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CoCo Bonds, Bank Stability, and Earnings Opacity

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CoCo Bonds, Bank Stability, and Earnings Opacity

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CoCo Bonds, Bank Stability, and Earnings Opacity*

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Abstract

This paper examines the effect of CoCo bonds that qualify as additional tier 1 capital on bank stability and reporting. The results reveal a significant reduction in the distance to insolvency following the hybrid bond issuance due to increased earnings volatility. Banks report less stable net income due to more volatile loss provisions, which increases earnings opacity rather than reflects changes in asset quality. The findings are consistent with the premise that persistent uncertainty and misconceptions among investors about bail-in likelihoods limit their monitoring engagement, which results in banks becoming less transparent.

Keywords: AT1 capital, bank stability, Basel III, CoCo bonds, earnings opacity

JEL classification: G21, G28, G32, M41

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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](#)). National governments bailed out several institutions considered too big to fail, creating a moral hazard problem. Regulators have since specified new bail-in rules and implemented higher capital requirements, which banks try to meet most cost-effectively ([Ammann et al., 2017](#); [Fiordelisi and Scardozzi, 2022](#)).

That is where contingent convertible (CoCo) bonds come in. A CoCo is a subordinated debt security with a fixed coupon rate that can qualify as regulatory capital while entailing a tax shield, giving it a cost advantage over equity instruments. The hybrid bond automatically converts from debt into equity or is subject to a write-down when a trigger event occurs that is prespecified at the time of issue. Unlike other convertible securities, CoCo bonds do not entail an option for the investor or the issuer. Hence, these bonds can absorb losses before the issuer encounters difficulties recapitalizing and contribute to bank stability (e.g., [Flannery, 2005](#)).

A large body of theoretical literature suggests that the bond design is decisive in that regard (e.g., [Chen et al., 2017](#); [Hilscher and Raviv, 2014](#); [Martynova and Perotti, 2018](#); [Pennacchi and Tchisty, 2019](#); [Sundaresan and Wang, 2015](#)). However, European regulators and legislators have implemented requirements for the design of CoCo bonds to qualify as

additional tier 1 (AT1) capital that contradict what the literature suggests is sensible from the stability perspective ([Glasserman and Perotti, 2017](#)). The Capital Requirement Regulation (CRR), which defines capital adequacy for EU banks, specifies the trigger event for AT1 CoCo bonds to be the Common Equity Tier 1 (CET1) ratio falling below 5.125% ([European Union, 2013b](#)). It also permits a write-down of the bond when the trigger occurs rather than requiring loss absorption through equity conversion. That effectively results in the breakdown of the seniority principle, with debt holders bearing losses before equity investors. Consequently, write-down CoCos can create a moral hazard problem as stockholders and management can shift losses to CoCo investors without having to share potential profits, which creates an incentive to engage in excessive risk-taking and undermines bank stability ([Berg and Kaserer, 2015](#); [Goncharenko, 2022](#)).¹

In contrast, the Banking Recovery and Resolution Directive (BRRD), which prescribes the procedures applicable to banks in or near failure, stipulates that shareholders must bear losses first (cf. BRRD, Art. 34), effectively restoring the order of seniority. Yet, the prerequisites for authorities to assume resolution powers are not unambiguous, as they include, for instance, the authority’s assessment of whether the bank is failing or likely to fail (cf. BRRD, Art. 32). That creates uncertainty around the likelihood of conversion or write-down of CoCos. In the prospectus of a write-down AT1 CoCo [Deutsche Bank AG \(2021\)](#) issued in May 2021, the bank itself points out the risk that “the competent authority may decide that the Issuer should allow a Trigger Event to occur at a time when it is feasible to avoid it.”

These uncertainties and information frictions about the bail-in likelihood might result in

¹[Section 2](#) provides an in-depth discussion of the institutional and legal background regarding the design of contingent convertible bonds and their use to meet capital requirements in the EU.

a net reduction of private monitoring, which could increase reporting opacity. Shareholders may reduce their monitoring efforts, expecting CoCo investors to be bailed in first, given the stipulations in the CRR. At the same time, CoCo investors might not compensate for this reduced monitoring, believing that equity holders must always bear losses first.²

Therefore, I investigate how issuing AT1 CoCo bonds impacts bank stability and reporting. The results can inform the debate on sound and incentive-compatible regulatory requirements for ‘going-concern’ capital and the importance of removing information frictions regarding bail-in likelihoods. That is crucial given that the aggregated annual outstanding volume of AT1 CoCos issued by listed banks in the EU has grown considerably since 2014 (see [Figure 1](#)), while the concentration in the issue market has declined, that is, more banks engage in issuing CoCos (see [Figure 2](#)).

I estimate the average effect issuing an AT1-qualifying CoCo bond has on the stability of the issuing bank based on the difference-in-differences estimator for staggered treatment adoption by [Callaway and Sant’Anna \(2021\)](#) and [Sant’Anna and Zhao \(2020\)](#) for event windows of up to three years after the issuance. My analysis draws upon an unbalanced panel of all listed banks in the EU for 2008-2021. I obtain annual bank balance sheet and income statement information from Bankfocus and augment my data set with stock market information retrieved from Refinitiv. Refinitiv also provides information on CoCo bond issuances, the issue amount, the amount outstanding over time, the regulatory classification, the loss absorption mechanism (i.e., equity conversion vs. write-down), and trigger level. I supple-

²The recent events surrounding the write-down of AT1 CoCos issued by Credit Suisse provide interesting anecdotal evidence for the substantial uncertainty and possibly misconceptions among CoCo investors about the risks they bear. For more details and the reaction of EU bank regulators, see [Section A.1](#) in the Appendix.

ment my data set with manually collected information from the bond prospectuses about how the bank plans to use the proceeds from the CoCo issues.

To address potential selection bias at the bank level, I control for bank size, regulatory capitalization, and capital quality, which previous studies have found to increase the likelihood of issuing contingent convertibles ([Avdjiev et al., 2020](#); [Fajardo and Mendes, 2020](#); [Goncharenko et al., 2021](#)) and also affect bank stability (e.g., [Bhagat et al., 2015](#); [Gornall and Strebulaev, 2018](#); [Laeven et al., 2016](#)). Additionally, I include a standard set of bank-level controls. I add two sets of important country-level control variables. First, I control for the corporate tax rate since contingent convertibles can entail a tax shield while outstanding due to tax deductible coupon payments. Hence, differences in the potential tax shield might affect a bank’s likelihood of issuing a hybrid instrument rather than preferred shares, which also qualify as AT1 capital. Second, I append the European Banking Union directives database assembled by [Koetter et al. \(2022b\)](#) to my data set to control for the time variation in the implementation of regulatory changes concerning bank capital (CRD IV) and resolution (BRRD) across countries.

The analysis reveals that a bank’s z-score, i.e., its distance to insolvency, significantly decreases after issuing AT1 CoCo bonds, all else equal. The effect is economically meaningful and corresponds to a drop of 0.8 standard deviations from the mean. I test for the absence of pre-trends and perform a placebo test, which substantiates the validity of my identification. My baseline results are robust to changes in the sample selection, additional bank-level confounders, and alternative computations of the outcome variable. The result is consistent with the premise that subordinated debt instruments, which meet the EU regulatory requirements

for AT1 capital, negatively impact bank stability.

Next, I turn to investigating the underlying mechanism that is driving my baseline effect. I do not find evidence for an increase in asset risk. Instead, the decomposition of the baseline effect on the z-score reveals that banks report more volatile earnings in the years after issuing an AT1 CoCo bond. Banks appear to move closer to insolvency due to higher income volatility following the issuance of an AT1-qualifying CoCo, all else equal, without increasing asset risk.

I perform additional estimations to identify the income component that becomes more volatile and find a higher standard deviation for loss provisions following the issuance of an AT1-qualifying CoCo. That could hint at banks issuing contingent convertibles in anticipation of a deterioration in asset quality and the need to increase provisions without eroding the existing capital base. However, I do not find a statistically significant treatment effect on loss provisions nor any evidence for loss provisions becoming more sensitive to future changes in asset quality after issuing an AT1 CoCo.

Alternatively, banks might become less transparent after issuing an AT1 CoCo due to a net reduction in monitoring. To test this hypothesis, I compute a proxy for earnings opacity based on the residuals from estimating a loss provision model that accounts for the fundamental determinants of changes in loss provisions (e.g., [Beatty and Liao, 2014](#); [Danisewicz et al., 2021](#); [Jiang et al., 2016](#); [Lin et al., 2018](#); [Yue et al., 2022](#)). I estimate the treatment effect and find that issuing an AT1 CoCo bond increases earnings opacity. The result is robust to several changes in the loss provision model underlying the earnings opacity proxy. Additional sensitivity analyses reveal that the effect increases with the relative size of the issue volume.

