Effects of regulatory policies on bank-specific risk and financial stability

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List of papers

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    * Melina Ludolph: Methodology, Validation, Formal analysis, Writing - original draft, Writing - review & editing, Visualization
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Abstract

This thesis comprises three independent essays evaluating the impact of different regulatory policies on bank risk and/or financial stability. First, we examine the effects of capital regulation on the link between bank size and volatility. Our panel data analysis reveals that more stringent capital regulation weakens the size-volatility nexus. Hence, large banks show, ceteris paribus, lower loan portfolio volatility when facing more stringent capital regulation. According to the granularity concept, that can increase macroeconomic stability. Next, I evaluate if MiFID II reduced the early information disclosure on analyst recommendation changes to selected investors - so-called tipping. I find absolute returns and turnover rise significantly on the day preceding the up- or downgrade release before and after MiFID II became law. Given that stock prices move further in the revision direction on publication day, selected investors continue to profit from an informational advantage, notwithstanding the regulatory change. That is likely harmful to the financial market overall. Lastly, I examine the impact of issuing contingent convertible (CoCo) bonds that qualify as regulatory additional tier 1 (AT1) capital on bank risk. My treatment effects analysis reveals that issuing AT1 CoCo bonds results in significantly higher risk-taking one to three years after the issuance. That is in line with previous theoretical studies suggesting that regulators have stripped CoCo bonds of their potential to strengthen the banks' capital bases.

JEL-Classification: E32, G14, G18, G21, G23, G24, G28, G32, G38

Keywords: Banking and financial regulation, capital regulation, bank risk, financial stability, bank size, Basel III, volatility, tipping, analyst recommendations, MiFID II, CoCo bonds, AT1 capital

Zusammenfassung

Diese Arbeit umfasst drei unabhängige Aufsätze, welche die Auswirkungen verschiedener regulatorischer Maßnahmen auf das Bankenrisiko und/oder die Finanzstabilität untersuchen. Zunächst wird der Einfluss von Eigenkapitalanforderungen auf den Zusammenhang zwischen Bankgröße und Volatilität analysiert. Unsere Panel-Datenanalyse zeigt, dass strengere Eigenkapitalanforderungen den Nexus zwischen Größe und Volatilität schwächt. Große Banken haben, ceteris paribus, einen weniger volatilen Kreditbestand, wenn sie strengerer Kapitalregulierung ausgesetzt sind. Gemäß dem Granularitätskonzept kann dies ebenfalls die makroökonomische Stabilität erhöhen. Als Nächstes untersuche ich, ob MiFID II die frühzeitige Informationsweitergabe über Änderungen von Analystenempfehlungen an einzelne Anleger, genannt Tipping, reduziert hat. Die Ergebnisse zeigen, dass die absoluten Renditen und Handelsvolumina einen Tag vor Veröffentlichung einer Hoch- oder Herabstufung vor und nach Inkrafttreten von MiFID II signifikant ansteigen. Da die Aktienkurse am Veröffentlichungstag weiter steigen bzw. fallen, profitieren ausgewählte Anleger trotz der regulatorischen Änderung weiterhin von einem Informationsvorteil. Dies hat vermutlich negative Auswirkungen auf den Finanzmarkt insgesamt. Zuletzt untersuche ich wie sich die Ausgabe von Contingent Convertible (CoCo) Anleihen, die als regulatorisches zusätzliches Kernkapital (AT1) geltend gemacht werden können, auf das Bankenrisiko auswirkt. Meine Analyse zeigt, dass AT1-CoCo-Anleihen ein bis drei Jahre nach Ausgabe zu einem signifikant höheren Bankenrisiko führen. Übereinstimmend mit theoretischen Studien deutet dies darauf hin, dass CoCo-Anleihen ihr Potenzial zur Stärkung der Eigenkapitalbasis der Banken durch die regulatorischen Anforderungen genommen wurde.

JEL-Klassifikation: E32, G14, G18, G21, G23, G24, G28, G32, G38

Schlagworte: Banken- und Finanzmarktregulierung, Eigenkapitalrichtlinien, Bankenrisiko, Finanzstabilität, Bankengröße, Basel III, Volatilität, Tipping, Analystenempfehlungen, MiFID II, CoCo Bonds, zusätzliches Kernkapital

Contents

Introduction

The global financial crisis proved that lax and fragmented financial regulations facilitate risk-taking. Both contributed to the onset of the crisis and the poor performance of many banks. In response, regulators and academics have made many proposals for improved banking regulation, some of which legislators have already implemented in recent years.

This promptness in times of crisis, although arguably warranted, can easily result in ineffectiveness due to a lack of time for proper analysis, testing, and evaluation of the applied measures. Therefore, it is all the more important to closely monitor the developments associated with regulatory changes to create a foundation for evidence-based policy adjustments and improvements if necessary. In this dissertation, I analyze the impact of three regulatory reforms on loan volatility and, by implication, macroeconomic stability, financial market functioning, and bank risk, respectively. Thereby, I add to the growing number of empirical studies on the consequences of different banking regulations on risk and stability (e.g., Barth et al., 2004; Houston et al., 2010; Kroszner and Strahan, 2014; Laeven and Levine, 2009).

I focus on three regulatory measures that aim at mitigating issues typically associated with large banks that are considered detrimental from a stability point of view: moral hazards, conflicts of interest or misaligned incentives, and high leverage ratios. Thus, I also contribute to the literature on large banks and risk (e.g., Hagendorff et al., 2018; Laeven et al., 2016; Landier et al., 2017; Poghosyan and de Haan, 2012).

Large banks face a moral hazard problem due to their systemic relevance and the associated "too big to fail" problem, i.e., they do not have to fear bankruptcy as they are sure to be bailed out. Consequently, they might act less responsibly and intentionally expose themselves to excessive risks to generate higher returns. Moreover, empirical studies (e.g., Amiti and Weinstein, 2018; Buch and Neugebauer, 2011; Galaasen et al., 2020) show that the granularity concept (Gabaix, 2011, 2016) according to which idiosyncratic shocks can translate into aggregate fluctuations of credit and output if the market concentration is sufficiently high applies to the banking sector. That emphasizes the importance of addressing volatility at the level of large banks to improve macroeconomic stability. In accordance with theoretical studies suggesting better capitalization increases the "skin in the game" and, thereby, mitigates moral hazards (e.g., Acharya et al., 2016; Barth and Seckinger, 2018; Gornall and Strebulaev, 2018), a key objective of regulatory reforms since the financial crisis has been to increase banks' capital bases (Basel Committee on Banking Supervision, 2011).

Thus, Chapter 1 evaluates the impact of more stringent capital regulation on large banks. To this end, my co-author and I estimate a power law that relates the volume of a bank's loan portfolio to the volatility of loan growth. Using bank-level data for 27 advanced economies over the 2000-2014 period, we examine how cross-country differences and changes over time in capital regulations impact the link between bank size and volatility, i.e., the power law coefficient. Building on the concept of granularity and focusing on bank lending rather than assets, our analysis provides insights into the effect stricter bank capital regulation has on banks' loan portfolio volatility when moving up the bank size distribution and, thus, implicitly on macroeconomic fluctuations. Thereby, we close a gap in the literature, which focuses on the effects of capitalization on systemic risk (i.e., macroprudential) (e.g., Anginer et al., 2018; Laeven et al., 2016) or bank-level risk (i.e., microprudential) (e.g., Barth et al., 2004; Devereux et al., 2015) rather than considering the granularity effect mechanism, i.e., that microprudential regulation can mitigate volatility at an aggregate level.

The main methodological challenge pertains to the causal effect identification of changes in capital regulation over time. In particular, time-varying confounding factors could easily affect both risk/stability and regulatory reform. To address endogeneity concerns, we perform instrumental variables regressions using the percentage of years since a country has become a democracy (loosely following Beck et al., 2006), religious population shares, and legal origin (e.g., Barth et al., 2004) as instruments for changes in capital regulation.

Chapter 1 reveals, first, that more stringent capital regulation weakens the size-volatility nexus. Hence, in countries with stricter capital regulation, large banks show, ceteris paribus, lower loan portfolio volatility. Second, the effect of tighter capital requirements on the size-volatility nexus becomes more pronounced for the upper tail of the bank size distribution. That is in line with capitalization decreasing with bank size, such that larger banks tend to be more affected by increasing capital requirements. Third, in countries with higher sectoral capital buffers, the size-volatility nexus is weaker. Our findings imply that more stringent capital regulation results in more stable credit extension and, by granularity concept, lower macroeconomic volatility.

Next, large financial institutions typically provide many services to their customers (e.g., corporate banking, asset management, brokerage services, equity research). These different activities and income sources can cause conflicts of interest or misaligned incentives within the banks due to information sharing across departments with negative consequences for other market participants (e.g., Fecht et al., 2018; Mehran and Stulz, 2007). One particular consequence of information sharing is the early disclosure of analyst recommendation changes to some investors, so-called tipping (Irvine et al., 2007). Given that stock prices adjust in the direction of a recommendation change when published (e.g., Bradley et al., 2014; Michaely and Womack, 2005), knowledge about the publication time and direction of an upcoming recommendation change arguably constitute inside information. The respective empirical literature establishes the negative consequences of insider trading. These include, most notably, the disruption of the signaling function of stock prices (e.g., Beny, 2006; Cheng et al., 2006; Du and Wei, 2004).

Before the financial crisis, regulators mainly relied on self-regulatory and corporate governance measures like Chinese walls, i.e., virtual information barriers, to address conflicts of interest or misaligned incentives within large financial institutions. However, the second Markets in Financial Instruments Directive (MiFID II), in effect since January 3, 2018, introduced a change to the payment structure for equity research and sales services. That is in line with policy implications from previous studies that suggest the formerly common practice of bundling research fees and sales commission, i.e., effectively providing research free of charge, incentivizes analysts to engage in tipping (Irvine et al., 2007; Juergens and Lindsey, 2009). Chapter 2 examines if this change introduced by MiFID II stopped the early information disclosure about analyst revisions. Based on 2,712 recommendation changes published by European banks and brokers from 04'2016-09'2019, I test the null hypothesis of no mean effect on trading over the three days leading up to and including the day of a recommendation change publication for the period before and after MiFID II became law, separately. That allows me to determine if the regulatory change stopped early trading activities related to the information disclosures. Thereby, I provide the first empirical evaluation of the effectiveness of MiFID II in reducing tipping. Further, my analysis focuses on European banks and brokers, which adds to the literature on tipping based on US data (e.g., Christophe et al., 2010; Hendershott et al., 2015; Juergens and Lindsey, 2009).

Chapter 2 also makes a methodological contribution to the empirical finance literature as it provides a conceptualization of the estimated treatment effects that allows for a structured discussion of the underlying assumptions versus varying event study frameworks in the past (e.g., Altınkılıç and Hansen, 2009; Chen and Cheng, 2006; Kadan et al., 2018). Moreover, I apply a comprehensive set of screening criteria, exceeding those used in previous studies on tipping (e.g., Irvine et al., 2007; Juergens and Lindsey, 2009; Kadan et al., 2018) to control for confounding factors. That allows me to isolate the causal effect information disclosure about a recommendation change has on trading from other firm-specific and market-wide news.

I find absolute returns and turnover rise significantly on the day preceding the release of an up- or downgrade before and after MiFID II became law. The analysis also reveals that stock prices move further in the revision direction on publication day. That suggests that selected investors continue to profit from an informational advantage likely harmful to the financial market overall. However, the extent of tipping taking place appears smaller in the European context than suggested by previous research based on US data. Nonetheless, the regulatory change does not effectively resolve the misaligned incentives that result from the different banking activities of financial institutions.

Lastly, high leverage ratios facilitate bank growth and allow financial firms to engage in excessive risk-taking (Bhagat et al., 2015), another contributing factor to the severity of the financial crisis (Günther, 2013). Increasing the leverage ratio, i.e., substituting debt for equity, decreases the total costs of capital, implies tax benefits, and boosts equity returns. However, there is a trade-off between these advantages and bankruptcy risk. When asset values depreciated, highly leveraged banks faced severe difficulties raising additional capital needed to meet their debt obligations. Financial instruments that were supposed to absorb losses proved ineffective, and the regulatory system lacked mechanisms to ensure that subordinated creditors and preferential shareholders bear their share of the costs rather than the public (Heldt, 2013).

Therefore, in addition to increasing the required quantity of bank capital, the

Basel Committee on Banking Supervision (2011) also aims at improving its quality to address the risks associated with high leverage ratios. To this end, Basel III and its European implementation Capital Requirement Directive IV (CRD IV)/ Capital Requirement Regulation (CRR) prescribe a particular design for so-called contingent convertible (CoCo) bonds to qualify as additional tier 1 (AT1) capital. A CoCo bond is a subordinated debt security that converts from debt into equity, or it is written down as soon as a predetermined trigger event occurs. These bonds can absorb losses early and contribute to bank stability (e.g., Barucci and Del Viva, 2012). Yet, this crucially depends on the security design (e.g., Maes and Schoutens, 2012). Based on a large body of theoretical literature (e.g., Allen and Tang, 2016; Avdjiev et al., 2017; Berg and Kaserer, 2015), I hypothesize that the Basel III and CRD IV/CRR requirements result in CoCo bonds that increase bank-level risk by allowing shareholders to shift losses to bondholders without having to share profits. I perform a difference-in-differences analysis for staggered treatment adoption with matching based on a statistical distance measure using bank-level data for 251 publicly traded European banks and 61 CoCo issues from 2008-2018 to analyze the impact of issuing CoCo bonds on bank-level risk. Thereby, Chapter 3 crucially contributes to the body of empirical research on the risk-effect of CoCos, which is scarce and limited to risk anticipation and perception rather than the actual realization at the bank level over time.

To identify the effect issuing CoCo bonds has on bank risk, I most closely follow Callaway and Sant'Anna (2020), Dettmann et al. (2020), and Imai et al. (2020) and apply a matching-based difference-in-differences approach for staggered treatment adoption. Thereby, I can control for selection bias and relative time dynamics. It also accounts for calendar time effects in the outcome variable and on treatment selection.

Estimation results in Chapter 3 show that issuing CoCo bonds that meet the regulatory criteria for additional tier 1 capital results in significantly higher risk-taking one to three years after the issuance versus the risk level of financial institutions not issuing AT1 CoCos. Rather than having a net negative impact, issuing CoCos seems to impede a positive time trend towards higher bank stability. The findings substantiate the hypothesis that currently outstanding CoCo bonds create incentives for excessive risk-taking, which raises serious concerns regarding the regulatory design requirements for AT1-qualifying CoCos.

Chapter 1

The nexus between loan portfolio size and volatility: Does bank capital regulation matter?

1.1 Motivation

This paper analyzes how bank capital regulation impacts loan growth volatility at the bank level. More precisely, we address the question of how differences in regulatory stringency between countries affect the relation between bank size, as measured by total net loans, and the volatility of loan growth. If capital regulation results in large banks becoming more stable because of, e.g., reduced moral hazards or less pro-cyclical lending, then not only may bank-level fluctuations be reduced, but also macroeconomic volatility.

Following the literature on firm or country size and volatility,[1] we estimate a power law that links bank size with the volatility of the bank's loan growth rates. We measure bank size and volatility as total net loans and the standard deviation of loan growth, respectively, then also match balance sheet data for the 2000-2014 period to information on bank regulations obtained from the World Bank Banking Supervision Survey (Barth et al., 2013) and the IBRN Prudential Instruments Database (Cerutti et al., 2017a). This results in a panel of 46,727 bank-year observations covering 27 advanced economies. The effect of bank capital regulation on the power law coefficient shows whether that regulation weakens or strengthens the link between bank size and volatility. Hence, it indicates whether

[1] For a summary, see Gabaix (2016).

capital stringency supports loss absorption capacity, mitigates pro-cyclical lending, affects moral hazards, or impedes diversification as banks become larger.[2] Thereby, we analyze whether more stringent capital regulation is associated with large banks' loan portfolios being more or less volatile.

To single out the effect of capital regulation on the nexus between loan portfolio size and volatility, we must control for time-varying confounding factors at both the bank and the country level. We follow the literature (e.g., Lambert et al., 2017) and control for standard bank-level characteristics that might affect loan growth volatility. Moreover, we include country-and-time fixed effects in all regressions. Importantly, to account for the potential endogeneity of capital regulation and bank-level loan volatility, we run instrumental variables (IV) regressions. We use three instrumental variables to single out cross-country differences and temporal changes in capital regulations that do not directly relate to bank-level loan volatility, namely the percentage of years since a country has become a democracy, religious population shares, and legal origin. Finally, to mitigate simultaneity concerns, we add interaction terms between bank size and time-varying country-specific economic, institutional and banking sector conditions that might explain part of the variation in the nexus between loan portfolio size and volatility.

Our analysis yields three main results. First, more stringent capital regulation coincides with a weaker size-volatility nexus, possibly due to fewer moral hazards and less pro-cyclical lending of large banks reflected in a less volatile loan portfolio. While loan volatility increases by 3% with a one standard deviation increase in bank size in countries with the most lenient capital regulatory stringency, it declines by 14% in those countries with the tightest standards on bank capital. Second, the tightening of capital requirements according to the implementation of the Basel-agreements produces the most robust results across a large set of model specifications and sanity tests. The magnitude of the volatility-reducing effect of the tightening of capital requirements becomes larger when considering the upper tail of the bank size distribution. Since capitalization decreases with bank size, the largest banks are likely to be more affected by higher capital requirements than the smaller ones. Third, we also find evidence for higher sectoral capital buffers to reduce the size-volatility nexus in banking and, hence, the relative volatility of large banks.

[2] Note that we do not focus on the direct effect that capital regulation has on all banks alike, small and large. Instead, we focus on the impact it has on the skewness of the volatility distribution, which is a function of size.

Since the global financial crisis, consolidation in the banking sector has led to increasing concentration (BIS, 2018; ECB, 2017). The share of total assets held by the five largest banks grew in several advanced economies. Moreover, bank sizes relative to GDP have been on the rise in many countries (Hagendorff et al., 2018). The continuing rise of big banks is fostering a renewed interest in bank size distributions and the implications of bank-specific shocks for macroeconomic volatility (Fernholz and Koch, 2017). As demonstrated by Amiti and Weinstein (2018), Buch and Neugebauer (2011) and Galaasen et al. (2020), macroeconomic fluctuations in investment or output can be explained by bank-level credit volatility to a significant degree, if market concentration is high; that is, if a few large players dominate the market. Consequently, our findings on the reduction in the volatility of large banks in response to stricter capital regulation have implications beyond the micro-level: more stringent capital regulation does not only make individual (large) banks' loan portfolios less volatile, but can also reduce macroeconomic volatility. The key mechanism is that bank-level volatility of loan growth translates into aggregate fluctuations in credit and output if bank concentration is very high (Bremus et al., 2018).[3] Yet, granular effects do not have to be strongest for the most concentrated markets but are also fostered if average volatility is high in a given market and if the largest players are relatively volatile (Di Giovanni and Levchenko, 2012). Thus, an analysis of the volatility of loans across banks of different sizes is important for better understanding the linkages between fluctuations at the bank level and macroeconomic volatility.

According to the granularity mechanism, macroeconomic volatility can be reduced through two channels. On the one hand, policies that mitigate bank concentration dampen the transmission of bank-level volatility to the macroeconomy. On the other hand, microprudential regulation might induce less excessive risk-taking, better diversification, and higher loss absorption capacity at the level of (large) banks and, thereby, also mitigate volatility at an aggregate level. Hence, it is crucial to understand which regulatory measures are effective in reducing volatility, especially at the level of the large banks, which dominate the credit market.

The remainder of this paper is organized as follows. Section 1.2 reviews the

[3]According to the concept of granularity, idiosyncratic shocks to firms can translate into macroeconomic fluctuations if market concentration is sufficiently high (Gabaix, 2011; Di Giovanni and Levchenko, 2012; Di Giovanni et al., 2014). Gabaix (2011) shows that Gibrat's law (i.e., that shocks to firm size are random and independent of a firm's absolute size) results in the firm size distribution following a power law. If this distribution is fat-tailed, the Central Limit Theorem ceases to hold and individual shocks to large firms do not average out in the aggregate.

related literature. Section 1.3 motivates and introduces the empirical model, discusses the selected regulatory indicators as well as the size and volatility measure, and describes our data. Section 1.4 presents the empirical results, before Section 1.5 concludes.

1.2 Related literature

Our analysis is closely related to the growing literature on large banks and their effect on the stability of the financial system as well as the real economy. There is a comprehensive body of literature on the relation between bank size and volatility or risk as well as on the effects of capital regulations on banks' risk-taking behavior.

1.2.1 Bank size and volatility or risk

The literature offers contrasting views on the effects that the size of a bank has on its volatility or risk profile. On the one hand, large banks that are "too big to fail" face moral hazards such that they engage in risky behavior, knowing that governments must bail them out in case of distress to prevent an economic meltdown. On the other hand, some studies indicate that large banks are better diversified and, thus, more stable.[4]

Stern and Feldman (2004) trace the roots of the moral hazard problem back to the expectations of large uninsured creditors of banks. If they can assume protection against any losses through government support, large banks will take on excessive risks due to suspended market discipline and distorted incentives. Laeven et al. (2016) find empirical evidence supporting this hypothesis by analyzing, in addition to systemic risk, the stock return performance of banks during the financial crisis. They show that banks with assets greater than 50 billion US dollars are riskier than small ones and that they tend to have a more fragile business model due to lower capitalization and less stable funding. Bhagat et al. (2015) confirm the positive correlation between bank size and risk, both using the z-score and the volatility of stock returns. In addition, Gropp et al. (2014) find

[4]In addition to the z-score as a common risk measure, the literature also assesses effects of moral hazard and bank size on the volatility of earnings (Gropp et al., 2014; Laeven and Levine, 2009), on the volatility of equity returns or return on assets (Bhagat et al., 2015; Laeven and Levine, 2009), and on the volatility of net interest margins (Houston et al., 2010).

that government guarantees promote banks' risk-taking, suggesting that implicitly insured large banks face moral hazards. The empirical results of Hagendorff et al. (2018) point in the same direction. As banks grow in size relative to GDP, their tail risk exposure increases, which can be partly linked to government guarantees. While Hagendorff et al. (2018) aim at identifying drivers for the largest banks taking on inordinate risks, as measured by expected shortfall, this paper aims at estimating the effect of capital regulation on the link between loan portfolio size and volatility, i.e. on the distribution of loan growth volatility.[5]

In contrast, Stever (2007) provides circumstantial evidence that small banks exhibit more risky loan portfolios as they lack the ability to properly diversify due to their smaller volume of total loans, due to less diversity with respect to the borrower type, since they cannot extend credit to large borrowers, and due to geographic restrictions.[6] Tschoegl (1983) considers a firm as a portfolio of projects that can be correlated implying that larger firms consist of more projects and, therefore, are better diversified. He finds that volatility of asset growth decreases with the size of a bank. While Landier et al. (2017) find larger banks to be less volatile as well, they hint at the relationship between bank size and volatility being rather weak.[7] Based on US data for the 2004-2009 period, Poghosyan and de Haan (2012) find a negative correlation between bank size and earnings volatility. However, this negative correlation becomes weaker the higher the banking sector concentration is.

Overall, the literature on the nexus between bank size and volatility or risk presents mixed results. While some studies find a negative relation, others show that larger banks, especially the largest ones, tend to be more risky. These seemingly contradicting results partly arise due to differences in how bank risk or volatility and size are measured as well as due to non-linearities in the link between size and risk or volatility.

In contrast to previous studies, this paper does *not* test whether the diversification or the moral hazard hypothesis is valid. Rather, we aim at evaluating

[5]Note, while Hagendorff et al. (2018) also present findings on the effect of a regulatory measure on risk-taking by large banks, their research question and methodology differ substantially from ours. They focus on estimating the direct effect that relative bank size (measured by liabilities to GDP) has on extreme bank risk exposures, i.e., on the tail of the risk distribution.

[6]For the case of non-financial firms, Herskovic et al. (2017) also point to a negative correlation between firm size and volatility as large firms have a broader customer base that improves diversification.

[7]According to their estimation, multiplying bank size by a factor of 1,000 results in a mere reduction of loan growth volatility of about 3.8 percentage points.

the strength of the link between bank size and volatility as a function of the stringency of bank capital regulation. Thus, we contribute to the literature by focusing on the implications of regulatory measures on the nexus between bank size and volatility.

1.2.2 Capital regulation and risk-taking

In response to tightened regulation following the crisis, particularly for large banks, a growing literature deals with the impact of bank capital regulation on banks' risk-taking incentives. Bhagat et al. (2015) show that financial firms engage in excessive risk-taking mainly through increased leverage. Therefore, supervision should focus on capital requirements. In line with this finding, different theoretical studies support that better capitalization raises the *skin in the game*, such that banks reduce risk-taking (e.g., Acharya et al., 2016; Barth and Seckinger, 2018; Gornall and Strebulaev, 2018; Holmstrom and Tirole, 1997).

Still, even if stricter capital requirements can increase skin in the game as well as loss absorption capacity (Admati and Hellwig, 2013), subsequent empirical studies show that banks (Devereux et al., 2015), particularly large ones (Dautovic, 2019), increase the riskiness of their asset portfolios in response to higher capital requirements. Similarly, Agoraki et al. (2011) find that the risk-reduction can be considerably weaker, if not reversed, for banks with market power. The estimation results of Jiménez et al. (2017) also point towards increasing credit portfolio risk and search for yield after increases in capital requirements. Based on historical data, Jordà et al. (2020) present evidence that higher capital ratios are not related to a reduced probability of systemic banking crises, thus questioning the skin in the game argument. However, they find the recovery after a crisis is faster.

Summing up, several studies focus on the effects of bank capital regulation on risk, also taking differences in bank characteristics into account. However, empirical evidence on how bank capital regulation affects the link between the size of a bank and the volatility of its loans is so far lacking. Hence, the question of whether bank capital regulations result in banks becoming less volatile - e.g., due to fewer moral hazards, better loss absorption capacity, or weaker pro-cyclical lending behavior - as we move up the bank size distribution remains.

1.3 Methodology and data

Bank-specific volatility does not just depend on a bank's size but also on a number of other variables. Those covariates can either be bank-specific, country-specific, or capture the global economic development. We exploit differences in the stance of capital regulation across countries and time to identify the effect of different regulatory measures on the size-volatility nexus in banking. Before analyzing the effectiveness of capital regulation at reducing volatility as banks grow in size, we briefly lay out the theoretical foundations of our analysis.

1.3.1 A power law linking bank size and volatility

Gabaix (2016) argues that the volatility of firm growth - in our case banks - varies with size. He suggests that not only does the distribution of sizes follow a power law, but also the volatility of growth rates as a function of the size: $sd(growth) = k(size)^{\alpha}$. Log linearizing yields

$$ln(sd(growth)) = ln(k) + \alpha ln(size), \tag{1.1}$$

where $sd(growth)$ is the standard deviation of banks' growth rates, *size* is bank size measured by total loans, k is a constant, and α is a parameter governing the relation between bank size and volatility.

If the volatility of the growth rate is independent of bank size, the observed power law coefficient will be $\alpha = 0$. If volatility increases (decreases) with bank size, α will be positive (negative). According to the moral hazard hypothesis, the power law coefficient should be positive ($\alpha > 0$), implying that larger banks take on higher risks, which results in a higher volatility of their growth rates compared to those of smaller banks. In contrast, a power law coefficient of $\alpha \in [-0.5, 0)$ suggesting that larger banks are better diversified and, hence, have more stable growth rates.[8]

Thus, regulations that increase moral hazard or reduce diversification of large banks, in particular, should increase the parameter α. In turn, regulatory measures that promote loss absorption capacity or reduce moral hazards of large

[8]Note that the power law coefficient has a lower bound since the Central Limit Theorem holds for $\alpha = -0.5$. Then, volatility converges toward zero as size goes to infinity given the sample of bank sizes is a sequence of independent and identically distributed random variables drawn from a distribution with $E[size_i] = \mu$ and finite variance. Hence, full diversification could be achieved.

banks, thereby making loan growth less volatile, will reduce the parameter α. Note that regulatory measures that affect all banks similarly, small and large ones, will have no effect on the nexus between bank size and volatility, i.e., the parameter α. Instead of affecting the skewness of the volatility distribution depending on size, these measures would shift the entire distribution.

1.3.2 Model specification

For our model specification, we follow Beck et al. (2013), who use a similar approach to estimate the impact of cross-country differences in market and institutional features on the link between bank competition and stability. Building on the power law linking bank size and volatility described in equation (1.1), we specify our model as follows:

$$ln(sd(growth_{i,j,t})) = ln(k) + \alpha ln(size_{i,j,t}) + \gamma X_{i,j,t-1} + \eta_{j,t} + \epsilon_{i,j,t} \quad (1.2)$$

with i being the index for banks, j indicating the country, and t the year. In order to analyze how regulatory measures affect the link between bank size and volatility, we assume that time-varying and county-specific regulations explain part of the heterogeneity in the conditional correlations between bank size and volatility, then we impose the following structure on the power law parameter: $\alpha(Z_{j,t}) = \theta_0 + \theta_1 Z_{j,t}$, with $Z_{j,t}$ being the time-varying, country-specific capital regulation. Then, equation (1.2) becomes

$$ln(sd(growth_{i,j,t})) = ln(k) + (\theta_0 + \theta_1 Z_{j,t}) ln(size_{i,j,t}) + \gamma X_{i,j,t-1} + \eta_{j,t} + \epsilon_{i,j,t}. \quad (1.3)$$

The parameter θ_0 reflects the direct link between bank size and the volatility of its loan growth. Our parameter of interest - θ_1 - indicates how differences in the stringency of capital regulation across countries $Z_{j,t}$ affect the size-volatility nexus. Positive (negative) values of θ_1 imply that a regulatory measure tends to have an increasing (decreasing) effect on the power law coefficient α and, hence, results in large banks being, *ceteris paribus*, more (less) volatile.

Given that we aim at investigating the role of cross-country differences in capital regulation on the size-volatility nexus, we include country-and-time fixed effects $\eta_{j,t}$ in all regressions to capture any direct effects of time-varying, country-specific regulations or other potential confounding factors on bank-level volatility.

By including country-and-time fixed effects, we only examine the link between the deviations of bank size from the annual country-mean $(Size_{i,j,t} - \overline{Size}_{j,t})$ and the corresponding deviation of bank-level volatility $(SD_{i,j,t} - \overline{SD}_{j,t})$, as well as how this link is influenced by regulation $Z_{j,t}$ that differs across countries and over time. Hence, only the within country-year variation is considered. We do not estimate the effects of regulation on the loan portfolio volatility of banks explicitly because we are only interested in the impact of differences in capital regulations on the nexus between volatility and size - as measured by θ_1.

