 × Stress × High market focus			0.236		
			(0.267)		
Post-14 × Stress × High st ov. lt				-0.036	
				(0.123)	
Post-14 × Stress × High z-score					2.267*
					(1.166)
Bank FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No
C-group-year FE	Yes	Yes	Yes	Yes	Yes
Observations	210	236	130	229	236
Adjusted R^2	0.924	0.927	0.895	0.922	0.923
Number of banks	51	57	28	55	57
Number of stressed banks	11	12	10	12	12
Mean dep. var.	5.259	5.226	5.437	5.244	5.226
Clustering	Bank	Bank	Bank	Bank	Bank
Sample period	2014-2018	2014-2018	2014-2018	2014-2018	2014-2018

Table 7: Robustness: covariate balance, binary stress, and bank-wide labor adjustment
This table reports estimates from regressions of the number of material risk taker (MRTs) outside the management board, bank-wide labor expenses and the total number of bank employees on a measure of bank distress. The sample covers European banks between 2014 and 2018 and has a bank-year structure. In columns 1, 3, and 4 the main independent variable is a bank's relative equity shortfall in the ECB's 2014 AQR stress test interacted year indicators. In column 2, the main independent variable is an indicator equal to one for banks with a non-zero equity shortfall in the AQR stress test. In column 1, I exclude banks with a very high or very low propensity of failing the AQR stress test based pre-AQR trends in size, performance, and capitalization. In columns 1 and 2, the dependent variable is the number of MRTs outside the management board. In column 3, the dependent variable is aggregate bank-wide labor expenses and in column 4 the dependent variable is a bank's total number of employees. All columns include bank fixed effects and interacted country-group times year fixed effects. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Num. MRTs non-MB		Lab. exp.	Num. empl
	(1)	(2)	(3)	(4)
15 × Stress	-0.176***		-0.030***	-0.022
	(0.045)		(0.011)	(0.018)
16 × Stress	-0.121**		-0.034	-0.048**
	(0.059)		(0.022)	(0.019)
17 × Stress	-0.027		-0.038**	-0.046***
	(0.144)		(0.015)	(0.016)
18 × Stress	-0.110		-0.055**	-0.070**
	(0.142)		(0.024)	(0.027)
15 × Stress (binary)		0.113		
		(0.264)		
16 × Stress (binary)		0.484		
		(0.416)		
17 × Stress (binary)		0.263		
		(0.346)		
18 × Stress (binary)		0.059		
		(0.327)		
Bank FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	No
C-group-year FE	Yes	Yes	Yes	Yes
Observations	220	236	258	252
Adjusted R^2	0.930	0.922	0.996	0.992
Number of banks	53	57	55	54
Number of stressed banks	10	12	12	11
Mean dep. var.	5.226	5.226	6.550	9.145
Clustering	Bank	Bank	Bank	Bank
Sample period	2014-2018	2014-2018	2014-2018	2014-2018

Appendix for
"Managerial Compensation in Stressed Banks: Evidence from the ECB's AQR Stress Test"

A Details on the MRT database

According to the template defined in EBA guideline EBA/GL/2014/08, MRTs in the MB are classified into the business units i) *the management body in its supervisory function* or ii) *the management body in its management function.* The remaining MRTs are classified into the business units iii) *investment banking*, iv) *retail banking*, v) *asset management*, vi) *corporate functions* (such as HR and IT), vii) *independent control functions* (such as risk management, compliance and internal audit), and the residual category viii) *all others.* The information in our database is hand-collected from a wide variety of documents such as annual reports, compensation reports, or CRR reports. Business units are manually classified whenever banks do not use the business unit names from the EBA template.

Note that the database used in this study is slightly modified relative to Stieglitz and Wagner (2020). Because I only differentiate between MRTs in- and outside the MB, I also count MRTs towards the group of non-MB MRTs that do not fit into any of the EBA categories, such as MRTs in regional business units.