This paper contributes to the limited number of empirical studies examining the impact of

going-concern contingent convertibles on bank-level stability. My results add to the findings of [De Spiegeleer et al. \(2017\)](#), who show that the volatility of CET1 ratios increases after banks issue write-down AT1 CoCos. My findings indicate that this could be attributable to more volatile retained earnings resulting from less stable net income. [Fiordelisi et al. \(2020\)](#) find that issuing equity conversion CoCo bonds lowers the stock return volatility of EU banks without affecting asset risk, which indicates that shareholders expect CoCos to convert to equity before shareholders are wiped out in insolvency. That lends support to my hypothesis that shareholders might reduce their private monitoring efforts. Thus, my paper builds on this analysis and shows that issuing CoCo bonds results in higher earnings opacity, suggest that CoCo investors do not offset the reduced monitoring. [Avdjiev et al. \(2020\)](#) examine the short-term announcement effect of (AT1) CoCo issues and find a negative effect on CDS spreads within an eleven-day window.³ Although CDS spreads often serve as a general proxy for insolvency risk, it is important to note that the analyzed CDS spreads pertain to senior unsecured debt, which ranks below equity and AT1-qualifying CoCo bonds in the seniority order. Hence, while this finding indicates that market participants expect the risk for senior creditors to fall after a bank issues the hybrid instrument, it does not speak to any uncertainties and misconceptions among shareholders and CoCo investors regarding their bail-in likelihood and potential adverse effects for their monitoring efforts. Nor does it reveal insights about the bank stability in the years following the issuance. Thus, my analysis adds to these insights. Lastly, [dos Santos Mendes et al. \(2022\)](#) analyze banks' contributions to systemic risk following the issuance of CoCo bonds, whereas my analysis focuses on the

³[Ammann et al. \(2017\)](#) find similar results for a smaller sample.

changes observable at the individual bank level.

My results also add to the literature on banks' earnings management and use of reporting discretion in the context of banking regulation ([Bischof et al., 2021](#); [Di Fabio et al., 2021](#); [Gallemore, 2023](#); [Nicoletti, 2018](#); [Yue et al., 2022](#); [Wheeler, 2019](#)), capital adequacy ([Bushman, 2016](#); [Huizinga and Laeven, 2012](#)), and risk ([Bushman and Williams, 2012, 2015](#); [Chen et al., 2022](#); [Cohen et al., 2014](#); [Leventis et al., 2011](#)). The evidence I provide indicates that more volatile loss provisions are associated with less stable reported income for banks after increasing their regulatory capital through issuing securities that can result in bond investors bearing losses before shareholders. In light of the persistent information frictions regarding the bail-in likelihood of AT1 CoCo investors, my findings further speak to the relevance of private monitoring for earnings transparency ([Bouvatier et al., 2014](#); [Fonseca and Gonzalez, 2008](#); [Gopalan, 2022](#); [Jiang et al., 2016](#)). My results are in line with an inverse relationship between incentives for creditor monitoring with earning opacity ([Danisewicz et al., 2021](#)).

Lastly, the study is also part of the broader literature on the effectiveness of requirements for bail-inable debt ([Bernard et al., 2022](#); [Cutura, 2021](#); [Kupiec, 2016](#); [Martynova et al., 2022](#)) and, more specifically, how recovery and resolution regulation impacts bank behavior and strategy ([Fiordelisi and Scardozzi, 2022](#); [Lambrecht and Alex, 2023](#); [Pandolfi, 2022](#)). My results imply that banks move closer to insolvency due to higher income volatility if regulatory requirements for subordinated debt instruments that are supposed to be bail-inable in a 'going concern' state leave room for shareholders to shift risk to bond investors and are associated with uncertainty for bond investors about their likelihood of bail-in.

2 Institutional and regulatory background

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](#)).

To that end, the [Basel Committee on Banking Supervision \(2010, 2011\)](#) has specified the following criteria for hybrid bonds to qualify as additional tier 1 capital that is supposed to absorb losses while the bank is still solvent (going-concern capital): Both a 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 should reserve the right to initiate conversion or write-down if necessary. Public sector assistance to avoid bankruptcy is only possible after the conversion or write-down of the AT1 instruments. Moreover, the issuer must be capable of suspending the coupon payments at any time, and AT1 bonds are to be perpetual bonds ([Basel Committee on Banking Supervision, 2022](#)).

2.1 Regulatory changes and market growth

While banks 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), gained traction. After some early issuances, aggregated outstanding issue volume of AT1 CoCos issued by listed banks in the EU remained fairly small and stable until 2012 ranging around 25 bn euros (see [Figure 1](#)), with the five most active banks accounting for 60-90% of new issues during those early years (see [Figure 2](#)). However, the figures also show that the outstanding AT1 CoCo issue volume increased after 2013, with more banks becoming active in the issue market. Noticeably, the rise in market dynamics coincides with the start of the Basel III phase-in period and its adoption into EU law.⁴ The [European Union](#) (2013a,b) implemented the Basel III guidelines and principles by passing the Capital Requirement Regulation (CRR) and the Capital Requirement Directive IV (CRD IV). The CRR has been binding for all member states since January 2014. It sets the standards for adequately measuring capital and risk. CRD IV further specifies the framework for capital buffers and the supervisory review process, which can result in additional capital requirements. In May 2014, the [European Union](#) (2014) passed the Banking Recovery and Resolution Directive (BRRD). It prescribes the procedures applicable to banks in or close to failure. To ensure sufficient funds are available for bail-in, BRRD also introduces minimum requirements for own funds and eligible liabilities (MREL). The 2019 amendment to the Capital Requirement Regulation adds the concept of Total Loss Absorbency Capacity (TLAC), which pursues a goal similar to MREL but only applies to global systemically important banks ([European Union, 2019](#)). Contingent convertible bonds typically meet both MREL and TLAC standards. Hence, increased regulatory capital requirements have presumably driven market growth in

⁴CoCo bonds can also qualify as tier 2 (T2) instruments. However, T2 capital intends 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. Therefore, T2 CoCo bonds are not the subject of this study.

the past, and CoCo bonds will likely only become more relevant as the new requirements are phased-in.

In contrast to the dynamic European market, US banks have largely refrained from issuing CoCos because all AT1 capital instruments must be treated like equity ([Flannery, 2014](#)). That implies that coupon payments on AT1-qualifying CoCos are not tax-deductible in the US, making issuing these bonds less cost-effective. In contrast, it has been a widespread practice in European countries to allow coupon payments to be tax-deductible for the issuer ([Bundgaard, 2017](#)). Sweden and the Netherlands are an exception to that. The former abolished the tax deductibility for coupon payments on securities that qualify as AT1 capital in 2017. In 2019, the Netherlands followed suit ([S&P Global, 2018](#)). Yet, in most European jurisdictions, tax shields on CoCos continue to apply, making CoCo bonds the financially more attractive option to increase AT1 capital versus, for instance, preferred shares. That is despite research showing that more similar tax rules for debt and equity instruments can significantly reduce bank risk ([Schepens, 2016](#)).

[Figure 3](#) displays the annual outstanding volumes of AT1-qualifying CoCo bonds issued by listed banks in the EU from 2008 to 2021 per country-year scaled by the banking sector size (total MFI assets) relative to the country- and year-specific corporate income tax rate. The scatter plot suggests a positive relation between banks choosing to issue AT1 CoCos and the corporate tax rate applicable to their income. Hence, the possibility to increase regulatory capital levels with contingent convertible bonds at lower costs due to a tax shield appears to be at least part of the reason EU banks issue these instruments.⁵

⁵[Avdjiev et al. \(2020\)](#) agree with this assessment. They further suggest that the growth could also be demand-driven: Due to the low-interest-rate environment, fixed-income investors are looking for opportunities

2.2 Regulatory design requirements

The European capital regulation CRR added one crucial detail to the requirements for financial instruments that are supposed to qualify as additional tier 1 capital to the Basel guidelines: CET1 capital falling short of constituting 5.125% of the bank's risk-weighted assets (RWAs) defines the trigger event (cf. CRR, Art. 54). That is a minimum requirement. Banks may choose higher levels for the CET1 ratio. They can also define additional trigger events.