More stringent capital regulation could affect diversification and moral hazard in different ways. A higher degree of leverage, for instance, might be associated with stronger asset diversification (Berg and Gider, 2017). According to studies analyzing risk-taking behavior by banks (Andriosopoulos et al., 2015; Agoraki et al., 2011; Houston et al., 2010; Laeven and Levine, 2009), higher capitalization can help prevent banks from taking on excessive risks. Yet, other studies point at increases in asset risk in response to stricter capital requirements, for example for banks that aim at increasing profitability (Devereux et al., 2015; Dautovic, 2019; Jiménez et al., 2017). Hence, the ultimate effect on the nexus between bank size and volatility remains ambiguous ($\theta_1 > 0$ or $\theta_1 < 0$).

To control for confounding factors that may affect volatility at the bank level, we include a set of standard variables that reflect bank performance $X_{i,j,t-1}$, all lagged by one year.

Standard errors are clustered at the bank level to account for potential remaining correlation of the residuals across time. Given that we only have 27 countries in our sample, clustering at the country-level entails the problem that the standard errors are biased due to the limited number of clusters (Petersen, 2009). Thus, we opt for clustering standard errors at the bank level, but also test the robustness of our results when clustering at the country level.

1.3.3 Measuring bank-level volatility, size and overall conditions

1.3.3.1 Bank-level volatility

Amiti and Weinstein (2018), Bremus et al. (2018), as well as Galaasen et al. (2020) show that the concept of granularity applies to the banking sector if idiosyncratic shocks are passed through to firms via changes in lending. Since we are interested in bank-level volatility that might cause macroeconomic fluctuations through the mechanism of granularity, we need a measure of the volatility of loans at the bank level, namely the standard deviation of a bank's growth in net loans. Thereby, our results might give some indications regarding the impact of financial regulations on macroeconomic volatility.

Directly calculating the annual standard deviation of the growth in net loans would require intra-year data on the lending of banks, which is not available for large cross-country datasets. Due to the unbalanced nature of our panel, calculating annual standard deviations based on rolling windows over several consecutive years is likely to produce inaccurate estimates. Instead, we follow Kalemli-Ozcan et al. (2014) and Loutskina and Strahan (2015), regressing bank-level loan growth on a set of time and bank fixed effects,

$$\frac{loans_{i,t} - loans_{i,t-1}}{loans_{i,t-1}} = \beta_t + \delta_i + shock_{i,t}, \tag{1.4}$$

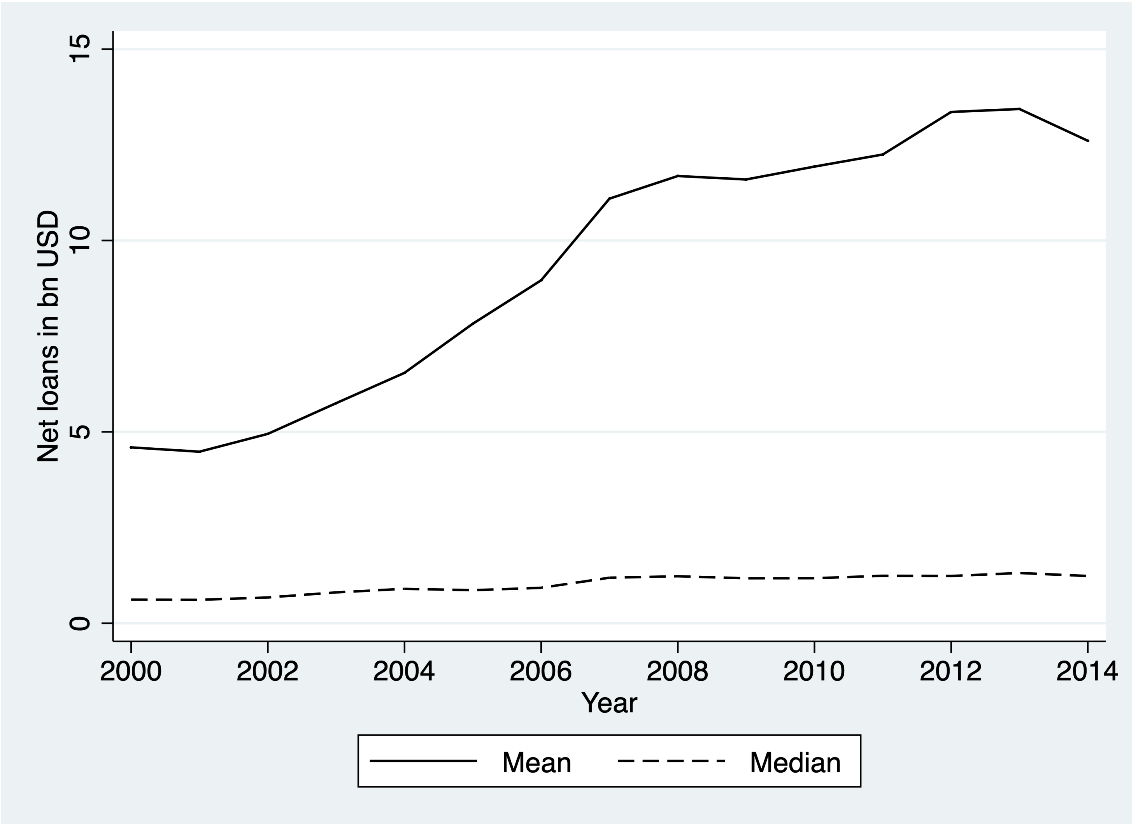
where β_t is the time fixed effect capturing the average growth of all banks, i.e., the effect of common macroeconomic factors in year t, and δ_i is the bank fixed effect capturing the average growth over time of bank i, i.e., the effect of time-invariant, bank-specific factors like the bank type or its business model. Consequently, the $shock_{i,t}$ reflects how much the loan growth of bank i differs from the average loan growth across all banks in year t and from the average loan growth of bank i over time. As equation (1.4) is estimated country-by-country, the shock captures deviations of bank-level loan growth from the annual country-mean. We then use the absolute value of the estimated residuals as our time-varying annual volatility measure:

$$sd(growth_{i,t}) := |\widehat{shock}_{i,t}|. \tag{1.5}$$

1.3.3.2 Bank size and control variables

Although bank size is often measured by total assets (Bhagat et al., 2015; Houston et al., 2010; Laeven et al., 2016), in our context, total net loans are a more appropriate measure of bank size as we are ultimately interested in the real macroeconomic effects of bank-level volatility, which are more closely related to credit supply rather than to total bank assets (Bremus and Buch, 2017). Moreover, granular effects from the banking sector are shown to transmit to the macroeconomy through loans (Amiti and Weinstein, 2018; Galaasen et al., 2020).

FIGURE 1.1: Average and median loan portfolio size over time



Note: This figure shows the development of the average and median size of banks' net loan portfolio in US dollar billions over the 2000-2014 sample period.

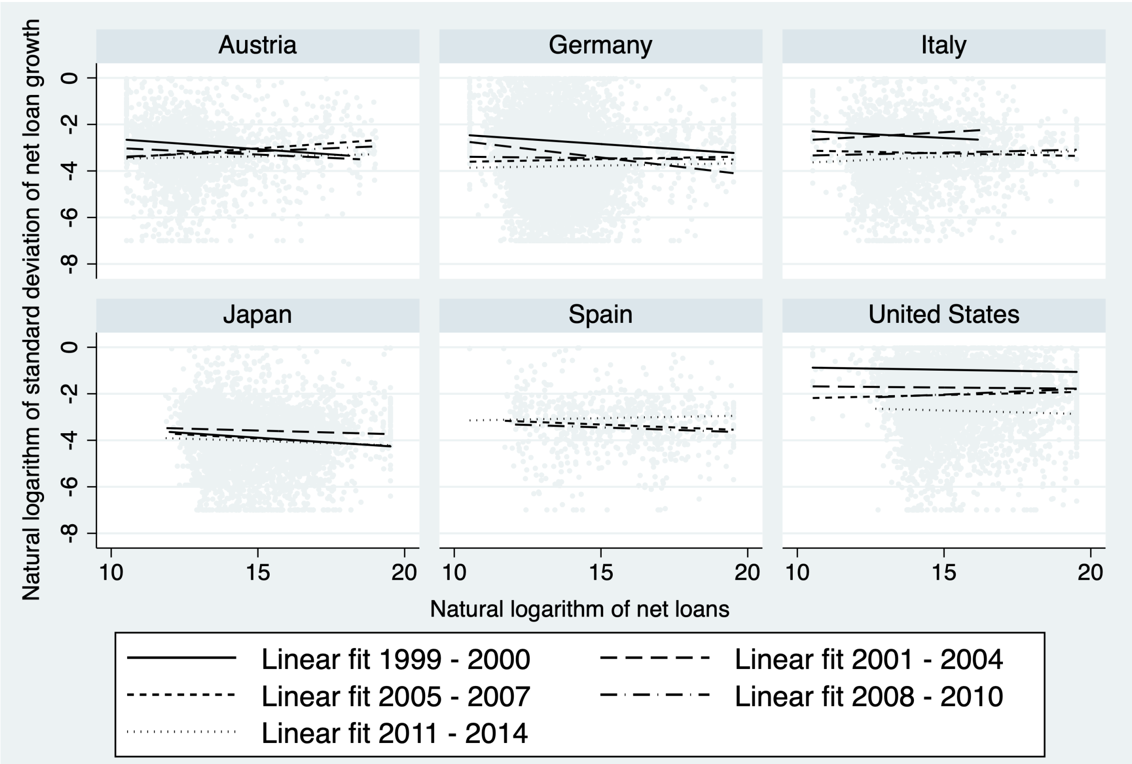
Figure 1.1 shows that the average bank loan portfolio size increases over the sample period, while the median remains fairly constant. This indicates that large banks disproportionately expanded their loan portfolios, hinting at an increase in market concentration in the credit market - a topic of increasing debate since the global financial crisis.[9]

To visualize the variation in the power law coefficient α that links size with volatility of a bank's loan portfolio over time and across countries, Figure 1.2 shows the linear fit of the power law introduced in equation (1.1) for five sub-periods in a subset of exemplary countries. Overall, the nexus seems to be negative in the majority of the cases, indicating that volatility increases with bank size, which is in line with the existing findings of Landier et al. (2017). However, while the size-volatility nexus remained fairly stable over time in some countries

[9]In the aftermath of the financial crisis, bank concentration significantly increased in several countries, e.g., due to mergers and takeovers of failing banks (BIS, 2018; ECB, 2017). In the United States, while the biggest five banks accounted for about ten percent of total bank assets at the beginning of the 1990s, in the mid-2010s, these banks owned nearly half of total bank assets (https://politicsofpoverty.oxfamamerica.org/2016/01/toobig-to-fail-and-only-getting-bigger/).

(e.g., Japan, Spain, United States), we can observe variation in the power law coefficient over time in others. In certain countries, the nexus even changed from positive to negative or vice versa (e.g., Austria, Germany, Italy).

FIGURE 1.2: Nexus between size and volatility of loan portfolios



Note: This figure shows the linear fit of the linearized power law that relates loan portfolio volatility to size as defined in Section (1.3.1) for five sub-periods in a subset of countries. The slope of the fitted lines reflects the power law parameter α.

In summary, the power law coefficient α varies substantially between countries and across time periods. The extent to which this variation across countries and over time in the power law coefficient α can be explained by differences and changes in regulatory frameworks for capital requirements is the question at hand.

To single out the effect of bank capital regulation on the link between size and volatility, we must control for confounding factors that drive volatility at the bank level. To account for the bank's overall condition, we follow the literature (e.g., Lambert et al., 2017) and include a set of standard bank-level characteristics that relate to banks' capital adequacy, asset quality, management capability, earning, and liquidity (CAMEL). We measure capital adequacy with the commonly used equity-to-assets ratio (Bhagat et al., 2015; Houston et al., 2010), asset quality by loan loss provisions over net interest revenues as in Beck et al. (2013), and management capability by non-interest expense over gross revenues (Agoraki et al., 2011). The ability of a bank to generate earnings is most commonly measured by the returns on average assets (ROAA) (Bhagat et al., 2015; Houston et al., 2010; Jiménez et al., 2013), while we control for liquidity using net loans relative to assets (Beck et al., 2013; Laeven et al., 2016). For details on the bank-level control variables, see Table A1.1 in the Data Appendix.

1.3.4 Data sources

1.3.4.1 Bank balance sheet data

To estimate our model, we use annual data covering the 1999-2014 period from the Bureau Van Dijck Bankscope database, which contains information on bank balance sheets worldwide.

We conduct several pre-processing steps to deal with issues typically arising when working with Bankscope (Duprey and Lé, 2016). First, we adjust for different accounting dates since we need consistent yearly observations to be able to match later the bank data to information on regulations. If a bank publishes its financial statements in the first half of a year, it is categorized to be referring to the previous full year. Second, to account for reporting errors, we perform some plausibility checks. We check whether reported net loans, loan loss provisions, and the measure for management capability being non-interest expense over gross revenues are larger than zero and whether the capital adequacy ratio measured as equity over total assets as well as the liquidity control variable being net loans over total assets are $\in [0, 1]$. All entries that do not fulfill these conditions - well below one percent of observations per variable - are dropped. As some banks publish consolidated as well as unconsolidated statements, the dataset might include double entries. Following previous literature (e.g., Buch and Neugebauer, 2011; Claessens and von Horen, 2015), we only keep banks that have a consolidation code of C1 (i.e., published statements are consolidated and companions are not in the dataset), C2 (i.e., published statements are consolidated and companions are in the dataset), U1 (i.e., published statements are unconsolidated and companions are not in the dataset or the bank does not publish consolidated accounts), or A1 (i.e., statements are aggregated with no companion). Since we are particularly interested in the effects capital regulation has on the link between the loan portfolio size and volatility of large banks, we disregard all banks with assets less than 0.005% of total bank assets in a given country-year. Thereby, we loosely follow a categorization by the ECB, which defines banks to be very small if a bank's assets as a percentage of total assets of EU banks is below this threshold. This selection results in a mere reduction of 2% in the sample mean of assets.

To ensure some degree of comparability, we analyze the effect of bank capital regulation on the nexus between loan portfolio size and volatility across advanced economies as defined by the International Monetary Fund (IMF, 2012). We disregard developing countries in our analysis to prevent any distorting effects arising from possible deviations between de jure and de facto banking regulations. Moreover, the financial crisis of 2007/08, which triggered many of the regulatory changes in recent years, had less impact on the banking sectors in developing countries compared to those in advanced economies.

We exclude all banks and country-years with fewer than five observations, as our time-varying and bank-specific volatilities are measured as the absolute value of the estimated residuals from regressing bank-level loan growth on a set of time and bank fixed effects in each country. Moreover, we only keep countries in the sample for which we have at least 50 bank-year observations.[10] In terms of bank types, the dataset includes bank holding and holding companies (11%), commercial banks (30%), cooperative banks (34%), and savings banks (26%). We exclude various specialized banks to focus on the traditional credit business.

After estimating the bank-specific volatilities according to equations (1.4) and (1.5), we winsorize all bank-level variables (volatility, size, and bank-specific controls) at a one percent level from above and below to reduce distorting effects arising from outliers and potential mismeasurement (Beck et al., 2013; Kalemli-Ozcan et al., 2014).[11] Our final regression sample covers the 2000-2014 period.[12]

[10]Our baseline results are not sensitive to these selection criterions. The main empirical findings do not change if the number of necessary bank-year observations for a country to be included in the panel is any number between 25 and 75. The same holds true for the number of country-year and bank observations being set as small as three and as large as seven.

[11]Winsorizing at any other level between 0.5 and 2.5 percent does not affect our baseline results.

[12]The pre-processed data covers the 2000 to 2014 period for the size and volatility measure, whereas it covers from 1999 to 2013 for the variables describing the banks' overall conditions as we use lagged controls.

1.3.4.2 Data on bank capital regulation

We obtain the data for the regulatory variables from the following sources: Information on the tightening of capital requirements due to the implementation of Basel II, II.5, and III is available from Cerutti et al. (2017a). Moreover, we use their aggregate index of the tightening or loosening of sector-specific capital buffers. These data are available starting from 2000.

Information on the overall stringency of bank capital regulation is taken from Barth et al. (2013), who use the World Bank Banking Supervision Surveys until 2011 to construct different indexes from individual survey questions. Unfortunately, the database does not cover each individual year in the sample period. Therefore, we must hold some variables constant over certain sub-periods as is done in previous studies that use indexes from the World Bank Banking Supervision Survey (Beck et al., 2013; Houston et al., 2012). The World Bank Banking Supervision Survey was conducted in five waves (1999, 2002, 2006, 2011, 2019). We choose to hold the capital regulatory index taken from Barth et al. (2013) constant for the periods from 2000, 2001-2004, and 2005-2007, and 2008-2010. Using the information from the 2019 survey, we extend the capital regulatory index from Barth et al. (2013) to the years 2011-2014. For more details on how we measure the regulations, see Table A1.2 in the Data Appendix.[13]

Other adjustments to bank capital regulation include capital surcharges for systemically important financial institutions (SIFIs). Information on whether a country imposes capital surcharges on large banks is available in the Macroprudential Policies Database (Cerutti et al., 2017b). However, there is almost no variation in the data for our sample as capital surcharges on SIFIs were rarely imposed in advanced economies prior to 2013.[14] Therefore, we disregard this capital regulation in our analysis.

After matching the bank data with the data on regulations, we again exclude all banks and country-years with fewer than five observations and all countries

[13]For the survey of 2019, we grant all those countries a value of 1 if question 3.1c indicates that the country has implemented Basel III. Moreover, for the surveys of 2011 and 2019, we assign all those countries a value of 1 if they implemented either Basel I or Basel II, i.e., if 3.1a or 3.1b are answered with "yes".

[14]In fact, only three countries in our sample, namely the Czech Republic, Singapore, and Switzerland, imposed SIFI surcharges in 2013 and 2014.

for which we do not have at least 50 bank-year observations.[15] This yields a panel consisting of 46,727 bank-year observations covering 4,708 banks in 27 advanced economies. Tables 1.1 and A1.3 present summary statistics and correlations of the regulatory variables for our sample, respectively. For a list of the countries included in the panel, see Table A1.4.

TABLE 1.1: Summary statistics

Bank-level variables	Obs.	Mean	SD	Min	Max
Standard deviation of net loans	46,727	0.1100	0.1574	0.0009	0.9661
Loan portfolio size in bn USD	46,727	9.5879	37.457	0.0372	299.73
Equity/total assets	46,727	0.0780	0.0414	0.0138	0.2620
Loan loss provision/net interest revenues	46,727	0.1888	0.2062	-0.0217	1.2400
Non-interest expense/gross revenues	46,727	0.6611	0.1414	0.2577	1.1915
Return on average assets	46,727	0.5036	0.7212	-2.1200	3.4000
Net loans/total assets	46,727	0.6239	0.1745	0.0851	0.9339
Country-level variables					
Capital regulatory index	46,727	6.7753	1.4999	2.0000	10.000
Change in capital requirements	46,727	0.1330	0.3410	0.0000	2.0000
Change in sectoral capital buffers	46,727	0.0199	0.1653	-3.0000	2.0000
Cumulative change in capital requirements	46,727	0.2856	0.6105	0.0000	2.0000
Cumulative change in sectoral capital buffers	46,727	0.0312	0.3613	-2.0000	4.0000

Note: This table presents summary statistics based on the pre-processed panel used for the baseline estimations in Table 1.2.

1.4 Estimation results

To estimate the effect of capital regulations on the nexus between bank size and volatility, we estimate the model presented in equation (1.3). All regressions include country-and-time fixed effects and control variables, and standard errors are clustered at the bank level.

1.4.1 Capital regulation and the size-volatility nexus

Table 1.2 presents our baseline regression results for a set of different indicators on bank capital regulation, namely the capital regulatory index by Barth et al. (2013), annual changes in capital requirements, changes in sectoral capital buffers, and the respective cumulative changes since 2000 provided by Cerutti et al. (2017b). Considering the variation in the different measures of capital regulation across countries and over time, the results reveal that effects on the bank size-volatility nexus show similar patterns across the regulatory measures.

[15]This reduces the total observations by approximately 1.5%. The average standard deviation and loan portfolio size of the sample shrinks by less than 1.5% and 0.5%, respectively. The results remain unchanged when estimating our baseline model with the larger panel.

TABLE 1.2: Determinants of heterogeneity in the loan volatility - size relationship, baseline model

Dependent variable: ln(volatility)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ln(size)	0.0196	-0.0147**	-0.0172***	-0.0141**	-0.0175***	-0.0142**	-0.0142**
	(0.0201)	(0.0059)	(0.0057)	(0.0060)	(0.0057)	(0.0058)	(0.0060)
Capital regulatory index x ln(size)	-0.0055*						
	(0.0029)						
Change in capital requirements x ln(size)		-0.0216*				-0.0204*	
		(0.0111)				(0.0111)	
Change in sectoral capital buffers x ln(size)			-0.0422*			-0.0396*	
			(0.0226)			(0.0226)	
Cumulative change in capital requirements x ln(size)				-0.0117*			-0.0099
				(0.0068)			(0.0068)
Cumulative change in sectoral capital buffers x ln(size)					-0.0207		-0.0180
					(0.0136)		(0.0134)
Equity/Assets	1.9632***	1.9565***	1.9560***	1.9560***	1.9609***	1.9569***	1.9606***
	(0.2706)	(0.2709)	(0.2708)	(0.2709)	(0.2707)	(0.2708)	(0.2707)
Loan loss provision/Net interest revenues	0.6571***	0.6579***	0.6569***	0.6572***	0.6583***	0.6560***	0.6569***
	(0.0412)	(0.0412)	(0.0412)	(0.0412)	(0.0412)	(0.0412)	(0.0412)
Non-interest expense/Gross revenues	0.3071***	0.3067***	0.3083***	0.3074***	0.3098***	0.3079***	0.3098***
	(0.0743)	(0.0744)	(0.0744)	(0.0744)	(0.0742)	(0.0744)	(0.0743)
ROAA	0.0159	0.0167	0.0168	0.0166	0.0169	0.0167	0.0167
	(0.0161)	(0.0161)	(0.0161)	(0.0161)	(0.0161)	(0.0161)	(0.0161)
Net loans/Assets	-0.9830***	-0.9802***	-0.9814***	-0.9800***	-0.9804***	-0.9805***	-0.9795***
	(0.0615)	(0.0616)	(0.0614)	(0.0616)	(0.0614)	(0.0615)	(0.0615)
Observations	46,727	46,727	46,727	46,727	46,727	46,727	46,727
Countries	27	27	27	27	27	27	27
Banks	4,708	4,708	4,708	4,708	4,708	4,708	4,708
R-squared	0.3401	0.3401	0.3400	0.3401	0.3401	0.3401	0.3401

Note: This table reports estimation results for the model specified in equation (1.3) with country-time fixed effects included in each regression. All control variables are lagged by one period. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

We find a negative and statistically significant effect for the capital regulation index, for changes in capital requirements and for changes in sectoral capital buffers (columns (1)-(3) and (6), $\theta_1 < 0$). Thus, more stringent capital regulation reduces loan portfolio volatility as banks become larger. This suggests that better capitalization results in less volatile loans issued by large banks. However, although the coefficients on the interaction terms between size and the cumulative indexes are also negative, they are mostly not statistically significant (columns (4)-(5) and (7)).

All bank-level control variables, except for ROAA, enter the model significantly and show the expected effects. A higher equity-to-assets ratio has a positive significant effect on loan portfolio volatility, suggesting that it might reduce the loan portfolio, rendering it more volatile. In line with this reasoning, an increase in net loans over total assets mitigates volatility, suggesting that a larger loan portfolio is likely to be better diversified. Weaker asset quality and management capability result in higher loan portfolio volatility.

Regarding economic significance, the estimation results reveal that as bank size increases by one standard deviation from the sample mean, loan portfolio volatility rises by approximately 3.4% in countries with the most lenient capital regulations. In contrast, in countries with the most stringent capital regulations, volatility declines by 13.8% with a one standard deviation increase in bank size.[16] For changes in capital requirements, namely tightening of capital requirements due to the implementation of the Basel-agreements, loan portfolio volatility is estimated to decline by 5.7% for the most lenient countries up to 22.6% in those countries with the largest adjustments in capital requirements in a given year.

Regarding the overall link between loan portfolio size and volatility, the power law coefficient α - computed from the estimated coefficients on $ln(size)$ and the effects of a certain regulatory measures times the sample mean of this regulation $(\theta_0 + \theta_1 \overline{Z}_{j,t})$ - is negative in all regression models, thus indicating that volatility generally decreases with loan portfolio size. This is in line with related studies (e.g., Landier et al., 2017).

[16] At the sample mean of bank size (9.59), an increase in size by one standard deviation (37.46) corresponds to a 390.6% increase in size. Capital regulatory stringency is measured by an indicator that varies between 2 and 10 for our sample (see Table 1.1). Hence, based on column (1) in Table 1.2, the marginal effect of bank size on volatility amounts to 390.6*(0.0196 - 0.0055*2) if the most lenient capital regulations are in place and to 390.6*(0.0196 - 0.0055*10) in case of the most stringent capital regulations.

TABLE 1.3: Determinants of heterogeneity in the loan volatility - size relationship, excluding small banks from the sample

Dependent variable: ln(volatility)	(1) >0.1bn USD	(2) >0.25bn USD	(3) >0.5bn USD	(4) >1bn USD	(5) >2bn USD
ln(size)	0.0196	0.0216	0.0339	0.0229	0.0324
	(0.0210)	(0.0230)	(0.0258)	(0.0307)	(0.0416)
Capital regulatory index x ln(size)	-0.0048	-0.0046	-0.0064*	-0.0046	-0.0070
	(0.0031)	(0.0034)	(0.0038)	(0.0044)	(0.0060)
R-squared	0.3401	0.3535	0.3727	0.3755	0.3916
ln(size)	-0.0086	-0.0043	-0.0014	0.0015	0.0013
	(0.0059)	(0.0063)	(0.0067)	(0.0079)	(0.0108)
Change in capital requirements x ln(size)	-0.0287**	-0.0336***	-0.0503***	-0.0623***	-0.0968***
	(0.0114)	(0.0123)	(0.0138)	(0.0167)	(0.0221)
R-squared	0.3401	0.3536	0.3730	0.3760	0.3926
ln(size)	-0.0124**	-0.0089	-0.0088	-0.0084	-0.0147
	(0.0058)	(0.0062)	(0.0066)	(0.0077)	(0.0106)
Change in sectoral capital buffers x ln(size)	-0.0362	-0.0428	-0.0552*	-0.0617*	-0.1218***
	(0.0237)	(0.0277)	(0.0310)	(0.0360)	(0.0457)
R-squared	0.3400	0.3535	0.3727	0.3756	0.3919
Observations	45,136	39,905	32,047	23,239	14,662
Countries	27	27	27	27	27
Banks	4,610	4,260	3,554	2,783	1,889
Minimal Size in USD mn	100	250	500	1000	2000

Note: This table reports estimation results of the model specified in equation (1.3) with country-time fixed effects included in each regression. In column (1)-(5) bank-year observations with a loan portfolio size smaller than 100, 250, 500, 1,000 and 2,000 USD mn respectively are excluded from the sample. All control variables are lagged by one period. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

The estimation results for our sample disregarding banks with loan portfolio sizes below certain thresholds are presented in Table 1.3. While the estimated effects of the capital regulatory index on the size-volatility nexus remain negative and close to the baseline model (top panel), they mostly lose statistical significance in the subsamples. Interestingly, changes in capital requirements and in sectoral capital buffers show a distinct pattern: the larger the cutoff for bank size is, the larger (and more significant) the effect of capital regulation on the reduction of loan portfolio volatility when moving up the bank size distribution becomes. The absolute value of the negative coefficient more than triples if only banks with a loan portfolio of at least 2 billion US dollar are included in the sample (column (5)) compared to the result for the sample including all banks with loan portfolios larger than 100 million US dollar (column (1)). Hence, imposing more stringent capital regulation on banks reduces the link between loan portfolio volatility and size for medium-sized and large banks, that is, for those banks more affected by the regulation.

In summary, our analysis shows that imposing stricter capital regulation affects the nexus between size measured in terms of total net loans and volatility measured as standard deviation of net loan growth.

1.4.2 Robustness tests

We run several alternative regressions in order to test the robustness of the results discussed above. Tables 1.4 - 1.8 present the results. The estimations are based on our model specified in equation (1.3). All regressions include country-and-time fixed effects and the above discussed lagged bank-specific controls.

First, we focus on potential endogeneity of bank capital regulation and loan volatility of (large) banks. Reverse causality does not seem to be an important issue in our context, as it would suggest a positive link between regulatory stringency and the size-volatility nexus, whereas we find a negative relationship. Thus, our estimates are conservative as they underestimate the true link if reverse causality plays a role. To account for potential common drivers of regulation and bank-level loan volatility, we run instrumental variables (IV) regressions. We use three instrumental variables that previous literature uses to explain cross-country differences and temporal changes in institutions and regulations but do not directly impact bank-level loan volatility.

Beck et al. (2006) use the percentage of years since 1776 that a country has been independent as an instrument for regulatory variables in their analysis of the effect that private monitoring and supervisory power have on corruption in lending. Similarly, Houston et al. (2012) rely on this variable when estimating the effects of regulatory differences on international bank flows. The argument for the relevance of this instrument goes back to Easterly and Levine (1997), who find that economic growth is stronger in countries that have been independent for longer and, thus, were able to better adopt respective policies. However, all these studies employ samples that include developing countries, i.e., also countries that were once colonialized. Since we focus our analysis on developed countries, we use the percentage of years since a country has become a democracy instead. The underlying argument remains the same. Countries with a longer democratic history are more likely to adopt stricter regulations. Boix (2018) provides data based on which we construct the share of years for which a country has been a democracy since 1800.

Stulz and Williamson (2003) find that countries with a Catholic majority are associated with weaker creditor rights than predominately Protestant countries. Barth et al. (2004) use religious population shares as instruments when analyzing cross-country differences in regulations. The Central Intelligence Agency (CIA) provides respective data in The World Factbook. Following Barth et al. (2004), we construct a variable measuring the share of Roman Catholic, Muslim, and other religious beliefs. We exclude Protestants as reference category to avoid multicollinearity. A potential concern regarding the assumption of instrument exogeneity might be lending restrictions in majority Muslim countries. However, our sample does not include any country-year with more than 18% Muslim population.