Table A.1: Definition of variables

Variable	Databases	Definition
Material risk taker variables		
Av. pay non-MB	Hand-collected	Natural logarithm of the average pay of material risk takers outside of the management board.
Av. pay MB	Hand-collected	Natural logarithm of the average pay of material risk takers in the management board.
Num. MRTs non-MB	Hand-collected	Natural logarithm of the number of material risk takers outside of the management board.
Num. MRTs MB	Hand-collected	Natural logarithm of the number of material risk takers in the management board.
Tot. pay non-MB	Hand-collected	Natural logarithm of the total pay of material risk takers outside of the management board.
Tot. pay MB	Hand-collected	Natural logarithm of the total pay of material risk takers in the management board.
Av. var. pay non-MB	Hand-collected	Natural logarithm of (one plus) the average variable pay of material risk takers outside of the management board.
Av. var. pay MB	Hand-collected	Natural logarithm of (one plus) the average variable pay of material risk takers in the management board.
Var. share non-MB	Hand-collected	Ratio of variable over fixed pay for material risk takers outside of the management board.
Var. share MB	Hand-collected	Ratio of variable over fixed pay for material risk takers in the management board.
Tot. var. pay non-MB	Hand-collected	Natural logarithm of (one plus) the total variable pay of material risk takers outside of the management board.
Tot. var. pay MB	Hand-collected	Natural logarithm of (one plus) the total variable pay of material risk takers in the management board.
Bank-wide variables		
Stress	ECB (2014), Bankscope and Bankfocus	Ratio (in %) of a bank's equity shortfall in the ECB's 2014 AQR stress test relative to total assets as of end-2013.
Labor expenses	Bankscope and Bankfocus	Natural logarithm of labor expenses comprised of wage- and non-wage labor costs of all bank-wide employees.
Number of employees	Bankscope and Bankfocus	Natural logarithm of the total number of employees.
Size	Bankscope and Bankfocus	Natural logarithm of total assets.
Tier 1	Bankscope and Bankfocus	Regulatory tier 1 capital ratio defined as tier 1 capital over risk-weighted assets.
Impaired loans	Bankscope and Bankfocus	Ratio of impaired loans over total assets.
ROA	Bankscope and Bankfocus	Ratio of net income over total assets.
Equity	Bankscope and Bankfocus	Ratio of equity over total assets.
Deposits	Bankscope and Bankfocus	Ratio of total customer deposits over total assets.
Cost-to-income	Bankscope and Bankfocus	Ratio of non-interest expenses over the sum of net interest income and other operating income.
Market focus	Bankscope and Bankfocus	Ratio of material risk takers in the investment banking business unit over material risk takers in the retail banking business unit.

(Continued)

Table A.1: – *Continued*

Short- ov. long-term funding	Bankscope and Bankfocus	The ratio of the sum of deposits from banks, repos and cash collateral, and other deposits and short-term borrowings over senior debt maturing after one year, subordinated borrowing, other funding, and shares and hybrid capital accounted for as debt.
Z-score	Bankscope and Bankfocus	The ratio of equity and net income over the standard deviation of ROA.

Table A.2: Robustness: euro area banks only
This table reports estimates from regressions of the number of material risk taker (MRTs) outside the management board on a measure of bank distress. The sample covers euro area banks between 2014 and 2018 and has a bank-year structure. The main independent variable is a bank's relative equity shortfall in the ECB's 2014 AQR stress test interacted with year indicators. Robust standard errors are clustered at the level of banks and displayed in brackets below parameter estimates. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. Refer to Appendix Table A.1 for variable definitions.

Dependent variable:	Num. MRTs non-MB
	(1)
15 × Stress	-0.160***
	(0.043)
16 × Stress	-0.124***
	(0.046)
17 × Stress	-0.059
	(0.100)
18 × Stress	-0.127
	(0.100)
Bank FE	Yes
Year FE	No
C-group-year FE	Yes
Observations	208
Adjusted R^2	0.914
Number of banks	49
Number of stressed banks	12
Mean dep. var.	5.103
Clustering	Bank
Sample period	2014-2018