2.2.1 Trigger event

The trigger event is supposed to identify the instant the issuer needs to recapitalize. Basing the trigger event on a book value like the CET1 ratio is widely criticized in the theoretic literature 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., 2020](#); [Flannery, 2005, 2014, 2016](#); [Maes and Schoutens, 2012](#); [McDonald, 2013](#)). Moreover, the trigger level should be reasonably high to ensure the bond is triggered before the bank faces insolvency ([Jaworski et al., 2017](#)). [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 likely not have absorbed any losses on time.⁶

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.

⁶The information defining a trigger event is not necessarily limited to a single financial institution. Some scholars propose to rely on industry-wide data to determine the point in time 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 financial system stability (e.g., [Avdjiev et al., 2020](#); [Flannery, 2016](#); [Maes and Schoutens, 2012](#)). Based on a contingent claim analysis, [Barucci and Del Viva](#)

[Avdjiev and Kartasheva \(2013\)](#), [Calomiris and Herring \(2013\)](#), [Flannery \(2005, 2016\)](#), [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 expectations of future performance. Hence, they are forward-looking and observable daily.⁷

In contrast to what the [Basel Committee on Banking Supervision \(2011\)](#) recommends, CRR does not prescribe that AT1 CoCos must give discretion to the regulator to decide whether it is necessary to trigger a CoCo bond conversion, which reduces uncertainty and the risk of regulatory forbearance ([Avdjiev and Kartasheva, 2013](#); [Maes and Schoutens, 2012](#)).

2.2.2 Loss absorption mechanism

CRR does not make any further specifications regarding the permissible loss absorption mechanism for AT1 instruments. Hence, 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 up all equity.

The European Securities and Markets Authority (ESMA) issued a warning about the risks

(2012) infer that systemic trigger CoCos do not reduce bankruptcy costs.

⁷[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. [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 \(2016\)](#) and [Pennacchi and Tchisty \(2019\)](#) show that, under certain conditions, a unique price equilibrium exists.

associated with investing in CoCo bonds in that regard, stating that “contrary to classic capital hierarchy, CoCo investors may suffer a loss of capital when equity holders do not” ([ESMA, 2014](#)).

Considering the specific CRR trigger event requirement, CoCo investors can get bailed in before shareholders if distressed banks draw down CET1 capital sources other than share capital first. These alternative sources are retained earnings, additional reserves, accumulated income, funds for general banking risk, and share premiums (Article 26 of CRR). The CET1 ratio of listed EU banks in my sample is 17.5% in 2021.⁸ Yet, the ratio of CET1 capital other than common equity, i.e., share capital and capital surplus, to risk-weighted assets amounts to almost 11%. Hence, the CET1 ratio of an average bank in my sample can drop to 6.5% before the share capital value depreciates. Since I cannot differentiate between share capital and capital surplus in my data, this can be considered an upper bound for the ratio of share capital to risk-weighted assets. Thus, a situation in which shareholders are only partly, or not at all, bailed in while the CET1 ratio falls below 5.125% of RWA seems feasible.

In contrast, if the resolution authority activates its powers under the BRRD (Art. 34), shareholders must always bear losses first, restoring the typical order of seniority. However, the criteria for authorities to assume resolution powers lack clarity, involving assessments of the bank’s potential failure or likelihood of failing (cf. BRRD, Art. 32). That creates uncertainty for CoCo investors about the possibility of conversion or write-down.

[Avdjiev et al. \(2020\)](#), [Flannery \(2014, 2016\)](#), and [Hilscher and Raviv \(2014\)](#) argue that

⁸Similarly, the [ECB \(2022\)](#) reports a CET1 ratio of 15.5% for significant institutions in the fourth quarter of 2021.

the possibility of shifting losses to investors in CoCos with a write-down mechanism likely encourages the management and shareholders of a bank to engage in excessive risk-taking.⁹ Moreover, 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). In the previously mentioned prospectus, Deutsche Bank AG (2021) discloses to that effect that they “may decide not to raise capital at a time when it is feasible to do so, even if that would result in the occurrence of a Trigger Event.”

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 inordinate risks (Flannery, 2005, 2014). If an AT1 CoCo can convert to equity, CRR does not prescribe a particular conversion ratio. The conversion ratio specifies the amount of stock a bondholder receives. Conditional on the conversion ratio, the incurred loss gets divided between bondholders and shareholders: A higher ratio results in more severe dilution.¹⁰ 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).¹¹

To summarize, scholars largely concur that the trigger event of a going-concern CoCo

⁹Martynova 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 assumption that the trigger activation is exogenous.

¹⁰A principal write-down CoCo is an extreme case in which the bondholder does not receive any equity and absorbs the entire loss.

¹¹Koziol and Lawrenz (2012) 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.

bond should depend on a high threshold for a market capitalization measure to ensure that the contingent convertible is triggered as soon as a bank needs to recapitalize. Any delay in activating the loss absorption mechanism impairs the bond’s capacity to reduce the bank’s default probability. Moreover, a CoCo bond should absorb losses by being converted into equity at a reasonably high conversion ratio to deter excessive risk-taking.

In contrast, the regulatory requirements for bonds to qualify as AT1 capital in the EU stipulate a trigger based on a low minimum regulatory capital ratio and leave the choice of a loss absorption mechanism open to the issuer. Two-thirds of AT1 CoCos issued by listed EU banks during 2008-2021 are write-down bonds (see [Table 1](#)). In less than 20% of issues, the trigger event is a CET1 ratio higher than the regulatory minimum requirement of 5.125%. Importantly, the substantial uncertainty surrounding the likelihood of bail-in might impede private monitoring. I hypothesize that issuing CoCo bonds that qualify as additional tier 1 capital according to European regulations has an adverse effect on bank stability.

3 Data and empirical strategy

3.1 Data on banks, CoCo bonds, and country controls

I retrieve balance sheet and income statement data for EU banks from Bankfocus. I augment my accounting data with stock market data from Refinitiv. I restrict my sample to banks listed for at least three years during the 2008-2021 sample period and the respective observations.¹² To eliminate double counting, I retain banks with consolidation codes C1 (published

¹²I also collect data for earlier years to estimate standard deviations for income statement variables based on a five-year rolling window and only later limit the data set to the sample period.

statements are consolidated, companions not in the data set), C2 (published statements are consolidated, companions in the data set), U1 (unconsolidated statements, no companions in the data set or bank doesn't publish consolidated accounts), or A1 (aggregated statements with no companion). I exclude specialized institutions, which results in almost two-thirds of my sample being commercial banks and the remainder comprising bank holding companies, cooperative banks, and saving banks.¹³ Next, I run plausibility checks to account for reporting errors on all balance sheet and income statement figures and replace false entries with missing values. Moreover, I winsorize the estimated bank-level outcomes at a one percent level from above and below to reduce distorting effects due to outliers.

Panel A of [Table 1](#) presents the summary statistics for the full sample comprising 1,376 observations of my main outcome variable for 210 banks. These numbers shrink to 834 annual data points for 124 banks in my final baseline estimation sample due to missing values in control variables and an additional sample restriction to eliminate the potential bias from repeated CoCo issuances. Yet, Panel A of [Table A.2](#) in the Appendix illustrates that this does not alter the summary statistics for the outcome and control variables in any meaningful way. Moreover, the statistics match the literature for similar samples (e.g., [Goncharenko et al., 2021](#); [Fiordelisi et al., 2020](#)).

I collect data on AT1-qualifying Contingent Convertibles issued by the banks in my sample from Refinitiv. Again, I must control for double counting. Banks often emit CoCos as 144A/Reg S offerings, which do not require registration with the Securities and Exchange Commission (SEC) while allowing qualified institutional buyers from the US to

¹³According to Bankfokus definitions, approximately 5% of the observations in my sample belong to other types of financial institutions. Yet, Refinitiv designates these as banks or commercial banks.

invest. 144A/Reg S offerings come with two distinct ISINs. Thus, I drop duplicates in all observable variables but the ISIN and double-check using the prospectuses. I convert the issue amounts to euros based on the year-end exchange rate extracted from the Bankfocus data to ensure consistency. I retrieve information on outstanding volumes over all subsequent years from Refinitiv to control for the redemption of a CoCo. Refinitiv also provides data on the loss absorption mechanism and the trigger level.