Legal origin is another variable used by Barth et al. (2004) when analyzing cross-country differences in regulations. The argument for the relevance of this instrument goes back to La Porta et al. (1998), who find that countries with common-law have the strongest investor protection rules followed by German and Scandinavian civil law countries. We also account for the Socialist/Communist legal origin of some countries in our sample. We exclude the category French civil law which provides the lowest degree of investor protection according to La Porta et al. (1998). We follow Barth et al. (2004) and use respective dummies as instruments for capital regulation.

Table 1.4 presents the IV regression results.[17] We can confirm our findings for the capital regulatory index (column (1)) as well as for changes in capital requirements (column (3)) and sectoral capital buffers (column (5)) when using all the above discussed instrumental variables. All coefficients of interest retain their negative sign and become more statistically significant. They also become larger in magnitude. This could hint at weak instruments.

However, we can reject this hypothesis based on Kleibergen-Paap F statistics (H_0 : weak instruments) for all three regressions. Yet, the Hansen J test statistics (H_0 : validity of over-identifying restrictions) suggest that not all instruments are uncorrelated with the error terms. To alleviate concerns regarding the validity of the entire set of instruments for each regulatory variable, we run IV regressions with specific subsets of instruments that prove to be valid (i.e., *p-value*(Hansen J) > 0.05). Columns (2) and (4) show that this neither changes the IV results for the capital regulatory index nor for the change in the capital requirements variable. The coefficient for the instrumented interaction of changes in sectoral

[17]First stage regression results can be found in Table A1.5 of the Data Appendix.

TABLE 1.4: Determinants of heterogeneity in the loan volatility - IV estimation results

Dependent variable: ln(volatility)	(1)	(2)	(3)	(4)	(5)	(6)
ln(size)	0.2492***	0.2339***	0.0385**	0.0419*	-0.0140**	-0.0016
	(0.0768)	(0.0781)	(0.0186)	(0.0242)	(0.0061)	(0.0080)
Capital regulatory index x ln(size)	-0.0393***	-0.0371***				
	(0.0115)	(0.0117)				
Change in capital requirements x ln(size)			-0.3680***	-0.3901***		
			(0.1150)	(0.1493)		
Change in sectoral capital buffers x ln(size)					-0.2238**	-0.9018***
					(0.1100)	(0.3228)
Equity/Assets	2.0075***	2.0043***	1.9694***	1.9703***	1.9553***	1.9617***
	(0.2705)	(0.2704)	(0.2733)	(0.2734)	(0.2714)	(0.2755)
Loan loss provision/Net interest revenues	0.6429***	0.6437***	0.6375***	0.6363***	0.6455***	0.6126***
	(0.0417)	(0.0417)	(0.0420)	(0.0422)	(0.0416)	(0.0443)
Non-interest expense/Gross revenues	0.3083***	0.3083***	0.3024***	0.3021***	0.3139***	0.3341***
	(0.0743)	(0.0743)	(0.0755)	(0.0756)	(0.0746)	(0.0764)
ROAA	0.0102	0.0106	0.0149	0.0148	0.0169	0.0162
	(0.0163)	(0.0163)	(0.0163)	(0.0163)	(0.0161)	(0.0161)
Net loans/Assets	-0.9950***	-0.9942***	-0.9656***	-0.9646***	-0.9828***	-0.9859***
	(0.0616)	(0.0616)	(0.0623)	(0.0624)	(0.0611)	(0.0621)
Kleibergen-Paap F	63.92	82.26	52.21	59.27	13.51	10.47
p-value	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*	*0.0000*
Hansen J	31.98	5.21	37.60	4.56	45.40	5.73
p-value	*0.0000*	*0.1572*	*0.0000*	*0.2068*	*0.0000*	*0.0569*
Durbin-Wu-Hausmann	10.04	8.13	9.95	6.54	2.92	1.36
p-value	*0.0015*	*0.0044*	*0.0016*	*0.0106*	*0.0873*	*0.2442*
R-squared	0.0314	0.0320	0.0111	0.0079	0.0344	0.0013
All instruments included	Yes	No	Yes	No	Yes	No

Note: This table reports two-stage estimation results of the model specified in equation (1.3) where the three capital regulatory variables are instrumented by all instruments discussed in Section 1.4.2 in columns (1), (3), and (5), while subsets of these instrumental variables are used in columns (2), (4), and (6) as shown in the first stage regressions in Table A1.5. All regressions are based on 46,658 observation for 4,697 banks in 27 countries. Country-time fixed effects are included in each regression. All control variables are lagged by one period. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

capital buffer and bank size becomes considerably larger. However, this should be of little concern, given that test results suggest that this regulatory variable is unlikely to be endogenous in the first place (*p-value*(Durbin-Wu-Hausmann) > 0.05). Hence, the baseline results should be consistent for this regulatory measure. In contrast, the Durbin-Wu-Hausmann test results (H_0 : exogenous regressors) show that size interacted with the capital regulatory index and with the changes in capital requirements are endogenous, warranting IV regressions. Parameter estimates for the control variables remain unchanged in all IV regressions. In summary, given the results of the Hansen test for the full set of instruments, we take the IV-results as tentative evidence supporting our main finding of a negative impact of more stringent capital regulation on the size-volatility nexus.

Second, to mitigate simultaneity concerns, we add interaction terms between bank size and time-varying country-specific variables that might explain part of the variation in the nexus between loan portfolio size and volatility. Tables 1.5 - 1.6 present the estimation results when including the interaction terms of size and different confounding factors that could have explanatory power for the power law coefficient α and that might be correlated with capital regulations.

As a first common driver, banking crises could be associated with particularly large banks taking on excessive risks, that is, with an increase in the nexus of loan portfolio size and volatility. However, neither dropping the 2007/08 period nor adding an interaction between size and a crisis dummy based on Laeven and Valencia (2012, 2018) changes the baseline results for any of the three regulatory variables, namely the capital regulatory index, changes in capital requirements, and changes in sectoral capital buffers (Table 1.5, columns (1)-(2)).

In unreported regressions, we further test whether capital regulation affects the size-volatility nexus differently in countries strongly hit by the financial crisis. To that goal, we extend the estimation model by triple interactions between a binary variable indicating whether a country belongs to the GIPS countries, i.e., a group of countries strongly hit by the global financial and economic crisis, or not, and our interaction of interest (log size x capital regulation). For changes in capital requirements, the results do not differ for GIPS-countries compared to the rest of the sample. Thus, the negative effect of the tightening of capital requirements on the size-volatility nexus does not seem to be driven by this group of crisis-hit countries. Yet, for the capital regulation index and for changes in sectoral capital buffers, the estimation results indicate a differential effect for crisis-hit countries, with large banks in GIPS-countries being less volatile under stricter

TABLE 1.5: Robustness checks - Sensitivity to country-specific economic and political conditions

Dependent variable: ln(volatility)	(1) w/o 2007-08	(2) ln(size) x Crisis	(3) ln(size) x Econ. risk	(4) ln(size) x Gov. effec.
ln(size)	0.0212	0.0207	-0.0213	0.0611**
	(0.0211)	(0.0202)	(0.0619)	(0.0248)
Capital regulatory index x ln(size)	-0.0063**	-0.0060**	-0.0053*	-0.0057*
	(0.0031)	(0.0030)	(0.0029)	(0.0030)
Crisis x ln(size)		0.0071		
		(0.0098)		
Econ. risk rating x ln(size)			0.0010	
			(0.0015)	
Gov. effectiveness x ln(size)				-0.0252**
				(0.0118)
R-squared	0.3289	0.3401	0.3401	0.3299
ln(size)	-0.0183***	-0.0145**	-0.0599	0.0318*
	(0.0061)	(0.0064)	(0.0585)	(0.0185)
Change in capital requirements x ln(size)	-0.0164	-0.0217*	-0.0211*	-0.0262**
	(0.0112)	(0.0112)	(0.0112)	(0.0112)
Crisis x ln(size)		-0.0006		
		(0.0095)		
Econ. risk rating x ln(size)			0.0012	
			(0.0015)	
Gov. effectiveness x ln(size)				-0.0291**
				(0.0118)
R-squared	0.3288	0.3401	0.3401	0.3299
ln(size)	-0.0203***	-0.0177***	-0.0750	0.0253
	(0.0060)	(0.0062)	(0.0578)	(0.0182)
Change in sectoral capital buffers x ln(size)	-0.0445	-0.0422*	-0.0450**	-0.0420*
	(0.0354)	(0.0226)	(0.0227)	(0.0222)
Crisis x ln(size)		0.0019		
		(0.0094)		
Econ. risk rating x ln(size)			0.0015	
			(0.0015)	
Gov. effectiveness x ln(size)				-0.0270**
				(0.0117)
R-squared	0.3288	0.3400	0.3401	0.3298
Observations	40,195	46,727	46,727	44,013
Countries	27	27	27	27
Banks	4,708	4,708	4,708	4,708

Note: This table reports robustness tests for the model specified in equation (1.3) with country-time fixed effects and the full set of control variables included in each regression (not reported). *Crisis* = 1 if a country experiences a systemic banking crisis according to Laeven and Valencia (2012, 2018) and zero otherwise. *Econ. risk* = index on economic risk by the ICRG, *Gov. effec.* = estimated government effectiveness provided by the Worldwide Governance Indicators. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

capital regulation than banks in other economies. When using the banking crisis dummy instead of the GIPS dummy for the triple interactions, a similar picture appears. Thus, the analysis underlines the robustness of the general negative effect of changes in capital requirements on the size-volatility nexus.

Next, the general condition of a country's economy might affect whether large banks diversify or rather take on inordinate risks. Thus, general country characteristics may be correlated with banking regulations. We include interactions between size and an economic risk-rating indicator provided by the International Country Risk Guides (ICRG) that accounts for a country's GDP per capita, real GDP growth, annual inflation, budget balance, and current account relative to GDP. We find no significant effect of economic risk on the power law coefficient (column (3)). Our baseline results also remain unchanged when controlling for any of the other ICRG risk indicators (financial, political, composite, and exchange rate risk). Moreover, countries with more stable and effective institutions are more likely to implement stringent banking regulations. Thus, a higher degree of government effectiveness should result in a lower power law coefficient. Column (4) supports this hypothesis. The government effectiveness indicator from the World Bank's Worldwide Governance Indicators (WGI) reveals a negative effect on the nexus at a 5%-significance level. Still, the coefficients for the interactions between size and the different capital regulation indexes again remain robust. We find similar results when controlling for control of corruption as provided by the WGI. Our baseline findings for bank capital regulation also remain robust when including interactions of size and indicators for political stability, regulatory quality, accountability, or rule of law.

Large banks in countries with efficient, accessible, and large banking sectors should be more diversified than in countries with less developed banking systems. Thus, we expect the IMF's Financial Institutions index (FI) to have a negative effect on the power law coefficient. At the same time, large banking sectors (relative to GDP) might also have more lobbying power to counteract regulatory efforts. Alternatively, countries that are more dependent on the stability of their banking sector due to its relevance for the domestic economy might be more prone toward regulation. Column (1) of Table 1.6 shows significantly negative effects of the interaction between size and FI. The estimates for the capital regulatory index and for changes in capital requirements remain very close to the baseline, while the coefficient on the interaction term between changes in sectoral capital buffers and size becomes statistically insignificant. In contrast, the results for all three capital regulation variables remain robust when controlling for the impact of financial market depth or bank concentration on our power law coefficient (columns (2)-(3)). Column (4) displays the results for estimating our model when Luxembourg and Switzerland are excluded from the sample to ensure that the results are not driven by countries with a very dominant banking sector. We

TABLE 1.6: Robustness checks - Sensitivity to country-specific banking sector characteristics

Dependent variable: ln(volatility)	(1) ln(size) x FI	(2) ln(size) x FMD	(3) ln(size) x Bank conc.	(4) w/o LU & CH
ln(size)	0.2156***	0.0471	0.0174	0.0179
	(0.0569)	(0.0294)	(0.0217)	(0.0206)
Capital regulatory index x ln(size)	-0.0051*	-0.0053*	-0.0057*	-0.0051*
	(0.0029)	(0.0029)	(0.0030)	(0.0030)
Fin. institutions index x ln(size)	-0.2468***			
	(0.0663)			
Fin. markets depth index x ln(size)		-0.0382		
		(0.0268)		
Bank concentratioin x ln(size)			0.0001	
			(0.0002)	
R-squared	0.3405	0.3401	0.3401	0.3295
ln(size)	0.1871***	0.0168	-0.0138	-0.0130**
	(0.0548)	(0.0229)	(0.0135)	(0.0062)
Change in capital requirements x ln(size)	-0.0219**	-0.0218*	-0.0215*	-0.0241**
	(0.0112)	(0.0112)	(0.0112)	(0.0116)
Fin. institutions index x ln(size)	-0.2506***			
	(0.0664)			
Fin. markets depth index x ln(size)		-0.0412		
		(0.0268)		
Bank concentratioin x ln(size)			-0.0000	
			(0.0002)	
R-squared	0.3405	0.3401	0.3401	0.3295
ln(size)	0.1774***	0.0114	-0.0168	-0.0166***
	(0.0547)	(0.0227)	(0.0135)	(0.0061)
Change in sectoral capital buffers x ln(size)	-0.0243	-0.0385*	-0.0421*	-0.0243
	(0.0210)	(0.0223)	(0.0226)	(0.0210)
Fin. institutions index x ln(size)	-0.2422***			
	(0.0663)			
Fin. markets depth index x ln(size)		-0.0376		
		(0.0267)		
Bank concentratioin x ln(size)			-0.0000	
			(0.0002)	
R-squared	0.3405	0.3401	0.3400	0.3294
Observations	46,727	46,727	46,727	42,908
Countries	27	27	27	25
Banks	4,708	4,708	4,708	4,330

Note: This table reports robustness tests for the model specified in equation (1.3) with country-time fixed effects and the full set of control variables included in each regression (not reported). LU & CH = Luxemburg and Switzerland, FI = IMF financial institutions index, FMD = IMF financial market access index, *Bank conc.* = World Bank 3-bank concentration (%). Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

find that our previous results for the capital regulatory index and changes in capital requirements are robust, while changes in sectoral capital buffer seem to matter less for the size-volatility nexus when the two countries are excluded.

We also check the sensitivity of our results with respect to bank characteristics (Table 1.7). Since bank types can play a role for the link between loan growth and volatility, we add investment banks to the sample. Compared to the baseline results (Table 1.2), the results including investment banks (columns (1) - (2)) remain broadly unchanged. Statistical significance and the size of the coefficients increase for both the capital regulatory index and the binary variable indicating changes in capital requirements. Changes in sectoral capital buffers matter less though once investment banks are included in the sample, which is not surprising since the objective of this regulatory measure is to limit a bank's credit exposure to certain sectors of the economy and is, therefore, less relevant for investment banks given their business model. Adding a dummy variable in the model that takes on a value of one for investment banks and zero otherwise shows a similar picture. The dummy itself is statistically significant and positive for all three models, indicating that investment banks have more volatile loan growth than the other banks in the sample.

Ownership structure does not seem to matter much for our results. Roughly 10% of our sample are banks listed on a stock exchange. Column (3) shows that adding a respective dummy neither alters the baseline results nor is the dummy statistically significant in any of the three models. To control for ownership diversity, we add a dummy that indicates if the number of a bank's shareholders does not exceed 10 (as of December 2016). Column (4) shows the dummy to be positive and significant at the 5%-level in all three models, suggesting that banks with a limited number of shareholders tend to have more volatile loan growth. Again, our findings for the capital regulatory index, changes in capital requirements, and changes in sectoral capital buffers remain similar to the baseline results.

As pointed out by Hagendorff et al. (2018), differences in accounting standards can have differential effects on banks. Thus, following these authors, we include a dummy variable indicating if accounting is done in accordance with IFRS or not (column (5)). We do not find any significant effects for the dummy variable and our baseline results for the three capital regulation variables prove to be robust. In unreported regressions, we further include interactions between all bank-level controls and the three capital regulatory measures in addition to the interaction between regulation and size. All our regulatory variables of interest retain their negative signs as well as statistical significance in the large majority of models. Moreover, we added bank fixed effects to the model in order to control for time-invariant bank characteristics. Apart from the capital regulatory index, changes in capital requirements and sectoral buffers still show negative and statistically significant effects on the size-volatility nexus.

TABLE 1.7: Robustness checks - Sensitivity to bank-characteristics

Dependent variable: ln(volatility)	(1) IBs included	(2) IB- dummy	(3) Listed dummy	(4) Owner div. dummy	(5) IFRS dummy
ln(size)	0.0442**	0.0321	0.0211	0.0224	0.0209
	(0.0199)	(0.0197)	(0.0201)	(0.0200)	(0.0201)
Capital regulatory index x ln(size)	-0.0091***	-0.0073**	-0.0056*	-0.0056*	-0.0061**
	(0.0029)	(0.0028)	(0.0029)	(0.0029)	(0.0030)
Dummy = 1 if bank is investment bank		0.7808***			
		(0.1018)			
Dummy = 1 if bank is listed			-0.0172		
			(0.0265)		
Dummy = 1 if max. # shareholders 10				0.0508**	
				(0.0247)	
Dummy = 1 if IFRS					0.0479
					(0.0574)
R-squared	0.3406	0.3442	0.3401	0.3402	0.3401
ln(size)	-0.0132**	-0.0126**	-0.0139**	-0.0123**	-0.0163***
	(0.0057)	(0.0057)	(0.0060)	(0.0060)	(0.0059)
Change in capital requirements x ln(size)	-0.0301***	-0.0299***	-0.0215*	-0.0210*	-0.0222**
	(0.0111)	(0.0110)	(0.0111)	(0.0112)	(0.0111)
Dummy = 1 if bank is investment bank		0.7892***			
		(0.1017)			
Dummy = 1 if bank is listed			-0.0149		
			(0.0266)		
Dummy = 1 if max. # shareholders 10				0.0498**	
				(0.0248)	
Dummy = 1 if IFRS					0.0382
					(0.0570)
R-squared	0.3405	0.3441	0.3401	0.3402	0.3401
ln(size)	-0.0174***	-0.0165***	-0.0164***	-0.0147**	-0.0188***
	(0.0056)	(0.0055)	(0.0058)	(0.0059)	(0.0057)
Change in sectoral capital buffers x ln(size)	-0.0275	-0.0380	-0.0424*	-0.0426*	-0.0420*
	(0.0260)	(0.0256)	(0.0226)	(0.0226)	(0.0226)
Dummy = 1 if bank is investment bank		0.7924***			
		(0.1018)			
Dummy = 1 if bank is listed			-0.0156		
			(0.0266)		
Dummy = 1 if max. # shareholders 10				0.0508**	
				(0.0247)	
Dummy = 1 if IFRS					0.0337
					(0.0569)
R-squared	0.3404	0.3441	0.3401	0.3402	0.3401
Observations	47,264	47,264	46,727	46,727	46,727
Countries	27	27	27	27	27
Banks	4,774	4,774	4,708	4,708	4,708

Note: This table reports robustness tests for the model specified in equation (1.3) with country-time fixed effects and the full set of control variables included in each regression (not reported). *IBs* = Investment banks, *IB-dummy* = 1 for investment banks and zero otherwise, *Listed-dummy* = 1 for listed banks and zero otherwise, *Owner div. dummy* = 1 if the number of shareholders is less than 11, *IFRS-dummy* = 1 if a bank applies International Financial Reporting Standards and zero otherwise. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

Finally, we test the sensitivity of our baseline results with respect to model specification. Given that we motivate and link our paper to the literature on granular effects from the banking sector (Amiti and Weinstein, 2018; Galaasen et al., 2020), we deliberately focus the analysis on the explanation of loan growth volatility. Other sources of bank risk like insolvency risk or liability structure potentially connect very differently to size and regulation, as pointed out, for example, by Devereux et al. (2019). Still, we check for alternative proxies of bank size and asset-side volatility - even if the link between bank assets and the real economy is less clear-cut than that for bank loans. The regressions with total assets (instead of total loans) as a measure of bank size and asset volatility on the right-hand side (Table 1.8, column (1)) reveal that our findings hold for the capital regulatory index. When looking at the interactions between changes in capital requirements or sectoral capital buffers and bank size as measured by total assets, the effect on the volatility of total assets turns statistically insignificant though.

In column (2), size is measured by net loans to GDP.[18] Thereby, we analyze the link between loan portfolio volatility and size relative to GDP, i.e., the bank's systemic size (Bertay et al., 2013). Our baseline results for all three measures of capital regulation stringency prove to be robust. This further substantiates our line of reasoning, based on the granularity hypothesis, that imposing capital regulation stringency reduces the volatility of banks that are large with respect to a country's economy, all else being equal, and, thus, might reduce macroeconomic fluctuations. In column (3), we add an interaction between size and a dummy variable indicating if a bank's assets are larger than 10% of the country's GDP. The results show that our findings are robust for the three capital regulation indicators even when controlling for the impact of too-big-to-fail banks.

When adding a quadratic term of the natural logarithm of size to the model in order to control for any non-linear effects in the log linearized size-volatility nexus, we find a positive effect for the quadratic term, whereas the direct effect of bank size measured by θ_0 is negative. Hence, our estimation results point to a U-shaped relationship between bank size and volatility. It suggests that, for small banks, increasing the loan portfolio size is associated with a reduction in volatility (diversification hypothesis). However, once a certain threshold of loan portfolio size is reached, further extending the loan portfolio results in higher levels of volatility (moral hazard hypothesis). Our baseline findings in the linear model remain unchanged in the non-linear model.

[18]Data on annual GDP for the countries included in the panel over the sample period is obtained from the World Bank.

TABLE 1.8: Robustness checks - Sensitivity to the model specifications

Dependent variable: ln(volatility)	(1) Asset size & vola.	(2) Loans/ GDP	(3) ln(size) x TBTF	(4) ln(size) x ln(size)
ln(size)	0.0886***	0.0199	0.0197	-0.1397**
	(0.0193)	(0.0193)	(0.0201)	(0.0631)
Capital regulatory index x ln(size)	-0.0084***	-0.0055*	-0.0053*	-0.0065**
	(0.0028)	(0.0028)	(0.0030)	(0.0029)
ln(size) x I(assets/GDP > 10%)			-0.0016	
			(0.0030)	
ln(size) x ln(size)				0.0055***
				(0.0020)
R-squared	0.2888	0.3401	0.3401	0.3403
ln(size)	0.0304***	-0.0140**	-0.0128**	-0.1763***
	(0.0060)	(0.0058)	(0.0062)	(0.0614)
Change in capital requirements x ln(size)	0.0084	-0.0216*	-0.0210*	-0.0250**
	(0.0107)	(0.0111)	(0.0111)	(0.0111)
ln(size) x I(assets/GDP > 10%)			-0.0020	
			(0.0030)	
ln(size) x ln(size)				0.0054***
				(0.0020)
R-squared	0.2886	0.3400	0.3401	0.3403
ln(size)	0.0313***	-0.0166***	-0.0152**	-0.1691***
	(0.0059)	(0.0057)	(0.0061)	(0.0614)
Change in sectoral capital buffers x ln(size)	0.0225	-0.0405*	-0.0417*	-0.0410*
	(0.0195)	(0.0223)	(0.0224)	(0.0232)
ln(size) x I(assets/GDP > 10%)			-0.0021	
			(0.0030)	
ln(size) x ln(size)				0.0050**
				(0.0020)
R-squared	0.2886	0.3400	0.3401	0.3402
Observations	46,727	46,727	46,727	46,727
Countries	27	27	27	27
Banks	4,708	4,708	4,708	4,708

Note: This table reports robustness tests for the model specified in equation (1.3) with country-time fixed effects and the full set of control variables included in each regression (not reported). $TBTF$ = too-big-to fail-dummy equal to one if total assets of a bank exceed 10% of its country's GDP. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

1.5 Conclusions

Building on the concept of granularity and motivating our model specification by the theory on power laws, our analysis provides empirical evidence that stricter bank capital regulation results in banks' loan portfolios being less volatile when moving up the bank size distribution. Thereby, we close a gap in the literature, which so far focuses almost exclusively on the direct effects that regulations have on the risk-taking behavior of banks.

Our analysis provides three main insights. First, imposing more stringent capital regulation has a significantly negative effect on the link between size and volatility; in countries with stricter capital regulation, volatility declines more with bank size than in countries with more lenient capital regulation, on average. Second, the impact of tightening capital requirements increases in magnitude for the upper tail of the bank size distribution. The larger the cutoff for minimum bank size included in the sample is set, the stronger is the volatility-reducing effect of higher capital requirements. Finally, we provide evidence that introducing sectoral capital buffers can also result in large banks having less volatile loan portfolios, all else being equal.

One limitation of our approach concerns the origins of loan portfolio volatility. Even if we control for bank characteristics and credit demand factors at the country level through bank-level controls and country-and-time fixed effects, respectively, we cannot fully account for the fact that changes in loan volatility result from credit demand at the firm level and, thus, cannot be affected by financial regulations. Moreover, test results indicate that our three instrumental variables that previous literature uses to explain cross-country differences and temporal changes in institutions and regulations might not fully address endogeneity concerns in our analysis. Hence, the evidence only allows for a tentative causal interpretation.

Overall, the estimation results reveal that the large heterogeneity in the link between bank size and volatility across countries and time is related to differences in the regulatory framework across countries. Interventions that increase bank capitalization appear to be effective at promoting lower loan portfolio volatility at the level of large banks, *ceteris paribus*, and, in turn, more stable credit extension. Thereby, the transmission of micro-level credit shocks to the macroeconomy through the channel of granularity can be mitigated.

1.A Data appendix

TABLE A1.1: Definitions of bank-specific control variables

Characteristic	Control	Bankscope Definition	+/–*	Used by
Capital adequacy	Equity/ Total assets	As equity is a cushion against asset malfunction, this ratio measures the amount of protection afforded to the bank by the equity they invested in it. The higher this figure the more protection there is.	–/+	Bhagat et al. (2015), Houston et al. (2010)
Asset quality	Loan loss provision/ Net interest revenues	This is the relationship between provisions in the profit and loss account and the interest income over the same period. Ideally, this ratio should be as low as possible and, in a well-run bank, if the lending book is higher risk this should be reflected by higher interest margins. If the ratio deteriorates, this means that risk is not being properly remunerated by margins.	+	Beck et al. (2013)
Management capability	Non-interest expense/ Gross revenues	This is an indicator of efficiency, measuring the overheads or costs of running the bank, the major element of which is normally salaries, as a percentage of net income before impairment charges. The lower the better.	+	Agoraki et al. (2011)
Earnings	Return on average assets	This is perhaps the most important single ratio in comparing the efficiency and operational performance of banks as it looks at the returns generated from the assets financed by the bank.	–	Bhagat et al. (2015), Houston et al. (2010), Jiménez et al. (2013)
Liquidity	Net loans/ Total assets	This liquidity ratio indicates what percentage of the assets of the bank are tied up in loans. The higher this ratio the less liquid the bank will be.	–	Beck et al. (2013), Laeven et al. (2016)

Note: * expected effect on loan portfolio volatility.

TABLE A1.2: Details on regulations and institutional variables

Regulation	Description	Range	Timing	Source
Capital regulatory index	Measures whether capital requirements reflect certain risk elements & certain market value losses are deducted before determining minimum capital adequacy, whether certain funds may be used to initially capitalize a bank and if they are officially verified; a higher number indicates greater stringency with respect to capital regulation.	{0,...,10}	[00], [01 02 03 04], [05 06 07], [08 09 10], [11 12 13 14]	World Bank Banking Supervision Survey
Change in capital requirements	Dummy variable that is 1 if capital requirements were tightened and 0 if not. Based on implementation of Basel II, II.5 and III.	{1, 0}	2000 - 2014	IBRN Prudential Instruments Database, Cerutti et al. (2017a)
Change in sectoral capital buffers	Aggregate index that captures adjustments in risk weights of bank exposures against different borrower types (consumer, real estate, other credit). For each individual sector, the index takes a value of 1 in case of a tightening, zero if there is no change and -1 in case of a loosening of sectoral capital buffers.	{-3,...,3}	2000 - 2014	IBRN Prudential Instruments Database, Cerutti et al. (2017a)
Cumulative change in capital requirements	Cumulative change in capital requirements relative to 2000.	{4,...,16}	2000 - 2014	IBRN Prudential Instruments Database, Cerutti et al. (2017a)
Cumulative change in sectoral capital buffers	Cumulative change in aggregate sector-specific capital buffers since 2000.	{1, 0}	2000 - 2014	IBRN Prudential Instruments Database, Cerutti et al. (2017a)
Percentage of years since democracy	Share of years since 1800 for that a country has been a democracy.	[0,1]	2000 - 2014	Boix (2018), own calculations
Religious composition	Share of Roman Catholic, Muslim, and other religious beliefs. Reference category: Protestants.	[0,1]	2000 - 2014	The World Fact Book, CIA
Legal origin	Dummy variables differentiating between Socialist, German, Scandinavian, and British legal origin. Reference category: French civil law.	{1, 0}	2000 - 2014	Barth et al. (2004)

TABLE A1.3: Correlations of regulatory variables

	Capital regulatory index	Change in capital requirements	Change in sectoral capital buffers	Cumulative change in capital requirements	Cumulative change in sectoral capital buffers
Capital regulatory index	1				
Change in capital requirements	0.1377*	1			
Change in sectoral capital buffers	-0.0589*	0.0859*	1		
Cumulative change in capital requirements	0.1812*	0.7727*	0.2627*	1	
Cumulative change in sectoral capital buffers	-0.0671*	0.0696*	0.7347*	0.2410*	1

Note: This table presents cross-correlations between the regulatory variables used in the regression analysis. * denotes statistical significance at the 1%-level.

TABLE A1.4: Countries included in the panel

Country	Observations	%
Australia	176	0.38
Austria	2,461	5.27
Belgium	384	0.82
Canada	78	0.17
Czech Republic	92	0.20
Denmark	749	1.60
Finland	81	0.17
France	1,822	3.90
Germany	13,787	29.51
Greece	124	0.27
Hong Kong	203	0.43
Israel	138	0.30
Italy	3,531	7.56
Japan	5,143	11.01
Luxembourg	525	1.12
Netherlands	225	0.48
New Zealand	87	0.19
Norway	909	1.95
Portugal	187	0.40
Singapore	60	0.13
Slovakia	129	0.28
Slovenia	189	0.40
Spain	720	1.54
Sweden	529	1.13
Switzerland	3,294	7.05
United Kingdom	530	1.13
United States	10,574	22.63
Total	**46,727**	**100**

Note: This table presents the total number of observations as well as the share by country of our baseline sample.

TABLE A1.5: First stage IV estimation results

	(1) Cap. reg. index	(2) Cap. reg. index	(3) Change in cap. req.	(4) Change in cap. req.	(5) Change in sect. buffer	(6) Change in sect. buffer
Share of democracy years since 1800 x ln(size)	-0.2463*	-0.2146	0.1014***		0.2368***	0.0459***
	(0.1289)	(0.1366)	(0.0147)		(0.0292)	(0.0100)
Share of Roman Catholic pop. x ln(size)	0.0799	0.7398***	0.1410***	-0.0300**	-0.0070	0.0247***
	(0.1900)	(0.1117)	(0.0242)	(0.0126)	(0.0215)	(0.0061)
Share of Muslim pop. x ln(size)	17.2501***	17.5751***	1.0569***		0.9290***	0.6568***
	(1.4182)	(1.3975)	(0.1185)		(0.1856)	(0.1748)
Share of other pop. x ln(size)	-0.4526***		0.2044***	0.0683***	0.0155	
	(0.1249)		(0.0167)	(0.0107)	(0.0134)	
Dummy for English common law x ln(size)	-0.0501	0.2582**	-0.0655***	-0.0961***	-0.1637***	
	(0.1424)	(0.1128)	(0.0178)	(0.0083)	(0.0243)	
Dummy for German civil law x ln(size)	-0.1988**		-0.0475***	-0.0976***	0.0111	
	(0.0991)		(0.0110)	(0.0074)	(0.0094)	
Dummy for Scandinavian civil code x ln(size)	-0.6670**		0.1682***		-0.0449*	
	(0.2705)		(0.0246)		(0.0272)	
Dummy for Socialist/Communist law x ln(size)	0.0962		-0.0115		0.0657***	
	(0.1847)		(0.0242)		(0.0124)	
Observations	46,658	46,658	46,658	46,658	46,658	46,658
Countries	27	27	27	27	27	27
Banks	4,697	4,697	4,697	4,697	4,697	4,697

Note: This table reports first stage estimation results for the IV estimations presented in Table 1.4. Dependent variables are the interactions of bank size and the regulatory variable indicated in the column title. Country-time fixed effects are included in each regression. All control variables are lagged by one period. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

Chapter 2

MiFID II and analyst recommendations: Does the unbundling of research fees from sales commissions reduce tipping?

2.1 Introduction

This paper evaluates whether the second Markets in Financial Instruments Directive (MiFID II), in effect since January 3, 2018, has reduced tipping. Tipping refers to a practice whereby large banks or brokers inform some of their clients about a change of a stock recommendation before its official publication (e.g., Irvine et al., 2007).

That is important because the knowledge about the time and direction of an upcoming recommendation revision seemingly meets the criteria for inside information under EU regulation[1] given that stock prices adjust in the recommendation change direction on the day an analyst employed by a large bank or broker[2] announces an up- or downgrade (e.g., Bradley et al., 2014; Michaely and Womack, 2005). Knowing about an upgrade in advance enables investors to profit by buying into that stock before the recommendation change is published and the share price increases. Similarly, knowing about an upcoming downgrade allows

[1] *Regulation (EU) No 596/2014*, OJ, L 173, 2014, p. 24, Art. 7.

[2] Note, this paper only focuses on sell-side analysts, which banks or brokerage firms employ, whereas investors employ so-called buy-side analysts. Buy-side analysts only provide research in-house, i.e., they do not publish their reports.

a market participant to unload its position at the share price before the publication and the associated decrease in the stock price (Juergens and Lindsey, 2009).

While there is, to the best of my knowledge, no research on the consequences of tipping, the adverse effects of insider trading are well-established in the respective empirical literature, suggesting it interferes with crucial roles of the financial market by impeding the signaling function of stock prices (e.g., Beny, 2006; Cheng et al., 2006; Du and Wei, 2004). Nevertheless, tipping is a practice not explicitly prohibited by law nor effectively prosecuted.

Previous studies suggest that the payment structure associated with equity research incentivized analysts to engage in this practice: Large banks and brokers provided reports and stock recommendations to their clients for free. Sales commissions and trading fees were supposed to cover research costs as well. Thus, tipping might have been an opportunity to engage clients in more trading resulting in higher commissions (Irvine et al., 2007; Juergens and Lindsey, 2009), i.e., profit for the bank or the broker, which ultimately also determines analysts' compensation (Groysberg et al., 2011).

Although there have been some efforts to contain the practice of tipping in the past, previous regulations fell well short of implementing a ban, acknowledging it as inside information dissemination, or, at least, addressing the payment structure for equity research. It was not until January 3, 2018, that MiFID II has come into effect, stipulating, among other things, the unbundling of fees for research and sales commissions. Now, investors must pay for reports either directly or via a research payment account irrespective of any trades to reduce hidden sales inducements.

This study examines a total of 2,712 recommendation changes between April 2016 - September 2019 published by European universal or investment banks and full-service brokers available via IBES Thomson Reuters to assess the potential effect of tipping on returns and turnover both before and after MiFID II became law. I test the null-hypothesis of no mean effect on trading over the three days leading up to and including the day of a recommendation change publication for both periods separately to determine if the regulatory change stopped trading activities related to early information disclosures.

To identify the effect on returns, I rely on the single-index model (Sharpe, 1963) and estimate expected returns in the absence of new information using a market portfolio and control period. I follow the literature on liquidity determinants (e.g., Benston and Hagerman, 1974; Lipson and Mortal, 2007) and use the control period average as the counterfactual estimate, i.e., the level of turnover expected without early information disclosure. I establish causality by applying an extensive set of screening criteria that is even more restrictive than in previous studies on tipping. Thereby, I ensure that no information other than knowledge about the time and direction of an upcoming recommendation change can drive the observed trading. Placebo tests validate my empirical methodology.

The contribution of my research is three-fold. This analysis is the first empirical evaluation of the effectiveness of MiFID II in reducing tipping. Moreover, I exclusively focus on tipping by European banks and brokers, also adding to the literature on tipping, in general, which mainly focuses on US data (e.g., Christophe et al., 2010; Hendershott et al., 2015; Juergens and Lindsey, 2009; Kadan et al., 2018). Additionally, by analyzing the impact of information disclosure on returns and turnover in terms of treatment effects, I provide a conceptualization that allows for a more structured discussion and better comparability of the underlying assumptions.

My analysis yields four main results: First, I can confirm previous findings of significant positive (negative) share price movements on the publication day of an upgrade (downgrade). That substantiates the argument that the knowledge about time and direction of a forthcoming revision constitutes inside information. Moreover, I find estimated treatment effects on returns and turnover to be statistically significant one day before the publication of an up- or downgrade, suggesting that a small group of investors makes an early profit or mitigates potential losses based on an informational advantage. However, the extent of tipping taking place appears smaller in the European context than suggested by previous research based on US data. Most notably, I cannot find any indication that the introduction of MiFID II reduced tipping. Consequently, the unbundling of research fees and sales commissions proves insufficient to overcome misaligned incentives within the financial institutions that are potentially harmful to the financial market and its participants overall.

The remainder of this paper proceeds as follows: Section 2.2 reviews the literature on analysts and tipping to motivate the research question. Section 2.3 outlines the relevant regulatory developments in Europe. Section 2.4 discusses the empirical strategy and data sources. Section 2.5 presents the results before Section 2.6 concludes.

2.2 The role of analysts

2.2.1 Dissemination of information

Previous research suggests that analysts increase the informational content of stock prices (Derrien and Kecskés, 2013). Amiram et al. (2016) find a significant reduction in bid-ask spreads following analysts' publications of earnings forecasts suggesting a reduction in information asymmetries due to equity research. Evidence that institutional investors outperform other market participants in part due to their reliance on research by analysts working for banks or brokers (Chen and Cheng, 2006), and findings by Busse et al. (2012) that suggest superior stock-picking abilities of sell-side analysts over mutual fund managers indicate a high informational quality of equity research. An extensive analysis of Hameed et al. (2015) even provides evidence for information spillover effects from shares widely covered by analysts to stocks with related fundamentals for which there is less equity research available. The researchers conclude that investors use the information provided by analysts for more widely covered shares to price some less regarded ones.[3]

In summary, equity research seems to contribute significantly to the price discovery process in financial markets. That is further supported by previous studies consistently finding significant trading activities and positive (negative) abnormal stock price movements in case of an upgrade (downgrade) on the day a recommendation change is published (Blau and Wade, 2012; Bradley et al., 2014; Brown et al., 2014; Christophe et al., 2010; Hendershott et al., 2015; Irvine et al., 2007; Kadan et al., 2018).

In other words, the publication of a recommendation change has a significant stock price effect. Hence, although publicly available information is presumably the basis for the financial analysis resulting in a recommendation change, not the content of the research itself but the knowledge about the time of publication and direction of the revision by a large investment bank or broker does meet the criteria for inside information: Under EU regulation, inside information includes

[3]The results by Altınkılıç and Hansen (2009) are somewhat of an exception in that they argue that analyst revisions mostly do not contain any relevant information. According to their piggybacking hypothesis, analysts use publicly available news to adjust their forecasts and only aim at emulating future returns. However, Bradley et al. (2014) explain these findings with delayed time-stamps for recommendation publications in their data set, which likely yields distorted results for intraday analyses.

> [. . .] information of a precise nature which has not been made public, relating, directly or indirectly, to one or more issuers of financial instruments or to one or more financial instruments and which, if it were made public, would be likely to have a significant effect on the prices of those financial instruments [. . .].[4]

Nonetheless, disseminating information that a particular recommendation change is forthcoming to a limited group of market participants so that they can make a profit on this information - called tipping in the literature (e.g., Irvine et al., 2007) - appears to be a practice not explicitly prohibited nor effectively prosecuted in the past.

2.2.2 Tipping

Previous studies find evidence for trading activities before the publication of up- and downgrades that significantly exceed the expected level of trading would there no revision be forthcoming.

Irvine et al. (2007) are the first to investigate tipping. The authors analyze the trading behavior of institutional investors five days before an analyst initiates a stock coverage. They find evidence for exceptionally high trading volumes and respective share price adjustments resulting in excessive returns. Kadan et al. (2018) find that large institutional investors, who are more likely informed about recommendation changes early, buy stocks before an upgrade. Busse et al. (2012) show that reverse trading patterns apply to downward revisions. Institutional investors sell shares in the five days leading up to the publication of a downgrade. Brown et al. (2014) provide evidence for tipping for both upgrades and downgrades. In particular, they find that mutual funds buy (sell) stocks in the case of a positive (negative) revision over the four days preceding the research report release.

More indirect evidence further substantiate these findings: Hendershott et al. (2015) show that institutional order flow increases (decreases) at least five days before positive (negative) news announcements. The analyzed news categories include broker research and recommendations. Furthermore, Amiram et al. (2016) state that their previously mentioned finding of a significant decrease in the bid-ask spread following the publication of an analyst forecast is in line

[4] *Regulation (EU) No 596/2014*, OJ, L 173, 2014, p. 24, Art. 7.

with the occurrence of tipping according to the disclosure literature. If sophisticated investors already processed some information, e.g., institutional investors are aware of the content of an upcoming analyst report before its publication, unsophisticated investors can only reduce the gap, i.e., narrow the spread once the information becomes public. Moreover, Christophe et al. (2010) find that the average daily short-selling in the three days before a downgrade is approximately four times the typical amount. Evidence that this increase in short-selling is significantly and negatively associated with stock returns on the day of the publication and the following day hints towards a short-term profit strategy consistent with the presence of tipping.[5] These studies mainly rely on US data. Thus, this analysis adds to this body of literature on tipping by focusing on European banks and brokers.

Unfortunately, research explicitly addressing the potentially adverse effects of tipping is so far lacking. However, there is ample empirical evidence on the negative consequences of insider trading. They include reduced market liquidity (Beny, 2006; Cheng et al., 2006; Chung and Charoenwong, 1998; Fishe and Robe, 2004) and excessive stock price volatility (Du and Wei, 2004; Fernandes and Ferreira, 2009). Both impede the signaling function of stock prices (Beny, 2006; Du and Wei, 2004; Fernandes and Ferreira, 2009), which eventually results in fewer equity issuances (Levine et al., 2017), more concentrated equity ownership (Beny, 2006), increased cost of equity (Bhattachary and Daouk, 2002; Fernandes and Ferreira, 2009), and less efficient resource allocation (Du and Wei, 2004).

Given that the publication of a recommendation change has a significant stock price effect and, therefore, meets the criteria for inside information (see Section 2.2.1), these results imply that trading on the knowledge that a stock will soon be up- or downgraded adversely affects the financial market. Thus, it should be in regulators' interest to prevent tipping.

2.2.3 Payment structure

Previous research suggests that the compensation structure under which analysts operate encouraged tipping in the past.

[5]Note, Blau and Wade (2012) cannot confirm excessive short-selling associated with recommendation changes published by sell-side analysts. However, this does not call into question the previously mentioned findings regarding early trading in long positions.

Investment and universal banks and full-service brokers provided research reports to their clients free of charge. The trading fees investors pay when they place their orders were to cover the research costs as well. Thus, tipping and, thereby, potentially boosting the trading commissions was seemingly a possibility to offset part of the costs equity research entails (Irvine et al., 2007; Juergens and Lindsey, 2009).

Furthermore, Groysberg et al. (2011) find that the analysts personally face monetary incentives to increase brokerage and investment-banking revenues. Using a proprietary data set comprising more than 400 analyst-year observations on compensation for the period from 1994-2005 and results of field interviews with several investment banks and analysts, they find that analyst compensation largely varies with the bonus pool. Yet, the size of that bonus pool depends almost exclusively on trading commissions and corporate finance fees.

These findings support the hypothesis that the payment structure for equity research incentivizes analysts to pre-release recommendation changes to institutional investors to allow them to trade on this information in advance and, thereby, boost trading commissions for the bank.

In summary, there is an explicit conflict of interest between the profit motive of banks and brokers determining the incentives for analysts and the public's interest in a well-functioning financial market.

2.3 Regulatory environment

European legislators and regulators have aimed at addressing this conflict of interest and aligning incentives within financial institutions since at least 2004.

2.3.1 MAD and MiFID

From 2004 to 2006, EU member states adopted the Market Abuse Directive (MAD).[6] It aims at creating a uniform framework for the presentation and dissemination of information by financial firms. However, the directive leaves considerable leeway allowing, for instance, for several self-regulatory measures. Among other things, it advocates for virtual information barriers. These so-called

[6] *Directive 2003/6/EC*, Official Journal of the European Union, L 96, 2003, p. 16-25.

Chinese walls are supposed to separate the research department from sales activities and forestall any selective disclosures of information. Moreover, Article 6 stipulates that research providers must disclose their conflicts of interest, further detailed in Articles 5 and 6 of a directive[7] that accompanied MAD.

In April 2004, the EU Commission published the first Markets in Financial Instruments Directive (MiFID).[8] It emphasizes the superiority of internal arrangements and preventive measures over external provisions to avoid conflicts of interest (see Articles 13 (3) and 18 (2)). The Commission's directive on the implementation of MiFID[9] points out that investment firms should pay special attention to the conflicts of interest resulting from their different activities - including research, trading, sales, and corporate finance activities - when designing internal policies and processes that ensure that investors' interests are protected. While this directive details the intended effects of organizational requirements and procedures within investment firms, e.g., that the exchange of information may not detrimentally affect any client and that payment for different provided services should be independent, it falls short of requiring any measurable provisions or regulations.

Hence, while MAD and MiFID aim, among many other things, to address the conflict of interest underlying the practice of pre-releasing analyst recommendation changes to institutional customers, these directives do not prohibit tipping.

Note, there is research on the efficacy of MAD and MiFID. However, they focus on the conflict of interest between the equity research department and corporate banking activities (e.g., Höfer and Oehler, 2014; Prokop and Kammann, 2018). To the best of my knowledge, no studies examine the impact of these regulations on early information disclosure associated with recommendation changes.

2.3.2 MAD Repeal and MiFID II

In 2014, the European Commission published a directive[10] that would repeal the Market Abuse Directive in July 2016 due to the rapid changes in the financial industry and market. While reiterating that research based on publicly available

[7] *Commission Directive 2003/125/EC*, Official Journal of the European Union, L 339, 2003, p. 73-77.
[8] *Directive 2004/39/EC*, Official Journal of the European Union, L 145, 2004, p. 1-44.
[9] *Directive 2006/73/EC*, Official Journal of the European Union, L 241, 2006, p. 26-58.
[10] *Regulation (EU) No 596/2014*, OJ, L 173, 2014, p. 1-61.

data does not constitute inside information, the directive qualifies this statement. It raises the possibility to label analyst research insider knowledge if its publication has an impact on prices. However, it is then left to the market participants to

> [...] consider the extent to which the information is non-public and the possible effect on financial instruments traded in advance of its publication or distribution, to establish whether they would be trading on the basis of inside information.[11]

One month later, EU member states adopted the second Markets in Financial Instruments Directive (MiFID II).[12] Article 16 states that investment firms must implement efficacious structures and measures to prevent any conflicts of interest from harming client interests. More importantly, Article 24 (7) stipulates that investment firms claiming to provide independent research may not receive any indirect payments for this service. The Directive 2017/593[13] supplementing MiFID II further specifies that investors must pay fees for trade execution and research services separately. Article 13 outlines that investment firms can either pay for research directly; or the clients can set up and fund a research payment account. The research fees may not depend on the volume or value of the clients' transactions.

Now, full-service brokers must unbundle research fees and transaction commission, i.e., they must set prices for the research they provide independent of trading fees. Hence, if tipping is the result of a payment structure for research and an analyst compensation both misaligning incentives (e.g., Irvine et al., 2007; Juergens and Lindsey, 2009), this regulatory change should be expedient. Equity research analysts should no longer face incentives to engage in tipping, as they become an income-generating entity. MiFID II came into effect on January 3, 2018.

2.4 Empirical methodology and data

I apply an event study framework to analyze whether the unbundling of fees for research and trading stipulated by the newly introduced European regulation MiFID II effectively reduces the practice of pre-releasing information about

[11] *Regulation (EU) No 596/2014*, OJ, L 173, 2014, p. 6, (28).
[12] *Directive 2014/65/EU*, OJ, L 173, 2014, p. 349-496.
[13] *Commission Delegated Directive (EU) 2017/593*, OJ, L 87, 2017, p. 500-517.

stock recommendation changes to selective market participants. Thereby, I follow numerous previous studies on the effect certain events might have on financial markets[14] and analyst recommendation changes, in particular (e.g., Altınkılıç and Hansen, 2009; Chen and Cheng, 2006; Irvine et al., 2007; Juergens and Lindsey, 2009; Kadan et al., 2018). However, I add to these studies by providing a conceptualization of the estimated treatment effects.

2.4.1 Average treatment effect on the treated

I estimate the average treatment effect on the treated (ATT) for each day on which banks or brokers might release information to determine whether market participants trade on knowledge about a forthcoming recommendation change.

From the moment an analyst decides to change the recommendation for a specific stock to the actual time of its publication, typically, several days pass during which analysts adjust the respective financial model, write and edit a research report, and the legal department reviews the results. In previous studies, this period ranges from three or four days (Juergens and Lindsey, 2009; Kadan et al., 2018) up to ten days (Blau and Wade, 2012; Hendershott et al., 2015). Since digitalization and globalization have likely resulted in a swifter process in recent years by automating some of the steps and ensuring a continuous work process due to offices in different time zones, I expect analysts to know about their upcoming publication three trading days beforehand.[15]

Hence, I am interested in four different treatment effects indexed with $j = -3, \ldots, 0$. Note, I assume brokers and banks indeed disclose information over the three days leading up to and including the publication, i.e., a stock is treated on each of these days. Therefore, I define the treatment dummies to be:

$$D_i^j = \begin{cases} 1, & \text{if a recommendation change for stock } i \text{ is published in } |j| \text{ days.} \\ 0, & \text{otherwise.} \end{cases} \tag{2.1}$$

The average effect of the information disclosure (i.e., treatment) on the trading measure (i.e., outcome variable) for stocks with a recommendation revision in $|j|$

[14] For a detailed review of event studies conducted between 1974 and 2000, see Kothari and Warner (2007).

[15] For better readability, the term *days* abbreviates *trading days* in the remainder of the paper.

days (i.e., treatment group) is:

$$ATT(Tr)^j = E[Tr^1_{i,t_{n_i}+j} - Tr^0_{i,t_{n_i}+j} | D^j_i = 1]. \tag{2.2}$$

$Tr^1_{i,t_{n_i}+j}$ measures trading $|j|$ days before an analyst publishes a recommendation change for stock i on day t_{n_i}, with $t_{n_i} \in \{1, \ldots, T\}$. T is the total number of trading days over my sample period. Note, there can be multiple recommendation changes for the same stock. Thus, n_i identifies a particular recommendation change for stock i, with $n_i = 1, \ldots, N_i$. $Tr^0_{i,t_{n_i}+j}$ indicates the level of trading if there is no revision published in $|j|$ days and, thus, no information disclosed, which is a counterfactual given that $D^j_i = 1$.

2.4.2 Measuring abnormal trading

Following previous studies on tipping, I measure trading in terms of returns and turnover (e.g., Irvine et al., 2007; Juergens and Lindsey, 2009; Kadan et al., 2018).

2.4.2.1 Returns

Stock return is the measure most commonly used when determining the effect a particular event has on financial markets. Expected returns can be estimated using the single-index model first proposed by Sharpe (1963):

$$E[R_{i,t}] = \alpha_i + \beta_i R_{m,t}, \tag{2.3}$$

with the market model parameters α_i and β_i, the return $R_{m,t}$ of the market portfolio m, and the actual stock return $R_{i,t}$ of stock i on any given day t. This results in the following identification assumption for the outcome variable return:

$$E[R^0_{i,t_{n_i}+j} | D^j_i = 1] = E[\alpha_i + \beta_i R_{m,t_{n_i}+j} | D^j_i = 1]. \tag{2.4}$$

From equations (2.2) and (2.4) follows for the estimated treatment effect on returns:

$$\widehat{ATT(R)}^j = \frac{1}{N} \sum_{i=1}^{I} \sum_{n_i=1}^{N_i} [R_{i,t_{n_i}+j} - (\hat{\alpha}_i + \hat{\beta}_i R_{m,t_{n_i}+j}) | D^j_i = 1], \tag{2.5}$$

for $j = -3, \dots, 0$. $N = \sum_{i=1}^{I} N_i$ is the sample size in terms of recommendation changes. Parameters α_i and β_i are estimated using data on returns of the stock i and of the market portfolio m over a control period of 25 days that starts 30 days before the publication at t_{n_i}. I exclude a two trading day buffer period ($t_{n_i} - 5$, $t_{n_i} - 4$) to ensure that potential information disclosures on earlier days do not distort my results.

This estimate for the ATT translates to the average over abnormal returns, i.e., the returns on event days surpassing the expected returns based on a control period, which is the definition often used in the literature analyzing tipping in an event-study setting (e.g., Irvine et al., 2007).

2.4.2.2 Turnover

Juergens and Lindsey (2009) and Kadan et al. (2018) additionally consider the effect of pre-releasing information about changes in recommendations on trading volume or turnover.

Assuming that turnover remains at a constant level in the absence of new information results in the following identification assumption for estimating the respective ATT:

$$E[To^0_{i,t_{n_i}+j} | D_i^j = 1] = E[To^0_{i,t_{n_i}+k} | D_i^j = 1], \quad \text{for } k \neq -3, \dots, 0. \tag{2.6}$$

Using the average over the control period to estimate the expected value of turnover for the counterfactual yields:

$$\widehat{ATT(To)}^j = \frac{1}{N} \sum_{i=1}^{I} \sum_{n_i=1}^{N_i} [\frac{Volume_{i,t_{n_i}+j}}{SharesOut_{i,t_{n_i}+j}} - (\frac{1}{25} \sum_{\tau=-30}^{-6} \frac{Volume_{i,t_{n_i}+\tau}}{SharesOut_{i,t_{n_i}+\tau}}) | D_i^j = 1], \tag{2.7}$$

for $j = -3, \dots, 0$. *Volume* is the daily number of traded shares, and *SharesOut* is the daily number of outstanding shares.

Again, this estimated ATT corresponds to the average over the so-called abnormal turnover $|j|$ days before a recommendation change is published.

2.4.3 Critical assessment of assumptions

In addition to being well-established in the literature, relying on the single-index model to identify the effect of information disclosure on stock returns seems a reasonable approach since market returns are unlikely to change significantly due to the revision of a single share. That is crucial, given that this identification assumption basically relies on the market portfolio as a control group weighted according to the estimated model parameters. Thus, the stable unit treatment value assumption (SUTVA), i.e., the outcome of the control may not be affected by the treatment, must hold.

The reasoning behind applying the average over a control period as a benchmark to identify the treatment effect on the variable measuring stock liquidity, i.e., turnover, is twofold: First, the literature on the determinants of stock liquidity suggests that firm characteristics like company size and ownership structure as well as the number of available market makers and analysts covering the stock affect the liquidity of securities (Benston and Hagerman, 1974; Easley et al., 1998; Heflin and Shaw, 2000; Irvine, 2003; Lipson and Mortal, 2007; Wahal, 1997). These factors are rigid and can be considered fixed effects in a 30-day period. Moreover, liquidity measures are highly sensitive to new information in the short-term. Averaging over a control period balances out the associated fluctuations. Hence, equation (2.7) estimates the turnover that surpasses the expected level based on the underlying fundamentals in the absence of new information. Note, to use contemporaneous data of a control group portfolio would require comprehensive data on all news events for these companies on the days of treatment to account for any confounding short-term effects. That would require an extensive data gathering process. Yet, evidence that control group data would result in better estimates for turnover levels is, to the best of my knowledge, lacking.

However, averaging over the control period comes at the cost of not accounting for calendar time effects, i.e., events that affect the entire market. Averaging over the different trading days should mitigate the associated bias as long as revisions in the sample are not severely clustered on specific days during my sample period.

Another point of criticism might concern the assumption that banks and brokers indeed disclose information about a forthcoming revision to a selected group of investors before its publication. Instead, investors might legitimately be privy to the same information the analyst changing the recommendation is. That would imply the information is widely available. Then, other analysts would likely also revise their recommendations for this specific stock around the same time. Hence, excluding recommendation changes for which there are revisions by other analysts around the same time should address this issue.

Similarly, a reverse causality problem arises if the observed treatment effect before the recommendation change publication is why an analyst revises this particular stock recommendation. However, this seems highly unlikely given the previously mentioned time intensity of the research process leading up to a revision publication.

Lastly, the research design does not control for time lags in the response of investors to the disclosed information. It is possible that, e.g., $\widehat{ATT(Tr)}^{-2}$ underestimates the effect of the information leakage two days before the official publication if market participants choose to trade on this information only on the following day. However, this is a rather unlikely scenario given that trading on an informational advantage is time-sensitive since stock prices adjust as soon as other market participants can gather the same information and trade accordingly.

2.4.4 Testing

In line with previous research on tipping, I analyze positive recommendation changes (i.e., upgrades) and negative recommendation changes (i.e., downgrades) separately since they will differ in their directional effect on returns. To assess the impact the introduction of MiFID II on January 3, 2018, has had on the practice of tipping, I follow the approach of Höfer and Oehler (2014) in their analysis of the effectiveness of MAD and MiFID and split my data into a pre- and a post-regulation sub-sample. Then, I estimate the average treatment effects on the treated according to equations (2.5) and (2.7) for the two sub-groups over the two time periods for each event day and test the null hypothesis

$$H_0: \quad ATT(Tr)^{j}_{p,rec} = 0, \tag{2.8}$$

for $j = -3, \ldots, 0$, $Tr = \{R, To\}$, $p = \{pre, post\}$, and $rec = \{up, down\}$. Hence, I test the null hypothesis that the cross-sectional average of the measure for daily abnormal returns or turnover over all up- or downgrades in the respective subperiod equals zero for each event day.

Since my sample comprises recommendation changes for different stocks possibly published on the same day, and because there can be multiple recommendation publications for the same share over time, I cluster standard errors on stock and date (Kadan et al., 2018).

However, this does not account for common firm characteristics like, for instance, belonging to the same industry. Thus, my sample likely suffers from cross-sectional residual correlation. To address this issue, I test the null hypothesis (equation (2.8)) using the non-parametric generalized rank (GRANK) test by Kolari and Pynnonen (2011). In addition to not relying on any distributional assumption, the test is also robust against autocorrelation and cross-correlation caused by event day clustering. Moreover, it accounts for event-induced volatility, i.e., the higher standard deviation in the event period due to the event effect on the trading measure. The generalized rank test is superior in performance to parametric and previous non-parametric tests in an event study framework (Kolari and Pynnonen, 2011; Pacicco et al., 2018).

If analysts pre-released information about upcoming recommendation changes to a sub-group of investors who traded on this information before the introduction of MiFID II and if the unbundling of research fees has indeed reduced the incentives to engage in tipping, the test results should show a rejection of the null hypothesis for the sub-sample *pre* for $j = -3, \ldots, -1$ but no rejection for $j = -3, \ldots, -1$ in the *post*-sub-sample.

Note that the absence of significant treatment effects for $j = -3 \ldots, -1$ does not necessarily imply that tipping does not occur. It is also possible that information on forthcoming recommendation changes is disclosed early but informed investors choose not to trade on this inside information. I can only infer that I cannot reject the null hypothesis that no abnormal trading took place. However, insider information only results in adverse effects on the market when market participants trade on it. Therefore, the inference suffices to evaluate the extent of the problem and the effectiveness of the regulation.

2.4.5 Data on analyst recommendations and stocks

I include all recommendation changes published for shares listed in the STOXX Europe 600 from 04'2016 to 09'2019. First, this ensures that the two periods determining my sub-samples - pre-MiFID II from April 1, 2016, to January 2, 2018, and post-MiFID II from January 3, 2018, to September 30, 2019 - are of approximately the same length. Second, European shares are more likely to be covered by European research producers who must comply with MiFID II. For prudence, I drop all recommendation changes published by investment banks and full-service brokers for which I cannot verify that their headquarters are in a European country. Moreover, I exclude all recommendation changes published by independent research houses. These companies do not offer sales and trading services. Thus, they have charged fixed prices or have provided their reports via a subscription model even before the introduction of MiFID II. Hence, they should not be affected by the unbundling of research from sales and trading fees. I retrieve the information about the publication date, the originator, and the recommendation from the database IBES Thomson Reuters. All stock data, additional information on the companies, and data on STOXX Europe 600 index returns, which serve as market returns in equation (2.5), is available via Thomson Reuters Eikon.