Panel B of [Table 1](#) presents summary statistics at the bond level, in parts scaled by bank data in the year of issue, for the entire sample, i.e., all AT1 CoCos issued by the sample banks in the sample period. However, many banks issued CoCos in multiple years of my sample period. To avoid any bias due to repeated issues, I only include instances in my final baseline estimation where a bank issues an AT1 CoCo for the first time, and I can observe at least one ‘clean’ post-treatment observation without another AT1 CoCo issue. That results in 56 treated banks in my final estimation sample (see Panel B of [Table A.2](#) in the Appendix). The issue volume is, on average, larger for equity conversion CoCos, not though in relative terms, i.e., scaled by the bank’s assets, total capital, or AT1 capital. The statistics further indicate first issues might be smaller than repeated issues in absolute terms. However, that is not true in relative terms. That hints at large banks being more likely to issue AT1 CoCos repeatedly.

Additionally, I hand-collect information on the use of proceeds from the prospectuses.¹⁴ [Figure 4](#) depicts the different uses of proceeds specified in the prospectuses of AT1 CoCo bonds issued by publicly traded banks in the EU between 2008 and 2021. The left chart refers

¹⁴Refinitiv also offers a variable on that. However, it is vague and has many missing values.

to the total issue volume of AT1 CoCos that convert to equity if triggered vs. loss absorption through write-down on the right-hand side. The category *strengthen capital base* includes all CoCos with stated use of proceeds like ‘strengthen regulatory capital base’, ‘included in Tier 1 capital base’, and ‘strengthen the leverage ratio and MREL’. If the prospectus does not explicitly refer to *strengthening the capital base* but indicates what the use of proceeds is, the bond is included in the category *other reasons*. That, for example, includes ‘general corporate purposes’ and ‘general financing purposes’. While less than 20% of the equity conversion CoCo volume is specifically issued to strengthen the capital, that is true for more than 45% of the CoCo issue volume that can be subject to a write-down.

I add country-level variables from various sources. I append the European Banking Union directives database assembled by [Koetter et al. \(2022b\)](#) to my data set and create two dummies that indicate if the key law implementing the BRRD and CRD IV in a country has been published for at least six months in a given year. I retrieve data on corporate tax rates from OECD Statistics and Tradeconomics. Data on GDP, unemployment rates, and residential property prices are from the ECB Data Portal. For information on whether general loan loss provisions are tax deductible, I rely on the information [Andries et al. \(2017\)](#) provide and further extend the time series using the latest wave of the World Bank Regulation and Supervision Survey (2019) that covers the time period 2011-2016. Panel C of [Table 1](#) and [Table A.2](#) in the Appendix present the summary statistics for the entire and final estimation sample, respectively.

3.2 Measuring bank risk and stability

My first outcome variable is the z-score, which measures the distance to insolvency and is widely used in the literature (e.g., [Bhagat et al., 2015](#); [Houston et al., 2010](#); [Hoque et al., 2015](#); [Laeven and Levine, 2009](#)):

$$\text{z-score}_{i,t} = \frac{(ROAA_{i,t} + \frac{\text{assets}_{i,t} - \text{liabilities}_{i,t}}{\text{assets}_{i,t}})}{sd(ROAA)_{i,t}}, \quad (1)$$

with $ROAA_{i,t}$ being the annual return on average assets and the associated standard deviation $sd(ROAA)_{i,t}$ measured over the preceding five-year rolling window. The z-score indicates how many standard deviations a bank's return on average assets would have to fall below its anticipated value for bank capital to become depleted ([Poczter, 2016](#)). The higher the score, the more stable a bank is. Using the logarithm of this measure alleviates issues associated with its skewed distribution. The z-score can be sensitive to the estimation procedure for $sd(ROAA)_{i,t}$, which I address in robustness checks. The issuance of an AT1-qualifying CoCo bond increases hybrid capital, which qualifies as equity rather than liabilities, resulting in a higher capital-to-assets ratio by construction. To preclude that distorting my results, I run a robustness check where I exclude hybrid capital from the numerator of the capital-to-assets ratio when computing the z-score.

In additional analyses, I decompose the z-score and estimate the effect that issuing AT1 CoCos has on the different components of the measure, i.e., $ROAA$, the capital-to-assets ratio (Capital/Assets), and earnings volatility $sd(ROAA)$. To delve further into the effect on earnings stability, I estimate the standard deviations of average assets ($sd(AA)$), net income ($sd(\text{net inc.})$), and different income components as additional outcome variables over five-

year rolling windows. These components are interest income ($sd(\text{int I})$), interest expense ($sd(\text{int E})$), operating revenues ($sd(\text{op I})$), staff expenses ($sd(\text{st E})$), administrative expenses ($sd(\text{adm E})$), other operating expenses ($sd(\text{oth E})$), and loss provisions ($sd(\text{LP})$).

I also look at some standard measures for asset risk, that is, the ratio of non-performing loans to gross loans (NPL ratio), the ratio of risk-weighted assets to total assets (RWA/Assets), and a proxy for asset volatility ($\ln(\text{asset risk})$). $\ln(\text{asset risk})$ is the logarithm of the product of annualized stock volatility and the market leverage ratio, i.e., market capitalization divided by the sum of total liabilities and market capitalization (Goncharenko et al., 2021; Gropp and Heider, 2010).

Table A.1 in the Appendix presents definitions of all variables.

3.3 Measuring earnings opacity

In a second set of analyses, I estimate the effect of issuing AT1 CoCo bonds on earnings opacity. I follow the literature (e.g., Beatty and Liao, 2014; Danisewicz et al., 2021; Jiang et al., 2016; Yue et al., 2022) and estimate a proxy for earnings opacity based on the following loss provision model:

$$\begin{aligned}
\text{LP/Loans}_{i,c,t} = & \beta_0 + \beta_1 \Delta \text{NPA}_{i,c,t+1} + \beta_2 \Delta \text{NPA}_{i,c,t} + \beta_3 \Delta \text{NPA}_{i,c,t-1} + \beta_4 \Delta \text{NPA}_{i,c,t-2} \\
& + \beta_5 \ln(\text{assets})_{i,c,t-1} + \beta_6 \Delta \text{Loans}_{i,c,t} + \beta_7 \text{CoCo}_{i,c,t} \\
& + \beta_8 \text{CoCo}_{i,c,t} \times \Delta \text{NPA}_{i,c,t+1} + \beta_9 \text{CoCo}_{i,c,t} \times \Delta \text{NPA}_{i,c,t} \\
& + \beta_{10} \text{CoCo}_{i,c,t} \times \Delta \text{NPA}_{i,c,t-1} + \beta_{11} \text{CoCo}_{i,c,t} \times \Delta \text{NPA}_{i,c,t-2} \\
& + \beta_{12} \text{CoCo}_{i,c,t} \times \ln(\text{assets})_{i,c,t-1} + \beta_{13} \text{CoCo}_{i,c,t} \times \Delta \text{Loans}_{i,c,t} \\
& + \beta_{14} \Delta \text{GDP}_{c,t} + \beta_{15} \Delta \text{RPP}_{c,t} + \beta_{16} \Delta \text{Unemp}_{c,t} + \gamma_c + \lambda_t + \epsilon_{i,t},
\end{aligned} \tag{2}$$

where the dependent variable is the ratio of loss provisions to lagged total loans (LP/Loans) of bank i in country c in year t . $NPA_{i,c,t}$ is the change in non-performing assets relative to lagged total loans and captures changes in asset risk and risk-taking. By also including the lead of this variable, I control for the extent to which current provisions correctly reflect an anticipation of a future deterioration of asset quality (e.g., [Bushman and Williams, 2012](#)). I include two lags to account for past changes in non-performing assets. $\ln(\text{assets})_{i,c,t-1}$ is the lagged value of the natural logarithm of total assets and accounts for potential differences in supervisory and private monitoring of banks dependent on their size (e.g., [Danisewicz et al., 2021](#)). $\Delta\text{Loans}_{i,c,t}$ is the growth rate of total loans, which could be negatively associated with loss provisions if quality deteriorates with a growing loan portfolio (e.g., [Jiang et al., 2016](#)). Alternatively, it could have a positive association if banks better diversify the lending portfolio as it grows.

Similarly to [Andries et al. \(2017\)](#), [Jiang et al. \(2016\)](#), [Nicoletti \(2018\)](#), and [Yue et al. \(2022\)](#), I include a dummy indicating whether a bank has issued an AT1 contingent convertible ($\text{CoCo}_{i,c,t}$) in the previous year and its interaction terms with the other bank-level variables relevant for loss provisions. That accounts for the fact that there might be systematic differences between the loss provisioning of control and treated banks. Most importantly, including $\text{CoCo}_{i,c,t} \times \Delta\text{NPA}_{i,c,t+1}$ accounts for the possibility that a bank issues an AT1 CoCo in anticipation of a deterioration in asset quality to create leeway for additional loss provisions without burdening its existing regulatory capital.