I exclude all stocks with an average quoted price below five euros over the sample period since such shares likely suffer from sparse coverage and low liquidity. That is in line with the five US dollars lower bound used in previous studies on recommendation changes based on US data (Brown et al., 2014; Irvine et al., 2007; Juergens and Lindsey, 2009; Madureira and Underwood, 2008). Furthermore, I must dismiss all recommendation changes for which necessary data on the stock and company is missing over the control and/or event period (Amiram et al., 2016; Blau and Wade, 2012; Madureira and Underwood, 2008). I follow Blau and Wade (2012) and only include common stocks in my analysis.

I follow previous studies on tipping (e.g., Juergens and Lindsey, 2009) and consider a recommendation change to be an upgrade if an analyst adjusts the recommendation from (strong) sell to hold or from hold to (strong) buy. Similarly, I define a downgrade to be a recommendation change from (strong) buy to hold or from hold to (strong) sell.

I base my analysis on recommendation changes, whereas Irvine et al. (2007) argue that focusing on coverage initiations would reduce the likelihood of distorted results due to confounding factors. However, Juergens and Lindsey (2009) believe that endogenous factors drive the analysts' decisions to initiate stock coverage. Moreover, ongoing research rather than initiations generates the majority of costs and income.

TABLE 2.1: Data sets

	Brokers	Revisions per broker				Revisions
		Mean	SD	Min	Max	
Revisions	77	106.0	75.2	1	226	2,712
Upgrades	73	56.1	39.1	1	117	1,358
Pre MiFID II	68	35.5	30.0	1	94	755
Post MiFID II	59	28.3	21.6	1	64	603
Downgrades	72	50.5	35.3	1	109	1,354
Pre MiFID II	64	29.4	23.6	1	79	747
Post MiFID II	57	25.6	18.8	1	60	607
	Stocks	**Revisions per stock**				**Revisions**
		Mean	SD	Min	Max	
Revisions	537	7.6	4.2	1	19	2,712
Upgrades	470	4.1	2.3	1	13	1,358
Pre MiFID II	382	2.7	1.5	1	8	755
Post MiFID II	339	2.3	1.2	1	5	603
Downgrades	468	4.3	2.4	1	12	1,354
Pre MiFID II	372	2.8	1.6	1	7	747
Post MiFID II	341	2.5	1.4	1	6	607
	Stocks	**Events per stock**				**Events**
		Mean	SD	Min	Max	
Placebo events	589	11.2	6.0	1	36	4,719
Positive return events	557	6.4	3.5	1	20	2,524
Pre MiFID II	473	3.5	2.0	1	10	1,194
Post MiFID II	504	3.7	2.0	1	10	1,330
Negative return events	540	5.7	3.1	1	19	2,195
Pre MiFID II	448	3.2	1.9	1	10	1,030
Post MiFID II	477	3.3	1.7	1	10	1,165

Note: First, this table displays the number of recommendation changes included in the baseline data set and the number of brokers that produced the research. The second part of the table shows the number of stocks the recommendation changes concerns. The bottom part displays the placebo data set.

I apply several screening criteria to control for confounding factors and isolate the effect of a recommendation change on trading from other firm-specific news. I do not include a recommendation change if during the buffer-period or on the event days: 1. another analyst publishes a revision for the same stock; 2. the company announces earnings or dividends; 3. the company informs the shareholders about an upcoming stock split, buyback, or exchange offer; 4. a (de-)merger, an investment or an acquisition is announced; 5. a company's management holds an analyst meeting or adjusts their guidance.[16]

In previous research on tipping, merely the first two screening criteria are typically applied (e.g., Amiram et al., 2016; Chen and Cheng, 2006; Irvine et al., 2007;

[16]Note, if a corporate announcement is released after 4 pm, I allocate the event to the following day as market participants will only be able to trade on the information once the markets reopen (Juergens and Lindsey, 2009). There is no time data available for mergers and investments announcements.

Juergens and Lindsey, 2009; Kadan et al., 2018). However, the latter three types of corporate announcements might also affect trading and the decision to change a recommendation. Not controlling for these kinds of events likely results in biased estimates. Data on corporate announcements are also available via IBES Thomson Reuters.

The final pre-processed data set comprises 2,712 recommendation changes published by 77 different brokers for 537 stocks. Table 2.1 shows that the number of recommendation changes, brokers, and stocks is split evenly between up- and downgrades. There is, however, a reduction in the sample size from the pre- to post-period. At first, this seems to confirm concerns raised by, e.g., Goldstein et al. (2009), that the unbundling of research fees and execution commissions could result in more specialization, competition, and concentration in the market for brokerage services. A recent survey by the CFA Institute[17] assessing the impact MiFID II might have on the investment research industry paints an even bleaker picture. Based on the responses of almost 500 participants working in various positions within the financial services industry, the survey indicates that research budgets have diminished, resulting in increased competition and rising concerns over research quality. Furthermore, respondents report a decrease in coverage of small- and medium-sized companies, in particular, as well as a reduction in analyst jobs.

However, the raw data for 2017 and 2018 (i.e., before pre-processing and screening) reveals that the overall number of published recommendation changes did not decrease after the MiFID II introduction. The size discrepancy after pre-processing stems from different intrayear sample periods: The pre-MiFID II sample only includes the second quarter of 2017, whereas the post-MiFID II sample spans Q2'2018 and Q2'2019. Since there are many public holidays from April-June, which unfortunately differ across European countries, I have to exclude roughly half of all revisions published in the second quarters due to missing data.

Lastly, I run fixed-effects regressions for each of the four sub-groups to check for time trends over the control period to substantiate the identification assumption for turnover. I cannot reject the null hypothesis of no linear time trend based on clustered standard errors in each regression.

Table 2.2 presents summary statistics for returns and turnover based on daily values for the control period.

[17] Available via URL: *https://www.cfainstitute.org/-/media/documents/survey/cfa-mifid-II-survey-report.ashx*.

TABLE 2.2: Summary statistics

	Return [%]				Turnover [%]			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Revisions	0.0403	1.71	-38.56	41.52	0.3290	0.62	0.00	35.29
Upgrades	-0.0187	1.73	-38.56	28.16	0.3347	0.65	0.00	35.29
Pre MiFID II	0.0128	1.70	-28.31	28.16	0.3525	0.80	0.01	35.29
Post MiFID II	-0.0580	1.76	-38.56	17.79	0.3125	0.38	0.00	12.31
Downgrades	0.0995	1.69	-27.29	41.52	0.3234	0.58	0.00	32.40
Pre MiFID II	0.1227	1.63	-27.29	41.52	0.3353	0.69	0.01	32.40
Post MiFID II	0.0709	1.75	-26.12	17.89	0.3086	0.42	0.00	12.51
Placebo events	0.0334	1.65	-27.29	45.28	0.3091	0.58	0.00	41.76
Positive return events	0.0256	1.70	-27.29	45.28	0.3124	0.63	0.00	41.76
Pre MiFID II	0.0442	1.69	-27.29	28.28	0.3429	0.86	0.00	41.76
Post MiFID II	0.0090	1.71	-21.67	45.28	0.2850	0.27	0.00	9.83
Negative return events	0.0423	1.58	-26.62	22.15	0.3053	0.52	0.00	20.23
Pre MiFID II	0.0830	1.54	-23.79	20.43	0.3146	0.69	0.00	20.23
Post MiFID II	0.0063	1.63	-26.62	22.15	0.2970	0.30	0.00	12.51

Note: This table presents summary statistics for daily returns and turnovers based on data for the control periods.

2.4.6 Placebo events

Following Kadan et al. (2018), I create a placebo group consisting of the previously mentioned corporate announcements, which are neither likely to be subject to tipping nor should trading around them be affected by MiFID II. I also add recommendation changes published by independent research firms to the placebo group.

I differentiate between positive and negative return events. The former group includes upgrades published by independent analysts and buyback announcements since they are likely to increase returns. Stock splits, exchange offers, and independent downgrades are negative return events. The type of news does not predetermine the direction of the return effect for the remaining corporate announcements. Therefore, the sign of abnormal returns on the announcement day defines group assignment.

I apply the same pre-processing and screening procedure used for the baseline data to the placebo set. Performing equivalent treatment effect estimations and tests presented in Section 2.4.2 and 2.4.4 should not result in the rejection of H_0 defined in equation (2.8) for $j = -3, \ldots, -1$. That must hold for tests based on both the pre- or post-MiFID II sub-sample.

The placebo set comprises 4,719 events for 589 stocks. Table 2.1 shows that the number of placebo events increases from pre- to post-MiFID II. While the negative intrayear sample split effect described for the recommendation change data set applies as well, there are two positive developments from one sample period to the other, off-setting the sample size reduction. First, the number of recommendation changes published by independent research firms increased after MiFID II became law. Second, companies typically report annual earnings in the first quarter of a calendar year, a period included twice in the post-sample but only covered once in the pre-sample.

The bottom part of Table 2.2 presents the respective summary statistics for returns and turnover based on daily averages for the control period. There are no substantial differences compared to the summary statistics of the recommendation change data set.

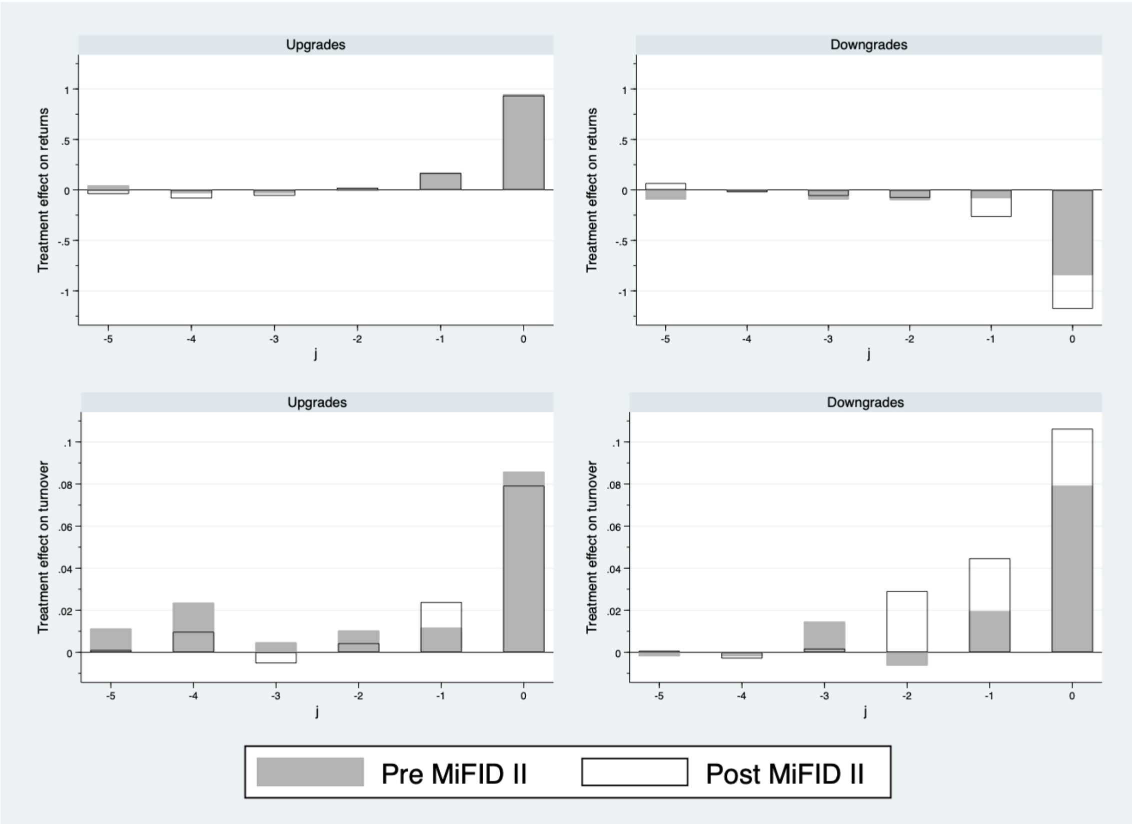
2.5 Results

2.5.1 Tipping and the effect of MiFID II

Figure 2.1 shows treatment effects on returns and turnover in percentage points estimated according to equations (2.5) and (2.7), respectively, for the baseline sample. The figure visualises estimation results for upgrades and downgrades, pre- and post-MiFID II, over the buffer and event period leading up to and including the publication of recommendation changes.

As expected, treatment effects on returns are positive (negative) on the day of the publication of upgrades (downgrades). Similarly, turnover increases on publication day to levels higher than expected in the absence of new information. Both trading measures also indicate abnormally high activities on the day preceding the publication of up- and downgrades, although to a smaller extent. Upon visual inspection, estimated treatment effects on returns do not suggest tipping in earlier days. In contrast, turnover also appears to be higher than expected in a no-news environment multiple days before the recommendation publication for some sub-groups. The introduction of MiFID II does not systematically alter any of these findings. If anything, post-MiFID II levels of abnormal trading appear to be even higher, particularly for downgrades.

FIGURE 2.1: Treatment effects due to tipping



Note: This figure displays treatment effects on returns (upper plots) and turnover (bottom plots) in percentage points estimated according to equations (2.5) and (2.7), respectively, for the baseline samples presented in Table 2.1. Note, the bar charts also display estimates for the buffer period, i.e., for $j = -4, -5$.

Test results presented in Table 2.3 largely corroborate these descriptive findings. First, my estimation results confirm previous findings by, e.g., Bradley et al. (2014) and Michaely and Womack (2005), that analysts' revisions do have a significant stock return impact on the day they are made available to the public. I find stock returns increase on average by 0.96 and 0.94 percentage points in response to the publication of an upgrade in the pre- and post-MiFID II period, respectively. That translates to more than half a standard deviation of control period returns ($\text{SD}(R_i) = 1.71$) with average returns close to zero, i.e., 0.04% (see Table 2.2). The effects are highly statistically significant according to the GRANK-test results.

Similarly, stock returns are on average 0.84 and 1.18 percentage points lower on a day a share is downgraded compared to the expected returns without a recommendation change pre- and post-MiFID II, respectively. That corresponds to half and more than two-thirds of the respective standard deviation mentioned above. Hence, the effects are economically meaningful. Again, I can reject the null-hypothesis of no mean effect at a 1%-level. These findings substantiate the argument that the knowledge about the publication of a recommendation change has a substantial share price effect and, thus, constitutes inside information.

TABLE 2.3: Trading due to tipping: baseline ATT estimates and test results

Upgrades	$j=0$	$j=-1$	$j=-2$	$j=-3$
$\widehat{ATT(R)}_{pre}$	0.9580***	0.1749***	-0.0109	-0.0332
	(0.0790)	(0.0537)	(0.0596)	(0.0646)
t_{GRANK}	16.4506	3.9491	-0.9370	0.3472
$\widehat{ATT(R)}_{post}$	0.9355***	0.1654***	0.0219	-0.0584
	(0.0839)	(0.0769)	(0.0681)	(0.0809)
t_{GRANK}	11.0914	2.5113	-0.3888	-0.3156
$\widehat{ATT(To)}_{pre}$	0.0859***	0.0117***	0.0104***	0.0047**
	(0.0108)	(0.0119)	(0.0114)	(0.0120)
t_{GRANK}	16.2827	3.1359	3.5119	2.4417
$\widehat{ATT(To)}_{post}$	0.0793***	0.0239**	0.0043**	-0.0053
	(0.0172)	(0.0170)	(0.0099)	(0.0109)
t_{GRANK}	10.6185	2.3759	1.8163	0.9322
Downgrades				
$\widehat{ATT(R)}_{pre}$	-0.8432***	-0.0818	-0.1013*	-0.0955
	(0.0802)	(0.0658)	(0.0582)	(0.0688)
t_{GRANK}	-14.6172	-0.3776	-1.7116	-0.6476
$\widehat{ATT(R)}_{post}$	-1.1784***	-0.2675***	-0.0789	-0.0600
	(0.1057)	(0.0795)	(0.0679)	(0.0698)
t_{GRANK}	-18.2473	-3.3640	0.7223	-1.6380
$\widehat{ATT(To)}_{pre}$	0.0792***	0.0195**	-0.0063	0.0146**
	(0.0124)	(0.0140)	(0.0145)	(0.0192)
t_{GRANK}	14.3483	2.6421	1.4583	2.1927
$\widehat{ATT(To)}_{post}$	0.1063***	0.0447***	0.0291*	0.0018
	(0.0152)	(0.0164)	(0.0137)	(0.0074)
t_{GRANK}	13.1595	2.6648	1.3568	0.4861

Note: This table reports the estimated treatment effects on the treated for the outcome variables returns and turnover in percentage points estimated according to equations (2.5) and (2.7), respectively. STOXX Europe 600 index returns serve as market returns. For details on the number of observations in each subgroup, see Table 2.1. Standard errors reported in parenthesis are clustered on stock and date. t_{GRANK} reports test statistics for the GRANK test (H_0: no mean effect) with *** p<0.01, ** p<0.05, * p<0.1. For details on the test procedure, see Kolari and Pynnonen (2011).

The estimated treatment effect on returns one day before the publication of an upgrade is 0.18 percentage points for the pre-MiFID II period. This effect is highly statistically significant and corresponds to an increase of 0.1 standard deviations of control period returns. That suggests that trading of selected investors based on disclosed information one day before the recommendation change becomes public resulted in statistically significant higher returns on that day than expected without tipping. Similarly, I find a statistically significant effect of 0.17 for $j = -1$ for the post-MiFID II period. There is little evidence for abnormal returns at earlier event days for neither sub-period. Furthermore, I find a negative treatment effect on returns for $j = -1$ of -0.27 percentage points (significant at 1%-level) for downgrades published after January 3, 2018, and -0.10 percentage points (significant at 10%-level) for $j = -2$ for the pre-period.

The estimated ATTs for turnover confirm the effect a publication of a recommendation change has on trading. Turnover increases on average between 0.08 and 0.11 percentage points on publication day in each sub-set. That translates to an increase of 0.13 and 0.18 standard deviations of turnover during the control period (SD(To_i) $= 0.62$) with an average of 0.33% (see Table 2.2). GRANK test results provide evidence for the statistical significance of these ATTs at the 1%-level. Treatment effect estimates for $j = -1$ indicate turnover levels to be on average 0.01 to 0.05 percentage points higher than expected without information disclosure concerning the next day's recommendation change publication. While these effects are less economically meaningful than the findings for returns, they are statistically significant at a level of at least 5%, according to GRANK test results. There is also some indication for tipping in earlier days based on the average treatment effects on turnovers for $j = -2, -3$, more so for upgrades than downgrades. Yet, this evidence is less conclusive.

Overall, the results suggest that banks and brokers give selected market participants hints about upcoming recommendation changes, who then engage in profitable trading based on this information, which meets the criteria for insider trading given its significant price effect once it is available to all market participants. For the most part, the trading activity due to tipping seems to take place one day before the recommendation change is officially published, and it seems to be more pronounced for up- than downgrades. Unreported t-test results largely confirm these findings.

However, I find trading activity due to tipping to be more limited than suggested by previous research. One reason could be the more restrictive screening criteria I apply (see Section 2.4.5). Thus, news events for which previous studies do not control could be a driver for their findings. I examine this hypothesis in the robustness checks in Section 2.5.3. Another explanation could be the fact that previous research mainly relies on US data. Thus, past European regulations, like

MiFID I and MAD, possibly already reduced the conflict of interest between the profit motives of the respective financial institutions and the public's interest in a well-functioning financial market, as suggested by Prokop and Kammann (2018) (see Section 2.3.1).

Most importantly, I find no evidence that the introduction of MiFID II had any mitigating effect on the practice of tipping. There is no systematic reduction in magnitude nor level of significance in the results over time. Changing the payment structure for research proves ineffective in preventing tipping.

2.5.2 Placebo tests with events not subject to tipping

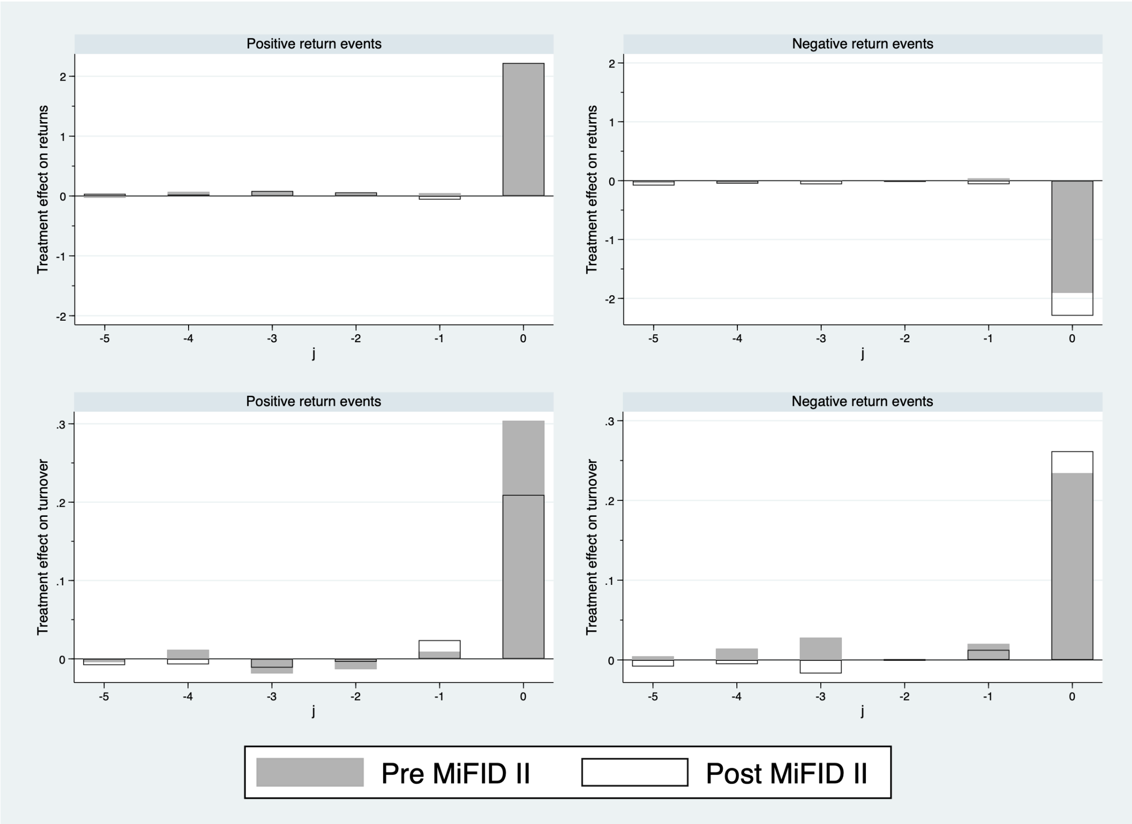
Figure 2.2 displays estimated treatment effects on returns and turnover in percentage points estimated according to equations (5) and (7), respectively, for the positive and negative return placebo events not subject to tipping instead of recommendation changes.[18] The figure indicates that market participants cannot generate returns above the expected levels without information disclosure before the actual announcement day. Yet, there is some indication for trading in earlier days based on the turnover treatment effects, though at a comparatively small scale.

Table 2.4 confirms this preliminary assessment. First, the extent of estimated treatment effects on the announcement day for both positive and negative return events is considerably larger than those for up- and downgrades. That is not surprising, given that these events include major corporate announcements like, for instance, earnings releases. I do not find significant treatment effects for returns one day before the placebo event announcement. That holds for both positive and negative return events, pre- and post-MiFID II. I find positive (weakly) significant effects for $j = -2, -3$ for positive return events after MiFID II became law. However, these are relatively small (0.06 and 0.09 percentage points) compared to both the placebo event effect for $j = 0$ and the baseline findings for recommendation changes.

The results show positive and (highly) statistically significant ATTs for turnover for $j = -1$ across all four subgroups. That is likely because earnings releases, dividend announcements, and analyst meetings are typically scheduled well in advance. Such events account for a large part of the placebo set (> 75%).

[18]For the definitions of positive and negative return events, see Section 4.6.

FIGURE 2.2: Treatment effects due to events not subject to tipping



Note: This figure displays treatment effects on returns (upper plots) and turnover (bottom plots) in percentage points estimated according to equations (2.5) and (2.7), respectively, but for placebo events instead of recommendation changes. Table 2.1 presents the data set. Note, the bar charts also display estimates for the buffer period, i.e., for $j = -4, -5$. For the definitions of positive and negative return events, see Section 2.4.6.

Hence, the results seem to suggest that market participants do trade significantly more over the days leading up to an anticipated corporate announcement compared to times of no expected news. But they do not seem to have an informational advantage, which would allow them to generate abnormal returns.[19]

In summary, the placebo tests substantiate the argument that early informed market participants must be driving abnormal returns before the publication of up- or downgrades. Moreover, I do not find systematic differences between the pre- and post-placebo subset, alleviating potential concerns that the apparent ineffectiveness of MiFID II found in the baseline results could be due to any general temporal changes in the market.

[19]Unreported results for the placebo set without scheduled events support this line of reasoning. I no longer find any significant turnover treatment effects for the days preceding the ad hoc events, while the ATTs remain statistically significant for the actual announcement day. However, excluding all earnings and dividend announcements and analyst meetings from the sample results in a small and unbalanced data set.

TABLE 2.4: Placebo set ATT estimates and test results

Positive return events	$j=0$	$j=-1$	$j=-2$	$j=-3$
$\widehat{ATT(R)}_{pre}$	2.2209***	0.0471	0.0442	0.0724
	(0.1204)	(0.0416)	(0.0360)	(0.0353)
t_{GRANK}	32.1774	0.8523	1.4036	1.12247
$\widehat{ATT(R)}_{post}$	2.2209***	-0.0602	0.0622*	0.0848**
	(0.1332)	(0.0441)	(0.0419)	(0.0444)
t_{GRANK}	33.9530	-1.2095	1.8526	2.0734
$\widehat{ATT(To)}_{pre}$	0.3040***	0.0091**	-0.0132	-0.0187
	(0.0413)	(0.0071)	(0.0079)	(0.0104)
t_{GRANK}	10.1990	2.6236	0.2196	-0.4283
$\widehat{ATT(To)}_{post}$	0.2093***	0.0239**	-0.0038	-0.0113
	(0.0216)	(0.0087)	(0.0058)	(0.0050)
t_{GRANK}	8.9390	2.7000	0.2030	-0.6075
Negative return events				
$\widehat{ATT(R)}_{pre}$	-1.9069***	0.0396	-0.0236	-0.0077
	(0.0921)	(0.0422)	(0.0442)	(0.0367)
t_{GRANK}	-30.1053	0.1619	-0.8495	-0.2860
$\widehat{ATT(R)}_{post}$	-2.2951***	-0.0602	-0.0183	-0.0626
	(0.1409)	(0.0495)	(0.0410)	(0.0439)
t_{GRANK}	-28.3855	-1.2687	-1.5712	-1.2935
$\widehat{ATT(To)}_{pre}$	0.2343***	0.0203***	-0.0001	0.0280
	(0.0244)	(0.0094)	(0.0106)	(0.0358)
t_{GRANK}	11.3253	3.7847	0.6448	0.1688
$\widehat{ATT(To)}_{post}$	0.2618***	0.0127***	0.0000	-0.0170
	(0.0262)	(0.0062)	(0.0094)	(0.0058)
t_{GRANK}	11.6078	3.1585	0.4372	-1.6628

Note: This table reports the estimated treatment effects on the treated for the outcome variables returns and turnover in percentage points estimated according to equations (2.5) and (2.7), respectively, for the placebo data set. STOXX Europe 600 index returns serve as market returns. For details on the number of observations in each subgroup, see Table 2.1, and for the definition of positive and negative return events, see Section 2.4.6. For details on the number of observations in each subgroup, see Table 2.1. Standard errors reported in parenthesis are clustered on stock and date. t_{GRANK} reports test statistics for the GRANK test (H_0: no mean effect) with *** p<0.01, ** p<0.05, * p<0.1. For details on the test procedure, see Kolari and Pynnonen (2011).

2.5.3 Robustness checks

I perform several robustness tests to check the validity of my findings.

First, I rerun the analysis only controlling for other recommendation changes and earnings announcements as the vast majority of previous studies on tipping does (e.g., Busse et al., 2012; Brown et al., 2014; Christophe et al., 2010; Irvine et al., 2007; Juergens and Lindsey, 2009; Kadan et al., 2018).

Table 2.5 shows that most of my findings become more pronounced. That suggests that other corporate announcements on dividends, stock splits, buybacks, (de-)mergers, exchange offers, investments, acquisitions, and informational events like analyst meetings and guidance calls result in trading activities unrelated to tipping. However, the changes are only small and do not fully account for the differences to previous studies, leaving the possible explanation that earlier regulatory changes in Europe might have already been effective in reducing tipping (see Section 2.4.3).

TABLE 2.5: Robustness test results:
less restrictive screening criteria

Upgrades	$j=0$	$j=-1$	$j=-2$	$j=-3$	N
$\widehat{ATT(R)}_{pre}$	0.9810***	0.2041***	0.0101	-0.0210	
	(0.0895)	(0.0543)	(0.0585)	(0.0620)	
t_{GRANK}	15.4455	4.1742	-0.9671	0.6152	801
$\widehat{ATT(R)}_{post}$	1.0037***	0.2002***	0.0252	-0.0414	
	(0.0984)	(0.0770)	(0.0682)	(0.0808)	
t_{GRANK}	12.0446	2.9378	-0.2453	-0.3749	625
$\widehat{ATT(To)}_{pre}$	0.0920***	0.0165***	0.0146***	0.0049**	
	(0.0114)	(0.0113)	(0.0109)	(0.0113)	
t_{GRANK}	16.5686	3.9524	3.9491	2.6531	806
$\widehat{ATT(To)}_{post}$	0.0863***	0.0240**	0.0045*	-0.0055	
	(0.0201)	(0.0165)	(0.0096)	(0.0106)	
t_{GRANK}	10.8202	2.5710	2.0372	0.8912	636
Downgrades					
$\widehat{ATT(R)}_{pre}$	-0.8178***	-0.0957	-0.1047**	-0.0789	
	(0.0847)	(0.0694)	(0.0557)	(0.0694)	
t_{GRANK}	-16.4448	-0.5135	-2.0997	-0.3997	801
$\widehat{ATT(R)}_{post}$	-1.1128***	-0.2111***	-0.0975	-0.0806*	
	(0.1072)	(0.0891)	(0.0645)	(0.0722)	
t_{GRANK}	-17.8823	-3.3816	0.3493	-1.7430	625
$\widehat{ATT(To)}_{pre}$	0.1163***	0.0326***	0.0009***	0.0185***	
	(0.0196)	(0.0142)	(0.0142)	(0.0188)	
t_{GRANK}	17.0444	4.0989	2.8986	3.1174	806
$\widehat{ATT(To)}_{post}$	0.12319***	0.1027***	0.0341*	0.0060	
	(0.0156)	(0.0338)	(0.0157)	(0.0087)	
t_{GRANK}	13.4053	3.2949	1.4178	0.5274	636

Note: This table reports the estimated treatment effects on the treated for the outcome variables returns and turnover in percentage points estimated according to equation (2.5) and (2.7), respectively, if only recommendation changes are excluded for which another recommendation change or an earnings announcement is published during the event and buffer period. The total sample size increases to 2,868 revisions. STOXX Europe 600 index returns serve as market returns. Standard errors reported in parenthesis are clustered on stock and date. t_{GRANK} reports test statistics for the GRANK test (H_0: no mean effect) with *** p<0.01, ** p<0.05, * p<0.1. For details on the test procedure, see Kolari and Pynnonen (2011).