Moreover, I include country-level controls for GDP growth ($\Delta\text{GDP}_{c,t}$), changes in the unemployment rate ($\Delta\text{Unemp}_{c,t}$), and changes in residential property prices ($\Delta\text{RPP}_{c,t}$), which

account for the relevant time-varying macroeconomic developments (Beatty and Liao, 2014). Similarly to Nicoletti (2018), I add country and time-fixed effects to control for time-invariant country-specific characteristics and EU-wide regulatory or supervisory actions that could affect banks' loss provisioning.

The error term of this model specified in equation (2) is the component of loss provisions that the included fundamental determinants cannot explain. Thus, the residuals indicate discretionary changes in loss provisions. To create a proxy for earnings opacity ($EO_{i,c,t}$), I take the natural logarithm of the absolute value of the residuals, which captures income-increasing and -decreasing earnings management (e.g., Andries et al., 2017; Danisewicz et al., 2021; Jiang et al., 2016; Yue et al., 2022). Larger values for EO suggest higher levels of opacity.

Column (1) of Table A.3 in the Appendix presents the estimation results for the loss provision model. I find a positive statistically significant association between changes in future non-performing assets ($\Delta NPA_{i,c,t+1}$) and loss provisions, suggesting banks anticipate a deterioration in asset quality and provision accordingly. There is no evidence of a systematic difference between banks issuing AT1 CoCos and control banks when it comes to loss provisions being reflective of forward looking asset quality ($CoCo_{i,c,t} \times \Delta NPA_{i,c,t+1}$).

I estimate several modified specifications of the loss provision model for additional robustness tests (columns (2) - (4) of Table A.3), which I describe in Section 5.1. Summary statistics for all resulting proxy measures for earnings opacity (EO^1 for the baseline, $EO^2 - EO^4$ for the alternative specifications) are available in Table 1 and Table A.2.

3.4 Model specification

Figure 2 illustrates that listed EU banks issued CoCo bonds qualifying as AT1 capital in different years from 2008-2021. Moreover, previous research has shown that the decision to issue an AT1-qualifying CoCo bond depends on time-varying bank characteristics like, for instance, size (e.g., Fajardo and Mendes, 2020), variables that can also directly affect bank-level stability (e.g., Bhagat et al., 2015).

To ‘uncover’ the causal effect in this setting where the treatment, i.e., the issuance of an AT1-qualifying CoCo, is staggered over time and there is a need to control for confounders when determining the effect on an outcome of interest $Y_{i,t}$, I adopt the methodology proposed by Callaway and Sant’Anna (2021).¹⁵

The key identifying assumption in a difference-in-difference setting is that, in the absence of treatment, the difference in outcomes between the treatment and control groups remains constant over time. That assumption also applies to staggered treatment adoption scenarios with the additional restriction that controls must only comprise non-treated observations. Despite some banks never issuing AT1 CoCos in my sample period, i.e., constituting ‘never-treated’ controls, I also allow for ‘not-yet treated’ banks to serve as control observation. The reason is that previous research shows that particularly larger banks are more likely to issue CoCos (Avdjiev et al., 2020; Fajardo and Mendes, 2020; Goncharenko et al., 2021).

¹⁵I refrain from applying the standard two-way fixed effects (TWFE) model due to the severe shortcomings it can have when treatment assignment changes over time and the treatment effect varies across different units or exhibits dynamic changes over different periods (e.g., Athey and Imbens, 2022; Baker et al., 2022; Borusyak et al., 2023; Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021). In the TWFE, one compares all cohorts with each other if there is a variation in treatment status within a given time frame. That can lead to comparisons between newly treated observations and already treated ones. In a worst-case scenario, that can result in the TWFE estimation procedure yielding a negative treatment effect, whereas participation in treatment positively affects the outcome.

Thus, only allowing ‘never-treated’ banks to constitute controls might result in treatment and control groups not being similar enough. Therefore, the parallel trends assumption must hold based on not-yet treated units to uncover a causal effect accurately:

$$E[Y_t(0) - Y_{t-1}(0)|G = g] = E[Y_t(0) - Y_{t-1}(0)|D_s = 0, G \neq g], \quad (3)$$

for all $g, s, t = 2, \dots, \mathcal{T}$ with $g \leq t$ and $t \leq s$, where $Y_t(0)$ is the untreated potential outcome in time t , and G indicates the treated group, i.e., the time of a bank’s first treatment. $D_s = 0$ identifies the group of ‘not-yet treated’ observations in time s . To substantiate the validity of this assumption, I check for the absence of pretrends in a robustness test.

Staggered treatment adoption implies the non-reversibility of treatment. Thus, once a bank has issued an AT1-qualifying CoCo bond in the previous year, the bank remains treated. That is reasonable since AT1 contingent convertibles are perpetual bonds. However, banks sometimes redeem them, resulting in a potential bias I address in a robustness check. I define an issuance in the previous year as the treatment since I am interested in changes in bank stability and reporting, which might need some time to materialize. Yet, I also find some evidence for an immediate effect in the issue year in additional tests.

I follow [Callaway and Sant’Anna \(2021\)](#) and define the group-time average treatment effect for all $g \leq t$ as follows:

$$ATT(g, t) = E[Y_t - Y_{g-1}|G = g] - E[Y_t - Y_{g-1}|D_t = 0, G \neq g], \quad (4)$$

where Y_t is the observed outcome at time t . This specification computes outcome differences solely on contemporaneous observations for treated and control banks, with the latter not having issued AT1 CoCos at time t .

I also add controls for time-varying bank characteristics that previous studies have shown to affect the likelihood of issuing CoCos ([Avdjiev et al., 2020](#); [Fajardo and Mendes, 2020](#); [Goncharenko et al., 2021](#)) and bank stability (e.g., [Bhagat et al., 2015](#); [Gornall and Strebulae, 2018](#); [Laeven et al., 2016](#)). [Callaway and Sant’Anna \(2021\)](#) show that the parallel trend assumption specified in equation (3) easily generalizes to allow for conditioning on covariates across groups.

Estimating the ATT conditional on covariates requires computing the change in outcomes for units in the control group given the confounding factors and accounting for the distribution of covariates for individuals in the different treatment groups. I apply the doubly-robust method [Callaway and Sant’Anna \(2021\)](#) and [Sant’Anna and Zhao \(2020\)](#) recommend, which combines the outcome regression and the inverse probability weighting approaches, to estimate the respective group-time average treatment effects. To determine the aggregate average treatment effect issuing AT1 CoCos has on my outcome variables, I average over event times and estimate the ATT for different event window lengths.¹⁶

4 Impact of AT1 CoCo issuances on bank stability

This section presents and examines the empirical results illustrating the negative impact of AT1 CoCo bonds on bank stability. To alleviate concerns regarding the identification, the sample selection, additional bank-level confounders, and the estimation of the outcome variable, I present the results of several robustness checks. Additionally, I show that more volatile earnings drive the baseline effect, and I further zoom in on the determining income

¹⁶In a robustness test, I also average over calendar time.

components.

4.1 CoCos and banks' distance to insolvency

I apply the approach of [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#) specified in [Section 3.4](#) and estimate the average treatment effect on the treated of issuing AT1 CoCos on a bank's $\ln(\text{z-score})$. [Table 2](#) displays the baseline results. The treatment dummy indicates whether the bank issued an AT1 CoCo the previous year. Panels A, B, and C display the average treatment effect on the treated over one, two, and three years after the issuance relative to two pre-treatment years.¹⁷

Many studies find that bank-level risk increases with bank size (e.g., [Bhagat et al., 2015](#); [Gropp et al., 2014](#); [Laeven et al., 2016](#)). Moreover, [Avdjiev et al. \(2020\)](#) show that the propensity to issue CoCos is higher for larger banks in advanced economies. [Fajardo and Mendes \(2020\)](#) and [Goncharenko et al. \(2021\)](#) confirm the positive association between bank size and the likelihood of issuing contingent convertibles for European banks. Thus, I include $\ln(\text{assets})$ as a proxy for bank size in all estimations to account for the potential selection bias associated with this variable.

I also control for the corporate income tax rate (Tax) in all specifications. A higher tax rate might increase incentives to issue AT1 CoCos due to the potential tax shield. Also, changes in corporate taxes might affect net income, which can affect a bank's z-score through the return on average assets (see equation (1)). Moreover, I account for cross-country and time-varying differences in the regulatory environment, possibly affecting the likelihood of a

¹⁷The issuance takes place at $t = -1$. Thus, $t = 0$ is the first full reporting year with the AT1 CoCo outstanding.

bank issuing AT1 CoCos and bank stability. Estimating group-time average treatment effects already accounts for any impact EU regulations, like the Capital Requirement Regulation from 2014 and its amendment from 2019, might have on the outcome since they directly apply to all member states. In contrast, federal governments must transpose EU directives into national law for them to become effective. Although the EU sets a transposition deadline, [Koetter et al. \(2022a\)](#) show considerable variation across countries, with some implementing BRRD and CRD IV early, while others delay passing the respective laws beyond the deadline. Cross-country and time-varying differences in the regulatory requirements could impact my outcome and treatment. Hence, I include dummies to account for the country-time variation in implementing BRRD and CRD IV into national law in all estimations.

All estimations include bank- and year-fixed effects. All controls are lagged. Standard errors are clustered at the bank level.

Column (1) of Panel A shows a negative treatment effect, implying that issuing AT1 CoCos reduces, on average, a bank's distance to insolvency, all else being equal. The effect is statistically significant at the 5% level. The size of the effect remains stable for longer event windows (Panels B & C), suggesting an immediate and sustained decrease in $\ln(\text{z-score})$.

Next, I follow the literature on bank stability (e.g., [Bremus and Ludolph, 2021](#)) and add a standard set of bank-level controls (CAMELs) for capital adequacy (Capital/Assets), asset quality (Llp/Int. inc.), management capability (Cost/Income), earnings (ROAA), and liquidity (Loans/Assets). [Goncharenko et al. \(2021\)](#) also find banks with larger loans-to-assets ratios to have a higher propensity to issue CoCo bonds, highlighting the importance of this confounder. The effect becomes slightly larger in size (column (2)).

In column (3), I restrict the post-treatment period to years before a bank issues another AT1 CoCo bond, which does not alter my findings.

I add bank-level controls for regulatory capitalization (Tier 1/RWA) and capital quality (Tier 1/Tot. capital) in column (4). Better capitalization is typically associated with lower bank risk (e.g., [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 mixed: While [Avdjiev et al. \(2020\)](#) find a positive effect of capitalization on the likelihood of issuing 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. [Figure 5](#) shows that the Tier 1 ratio of listed banks in the EU that issued AT1 CoCos in the 2008-2021 period is, on average, lower than for banks not issuing these hybrid securities despite both groups following a similar growth path. Yet, the lines converge as the issue activity gains greater dynamism in 2014 (see [Section 2.1](#) and [Figure 2](#)) before diverging again in 2018. Adding regulatory capital controls in my final baseline estimation precludes pre-treatment differences in the regulatory capital quantity and quality from confounding my results. That further strengthens my findings.

The treatment effect is statistically significant at a 1% level and ranges from -0.25 to -0.29. The effect is also economically meaningful. Issuing an AT1-qualifying CoCo bond results in a bank's distance to insolvency dropping by almost 25%, all else equal. That corresponds to a drop of 0.8 standard deviations from the mean of 3.43 in the baseline estimation sample. [Table A.4](#) shows that extending the post-treatment period by another year does not substantially alter my results (panel A), nor does including an additional

pre-treatment period (panels B & C) or averaging over calendar time (panel D).

4.2 Robustness of the baseline results

I perform several robustness checks to substantiate the validity of my baseline findings.

4.2.1 Identification

First, I test the validity of the identifying assumption underlying my empirical strategy, i.e., the parallel trends assumption based on not-yet treated observations (see equation (3)) conditional on the covariates. I first check the pre-treatment ATTs for my final baseline estimations (i.e., including the full set of controls and limiting the post-treatment years to ‘clean’ observations). Panel A of [Table 3](#) presents the results for the year of issuance (τ_{-1}) and the three preceding years (τ_{-2} , τ_{-3} , and τ_{-4}). The effect is weakly statistically significant in $t = -1$. That indicates a bank’s distance to insolvency already shrinks in the year of issuance. However, given that the direction of this ‘pre-treatment’ effect is also negative, it only suggests that I might underestimate the actual treatment effect in my baseline estimations. I find no statistically significant results for earlier years. The average over all pre-treatment ATTs is also not statistically significant (Panel B).

Next, I perform a placebo test to substantiate my identification. To that end, I only keep never-treated and not-yet-treated observations in my baseline estimation sample. I randomly choose a set of banks to issue a placebo CoCo in a randomly assigned year, such that the share of treated observation is approximately similar to the baseline case. In line with my baseline estimations, the placebo treatment is non-reversible. I include the same set of fixed effects and controls as before. [Table 4](#) reports the results for different event windows. The

placebo treatment does not result in a significant average treatment effect, which further verifies the validity of my identification.

4.2.2 Sample selection

Table 5 presents the robustness test results for the baseline findings (column (1)) pertaining to the sample selection. First, I check that AT1 CoCo bonds issued before the regulatory change that specified the design requirements (CRR, applicable since 2014) or that might have altered the bail-in likelihood (BRRD, in effect in most countries since 2016) do not drive my results. Thus, I restrict the sample to AT1 CoCo issues from 2014-2021 and 2016-2021, respectively. Column (2) shows that the effects remain robust in size and statistical significance. Limiting the sample to the issues since 2016 does not substantively alter the effect sizes, but the ATTs for the longer event windows become weaker in statistical significance (column (3)). However, dropping earlier observations implies losing disproportionately more treated units for which I can observe more post-treatment periods. Hence, I would refrain from interpreting that as evidence that the BRRD resolved the negative association between issuing AT1 CoCos and bank stability.

Column (4) presents the results for the sample only comprising post-treatment observations for which the outstanding bond volume remains constant. Disregarding redemptions does not result in my baseline estimates being overly upward-biased. In column (5), I drop all banks delisted at some point during my sample period. The ATTs become smaller in size and significance. That suggests banks that do not survive as standalone listed banks drive my baseline findings in parts. Yet, the results also show that issuing AT1-qualifying CoCos reduces the distance to insolvency, even conditional on remaining listed.

Lastly, I check whether AT1 CoCos that banks issue for purposes other than increasing capital drive my results. Column (6) presents the treatment effects for the subset of CoCos for which the prospectuses explicitly state that the proceeds will strengthen the capital base. Despite the sample size reduction, the effects persist. Hence, contingent convertibles issued for other purposes, like funding high-risk projects, are not behind my results.

4.2.3 Bank-level confounders

[Table 6](#) presents robustness tests for the baseline results (column (1)) concerning additional bank-level confounders. [Fajardo and Mendes \(2020\)](#) find the RWA-to-assets ratio to be associated with a lower propensity to issue CoCo bonds, while they do not uncover a significant effect for capitalization. Thus, I replace the regulatory capital control variables with RWA/Assets. Column (2) shows that my baseline findings remain unchanged. Panel D reports the summary statistics of the added confounder.

Next, I include a proxy for the the charter value of bank, that is, its future profit-generating potential. Previous studies find that banks with lower charter values are associated with higher risk (e.g., [Hugonnier and Morellec, 2017](#); [Gropp and Vesala, 2004](#)). Moreover, if a bank expects to generate less profits, it might be more likely to issue AT1 CoCos to shift losses to the investors without going insolvent. Column (3) shows that treatment effects become even more pronounced.

I also control for the banks' regulatory leverage ratio, which is the ratio of Tier 1 capital to total on- and off-balance sheet exposure. The leverage ratio is not linked to risk-weighted assets and serves as a backstop to capital requirements. Banks with lower leverage ratios are likely associated with higher levels of instability but might also be more willing to issue

CoCos that increase Tier 1 capital. Adding the regulatory leverage ratio to my set of controls considerably reduces the sample size. Again I predominantly lose earlier observations as the leverage ratio has only become a regulatory requirement with the amendment to CRR. Nonetheless, the effect in the one-year post-treatment window more than doubles in size and remains statistically significant at a 5% level (column (4) of [Table 6](#)).

4.2.4 Calculation of z-score

Lastly, I scrutinize the estimation of my outcome variable. Column (1) of [Table 7](#) replicates the baseline results in its final specification. Column (2) illustrate that my results remain stable when only including equity rather than the sum of equity and hybrid capital in the capital ratio numerator of the z-score (cf. equation (1)). That alleviates concerns that my findings are biased due to a ‘mechanical’ change in the capital ratio following the issuance of a contingent convertible.