TABLE 2.6: Robustness test results:
different identification assumptions for returns

Upgrades	$j=0$	$j=-1$	$j=-2$	$j=-3$	
$\widehat{ATT(R)}_{pre}$	0.9347***	0.1503***	-0.0305	-0.0646	Market model & market returns
	(0.0742)	(0.0526)	(0.0585)	(0.0665)	
t_{GRANK}	16.0635	3.5956	-1.0131	0.0402	
$\widehat{ATT(R)}_{post}$	0.9009***	0.1111**	-0.0454	-0.1090	
	(0.0816)	(0.0790)	(0.0666)	(0.0796)	
t_{GRANK}	10.2427	2.2877	-0.7417	-0.4684	
$\widehat{ATT(R)}_{pre}$	0.9415***	0.1587***	-0.0170	-0.0492	Single-index model & sice-decile returns
	(0.0779)	(0.0528)	(0.0600)	(0.0640)	
t_{GRANK}	16.5639	3.7800	-1.2189	-0.3238	
$\widehat{ATT(R)}_{post}$	0.9211***	0.1446**	0.0392	-0.0678	
	(0.0849)	(0.0751)	(0.0654)	(0.0781)	
t_{GRANK}	10.7827	2.3415	-0.3099	-0.9362	
$\widehat{ATT(R)}_{pre}$	0.9135***	0.1230***	-0.0536	-0.0825	Market model & sice-decile returns
	(0.0735)	(0.0523)	(0.0598)	(0.0652)	
t_{GRANK}	15.9963	3.4164	-1.3720	-0.2731	
$\widehat{ATT(R)}_{post}$	0.8742***	0.1117**	-0.0442	-0.1228	
	(0.0808)	(0.0774)	(0.0643)	(0.0757)	
t_{GRANK}	9.9497	2.6222	-0.4296	-0.6404	
Downgrades					
$\widehat{ATT(R)}_{pre}$	-0.7732***	0.0052	-0.0273*	-0.0031	Market model & market returns
	(0.0787)	(0.0634)	(0.0569)	(0.0714)	
t_{GRANK}	-14.3802	-0.2030	-1.8847	-0.3633	
$\widehat{ATT(R)}_{post}$	-1.1170***	-0.2290***	-0.0183	0.0137	
	(0.1043)	(0.0799)	(0.0678)	(0.0686)	
t_{GRANK}	-16.5339	-3.5421	0.3600	-1.3781	
$\widehat{ATT(R)}_{pre}$	-0.8456***	-0.0807	-0.1005**	-0.0776	Single-index model & sice-decile returns
	(0.0773)	(0.0636)	(0.0583)	(0.0663)	
t_{GRANK}	-16.6995	-0.5360	-2.2516	-0.3402	
$\widehat{ATT(R)}_{post}$	-1.1498***	-0.2521***	-0.0885	-0.03403	
	(0.1005)	(0.0765)	(0.0664)	(0.0686)	
t_{GRANK}	-18.2152	-3.6066	0.4444	-0.9063	
$\widehat{ATT(R)}_{pre}$	-0.7811***	-0.0176	-0.0503**	-0.0171	Market model & size-decile returns
	(0.0750)	(0.0614)	(0.0566)	(0.0691)	
t_{GRANK}	-16.2216	-0.4738	-2.2624	-0.4508	
$\widehat{ATT(R)}_{post}$	-1.1033***	-0.2244***	-0.0323	0.0249	
	(0.0996)	(0.0791)	(0.0670)	(0.0669)	
t_{GRANK}	-18.3634	-3.8071	0.3382	-0.8601	

Note: This table reports the estimated treatment effects on the treated for the outcome variables returns in percentage points estimated according to equation (2.2) with the identification assumption specified in equations (2.4.1), (2.4.2), and (2.4.3). STOXX Europe 600 index returns serve as market returns. Standard errors reported in parenthesis are clustered on stock and date. t_{GRANK} reports test statistics for the GRANK test (H_0: no mean effect) with *** p<0.01, ** p<0.05, * p<0.1. For details on the test procedure, see Kolari and Pynnonen (2011).

Next, I analyze the sensitivity of the finding to my identification assumption for the return treatment effect. Most previous studies on the impact of tipping and insider trading rely on a simplified version of the single-index model to estimate expected returns. For the market-adjusted model, the parameters in equation (2.3) become $\alpha_i = 0$ and $\beta_i = 1$ (e.g., Jeng et al., 2003; Kadan et al., 2018). That changes the identification assumption stated in equation (2.4) to

$$E[R^0_{i,t_{n_i}+j}|D^j_i = 1] = E[R_{m,t_{n_i}+j}|D^j_i = 1]. \tag{2.4.1}$$

Blau and Wade (2012) and Irvine et al. (2007) estimate abnormal returns using the mean return of all firms in the same size decile rather than the total market return, resulting in the identification assumptions

$$E[R^0_{i,t_{n_i}+j}|D^j_i = 1] = E[\alpha_i + \beta_i R_{sd_i,t_{n_i}+j}|D^j_i = 1] \tag{2.4.2}$$

for the single-index model, and

$$E[R^0_{i,t_{n_i}+j}|D^j_i = 1] = E[R_{sd_i,t_{n_i}+j}|D^j_i = 1] \tag{2.4.3}$$

for the market-adjusted model, where R_{sd_i} is the average return across all firms in the same size decile as stock i. Table 2.6 presents the respective treatment effects and test results. The baseline results prove robust against these alterations of the identification assumption, both in size and significance level.

Moreover, I address the potential bias due to event day clustering (see Section 2.4.3). The average number of recommendation changes on each trading day is 3.02 in my sample. I exclude all observations published on a trading day on which there are five revisions or more to ensure that calendar time effects do not drive my results. Table 2.7 shows that my main findings remain unchanged despite the severe reduction of the sample size to 1,702 recommendation changes.

I also evaluate the sensitivity of my findings to the control period length. First, I rerun my analysis using an extended control period starting forty days before the publication and a shortened one beginning twenty days in advance of the official recommendation change announcement. Then, I also use the twenty days after the publication of a recommendation change as the control period, including the two-day buffer period. None of these changes affect my main findings.

TABLE 2.7: Robustness test results: calendar day clustering

Upgrades	$j=0$	$j=-1$	$j=-2$	$j=-3$	N
$\widehat{ATT(R)}_{pre}$	0.9974***	0.2479***	0.0061	-0.0620	
	(0.1017)	(0.0722)	(0.0732)	(0.0875)	
t_{GRANK}	10.7316	3.2497	-1.1714	-0.4143	418
$\widehat{ATT(R)}_{post}$	0.9910***	0.2837**	0.0656	-0.0180	
	(0.1018)	(0.0762)	(0.0808)	(0.0753)	
t_{GRANK}	11.0651	2.7377	-0.0861	-0.0750	450
$\widehat{ATT(To)}_{pre}$	0.0904***	0.0150***	-0.0060**	-0.0022*	
	(0.0168)	(0.0144)	(0.0134)	(0.0138)	
t_{GRANK}	14.2427	2.8409	2.5108	1.7336	403
$\widehat{ATT(To)}_{post}$	0.0933***	0.0258**	0.0084*	-0.0003	
	(0.0211)	(0.0203)	(0.0098)	(0.0106)	
t_{GRANK}	8.2501	2.1192	2.0451	1.6438	431
Downgrades					
$\widehat{ATT(R)}_{pre}$	-1.0067***	-0.1090	-0.1212**	-0.0246	
	(0.1128)	(0.1025)	(0.0815)	(0.0829)	
t_{GRANK}	-13.0188	0.2261	-2.2960	-0.3872	418
$\widehat{ATT(R)}_{post}$	-1.1964***	-0.2142**	-0.0252	-0.0576	
	(0.1063)	(0.0871)	(0.0777)	(0.0833)	
t_{GRANK}	-13.8953	-2.2535	0.9404	-1.3321	450
$\widehat{ATT(To)}_{pre}$	0.0879***	0.0325***	-0.0257	-0.0013**	
	(0.0166)	(0.0245)	(0.0221)	(0.0269)	
t_{GRANK}	13.0672	3.0415	0.4819	2.4300	403
$\widehat{ATT(To)}_{post}$	0.0962***	0.0308	0.0429*	0.0074	
	(0.0178)	(0.0182)	(0.0184)	(0.0099)	
t_{GRANK}	12.7007	1.2449	1.7742	1.5835	431

Note: This table reports the estimated treatment effects on the treated for the outcome variables returns and turnover in percentage points estimated according to equations (2.5) and (2.7), respectively, if only recommendation changes are included that are published on calendar days on which no more than five revisions are reported. The total sample size decreases to 1,702 revisions. STOXX Europe 600 index returns serve as market returns. Standard errors reported in parenthesis are clustered on stock and date. t_{GRANK} reports test statistics for the GRANK test (H_0: no mean effect) with *** p<0.01, ** p<0.05, * p<0.1. For details on the test procedure, see Kolari and Pynnonen (2011).

Lastly, I check for differences in treatment effects depending on the type of up- or downgrade. Roughly 46% of my sample are revisions that result in a "hold" recommendation, i.e., analysts downgrade stocks from "buy" to "hold" and upgrade from "sell" to "hold". Splitting the sample in accordance reveals that there is slightly more evidence for tipping in the subset of upgrades that result in a "buy" recommendation. Similarly, if I only consider downgrades from "buy" to "hold", the estimated treatment effects become more pronounced. That hints towards a greater relevance of revisions that involve "buy" recommendations. However, the overall results remain unchanged.

2.6 Conclusions

This paper provides a first assessment concerning the effectiveness of the so-called unbundling of research and trading fees stipulated by the newly introduced European regulation MiFID II in reducing tipping.

This study provides four main insights by comparing treatment effects of information disclosure about stock revision on returns and turnover, before and after MiFID II became law, based on non-parametric test results. First, the results confirm previous findings that analyst revisions have a significant and economically meaningful impact on stock returns and turnover on the day they become public. That supports the argument that the knowledge about the publication of a recommendation constitutes inside information, and trading based on this information is likely adversely affecting financial markets.

Second, estimated treatment effects on returns and turnover are statistically significant one day before an upgrade gets published, suggesting that a small group of investors knows about the upcoming recommendation change in advance and can make an early profit. Placebo test results substantiate that. They show that returns do generally not move significantly in the expected direction before a corporate announcement or an independent revision is published. There is also some indication for negative treatment effects on returns preceding a downgrade publication. That hints at the possibility for some market participants to mitigate losses based on an informational advantage.

Third, tipping seems to take place less frequently or to a smaller extent than suggested by previous research with US data. That indicates earlier EU regulations might have already been effective in mitigating tipping.

Most importantly, the results do not suggest that the introduction of MiFID II reduced tipping. The unbundling of research fees and sales commissions seems insufficient to overcome misaligned incentives within the financial institution. Given the many different conflicts of interests within large financial institutions partly due to the various services they provide (e.g., Fecht et al., 2018), this could suggest that tipping is not only an opportunity to boost sales commissions but also to build relationships with large institutional clients that might result in income for other divisions of the bank or broker. It might, for example, persuade institutional investors to utilize the bank's corporate banking services, e.g., for leveraged buyouts or mergers and acquisitions. It could also increase the likelihood that these investors will buy into initial public offerings brokered by the financial institution.[20]

Prohibiting large banks and brokers from providing equity research could mitigate the conflict of interest rooted in the variety of services these financial institutions offer their clients. Only advising clients and not releasing reports or recommendations would stop stock prices from moving in an expected direction, which would prevent analysts from creating the inside information in the first place. However, to conclusively assess whether such a limitation on services banks and brokers may provide is expedient, further research is necessary. First, future studies must analyze the direct effects of tipping on the signaling function of share prices to verify the conclusions derived from the literature on insider trading regarding its detrimental impact on financial markets. Moreover, the extent to which a prohibition of sell-side research publications could, in turn, be an impediment to the price discovery process due to a reduction of research production and information sharing - a point of criticism raised by opponents of tipping prohibitions (e.g., Madureira and Underwood, 2008) and insider trading bans (e.g., Meulbroek, 1992) - must be assessed. These benefits and drawbacks should form the basis for future policy decisions while taking the results presented in this paper into account.

[20] Another explanation might be that banks and brokers use the information themselves rather than passing it on to clients, i.e., for proprietary trading (e.g., Juergens and Lindsey, 2009). However, due to regulatory change following the financial crisis, proprietary trading has since become insignificant from an overall profit perspective for a universal bank, rendering this a less likely scenario.

Chapter 3

The adverse effect of contingent convertible bonds on bank stability

3.1 Introduction

When asset values depreciated during the global financial crisis, highly leveraged banks faced severe difficulties raising additional capital needed to meet their debt obligations. Financial instruments that were supposed to absorb losses proved ineffective, and the regulatory system lacked mechanisms to ensure that subordinated creditors and preferential shareholders bear their share of the costs (Basel Committee on Banking Supervision, 2010, 2011). Eventually, national governments bailed out several institutions, which were considered too big to fail, thereby creating a moral hazard problem. Consequently, regulators have implemented higher capital requirements over recent years, which banks try to meet most cost-effectively.

That is where contingent convertible (CoCo) bonds come in. A CoCo bond is a subordinated debt security with a fixed coupon rate. It converts from debt into equity, or it is written-down as soon as a predetermined trigger event occurs. Hence, contingent convertibles can entail a tax shield as long as the trigger event is not met. Unlike other convertible securities, CoCos do not come with an option for the investor or the issuer. Therefore, these bonds can absorb losses, possibly even before the issuer encounters difficulties to recapitalize. Thereby, they can contribute to bank stability (e.g., Barucci and Del Viva, 2012).

An increasing body of theoretic literature suggests that the bond design is decisive for its effect on bank stability (e.g., Ammann et al., 2017). Poorly designed, contingent convertibles could even increase bank-level risk due to a moral hazard problem: If management and stockholders can shift losses to CoCo investors but do not have to share potential profits, this is an incentive to engage in excessive risk-taking (e.g., Avdjiev et al., 2017).

Yet, regulators and legislators have implemented requirements for the design of CoCo bonds to qualify as regulatory capital that are contrary to most of what research suggests is sensible from the stability perspective. In fact, regulators seem to have stripped contingent convertibles of their potential to strengthen banks' capital bases in any meaningful way (Glasserman and Perotti, 2017). Given the cost advantage of this security due to its tax shield over, for instance, preferred shares, which also qualify as additional tier 1 (AT1) capital, it is little surprising that banks increasingly opt to issue CoCo bonds to raise their regulatory capital bases.

That raises the question if issuing CoCo bonds that qualify as additional tier 1 capital according to European regulations increases bank-risk.

I apply a matching-based difference-in-differences approach for a staggered treatment adoption to analyze the effect of 61 AT1 CoCo bond issues over the 2008-2018 period among 251 listed European banks. I obtain annual bank balance sheet data, daily stock data, and information about CoCo bond issuances from Thomson Reuters Eikon.

My analysis reveals that issuing AT1 CoCo bonds results in significantly higher risk-taking one to three years after the issuance compared to the expected development of bank risk would a financial institution not issue AT1 contingent convertibles. The effect is economically large and robust. The data suggest a negative time trend in bank risk which is less pronounced for treated units. I also find tentative evidence indicating the treatment effect on risk might be more pronounced for larger banks and weaker for banks focusing on credit supply. My results substantiate theoretical predictions that outstanding CoCo bonds encourage excessive risk-taking (e.g., Berg and Kaserer, 2015).

Thereby, I close a gap in the empirical literature on the risk-effect of contingent convertibles, which is so far limited and focuses on changes in market participants' anticipation and perception of risk rather than the actual realization at the bank-level over time.

The remainder of this paper is organized as follows: Section 3.2 briefly reviews the relevant literature on CoCo bond design and bank risk. It also describes the respective regulatory and market developments motivating the research question. Section 3.3 introduces the empirical strategy and data sources. Section 3.4 presents the results before Section 3.5 concludes.

3.2 CoCo bond design and risk

CoCo bonds have great potential for contributing to bank stability. By absorbing losses and increasing the equity base in times of financial difficulties without additional outside liquidity, this type of security can restore confidence in the bank's financial well-being, reduce the default probability of a bank and, eventually, prevent bailouts (e.g., Barucci and Del Viva, 2012; Flannery, 2005). However, the realization of this potential crucially depends on the bond's features (e.g., Ammann et al., 2017; Hilscher and Raviv, 2014; Maes and Schoutens, 2012). Poorly designed, contingent convertibles can even increase bank risk due to a moral hazard problem possibly associated with the security: If management and stockholders can shift risks to contingent convertible investors without having to share profits, they face incentives to take on excessive risk (e.g., Allen and Tang, 2016; Avdjiev et al., 2017; Berg and Kaserer, 2015).

3.2.1 Theory on CoCo bond design

Over the past decade, a body of literature has developed analyzing the benefits and shortcoming of distinct design features of contingent convertible bonds concerning the security's potential to contribute to financial stability.

Loss absorption mechanism: Contingent convertibles can absorb losses and appreciate the value of a bank's equity by a write-down or a conversion into a certain number of shares. The main criticism regarding the principal write-down mechanism concerns its implicit reversal of the seniority principle. It leaves bond investors liable before using the remaining equity. That likely encourages the management and shareholders to engage in excessive risk-taking (Avdjiev et al., 2017; Flannery, 2014, 2016; Hesse, 2018; Hilscher and Raviv, 2014).[1] Moreover,

[1] Martynova and Perotti (2018) present a theoretical model suggesting the opposite, i.e., that principal write-down CoCos reduce risk-taking incentives. They argue that the leverage reduction after conversion reduces returns on equity and, thus, risk incentives. However, this hinges on the questionable assumption that the trigger activation is exogenous.

it would not be in the shareholders' interest to raise new equity to overcome financial difficulties due to the debt overhang problem (Pennacchi et al., 2014). Conversely, stockholders of banks that issue conversion-to-equity CoCos face the possibility of share dilution if the bond is triggered. Hence, this likely deters them from taking on inordinate risks (Flannery, 2005, 2014).

Conversion ratio: The conversion ratio specifies the amount of stock a bondholder receives once the security is triggered. Conditional on the conversion ratio, the incurred loss gets divided between bondholders and shareholders:[2] A higher ratio results in more severe dilution. Thus, existing shareholders face incentives to exercise more prudent risk management (Berg and Kaserer, 2015; Calomiris and Herring, 2013; Hilscher and Raviv, 2014; Maes and Schoutens, 2012) and possibly even inject additional equity to prevent conversion (Calomiris and Herring, 2013; Chen et al., 2017).[3]

Trigger event: The trigger event is supposed to determine the point in time the issuer needs to recapitalize. In general, the earlier a conversion-to-equity CoCo bond converts, the more disciplining power the security has and the more likely it is that bankruptcy costs are reduced (Barucci and Del Viva, 2012). Moreover, the bond decreases the issuer's default probability as long as the likelihood to convert is sufficiently high (Jaworski et al., 2017).

A minimum value for the common equity tier 1 (CET1) ratio, for example, can serve as a threshold. However, basing the trigger event on a book-value is widely criticized for neither ensuring timeliness of conversion nor robustness against management manipulation in case of lax accounting rules and regulatory forbearance (Avdjiev and Kartasheva, 2013; Avdjiev et al., 2017; Flannery, 2005, 2014, 2016; Maes and Schoutens, 2012; McDonald, 2013). Pennacchi et al. (2014) find that if banks affected by the financial crisis had issued contingent convertible bonds with trigger events based on regulatory capital ratios, these instruments would most likely not have absorbed any losses on time.

Avdjiev and Kartasheva (2013), Calomiris and Herring (2013), Flannery (2005,

[2]A principal write-down CoCo is the extreme case in which the bondholder does not receive any equity and absorbs the entire loss.

[3]Koziol and Lawrenz (2012) and Tan and Yang (2017) disagree with this widespread reasoning. Based on theoretical models, they predict CoCo bonds to incentivize excessive risk-taking if shareholders can change investment policies ex-post. However, Tan and Yang (2017) show that the distorting incentive does get weaker with an increased conversion ratio as long as shareholders cannot influence the trigger.

2016), Glasserman and Perotti (2017), Maes and Schoutens (2012), and McDonald (2013) are among the many scholars arguing that these shortcomings could largely be overcome by simply defining the trigger contingent on a market value like, for instance, the stock price. Market triggers are more robust towards balance sheet manipulations and diverging accounting standards. Furthermore, it mitigates the issue of regulatory forbearance. The figures underlying market triggers reflect the market's expectation and evaluation of the bank's financial standing. Hence, they are forward-looking and observable daily.

While this trigger type might theoretically create incentives for old shareholders, CoCo bond investors, and speculators to engage in share price manipulations to prevent or to trigger a conversion at their convenience (Avdjiev and Kartasheva, 2013; Flannery, 2016; Maes and Schoutens, 2012; Pennacchi et al., 2014), short-selling restrictions likely limit the actual extent of this misconduct in practice. Sundaresan and Wang (2015) raise the concern that, under certain circumstances, neither an equilibrium stock nor bond price exists for CoCos with a market-value-based trigger event.[4] Pennacchi et al. (2014) suggest relying on a trigger threshold defined by the market value of total capital to overcome this issue. Moreover, Glasserman and Nouri (2016) develop a continuous time-frame model that shows that price equilibria for CoCo bonds with a stock price trigger are attainable if the conversion happens early; it results in dilution for old shareholders; and information regarding the trigger event is available to all parties promptly.

It can also be at a regulator's discretion to decide whether it is necessary to trigger a CoCo bond conversion. Conveniently, neither drawbacks of book values, like rigidity and manipulability, nor ambiguity of prices associated with market triggers is an issue when relying on a regulatory trigger (Avdjiev and Kartasheva, 2013; Berg and Kaserer, 2015). Yet, experience shows that regulators do not necessarily have proper knowledge about the fundamental values of a bank, nor are they immune against forbearance (Berg and Kaserer, 2015; Calomiris and Herring, 2013; Flannery, 2005, 2014; Maes and Schoutens, 2012).

The information defining a trigger event is not necessarily limited to a single

[4]Sundaresan and Wang (2015) argue that outside investors anticipate a value transfer in favor of shareholders. Therefore, stock demand increases if the stock price falls close to the threshold. That, in turn, drives up the stock price and prevents the contingent convertible from being triggered. Conversely, if investors are sure the bank will not fall short of the minimum value defining the market trigger, the stock might indeed lack demand and, eventually, drop below the threshold. Hence, rational expectations and actual stock price developments are inconsistent that precludes the persistence of an equilibrium.

financial institution. Some scholars propose to use industry-wide data to determine the point when banks should recapitalize to overcome the moral hazard problem (e.g., Allen and Tang, 2016; McDonald, 2013). Critics argue that a systemic trigger can cause a domino effect jeopardizing the stability of the entire system (e.g., Avdjiev et al., 2017; Flannery, 2016; Maes and Schoutens, 2012). Based on a contingent claim analysis, Barucci and Del Viva (2012) infer that systemic trigger CoCos do not reduce bankruptcy costs.

In summary, scholars largely concur that a CoCo bond must absorb losses by being converted into equity at a reasonably high conversion ratio to deter excessive risk-taking on the part of existing shareholders and managers. Moreover, the trigger event should depend on a high threshold for a market capitalization measure to ensure that a contingent convertible is indeed triggered as soon as a bank needs to recapitalize. Any delay in the activation of the loss absorption mechanism significantly impairs the bond's capacity to reduce the bank's default probability.

3.2.2 Regulation as a driving force

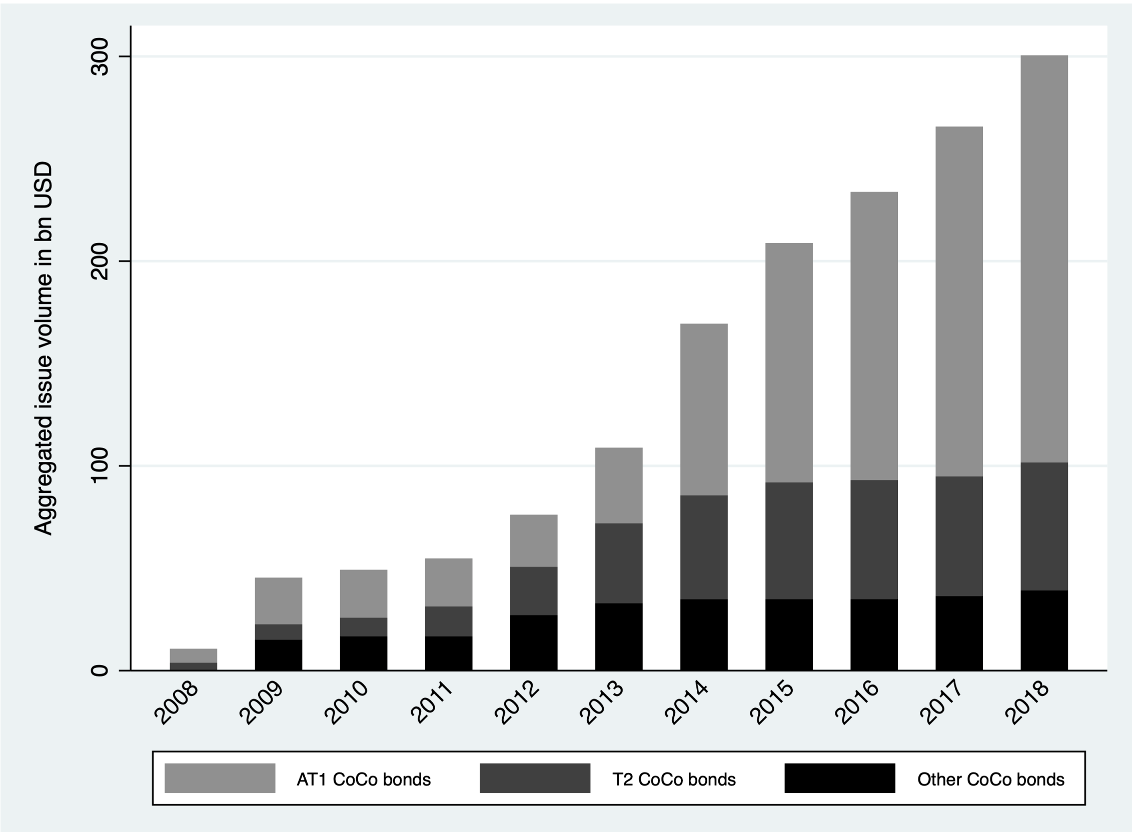
The financial crisis marks a fundamental shift in banking regulation towards higher capital requirements. When governments bailed out large banks, they mainly injected common equity to safeguard savers' deposits. As an unintended side effect, subordinated debt holders did not incur any losses, either (Basel Committee on Banking Supervision, 2011). Thus, part of the Basel Committee's three-pronged strategy to improve bank capitalization has been to rectify the definitions that specify which financial instruments shall be accepted as part of the regulatory capital to ensure that all capital types satisfy their respective loss absorbency capacity (Basel Committee on Banking Supervision, 2010).

The Basel Committee on Banking Supervision (2010, 2011) has specified the following criteria for CoCo bonds to qualify as additional tier 1 capital that is supposed to absorb losses while the bank is still solvent (going-concern capital): Both conversion into common equity and principal write-off is an acceptable loss absorption mechanism. In addition to an unspecified trigger event set by the issuer, regulators ought to reserve the right to initiate conversion or write-down if they deem necessary. Moreover, the issuer must be capable of suspending the coupon payments at any time, and AT1 CoCos need to be perpetual bonds.

Note, CoCo bonds can also qualify as tier 2 (T2) instruments. However, T2 capital is intended to offset losses following bankruptcy and upon liquidation (gone-concern capital) (Basel Committee on Banking Supervision, 2010). Hence, these securities are not supposed to reduce the default probability of a single institution but rather mitigate the risk of a systemic crisis once a bank becomes insolvent. Thus, these T2 CoCo bonds are unlikely to affect the individual bank's risk-taking behavior and are, therefore, neither referred to by the theoretical studies on CoCo bond design and risk outlined in Section 3.2.1 nor subject of this study.

While banks have issued hybrid securities similar to CoCo bonds before 2008, it was not until after the financial crisis that the idea of contingent convertibles, first proposed by Flannery (2005), got any traction. Figure 3.1 shows the aggregated issue volume of CoCo bonds by publicly traded and privately owned European banks over the 2008-2018 period in billion US dollars.

FIGURE 3.1: Aggregated issue volume of CoCo bonds by European banks



Note: This figure shows the aggregated issue volume of CoCo bonds by publicly traded and privately owned European banks over the 2008-2018 period in billion US dollars. CoCo bonds can either qualify as additional tier 1 (AT1) capital, tier 2 (T2) capital or not qualify as regulatory capital under Basel III (Other).

After some early issuances in 2009, aggregated issue volume of AT1 CoCos grew at an average rate of only around 4% each year until 2012. Increasing issue volume of CoCos qualifying as T2 or not qualifying as regulatory capital account for the overall market growth for contingent convertibles in this earlier period. However, the figure shows a sharp increase in AT1 CoCo issuances after 2013 and a continuous growth for these bonds at an average annual rate of approximately 45% until 2018, while the aggregated issue volume of T2 and other CoCos stagnated over recent years. Noticeably, this increase in the growth rate of the aggregated AT1 CoCo issue volume and the start of the Basel III phase-in period on January 1, 2013, coincide.

The European Union signed the guidelines and principles set out by Basel III into law by passing the Capital Requirement Directive IV (CRD IV)[5] and the Capital Requirement Regulation (CRR)[6] in July 2013. The European regulation adds one crucial detail to the requirements for financial instruments that are supposed to qualify as additional tier 1 capital: CET1 capital falling short of constituting 5.125% of the bank's risk-weighted assets (RWAs) defines the trigger event (cf. CRR, Art. 54).

In Switzerland, the Capital Adequacy Ordinance (CAO)[7] stipulates that CoCo bonds must absorb losses as soon as the bank's CET1 capital ratio drops below 7%. These high trigger level bonds must account for 4.3% of RWAs, whereas it is optional to use CoCo bonds to meet the 1.5% AT1 capital requirement under EU regulation. Note, systemically important Swiss banks had to cover only 3% of RWAs with high trigger level CoCos from March 2012 until June 2016. Also, they had to provide low trigger level contingent convertibles accounting for 6% of RWAs of regulatory capital that were to convert if CET1 capital falls below 3% of RWAs.

Moreover, it has been a widespread practice in European countries to allow coupon payments to be tax-deductible for the issuer (Bundgaard, 2017). Thus, CoCo bonds are the financially more attractive option to meet AT1 capital requirements versus like, for instance, preferred shares that are associated with higher costs of equity. In particular, Great Britain approved CoCo bond coupons

[5] *Directive 2013/36/EU*, Official Journal of the European Union, L 176, 2013, p. 338-436.

[6] *Regulation (EU) No 575/2013*, Official Journal of the European Union, L 176, 2013, p. 1-337.

[7] *Verordnung über die Eigenmittel und Risikoverteilung der Banken und Wertpapierhäuser* (RS 952.03), as of March 28, 2020. Available at: https://www.admin.ch/opc/de/classified-compilation/20121146/index.html (Accessed: 11 June 2020).

to be tax-deductible at the beginning of 2014.[8] Moreover, in April of 2014, the German Federal Ministry of Finance clarified that any interest payments on AT1 classified instruments are tax-deductible.[9] Both announcements coincide with a surge in the respective national CoCo issue volumes.

In summary, the possibility to increase regulatory capital levels with contingent convertible bonds in a cost-effective manner due to the favorable tax treatment appears to be decisive for the CoCo market growth.[10] Yet, considering the theoretical research on the topic of optimal contingent convertible bond design (see Section 3.2.1), the regulatory requirements for bonds to qualify as AT1 capital are puzzling at best. While conversion-to-equity at a high conversion ratio is superior to a write-down mechanism from a risk-taking incentive perspective, only Swiss regulation demands the former *loss absorption mechanism* and specifies a high *conversion ratio*. Most notably, scholars widely reject the idea of a *trigger event* based on regulatory capital ratios and/or a regulator's discretion, both stipulated by European regulations. Glasserman and Perotti (2017) concur with this assessment and argue that regulators have stripped CoCo bonds of their potential to strengthen the banks' capital bases.

Consequently, I hypothesize that issuing CoCo bonds that qualify as additional tier 1 capital according to European regulations increases bank risk.

3.2.3 CoCos and bank risk in practice

The number of empirical analyses on the implications of issuing CoCo bonds on bank risk is limited.

Ammann et al. (2017), Avdjiev et al. (2017), and Goncharenko et al. (2020) conduct event studies based on daily data to analyze the announcement effects of issuing CoCos on credit default swap (CDS)-spreads of the bank. While Ammann

[8] *The Taxation of Regulatory Capital Securities Regulations 2013* (SI 2013/3209). Available at: http://www.legislation.gov.uk/uksi/2013/3209/made (Accessed: 11 June 2020).

[9] Bundesministerium der Finanzen. (April 10 2014). *Steuerliche Behandlung von Instrumenten des zusätzlichen Kernkapitals nach Art. 51 ff. CRRR; Musterbedingungen AT1-Instrumente Typ A und Typ B des Bundesverbands deutscher Banken e.V. vom 20. Februar 2014*. Available at: https://bankenverband.de/media/uploads/2016/11/02/bmf-schreiben-10-04-2014.pdf (Accessed: 11 June 2020).

[10] While Avdjiev et al. (2017) agree with this assessment, they also suggest that part of the supply could be demand-driven: Due to the low-interest-rate environment, fixed income investors are looking for opportunities in line with their investment restrictions. CoCo bonds that are written-off and do not convert into equity can meet their demand and provide reasonably high coupon payments.

et al. (2017) and Avdjiev et al. (2017) argue their findings of decreasing effects on CDS-spreads is evidence for reduced bankruptcy risk and cost advantages due to the associated tax shield, Goncharenko et al. (2020) do not find any significant announcement effects on CDS spreads. Ammann et al. (2017) and Avdjiev et al. (2017) also look at the announcement effect on stock prices but find mixed results. In contrast to these studies, I aim at estimating the causal impact issuing contingent convertibles qualifying as going-concern capital has on bank-level risk one to three years after the issuance of AT1 CoCos. Thus, I contribute to the literature by focusing on the actual realization of risk changes rather than market participants' anticipation and perception.

De Spiegeleer et al. (2017) and Echevarria-Icaza and Sosvilla-Rivero (2018) also focus on the effect of issuing AT1 CoCos on the stability and level of regulatory capital. De Spiegeleer et al. (2017) find that the standard deviation of common equity tier 1 ratios rises after issuing write-down AT1 CoCos, suggesting that their capital bases have become less stable, albeit at a higher level. However, their empirical analysis is more of a case study given that their sample only comprises nine banks for the 2014-2016 period. Echevarria-Icaza and Sosvilla-Rivero (2018) perform a difference-in-difference analysis using data on 260 listed large European financial institutions between 2011 and 2015. They argue that their results suggest that systemically important banks improve their capital ratios, among other things, by issuing CoCo bonds. However, they merely include a dummy indicating if an observation is from before or after 2013. That is, they use a time dummy as a proxy rather than controlling for whether a bank issues contingent convertibles, rendering their findings indirect at best. Thus, I further close a gap in the literature by explicitly linking the issuance of AT1 CoCos to changes in bank risk profiles based on a comprehensive sample.

3.3 Empirical strategy and data

As indicated by Figure 3.1, European banks issued CoCo bonds qualifying as AT1 capital (i.e., treatment) in different years over the 2008-2018 period. The methodological challenges in identifying the causal effect in case of such a so-called staggered treatment adoption and suggestions on how to resolve them have been the subject of recent research (e.g., Abraham and Sun, 2020; Athey and Imbens, 2018).

3.3.1 Matching-based difference-in-differences approach

Several studies propose some version of a conditional or matching-based difference-in-differences (DiD) approach for staggered treatment adoption: Differences in outcomes are only computed based on treated and control units that are, first, comparable in the variables affecting treatment assignment and the outcome variable, and, second, measured at the same time (Callaway and Sant'Anna, 2020; Dettmann et al., 2020; Imai et al., 2020). Thereby, the following issues associated with identifying and estimating the effect issuing AT1 CoCo bonds has on banks risk, in particular, can be addressed:

First, this approach accounts for calendar time effects in the aggregate outcome and on treatment selection. Bank risk (i.e., outcome variable) likely varies across the industry over time due to changes in the economic and regulatory environment. Thus, comparing banks that issued AT1 CoCo bonds early after the financial crisis, for example, with control units in more recent years, would likely result in upwards biased estimates given the general time trend towards more financial stability over the past decade. Moreover, changes in the economic or regulatory environment like, e.g., the EU passing CRD IV and CRR in 2013, might affect treatment selection, for which matching based on contemporaneous data accounts.

Second, this method also accounts for the relative time dynamics both before and after the treatment. A bank might decide on issuing CoCo bonds at some time before the security is available to investors. During this period, shareholders could already choose to take on higher risks in anticipation of the possibility of being able to shift them to future bondholders, which would result in a temporary increase of the outcome variable, i.e., an Ashenfelter's dip (Ashenfelter, 1978). Matching based on an earlier time relative to the treatment addresses concerns regarding the associated bias. Furthermore, AT1 CoCos are perpetual bonds, and their effect on bank risk might change over time after the treatment. The approach can support the estimation of treatment effects for different relative post-treatment periods.

3.3.2 Model specification

To estimate the average treatment effect on the treated, I define the following model:

$$risk_{i,t} = \gamma_\tau \cdot D_i \cdot d\tau_t + \delta_\tau \cdot d\tau_t + c_i + u_{i,t}, \quad (3.1)$$

where i identifies the observational unit; $t = -1, \ldots, \tau$ indicates the time in years relative to the treatment at $t = 0$; and $\tau \in \{0, \ldots, T\}$ specifies how many post treatment observations are included. D_i is the treatment dummy with $D_i = 1$ for banks that are treated and zero otherwise. $d\tau_t$ is a time dummy with $d\tau_t = 1$ if $0 \leq t \leq \tau$, zero otherwise, and c_i are individual fixed effects.

The parameter of interest in this regression is γ_τ as it measures the average treatment effect of issuing AT1 CoCo bonds on bank risk. However, this model specification does not allow for heterogeneous treatment effects over time. Instead, γ_τ estimates the temporal mean of the outcome over the entire (post-)treatment period included in the estimation, e.g., over three years if $\tau = 2$.

To differentiate between treatment effects for the post-treatment periods, I augment the model as follows:

$$risk_{i,t} = \sum_{\tau=0}^{T} \gamma_\tau \cdot D_i \cdot d\tau_t + \sum_{\tau=0}^{T} \delta_\tau \cdot d\tau_t + \sum_{k=1}^{K} \beta_k \cdot x_{k,i,t} + c_i + u_{i,t}, \quad (3.2)$$

with i identifying the observational unit, D_i specifying the treatment dummy (i.e., $D_i = 1$ for banks that are treated, zero otherwise), and c_i being individual fixed effects. In this model, however, all time periods are included simultaneously with $t = -1, \ldots, T$ indicating the time in years relative to the treatment at $t = 0$. Here, $d\tau_t$ with $\tau = 0, \ldots, T$ are time dummies for each relative year, i.e., $d\tau_t = 1$ if $\tau = t$, zero otherwise. Additional control variables ($x_{k,i,t}$) can also be included.

The parameters of interest in this analysis are $\gamma_0, \ldots, \gamma_T$, measuring the treatment effects of issuing AT1 CoCo bonds on bank risk in the year of the issuance (γ_0) as well as one (γ_1), two (γ_2), ..., and T years (γ_T) after the initial treatment.

3.3.3 Measuring bank risk

The z-score measures the distance from insolvency and is widely used in the literature to measure bank-level risk (e.g., Bhagat et al., 2015; Houston et al., 2010;

Hoque et al., 2015; Laeven and Levine, 2009):

$$\text{z-score}_{i,t} = \frac{(ROA_{i,t} + \frac{assets_{i,t} - liabilities_{i,t}}{assets_{i,t}})}{\sigma^{ROA}_{i,t}}, \tag{3.3}$$

with $ROA_{i,t}$ being the annual average return on assets and the associated standard deviation $\sigma^{ROA}_{i,t}$ measured over the preceding five-year rolling window. The variables $assets_{i,t}$ and $liabilities_{i,t}$ measure annual total assets and liabilities, respectively. The higher the score, the less risk is taken. Using the logarithm of this measure alleviates issues associated with its skewed distribution.

Note that I do not use bank-level risk measures based on stock data like, for instance, the volatility of returns for my estimations since this analysis aims at estimating the actual realization of changes in risk rather than the market's anticipation. Moreover, since theory suggests that AT1 CoCo bonds are largely designed such that shareholders can shift potential losses associated with higher risk-taking to bondholders (cf. Section 3.2.1), the treatment should not affect shareholders' risk assessment.

Furthermore, I focus on bank-level rather than systemic risk for two reasons: First, systemic risk measures depend on the state of the financial system, which is a function of aggregate bank risk. Hence, the treatment would likely affect the outcome for control units which constitutes a violation of the stable unit treatment value assumption. Second, previous research provides evidence that large banks only hold a small fraction of outstanding CoCo bonds.[11] This indicates that contingent convertibles are unlikely to contribute to severe risk shifting within the banking sector, alleviating any concern regarding a significant increase in systemic risk.

3.3.4 Matching

There is a growing body of literature on bank characteristics that are associated with higher risk-taking. Several of these identified factors are also decisive for the

[11] According to Avdjiev and Kartasheva (2013), large incorporated banks have held a mere three percent of CoCos issued between 2009 and mid-2013 globally. Boermans and van Wijnbergen (2018) find that Euro area banks accounted for approximately 1.5% of European contingent convertibles holdings between 2009 and 2015.

propensity of a bank to issue CoCo bonds. Hence, not matching treated with control banks based on these observable selection variables would render treatment effects estimates biased.

First, many studies have found that bank-level risk-taking increases with *bank size* (e.g., Bhagat et al., 2015; Bostandzic and Weiß, 2018; Gropp et al., 2014; Laeven et al., 2016). Moreover, Avdjiev et al. (2017) find that the propensity to issue CoCos is higher for larger banks in advanced economies. Fajardo and Mendes (2020) and Goncharenko et al. (2020) confirm the positive association between bank size and the likelihood to issue contingent convertibles for European banks in particular.

Next, better *capitalization* is typically associated with lower bank risk (e.g., Barth and Seckinger, 2018; Gornall and Strebulaev, 2018; Hoque et al., 2015; Laeven et al., 2016). Empirical results on its impact on the propensity to issue contingent convertibles are more mixed: While Avdjiev et al. (2017) and Hesse (2018) find a positive effect of capitalization on the likelihood to issue CoCo bonds, Fajardo and Mendes (2020) do not find evidence for a significant relation. Vallée (2019) even argues that banks with lower tier 1-capital ratios might be more willing to rely on contingent capital securities. However, this argument for a negative link between these two variables is based on indirect evidence.[12]

Furthermore, banks with a *business model* characterized by low liquidity, typically approximated by net loans relative to assets, are often associated with higher risk (Beck et al., 2013; Laeven et al., 2016; Lambert et al., 2017). Goncharenko et al. (2020) also find banks with larger loans-to-assets ratios to have a higher propensity to issue CoCo bonds.

Similarly, Bhagat et al. (2015) find *investment banks* to be prone to excessive risk-taking. Given the findings of Goncharenko et al. (2020) regarding the business model proxy loans-to-assets ratio, I also include a dummy indicating if the bank belongs to the investment banking and services industry in my set of matching variables.

[12] Vallée (2019) analyzes banks that imposed their losses onto debt-holders during the financial crisis by refusing to call subordinated bonds at par at the first call date, a common practice before the financial crisis. Instead, they launched highly discounted tender offers. Capital gains on liabilities resulting from these liability management exercises (LMEs) can increase banks' core equity. Vallée (2019) finds that weakly capitalized banks were associated with larger increases in CET1 capital resulting from LMEs.

Lastly, I match treated and control banks based on two additional dummy variables. The first dummy equals one if a bank has its headquarter in Germany or the United Kingdom and if the observation year is 2013 or later. The second dummy is equal to one if a bank is Swiss and if the observation year falls in the 2012-2016 period. Thereby, I control for selection bias due to country-time-specific *tax incentives* and *requirements to issue AT1 CoCos*, respectively.[13]

Note, Table A3.1 provides definitions of all variables used in this paper.

To find adequate controls for each treated unit around the specific treatment time, I follow Dettmann et al. (2011, 2020), who propose a matching procedure based on a statistical distance function which they find to perform superior to balancing scores, like the propensity or index score, for small samples, in particular.

The aggregate distance function is defined to be the average over the absolute difference for continuous matching variables, i.e., a measure similar to the Mahalanobis distance but normalized by the maximum in observed differences, and a distance measure based on the generalized matching coefficient for categorical variables, both weighted according to the number of continuous and categorical matching variables.

Estimating the difference-in-differences models specified in equations (3.1) and (3.2), which account for selection on unobservable bank characteristics, matching also allows to control for selection on observable characteristics. Thus, respective regressions estimate the average treatment effect on the treated as the mean over individual differences in the development of bank risk at the same time relative to treatment.[14]

3.3.5 Data on CoCo bonds and banks

I obtain balance sheet and stock data for listed financial institutions that belong to the banking and investment services sector from Thomson Reuters Eikon for

[13]See Section 3.2.2 for details on the relevant referenced legislative developments.

[14]Note, Abraham and Sun (2020) and Callaway and Sant'Anna (2020) first average over groups defined by the specific post-treatment period and treatment-time, respectively, before computing a weighted average. My approach follows Dettmann et al. (2020) and Imai et al. (2020) more closely. It translates to the particular case in which each observational unit and its match constitute a single, equally-weighted group.

the period from 2000 to 2019.[15] I only include banks from the European Economic Area, the United Kingdom, and Switzerland. I cross-check the selection with information from banks' websites, annual reports, public financial services platforms, and the bank directory TheBanks.eu. That results in a list of 258 banks. For 12 of these banks, Thomson Reuters Eikon does not provide data. Next, I run plausibility checks to account for reporting errors on all balance sheet figures. I check, for instance, if total assets only includes positive values and replace the entry with a missing value otherwise. I also revise inconsistent data entries using annual reports.

After estimating the standard deviation of return on assets (σ^{ROA}) based on five-year rolling windows and computing the distance from insolvency (ln(z-score)) for each bank-year according to equation (3.3), I winsorize the risk measure at a one percent level from above and below to reduce distorting effects due to outliers or remaining reporting errors.

TABLE 3.1: Summary statistics

Risk measures	**Obs.**	**Mean**	**SD**	**Min.**	**Max.**
Distance from insolvency - ln(z-score)	3,142	3.3278	1.1920	-0.0728	5.9222
Earnings volatility - σ^{ROA}	3,191	1.0130	2.0520	0.0250	13.370
Continuous matching variables					
Log assets	3,294	23.079	2.5535	12.268	29.929
Tier 1-ratio	2,363	0.1481	0.0562	0.0280	0.4910
Loans/assets	3,217	0.5973	0.1897	0.0000	0.9674
Risk-weighted assets/assets	990	0.4981	0.5877	0.0003	16.791
Charter value	2,960	1.2792	1.3880	0.0002	21.557
Categorical matching variables					
Tax incentives dummy	3,676	0.0808	0.2726	0.0000	1.0000
Higher AT1 requirements dummy	3,676	0.0381	0.1914	0.0000	1.0000
Investment bank dummy	3,676	0.1254	0.3312	0.0000	1.0000
Systemically important bank dummy	3,676	0.1064	0.3084	0.0000	1.0000
Additional control variables					
Equity/assets	3,282	0.1226	0.1302	0.0168	0.7920
Loan loss provisions/net interest income	2,622	0.2738	0.4513	0.0000	3.2537
Non-interest expense/gross revenues	2,837	0.6949	0.2363	0.2878	1.9852
Return on average assets	3,247	0.0115	0.0267	-0.0619	0.1652

Note: This table presents summary statistics based on the pre-processed unbalanced panel for the period from 2005 to 2019. All variables are defined in Table A3.1.

[15] The length of the sample on banks allows me to estimate σ^{ROA} based on five-year rolling windows and still have at least three pre-treatment observations before the earliest AT1 CoCo bond issue analyzed in this study, that is in 2008. I also have at least one post-treatment observation for the latest issue in 2018.

I match the bank data with information about CoCo bond issuances for the 2008-2018 period, which I also retrieve from Thomson Reuters Eikon. Again, I must manually search through banks' annual reports to correctly identify or verify certain issuers, given that some banks have issued contingent convertibles through special purpose entities or private subsidiaries.

Note, several banks issued CoCos multiple times in different years of the sample period. For the baseline model specification, I only consider the first time a bank issued a contingent convertible bond, and I regard this bank as treated in all subsequent periods. That is in line with a common assumption for the staggered treatment adoption design, referred to as irreversibility of treatment (Callaway and Sant'Anna, 2020; Dettmann et al., 2020). I do, however, regard a bank as a new observational unit if at least three years pass between two issuances. Similarly, I add observational periods of banks that later issue CoCos to the pool of controls if this period before the treatment spans at least five years in addition to a one-year buffer period.

That results in a total of 61 treated units for which I can at least observe one post-treatment outcome and the matching variables described in Section 3.3.4 at $t = -1$, that is, the time of matching for the baseline model. The pool of controls comprises 238 observational units.

Table 3.1 presents summary statistics for the unbalanced panel over the 2005-2019 period.

3.4 Results

To determine the causal effect issuing AT1-qualifying CoCo bonds has on bank risk, I estimate the models specified in equations (3.1) and (3.2).

3.4.1 Bank risk and AT1 CoCo issuances

Table 3.2 presents the baseline regression results. All estimations are based on the matched sample using the combined statistical distance function and matching variables outlined in Section 3.3.4. The time of matching for the baseline model is $t = -1$. Standard errors are clustered at the bank level. This table reports estimation results for the model specified in equation (3.1) with $\tau = 0, \ldots, 3$ in columns (1)-(4), respectively, and the model specified in equation (3.2) for $T = 3$ in columns (5)-(8).

The findings suggest that issuing AT1 CoCo bonds results in significantly higher risk-taking in the years after the issuance compared to the expected development of bank risk would a financial institution not issue AT1 contingent convertibles. Note, the outcome variable in all regressions is ln(z-score), which measures the bank's distance from insolvency, i.e., a smaller number indicates higher risk. Columns (1) and (2) present only week evidence for a change in risk-taking in the treatment year and, on average, with one added post-treatment observation period, respectively. However, estimating the treatment effect as the temporal mean of the outcome over a two and three post-treatment year period (columns (3) and (4), respectively) shows an increasingly negative effect on ln(z-score) that is statistically significant at a 5%-level. That hints towards heterogeneity of the treatment effect over the post-treatment period.

Column (5) confirms this: There is no statistically significant treatment effect on bank risk in the year AT1 CoCos are issued. For the following year, however, the results show ln(z-score) to be, on average, 0.37 lower for treated banks versus the level of risk-taking expected without issuing AT1 contingent convertibles, all else being equal. For $t = 2$ ($\hat{\gamma}_2$) and $t = 3$ ($\hat{\gamma}_3$), the estimated treatment effects increase in absolute size by an additional 0.15 and 0.12 over the second and third post-treatment year, respectively. These effects correspond to changes of ln(z-score) by 0.31 to 0.54 standard deviations (SD(ln(z-score)) $=$ 1.19 for the panel, see Table 3.1). Given that the mean of ln(z-score) is 3.33, the effects are economically meaningful. All three estimates for the post-treatment period ($\hat{\gamma}_1$, $\hat{\gamma}_2$, and $\hat{\gamma}_3$) are also statistically significant at a 5%-level.

Given that I include CoCo issuances from 2008 to 2018 while my bank-level data ends in 2019, I estimate $\hat{\gamma}_0$ and $\hat{\gamma}_1$ based on all 61 issues, whereas I can only rely on issuances before 2017 and 2016 to compute $\hat{\gamma}_2$ and $\hat{\gamma}_3$, respectively. To ensure sample consistency, I re-run my regression only for AT1 CoCo issues for which I can observe all three post-treatment periods, which are 48 issuances (column (6)). The results show that the treatment effects become even more pronounced, both in size and level of significance, including a weakly significant effect in the year of treatment ($\hat{\gamma}_0$).

The findings suggest that ln(z-score) is almost half a standard deviation lower one year after a bank issues AT1 CoCos compared to the expected stability-level

TABLE 3.2: Effects of AT1 CoCo bonds on bank risk - baseline results

Outcome: ln(z-score)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{\gamma}_0$	-0.1451				-0.1408	-0.2477*	-0.2627*	-0.2325*
	(0.1213)				(0.1232)	(0.1385)	(0.1413)	(0.1370)
$\hat{\gamma}_1$		-0.2687*			-0.3734**	-0.5847***	-0.4928***	-0.4740***
		(0.1416)			(0.1844)	(0.1817)	(0.1718)	(0.1611)
$\hat{\gamma}_2$			-0.3465**		-0.5229**	-0.7208***	-0.5774***	-0.5668***
			(0.1575)		(0.2117)	(0.2253)	(0.2041)	(0.1967)
$\hat{\gamma}_3$				-0.3976**	-0.6419**	-0.7932**	-0.7383**	-0.7340**
				(0.1616)	(0.2785)	(0.3089)	(0.2951)	(0.3101)
Equity/assets							11.3480*	16.1434*
							(6.6474)	(8.4029)
Loan loss prov./net interest income							-0.4879	-0.5724
							(0.3309)	(0.3447)
Non-interest exp./revenues							-0.4463**	-0.5131**
							(0.2231)	(0.2077)
ROA							-0.4255	-5.5362
							(18.4400)	(18.6843)
Observations	240	362	470	566	566	471	436	432
Treated banks	61	61	61	61	61	48	48	48
R^2	0.1116	0.1220	0.1298	0.1130	0.2067	0.2273	0.2565	0.2711
Temporal mean over post-treatment outcomes	Yes	Yes	Yes	Yes	No	No	No	No
Constant sample for post-treatment observations	No	No	No	No	No	Yes	Yes	Yes
Bank risk controls included	No	No	No	No	No	No	Yes	Yes
Matching variables included	No	No	No	No	No	No	No	Yes

Note: This table reports estimation results for the model specified in equation (3.1) with $\tau = 0, \ldots, 3$ in columns (1)-(4), respectively, and the model specified in equation (3.2) for $T = 3$ in columns (5)-(8). All estimations are based on the matched sample using a combined statistical distance function for matching following Dettmann et al. (2011, 2020). The outcome variable is ln(z-score), which measures the bank's distance from insolvency, i.e., a smaller number indicates higher bank risk. Matching variables for all estimations are log assets, tier 1-ratio, loans/assets, tax incentive dummy, higher AT1 requirements dummy, investment bank dummy. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

without treatment ($\hat{\gamma}_1 = -0.59$ with SD(ln(z-score)) = 1.19). Over the subsequent two years, this gap grows to -0.72 and -0.79, respectively. Interestingly, this restriction results in the sample no longer comprising investment banks, possibly hinting towards a larger effect of AT1 CoCo issues on commercial banks' risk-taking. Moreover, the average size of treated banks at the time of matching slightly increases. Hence, this might indicate a non-linear effect, suggesting that larger banks engage, ceteris paribus, in more excessive risk-taking compared to smaller banks in response to the treatment. However, a decrease in precision due to the smaller sample size could also drive the more pronounced results.

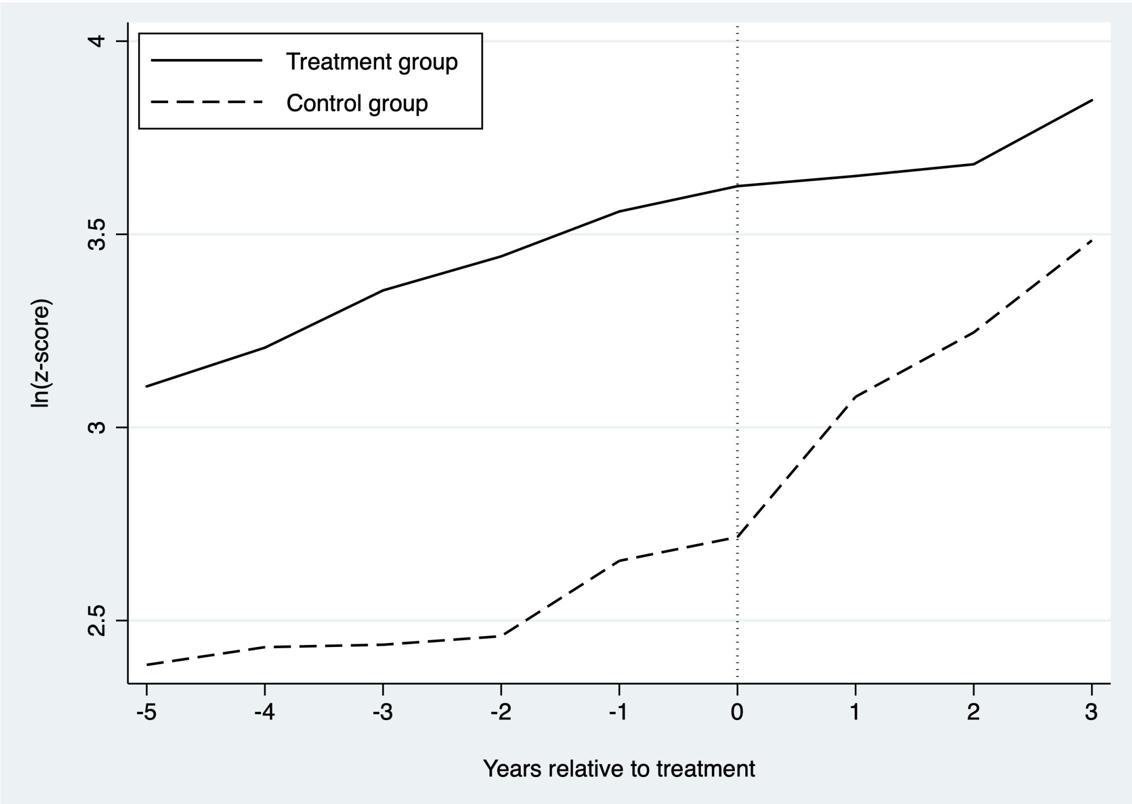
Next, I follow the literature and control for bank-level variables that might impact risk-taking (column (7)). First, I account for capital adequacy by including the commonly used equity-to-assets ratio (Bhagat et al., 2015; Houston et al., 2010). As expected, a higher equity-to-assets ratio is associated with a higher degree of bank stability. The parameter estimate is statistically significant at a 10%-level. Following Beck et al. (2013), I measure asset quality by loan loss provisions over net interest income, for which I do not find a significant effect. Non-interest expense over revenues is a proxy for management capability (Agoraki et al., 2011). A higher ratio indicates a less efficiently operating bank, leading to more instability. Thus, it is negatively associated with ln(z-score) (p-value < 0.05). Lastly, returns on assets (ROA) measures the ability to generate earnings (Bhagat et al., 2015; Houston et al., 2010; Jiménez et al., 2013). I find this factor not to be significant in my estimations. Note, these are a set of standard bank-level characteristics used in the literature (e.g., Lambert et al., 2017). In addition to the measures for banks' capital adequacy, asset quality, management capability, and earning, they typically also include the loans-to-assets ratio to account for liquidity (CAMEL). However, given that the liquidity control is one of my baseline matching variables, I do not include this ratio in my set of bank controls.

Results show no changes in the statistical significance levels of estimated treatment effects compared to column (6). The absolute effect sizes slightly decrease and become more comparable to the estimates for the entire 2008-2018 period reported in column (5).