The element of the z-score computation that typically exhibits the highest sensitivity to modifications is the method for calculating the standard deviation of returns on average assets. In addition to calculating $sd(ROAA)_{i,t}$ over rolling windows, I adopt another approach applied in various contexts in the literature (e.g. [Bremus and Ludolph, 2021](#); [Loutskina and Strahan, 2015](#); [Kalemli-Ozcan et al., 2014](#)) and regress $ROAA$ on a set of time- and bank-fixed effects:

$$ROAA_{i,t} = \beta_t + \delta_i + shock_{i,t}, \quad (5)$$

where β_t is the time-fixed effect accounting for the average growth of all banks, and δ_i is the bank-fixed effect capturing the average growth over time of bank i . I run these regressions for each country separately and only for banks for which I observe $ROAA$ for at least three

years. The $shock_{i,t}$ quantifies the extent to which bank i 's return on assets deviates from both the average $ROAA$ across all banks in year t and the average $ROAA$ of bank i over time. The absolute value of the estimated residuals is a proxy for the time-varying annual volatility, which I use as the denominator in the z-score calculation for columns (3). The effect size slightly increases. However, the summary statistics presented in panel D show that the mean and standard deviation for $\ln(\text{z-score})$ also grow due to the altered $sd(ROAA)$ estimation. Despite this substantial change in the computation approach, the ATTs for the two- and three-year post-treatment windows remain statistically significant at the 10% and 5% levels, respectively.

In summary, the results show that issuing AT1 CoCo bonds results in a significantly lower z-score versus the expected levels in the absence of treatment. The treatment effect is statistically significant, robust, and economically meaningful.

4.3 Increased earnings volatility drives baseline effect

Interestingly, a discernible positive treatment effect is absent when estimating the treatment effect on various asset risk measures commonly employed in the existing literature. [Table A.5](#) shows, for instance, no significant estimates for an asset volatility proxy ($\ln(\text{asset risk})$), the ratio of risk-weighted assets to total assets (RWA/ Assets), or the ratio of non-performing loans to gross loans (NPL ratio).¹⁸ That corresponds with other studies not finding statistically significant effects for accounting-based risk measures ([Fiordelisi et al., 2020](#)). While the lack of statistical significance is by no means evidence for the absence of a relation, it

¹⁸For details on how the variables are defined, see [Table A.1](#). All estimations include the full set of controls and fixed effects. The sample for these estimations is equivalent to the baseline estimation sample in column (4) of [Table 2](#), except for missing observations in the alternate outcomes.

does prompt the question of what drives the baseline effect on the z-score if evidence for changes in asset risk is lacking. That holds particularly true, given the robust nature of the baseline effects, which persist even when altering crucial elements of the z-score calculation. Therefore, I investigate which z-score components drive my baseline findings next.

I decompose the z-score (cf. equation (1)) and estimate the treatment effect of issuing AT1 CoCo bonds on a bank's return on average assets ($ROAA$), its capital-to-assets ratio (CA ratio), and the standard deviation of its return on average assets ($sd(ROAA)$). Columns (2) - (4) of Table 8 present the respective results. While I do not find any significant effects on the numerator components ($ROAA$ and CA ratio), the standard deviation of returns on average assets significantly increases in all event windows. That suggests that an increase in the denominator of the z-score is driving the negative baseline effect (column (1)). $ROAA$ is the ratio of net income over average assets. Thus, I estimate the treatment effect on the standard deviation of net income ($sd(\text{net I})$) in column (5) and of average assets ($sd(AA)$) in column (6).¹⁹ The results show a positive ATT for the earnings volatility measure $sd(\text{net I})$, which is consistently statistically significant at a 5% level for all event windows (column (5)). The effect size is less economically meaningful than the baseline effect for the z-score but still corresponds to a change in earnings volatility of half a standard deviation in the baseline estimation sample (see Table A.2 for the summary statistics). ATTs for the standard deviation of average assets are not significant. Banks appear to move closer to insolvency due to higher income volatility following the issuance of an AT1-qualifying CoCo, all else

¹⁹The standard deviation of a ratio is not the ratio of its standard deviations. It also depends on the covariance of the numerator and denominator and can be approximated with a first-order Taylor expansion. Yet, these estimations can indicate whether either one is a dominating factor.

equal, while I find no evidence for an increase in asset risk.

I extend my analysis by another step to identify which income components exhibit diminished stability following the issuance of AT1-qualifying CoCo bonds. First, I estimate the treatment effects on the standard deviation of interest income ($sd(int. I)$) and interest expense ($sd(int. E)$), which I find not to be significant (see columns (2) and (3) of [Table 9](#)). Next, I turn to the volatility of operating income and expenses. Column (2) of [Table 10](#) shows that issuing an AT1 CoCo bond does not impact the stability of operating income. Similarly, there is no evidence that operating expenses related to staff (column (3)) or administration (column (4)) become more volatile. Yet, issuing AT1 CoCos results in more volatile other operating expenses (column (5)), which include, for instance, depreciation on operating assets but also audit and legal fees. Yet, the effect is small (approximately 0.04) and not necessarily economically meaningful (SD of 0.29, see the summary statistics in Panel D of [Table 10](#)).²⁰ Most notably, Column (6) of [Table 10](#) reveals a statistically significant and positive ATT for the standard deviation of total loss provisions. The effect is relatively stable in size over the different event windows and corresponds to approximately 0.35 standard deviations.

Overall, I find that issuing an AT1 qualifying CoCo bond results, on average, in a significantly lower z-score for the emitting bank, all else being equal. The treatment effect is economically meaningful and robust. Additional analyses suggest that a decrease in earnings stability, mainly due to more volatile loss provisions, drives the baseline result rather than an increase in asset risk.

²⁰I also consider non-operating income and expense volatility for which I do not find statistically significant results. Yet, the variables have many missing values making the results less reliable.

5 AT1 CoCos and earnings opacity

There are two possible explanations for more volatile loss provisions following the issuance of AT1-qualifying CoCo bonds. Banks might issue CoCo bonds in anticipation of a justified need to increase loss provisions to prevent a future deterioration in asset quality from eroding the capital base. However, I do not find a statistically significant treatment effect on loss provisions in unreported additional analyses. Moreover, the absence of a positive statistically significant parameter estimate for $CoCo_t \times \Delta NPA_{t+1}$ in the estimation results for the loss provision model presented in [Table A.3](#) in the Appendix also suggests banks issuing AT1 CoCos not necessarily being more sensitive in their loss provisioning to future changes in asset quality.

Alternatively, my findings might reflect loss provisioning unrelated to future changes in non-performing assets, which reduces transparency. [Danisewicz et al. \(2021\)](#) provide evidence for an inverse relationship between non-depositor monitoring and earning opacity. Similarly, prevailing uncertainty and misconceptions concerning the likelihood of bail-in might prevent AT1 CoCo investors from adequately engaging in monitoring while shareholders reduce their efforts expecting CoCo investors to be bailed in first, allowing banks to become less transparent in their earnings reporting.

In this section, I test this hypothesis and evaluate the impact of issuing AT1 CoCo bonds on earnings opacity. In addition, I examine whether my results are sensitive to differences in the characteristics of the CoCo bonds and issuers.

5.1 Issuing AT1 CoCos results in higher earnings opacity

[Table 11](#) presents the estimated treatment effects of issuing AT1 CoCo bonds on earnings opacity. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant’Anna \(2021\)](#) and [Sant’Anna and Zhao \(2020\)](#). Panels A-C present the results for different event windows. Panel D displays the summary statistics for the outcome variables in the regression samples. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level.

The outcome variable in column (1) is EO^1 , which is the natural logarithm of the absolute values of the residuals from estimating the baseline loss provision model specified in equation (2) and presented in column (1) of [Table A.3](#). Higher values indicate greater earnings opacity.²¹ The results show that issuing an AT1 CoCo bond increases earnings opacity by approximately 0.4 the year after the issuance (Panel A). This effect is economically meaningful (SD 1.08) and statistically significant at the 5% level. The treatment effect remains similar in size over longer event windows (Panels B & C) and is weakly statistically significant. That hints at a sustained increase in earnings opacity.

I test the robustness of this finding and run several alternative specifications of the loss provision model that is the basis for the earnings opacity proxy.