Finally, I also add the matching variables to my set of controls, which does not change my estimates in any meaningful way (column (8)). In unreported results, I find none of the continuous matching variables to be statistically significant. That alleviates concerns regarding the possibility that the matching process selected control units with extreme values in my continuous matching variables, possibly resulting in biased estimates if control units then reverse towards the mean of log assets, tier 1-ratio, and loans/assets over the post-treatment period.

I perform additional tests to further investigate the validity of the assumptions underlying my empirical strategy. Figure 3.2 displays the development of the average ln(z-score) before and after the treatment, i.e., the issue of AT1 CoCo bonds at $t = 0$, for the treated and the matched control units. Note, I only include banks for which data on bank risk is available for all post-treatment periods, i.e., the sample used for the estimations in columns (5)-(8).

FIGURE 3.2: Common trend of control and treatment group



Note: This figure shows the development of the average ln(z-score) before and after the treatment, i.e., the issue of AT1 CoCo bonds at $t = 0$, for the treatment and control group. Note, ln(z-score) measures the bank's distance from insolvency, i.e., a smaller number indicates higher bank risk. The figure is based on the matched sample used for the baseline estimation presented in Table 3.2. Only banks are included for which data on bank risk is available for all post-treatment periods.

First, the figure shows a general time trend towards higher bank stability, which might reflect the positive impact of the sweeping regulatory changes since the financial crisis and the period of economic growth and stability of the real economy over the past decade. Second, the figure clearly indicates a parallel

TABLE 3.3: Placebo test results

Outcome: ln(z-score)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{\gamma}_0$	0.0164	0.0140	0.0543	0.0174	0.0141	0.1013	-0.0148	0.0872
	(0.1300)	(0.0942)	(0.1276)	(0.1089)	(0.1363)	(0.1112)	(0.1592)	(0.0947)
$\hat{\gamma}_1$	-0.1288	-0.1655	-0.0978	-0.2058	0.0331	0.1544	0.0424	0.1327
	(0.1797)	(0.1422)	(0.1833)	(0.1467)	(0.1933)	(0.1328)	(0.1979)	(0.1234)
$\hat{\gamma}_2$					-0.1122	0.0250	-0.1097	-0.0362
					(0.2428)	(0.1792)	(0.2584)	(0.1697)
Equity/assets		10.2917**		11.9348***		15.1035***		18.6065***
		(4.3450)		(4.0385)		(4.0098)		(4.0657)
Loan loss prov./net interest income		-0.2454**		-0.3195***		-0.2114*		-0.2392**
		(0.0974)		(0.0685)		(0.1149)		(0.0905)
Non-interest exp./revenues		-0.1804		-0.2698		-0.4235*		-0.4201**
		(0.2555)		(0.2665)		(0.2137)		(0.1814)
ROA		10.0372*		14.1184***		4.8801		13.6621***
		(5.4860)		(5.0888)		(7.3411)		(3.9911)
Observations	336	309	260	244	429	395	331	309
Treated banks	61	61	48	48	61	61	48	48
R^2	0.0855	0.2599	0.0565	0.3273	0.1050	0.2996	0.0881	0.4287
Placebo treatment at $t = -2$	Yes	Yes	Yes	Yes	No	No	No	No
Placebo treatment at $t = -3$	No	No	No	No	Yes	Yes	Yes	Yes
Constant sample for post-treatment observations	No	No	Yes	Yes	No	No	Yes	Yes
Matching variables included	No	Yes	No	Yes	No	Yes	No	Yes
Bank risk controls included	No	Yes	No	Yes	No	Yes	No	Yes

Note: This table reports estimation results for the model specified in equation (3.2). The time of the placebo event is set to $t = -2$ in columns (1)-(4) and to $t = -3$ in columns (5)-(8). All estimations are based on the matched sample using a combined statistical distance function for matching following Dettmann et al. (2011, 2020). The outcome variable is ln(z-score), which measures the bank's distance from insolvency, i.e., a smaller number indicates higher bank risk. Matching variables for all estimations are log assets, tier 1-ratio, loans/assets, tax incentive dummy, higher AT1 requirements dummy, investment bank dummy. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

trend for treated and matched control units for $t < 0$. Most interestingly, the development after the treatment suggests that issuing AT1 CoCos does result in a weaker increase in bank stability relative to the expected positive development in the absence of the treatment rather than an absolute increase in bank-level risk. Unreported results for the time dummies ($\hat{\delta}_1, \hat{\delta}_2$, and $\hat{\delta}_3$) in my baseline estimations substantiate this assessment.

Furthermore, I perform placebo tests to check the validity of the conditional independence assumption. Table 3.3 reports estimation results for the model specified in equation (3.2). The time of the placebo event is at $t = -2$ in columns (1)-(4), hence I can observe only one post-placebo-treatment observation before the actual treatment, and at $t = -3$ in columns (5)-(8), which results in two post-placebo-treatment observations. I run regressions for both placebo events based on the larger sample (columns (1)-(2) and columns (5)-(6)) as well as the constant smaller sample (columns (3)-(4) and columns (7)-(8)). For each placebo event and sample, I estimate the model, first, without controls and, second, with the full set of bank-controls and matching variables. None of the model specifications reveal significant effects due to the placebo treatments. All significant bank-level control variables show the expected signs.

Lastly, Table 3.4 shows balancing test results for all matching variables at the time of matching, i.e., at $t = -1$. The first and second half of the table present test results based on the sample used for the baseline estimations in columns (1)-(5) and columns (6)-(8) of Table 3.2, respectively. The table reports the means of the matching variables for treated banks ($D_i = 1$), i.e., banks that issue AT1 CoCo Bonds at $t = 0$, and the control banks ($D_i = 0$). T-test statistics with p-values for $H_0 : E[var_{i,t}|D_i = 1] = E[var_{i,t}|D_i = 0]$ (i.e., equality of means) and Kolmogorov-Smirnov (KS) test statistics with exact p-values for $H_0 : F_{D_i=1}(var_{i,t}) = F_{D_i=0}(var_{i,t})$ (i.e., equality of distribution functions) are displayed. The results show that I cannot reject the null hypotheses of equality of means and equality of distribution functions for any of the matching variables at a level of statistical significance of at least 5%. In fact, p-values are larger 0.1 for all but the t-test for log assets in the larger sample (p-value = 0.072). That should alleviate concerns regarding a violation of the common support assumption.[16]

[16]I do not report Fisher exact test results for the categorical variables since they are perfectly balanced as indicated by the reported mean values in the first two columns.

TABLE 3.4: Balancing tests for matching variables

Variable	Mean D=1	Mean D=0	t-test t_{emp}	t-test p>|t|	KS-test KS_{emp}	KS-test p>KS
Log assets	25.85	25.20	1.81	0.072	0.213	0.125
Tier 1-ratio	0.136	0.137	-0.10	0.924	0.098	0.933
Loans/assets	0.584	0.578	0.20	0.841	0.164	0.388
Tax incentive dummy	0.180	0.180	0.00	1.000		
Higher AT1 requirements dummy	0.066	0.066	0.00	1.000		
Investment bank dummy	0.033	0.033	0.00	1.000		
Observations						122
Treated banks						61
Constant sample for post-treatment observations						No
Log assets	25.99	25.50	1.19	0.237	0.250	0.100
Tier 1-ratio	0.133	0.131	0.25	0.804	0.125	0.853
Loans/assets	0.579	0.582	-0.08	0.938	0.167	0.522
Tax incentive dummy	0.167	0.167	0.00	1.000		
Higher AT1 req. dummy	0.063	0.063	0.00	1.000		
Investment bank dummy	0.000	0.000	0.00	1.000		
Observations						96
Treated banks						48
Constant sample for post-treatment observations						Yes

Note: The balancing tests are performed at the time of matching, i.e., at $t = -1$. The first and second part of the table present test results based on the sample used for the baseline estimations in columns (1)-(5) and columns (6)-(8) of Table 3.2, respectively. The table reports the means of the matching variables for treated banks ($D_i = 1$), i.e., banks that issue AT1 CoCo Bonds at $t = 0$, and the control banks ($D_i = 0$). T-test statistics with p-values for $H_0 : E[var_{i,t}|D_i = 1] = E[var_{i,t}|D_i = 0]$ (i.e., equality of means) and Kolmogorov-Smirnov (KS) test statistics with exact p-values for $H_0 : F_{D_i=1}(var_{i,t}) = F_{D_i=0}(var_{i,t})$ (i.e., equality of distribution functions) are displayed.

In summary, the results show that issuing CoCo bonds that meet the criteria for additional tier 1 capital results in significantly higher risk-taking in subsequent years versus the expected levels in the absence of treatment. Rather than having a net negative impact, issuing AT1 CoCos seems to impede the positive development towards greater bank stability. The treatment effect is statistically significant and economically meaningful.

3.4.2 Robustness checks

I perform several robustness checks to substantiate the validity of my baseline findings. I re-run all model specifications of the baseline estimations (Table 3.2) using earnings volatility (σ^{ROA}) as the dependent variable - an alternative way to measure bank-level risk without relying on stock market data as suggested by Laeven and Levine (2009). Table 3.5 present the results. Note, a higher earnings volatility indicates higher bank risk.

TABLE 3.5: Robustness check - alternative risk measure: earnings volatility

Outcome: σ^{ROA}	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{\gamma}_0$	0.1101**				0.1103**	0.1780**	0.2216***	0.2313**
	(0.0481)				(0.0491)	(0.0680)	(0.0767)	(0.0921)
$\hat{\gamma}_1$		0.2008**			0.2899**	0.4033**	0.4355***	0.4661***
		(0.0830)			(0.1388)	(0.1810)	(0.1623)	(0.1691)
$\hat{\gamma}_2$			0.2476**		0.3555**	0.4693**	0.4504**	0.4656**
			(0.1083)		(0.1713)	(0.2121)	(0.1852)	(0.1922)
$\hat{\gamma}_3$				0.3140**	0.5832*	0.6577*	0.7408*	0.7537*
				(0.1350)	(0.2985)	(0.3426)	(0.3769)	(0.3842)
Equity/assets							-6.9428	-5.6531
							(7.5309)	(7.0074)
Loan loss prov./net interest income							0.6164	0.5372
							(0.5358)	(0.4991)
Non-interest exp./revenues							0.2881	0.1789
							(0.4424)	(0.4034)
ROA							10.9050	5.4681
							(26.2757)	(24.9443)
Observations	241	363	473	569	569	477	439	435
Treated banks	61	61	61	61	61	48	48	48
R^2	0.1112	0.0940	0.0959	0.0793	0.1564	0.1732	0.2522	0.2720
Temporal mean over post-treatment outcomes	Yes	Yes	Yes	Yes	No	No	No	No
Constant sample for post-treatment observations	No	No	No	No	No	Yes	Yes	Yes
Bank risk controls included	No	No	No	No	No	No	Yes	Yes
Matching variables included	No	No	No	No	No	No	No	Yes

Note: This table reports estimation results for the model specified in equation (3.1) with $\tau = 0, \ldots, 3$ in columns (1)-(4), respectively, and the model specified in equation (3.2) for $T = 3$ in columns (5)-(8). All estimations are based on the matched sample using a combined statistical distance function for matching following Dettmann et al. (2011, 2020). The outcome variable is σ^{ROA}, which measures the bank's earnings volatility, i.e., a larger number indicates higher bank risk. Matching variables for all estimations are log assets, tier 1-ratio, loans/assets, tax incentive dummy, higher AT1 requirements dummy, investment bank dummy. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

All regressions confirm my baseline findings. The results show, however, effects in earlier post-treatment periods, i.e., $\hat{\gamma}_0$ and $\hat{\gamma}_1$, to be of a higher level of statistical significance (at least at a 5%-level) whereas $\hat{\gamma}_3$ is only weakly statistically significant in most regressions. That could hint towards increases in earnings volatility preceding a reduction in a bank's distance to insolvency.

Note, all regression results discussed in the remainder of this section are based on the model specified in equation (3.2) for $T = 3$, and the underlying sample only includes banks for which data on bank risk is available for all post-treatment periods (i.e., constant sample for post-treatment observations). All estimations include bank risk controls. Standard errors are clustered at the bank level.

Table 3.6 reports robustness checks results concerning matching variable selection, matching timing, and the matching method. First, I exclude the matching variable loans/asset since evidence for the impact of this business model proxy on the propensity to issue CoCos is so far scarce compared to the more widespread evidence for the relevance of bank size and capitalization (see Section 3.3.4). Column (1) shows that estimated treatment effects remain largely unchanged.

Next, I match based on the ratio of risk-weighted assets to total assets rather than the tier 1-ratio. Thereby, I follow Fajardo and Mendes (2020), who do not find a significant effect for capitalization. Yet, they identify RWA/assets to be associated with a lower propensity of a bank to issue CoCo bonds. Column (2) shows that, despite a severe sample size reduction due to missing data, my main finding of a negative statistically significant treatment effect that persists over several years after the issuance of AT1 CoCos on the z-score remains unchanged.

I also include additional matching variables to address moral hazards possibly affecting treatment selection. In a theoretical study, Hesse (2018) shows that banks with lower price-to-book ratios, a proxy for a banks' charter value which indicates higher moral hazard levels, must pay higher coupons on their CoCos, suggesting that an efficient pricing mechanism might deter low-quality banks from issuing CoCos in the first place. Column (3) shows that treatment effects become less pronounced when matching based on charter values. Yet, matching based on another proxy for moral hazards, that is a dummy indicating whether a bank is systemically important (Goncharenko et al., 2020) and, thus, is likely considered too-big-to-fail, fully confirms the baseline results (column (4)).

Furthermore, my baseline findings are robust against changing the matching

TABLE 3.6: Robustness checks - sensitivity to matching variables, time, and method

Outcome: ln(z-score)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\hat{\gamma}_0$	-0.3001**	-0.2261	-0.1038	-0.2584	-0.2885	-0.1995	-0.2433**
	(0.1385)	(0.1505)	(0.1700)	(0.1961)	(0.1854)	(0.1487)	(0.1171)
$\hat{\gamma}_1$	-0.5346***	-0.5518**	-0.2941	-0.6508***	-0.5018**	-0.5334***	-0.5169***
	(0.1894)	(0.2458)	(0.1928)	(0.2201)	(0.2183)	(0.1927)	(0.1456)
$\hat{\gamma}_2$	-0.5505**	-0.8249**	-0.3996*	-0.7730***	-0.6830***	-0.7880***	-0.5603***
	(0.2551)	(0.3259)	(0.2192)	(0.2639)	(0.2279)	(0.2612)	(0.1553)
$\hat{\gamma}_3$	-0.5167*	-0.8957*	-0.5968**	-0.5852**	-0.5700**	-0.6598***	-0.4347**
	(0.2923)	(0.5023)	(0.2937)	(0.2506)	(0.2360)	(0.2337)	(0.1715)
Observations	442	281	395	439	444	401	442
Treated banks	49	31	42	48	48	43	45
R^2	0.1930	0.3193	0.2823	0.3168	0.2544	0.3651	0.3763
Continuous matching variables:							
Log assets	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tier 1-ratio	Yes	No	Yes	Yes	Yes	Yes	Yes
Loans/assets	No	Yes	Yes	Yes	Yes	Yes	Yes
Risk-weighted assets/assets	No	Yes	No	No	No	No	No
Charter value	No	No	Yes	No	No	No	No
ln(z-score)	No	No	No	No	No	Yes	No
Lagged ln(z-score)	No	No	No	No	No	Yes	No
Categorical matching variables:							
Tax incentives dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Higher AT1 requirements dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Investment bank dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Systemically important bank dummy	No	No	No	Yes	No	No	No
Matching time	$t=-1$	$t=-1$	$t=-1$	$t=-1$	$t=-2$	$t=-1$	$t=-1$
Matching method	Stat. distance	Stat. distance	Stat. distance	Stat. distance	Stat. distance	Stat. distance	Synth. control

Note: This table reports estimation results for the model specified in equation (3.2) for $T = 3$. The sample only includes banks for which data on bank risk is available for all post-treatment periods (i.e., constant sample for post-treatment observations). The outcome variable is ln(z-score), which measures the bank's distance from insolvency, i.e., a smaller number indicates higher bank risk. All estimations include bank risk controls. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

period to two years before a bank issues AT1 CoCos (column (5)) as well as against adding the (lagged) outcome variable (i.e., ln(z-score) in $t = -1$ and $t = -2$) to the set of matching variables (column (6)).

Lastly, I apply the synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al., 2010) to find the weighted combination of controls for each treated bank that minimizes the mean squared prediction error in the matching period $t = -1$.[17] Each donor pool consists of all control banks in the respective calendar years with matching values in the categorical variables. Column (7) presents the regression results for the treated banks and their synthetic controls. I find negative and highly statistically significant treatment effects one and two years after the issuance of AT1 CoCo bonds that are very similar in size to my respective baseline findings (Table 3.2, column (7)). Moreover, the results based on the synthetic control method show a smaller treatment effect in the third post-treatment period, whereas $\hat{\gamma}_0$ is comparable in size (both significant at 5%-level). Overall, my baseline findings prove to be robust against various changes in the matching procedure.

Table 3.7 displays the results for several robustness checks addressing potential treatment effect heterogeneity depending on issuer characteristics. First, I exclude all issuers headquartered in Switzerland due to the regulatory differences to EU capital regulations outlined in Section 3.2.2. Second, I exclude all German and British banks that issued AT1 CoCo bonds after 2013 due to the clarification of the tax deductibility rules around that time.[18] Columns (1) and (2) show that neither of these sample restrictions affects my main findings in a meaningful way.

Results become considerably weaker when excluding all treated banks with total assets above the 75^{th} percentile at $t = 0$ from the sample. Column (3) shows significant treatment effects only for the year of the issue and two years later at a 10%-level. The latter is also smaller in absolute size versus the baseline findings in column (7) of Table 3.2. This hints, again, in the direction of the treatment effect not being linear in bank size. However, column (4) of Table 3.7 does not present evidence for larger or statistically stronger treatment effects for the sub-sample when the bottom 25% of treated banks in terms of size are excluded compared to the baseline findings.

[17]Note, minimizing the mean squared prediction error requires the outcome variable to be observable for the matching period. That reduces the number of treated banks to 45.

[18]My baseline sample does not include AT1 CoCo issues by German or British banks before 2013. Thus, this restriction effectively results in excluding German and British issuers from the sample.

TABLE 3.7: Robustness checks - sensitivity to heterogeneity of issuers

Outcome: ln(z-score)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{\gamma}_0$	-0.2623	-0.2055	-0.2528*	-0.3112*	-0.2648	-0.3143*	-0.3437*	-0.2115
	(0.1575)	(0.1364)	(0.1505)	(0.1649)	(0.1588)	(0.1595)	(0.1760)	(0.1546)
$\hat{\gamma}_1$	-0.4851**	-0.4063**	-0.3044	-0.5500**	-0.5181***	-0.5170***	-0.6349***	-0.3223*
	(0.1900)	(0.1802)	(0.1837)	(0.2094)	(0.1750)	(0.1791)	(0.2006)	(0.1734)
$\hat{\gamma}_2$	-0.5669**	-0.5458**	-0.3966*	-0.5024*	-0.5747***	-0.5983***	-0.7661***	-0.4112*
	(0.2290)	(0.2312)	(0.2344)	(0.2815)	(0.1976)	(0.2202)	(0.2419)	(0.2259)
$\hat{\gamma}_3$	-0.7111**	-0.8156**	-0.5561	-0.6968*	-0.6924**	-0.7562**	-0.9715**	-0.5939*
	(0.3294)	(0.3803)	(0.3554)	(0.3764)	(0.2986)	(0.3123)	(0.3708)	(0.3332)
Observations	387	343	312	331	317	312	321	328
Treated banks	42	39	36	36	35	36	36	36
R^2	0.2546	0.2794	0.1862	0.2970	0.2654	0.2724	0.3275	0.2104
Sample selection criteria:	Country of issuer		Log assets		Tier 1-ratio		Loans/assets	
	≠CH	≠UK & G	< 75%ile	> 25%ile	< 75%ile	> 25%ile	< 75%ile	> 25%ile

Note: This table reports estimation results for the model specified in equation (3.2) for $T = 3$. The sample only includes banks for which data on bank risk is available for all post-treatment periods (i.e., constant sample for post-treatment observations). All estimations are based on the matched sample using a combined statistical distance function for matching following Dettmann et al. (2011, 2020). The outcome variable is ln(z-score), which measures the bank's distance from insolvency, i.e., a smaller number indicates higher bank risk. Matching variables for all estimations are log assets, tier 1-ratio, loans/assets, tax incentive dummy, higher AT1 requirements dummy, investment bank dummy. All estimations include bank risk controls. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

Neither excluding treated banks that are relatively low capitalized, i.e., tier 1-ratio $< 75^{th}$ percentile at time of issue (column (5)), nor estimating the model based on treated banks that have higher tier 1-ratio than the bottom 25% (column (6)) changes the results. Yet, all treated banks in the baseline sample reported a tier 1-ratio of at least 6% at $t = 0$. Hence, the baseline sample only comprises comparatively well-capitalized banks.

Finally, columns (7) and (8) suggest that banks with lower loans-to-assets ratios might engage, ceteris paribus, in more excessive risk-taking after issuing AT1 CoCo bonds than banks with a business model more geared towards lending. Treatment effects are larger in absolute size and statistically significant at a higher level if I exclude treated banks with a loans-to-assets ratio above the 75^{th} percentile compared to my baseline findings. In contrast, results for the sub-sample excluding treated banks with the lowest loan volume relative to assets are weak.

In a final set of robustness checks, I examine whether my results are sensitive to differences between the issuances. Table 3.8 presents the results. First, I exclude all cases in which a treated bank issues multiple CoCo bonds over the outcome period. In other words, I only consider isolated instances of AT1 CoCo bond issuances. As expected, column (1) shows treatment effects to shift to earlier (post-)treatment periods. More specifically, I find ln(z-score) to be, on average, 0.50 lower for treated banks in the year the bank issues the CoCo bond versus the level of expected risk-taking in the absence of the treatment. This effect is statistically significant at a 5%-level. I find weakly statistically significant treatment effects, similar in size, for both $t = 1$ and $t = 2$. Considering the severe reduction in sample size when only analyzing single AT1 CoCo issues, I take these findings as tentative evidence that repeated AT1 CoCo issuances in later post-treatment periods might drive some of my baseline findings for those years. However, the main finding of a persistent positive impact of issuing AT1 CoCo bonds on bank-risk remains robust.

Furthermore, I aim at evaluating whether treatment effects increase with the relative size of the aggregate issue volume. Thus, I exclude all treated units that issued AT1 CoCo bonds amounting to more than 0.6% of the bank's average assets over the outcome period.[19] That is, I only keep the 75% of banks that issued the smallest relative amounts of AT1 CoCos. Next, I drop all treated units with aggregate issue volume accounting for less than 0.27%, i.e., the 25^{th} percentile.

[19] In the baseline sample, aggregate issue volume/average assets ranges from 0.005% to slightly above 4%, with an average of 0.55%.

TABLE 3.8: Robustness checks - sensitivity to heterogeneity of CoCos

Outcome: ln(z-score)	(1)	(2)	(3)	(4)	(5)
$\hat{\gamma}_0$	-0.4962**	-0.2758	-0.2880**	-0.2870	-0.2869
	(0.2096)	(0.1862)	(0.1359)	(0.1793)	(0.2091)
$\hat{\gamma}_1$	-0.4873*	-0.4798**	-0.4706**	-0.5714**	-0.4455*
	(0.2494)	(0.2264)	(0.1823)	(0.2328)	(0.2538)
$\hat{\gamma}_2$	-0.5361*	-0.6760**	-0.5799***	-0.7634***	-0.3771
	(0.3141)	(0.2542)	(0.2159)	(0.2790)	(0.2803)
$\hat{\gamma}_3$	-0.6053	-0.8901**	-0.7263**	-0.8754**	-0.2633
	(0.3832)	(0.3467)	(0.3038)	(0.3760)	(0.2750)
Observations	179	326	320	266	135
Treated banks	20	36	36	30	14
R^2	0.2631	0.3262	0.2612	0.2510	0.3618
Sample selection criteria:	Single issues only	Issue volume/assets		Loss absorption	
		< 75%ile	> 25%ile	write-down	equity conversion

Note: This table reports estimation results for the model specified in equation (3.2) for $T = 3$. The sample only includes banks for which data on bank risk is available for all post-treatment periods (i.e., constant sample for post-treatment observations). All estimations are based on the matched sample using a combined statistical distance function for matching following Dettmann et al. (2011, 2020). The outcome variable is ln(z-score), which measures the bank's distance from insolvency, i.e., a smaller number indicates higher bank risk. Matching variables for all estimations are log assets, tier 1-ratio, loans/assets, tax incentive dummy, higher AT1 requirements dummy, investment bank dummy. All estimations include bank risk controls. Standard errors are clustered at the bank level. *,**, and *** denote statistical significance at the 10-, 5-, and 1%-level.

While columns (2) and (3) show the treatment effect in the year of issue to be statistically significant only in the case of larger aggregate issue volumes, my findings remain unchanged overall.

Lastly, I analyze treatment effects heterogeneity related to the only CoCo design feature not defined by regulations - the loss absorption mechanism. When running the regression solely based on contingent convertible bonds written-down once the trigger event is met, estimated treatment effects become more pronounced in size (column (4)). In contrast, issuing CoCos that convert to equity if the bank becomes financially distressed does not seem to result in comparable risk growth (column (5)). That supports a prevailing view in the literature: Write-down CoCos are more likely to exacerbate the moral hazard problem than conversion-to-equity contingent convertibles.[20] Nevertheless, given the small sample sizes, particularly for the CoCos that convert to equity, results can only be considered indicative. Yet, the fact that the large majority of banks opt to issue write-down rather than conversion-to-equity bonds attests to the argument.

3.5 Conclusions

Building on the insights from previous studies on what determines a bank's propensity to issue contingent convertibles, I apply a matching-based difference-in-differences approach for a staggered treatment adoption to provide empirical evidence for the effect issuing additional tier 1 capital-qualifying CoCo bonds has on bank-level risk. Thereby, I close a gap in the empirical literature, which is limited in scope and only focuses on changes in market participants' anticipation and perception of bank risk rather than the actual realization over time.

My analysis reveals that issuing AT1 CoCo bonds results in significantly higher risk-taking one to three years after the issuance compared to the expected development of bank risk would a financial institution not issue AT1 contingent convertibles. The effect is economically large and robust against changes in the model specifications and matching procedure. Rather than having a net negative impact, issuing contingent convertibles seems to impede an overall positive time trend towards greater bank stability.

Additional regressions provide tentative evidence indicating the treatment effect on risk might be increasing in bank size and decreasing in loans/assets. In

[20]See Section 3.2.1 for a detailed review of the relevant literature.

contrast, I do not find strong evidence suggesting that the relative aggregate issue volume is a determining factor for the risk effect. As expected, write-down CoCos appear to aggravate the detrimental effect on bank stability.

Overall, my results confirm theoretical predictions that currently outstanding CoCo bonds create incentives contrary to bank stability objectives (e.g., Berg and Kaserer, 2015).

However, due to the regulatory requirements regarding the characteristics of AT1-qualifying CoCos it is inherently impossible to assess whether a different bond design would indeed contribute to bank stability as also suggested by theoretical studies. Hence, I cannot provide any affirmative evidence for the stability-strengthening effect of CoCo bonds that convert into equity at a reasonably high conversion ratio when a market-value-based trigger event occurs. Another limitation concern the small sample size, which limits the power of my statistical inferences. Yet, given that all results consistently point in the same direction, banks relying on these poorly-designed CoCo bonds as regulatory capital should raise serious concerns regarding its impact on financial stability.

Insights from the United States reveal how banks could easily be deterred from issuing contingent convertibles. US banks largely refrain from issuing CoCos because all financial instruments that are supposed to qualify as additional tier 1 capital must be handled as if they were equity (Flannery, 2014). That implies that coupon payments are not tax-deductible, making issuing CoCo bonds less cost-effective.

In 2017, Sweden abolished the tax deductibility for coupon payments on securities that qualify as additional tier 1 capital.[21] In 2019, the Netherlands followed suit.[22]

[21] https://www.bruegel.org/2016/09/taxpayer-should-not-facilitate-risky-bank-cocos/

[22] https://www.government.nl/latest/news/2018/06/29/cocos-not-tax-deductible-anymore-as-from-1-january-2019

3.A Data appendix

TABLE A3.1: Variable definitions

Risk measures	Definition	References
Distance from insolvency	Logarithm of the z-score, i.e., the sum of ROA and the capital to asset ratio divided by the standard deviation of ROA.	Bhagat et al. (2015); Houston et al. (2010); Hoque et al. (2015); Laeven and Levine (2009)
Earnings volatility	Standard deviation of ROA over a five-year rolling window.	Laeven and Levine (2009)
Continuous matching variables		
Log assets	Logarithm of total assets to measure bank size.	Avdjiev et al. (2017); Fajardo and Mendes (2020); Goncharenko et al. (2020)
Tier 1-ratio	Tier 1 capital over total assets to measure capitalization.	Avdjiev et al. (2017); Boyson et al. (2016); Hesse (2018); Vallée (2019)
Loans/assets	Net loans over total assets as a proxy for the business model and the liquidity.	Beck et al. (2013); Goncharenko et al. (2020); Laeven et al. (2016)
Risk-weighted assets/assets	Risk-weighted assets over total assets to proxy the riskiness of the assets.	Fajardo and Mendes (2020)
Charter value	Market capitalization over total equity to proxy the moral hazard level.	Hesse (2018)
Categorical matching variables		
Tax incentives dummy	Dummy = 1 if the bank is headquartered in Germany or the United Kingdom and if the observation year is $\geq$ 2013.	
Higher AT1 requirements dummy	Dummy = 1 if the bank is headquartered in Switzerland and if the observation year is $\geq$ 2012 and $\leq$ 2016.	
Investment bank dummy	Dummy = 1 if the bank is classified to belong to the TRBC Investment banking and investment services industry group.	Bhagat et al. (2015)
Systemically important bank dummy	Dummy = 1 if the bank is a global systemically important institution according to the ECB and FSB as of 2019.	Goncharenko et al. (2020)
Additional control variables		
Equity/assets	This ratio is a proxy for capital adequacy.	Bhagat et al. (2015); Houston et al. (2010)
Loan loss provisions/net interest income	This ratio is a proxy for asset quality.	Beck et al. (2013)
Non-interest expense/gross revenues	This ratio is a proxy for management capability.	Agoraki et al. (2011)
Return on average assets	This ratio is a proxy for earnings capability.	Bhagat et al. (2015); Houston et al. (2010); Jiménez et al. (2013)

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