First, I scale the dependent variable in the loss provision model by lagged total assets rather than loans, like, for instance, [Di Fabio et al. \(2021\)](#), to ensure that changes in assets

²¹For more details on the loss provision model and the proxy for earnings opacity, see [Section 3.3](#).

other than loans do not distort the outcome. The estimation results of the modified loss provision model are presented in column (2) of [Table A.3](#) in the Online Appendix. Column (2) of [Table 11](#) shows that my main findings persist but become slightly weaker. That suggests that changes to assets other than loans might also contribute to increased earnings opacity following the issuance of an AT1 CoCo bond, albeit to a limited extent.

Next, I add the interaction of $\Delta\text{NPA}_{i,c,t+1}$ with a dummy that equals one for all observations starting in 2018, when IFRS 9 became effective, to my loss provision model (column (3) of [Table A.3](#)). The new accounting standard requires using forward-looking indicators to estimate loss provisions. Hence, it might systematically increase the sensitivity of loss provisions to future changes in non-performing assets. Also, [Oberson \(2021\)](#) finds it to result in a more aggressive use of loss provisions to smooth earnings. Column (3) of [Table 11](#) shows that the impact of issuing an AT1 CoCo bond on the resulting proxy for earnings opacity (EO^3) increases in size but weakens in statistical significance.

[Andries et al. \(2017\)](#) provide evidence for a positive association between loan loss provisions and the tax rate if countries allow provisions for existing loans not yet specifically identified as impaired to be deductible. Thus, I add the interaction of the corporate tax rate with a dummy indicating whether general loan loss provisions are tax-deductible to my loss provision model (column (4) of [Table A.3](#)). That results in a proxy for earnings opacity EO^4 for which I find considerably larger treatment effects (column (4) of [Table 11](#)). However, part of this increase is attributable to the sample selection resulting from limited information on the tax deductibility of loss provisions. In additional tests, I also find more sizeable effects for the baseline earnings opacity measure EO^1 when estimating the treatment effect based

on the limited sample.

5.2 Sensitivity to CoCo and bank characteristics

Next, I check whether my results are sensitive to particular characteristics of the CoCo or bank.

[Table 12](#) presents the treatment effects of issuing AT1 CoCo bonds on earnings opacity (EO^1) in the one-year post-treatment window for subsamples of the treated dependent on characteristics of the issued bond. First, I differentiate between loss absorption mechanisms. I find a weakly statistically significant effect for write-down CoCos (column (2)). For equity conversion, the treatment effect is slightly smaller than the baseline effect and has a p-value that exceeds 0.1 (column (3)). Given that investors in write-down CoCos could face the loss of their entire investment, whereas equity conversion would leave investors with a claim on future profits (e.g., [Avdjiev et al., 2020](#); [Flannery, 2016](#)), this lends support to the notion that particularly write-down CoCo investors do not fully appreciate the risks they are facing, resulting in a lack of monitoring.

In columns (4) and (5), I exclude the bottom and top quartiles of CoCo issues with respect to issue volume relative to lagged total assets. In line with expectations, the estimate becomes more pronounced for relatively larger issue volumes (column (4)). For smaller issues, the subsample ATT is no longer statistically significant (column (5)). Panel B reports the means of the issue volume relative to lagged total assets and capital for the selected subsample. I find similar results when measuring issue size relative to total capital.

Lastly, I check whether the treatment effects are larger in or exclusive to specific sub-

samples of banks. Columns (2) and (3) of [Table 13](#) suggest that earnings of smaller banks become particularly opaque after issuing an AT1-qualifying CoCo. At the first glance, that seems to contradict previous studies that find larger banks are more prone to earnings opacity ([Huizinga and Laeven, 2012](#)) and creditor monitoring being more relevant for earnings transparency of large banks ([Danisewicz et al., 2021](#)). However, further inspection of the summary statistics in Panels B and C reveals an inverse relationship between bank size and relative issue size. Focusing on larger (smaller) banks in the treatment group results in estimating the treatment effect for CoCo issues that are smaller (larger) relative to total assets. Hence, relative issue size is possibly driving this result.

Columns (4) and (5) tentatively suggest that the adverse effect of issuing an AT1 CoCo on earnings opacity is larger for banks that are weaker capitalized ex-ante. I exclude the bottom and top quartile of treated banks with respect to the issuer’s capitalization (Tier1/RWA) measured as the average over the years before the issuance. However, neither effect is statistically significant.

Overall, the evidence suggests that issuing AT1-qualifying CoCo bonds increases earnings opacity, especially when the issue volume is relatively large.

6 Conclusions

In this paper, I provide empirical evidence that AT1 CoCo bonds significantly reduce bank stability due to increased earnings volatility. Additional analyses reveal that net income becomes less stable as total loss provisions fluctuate more. Rather than banks issuing CoCo bonds in anticipation of a justified need to increase loss provisions to prevent a future de-

terioration in asset quality from eroding the capital base, I find earnings opacity increases following the issuance of AT1-qualifying CoCos. That effect gets more pronounced with relative issue size.

My results are consistent with the premise in the literature that the regulatory requirements in the EU result in ‘going-concern’ subordinated debt instruments that negatively impact bank stability. Existing studies establish that market participants like shareholders and investors in CDS spreads pertaining to senior unsecured debt consider CoCos to alter bank risk. I add to that by providing the first empirical evidence of how earnings stability and reporting change. My results support an inverse relation between creditor monitoring and earnings opacity. The uncertainty and misconceptions among CoCo investors and shareholder about the bail-in likelihood seem to limit their monitoring engagement, which results in more volatile loss provisions and higher earnings opacity.

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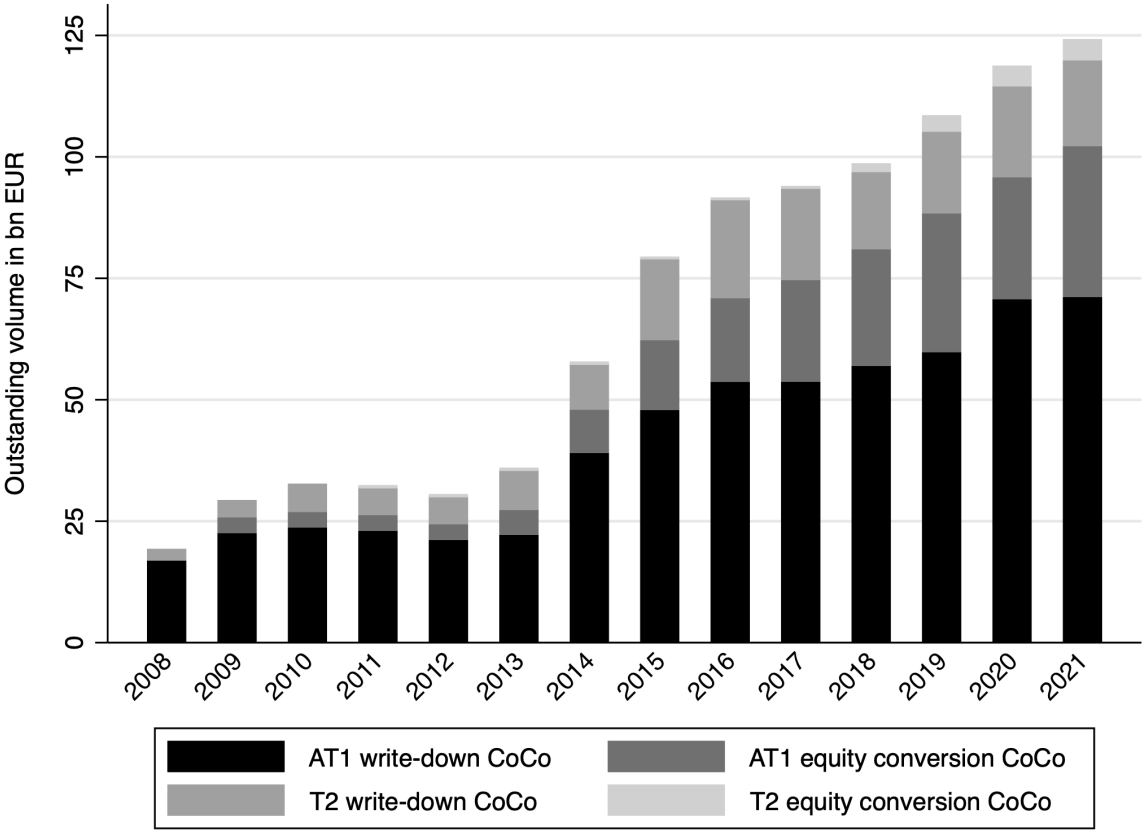
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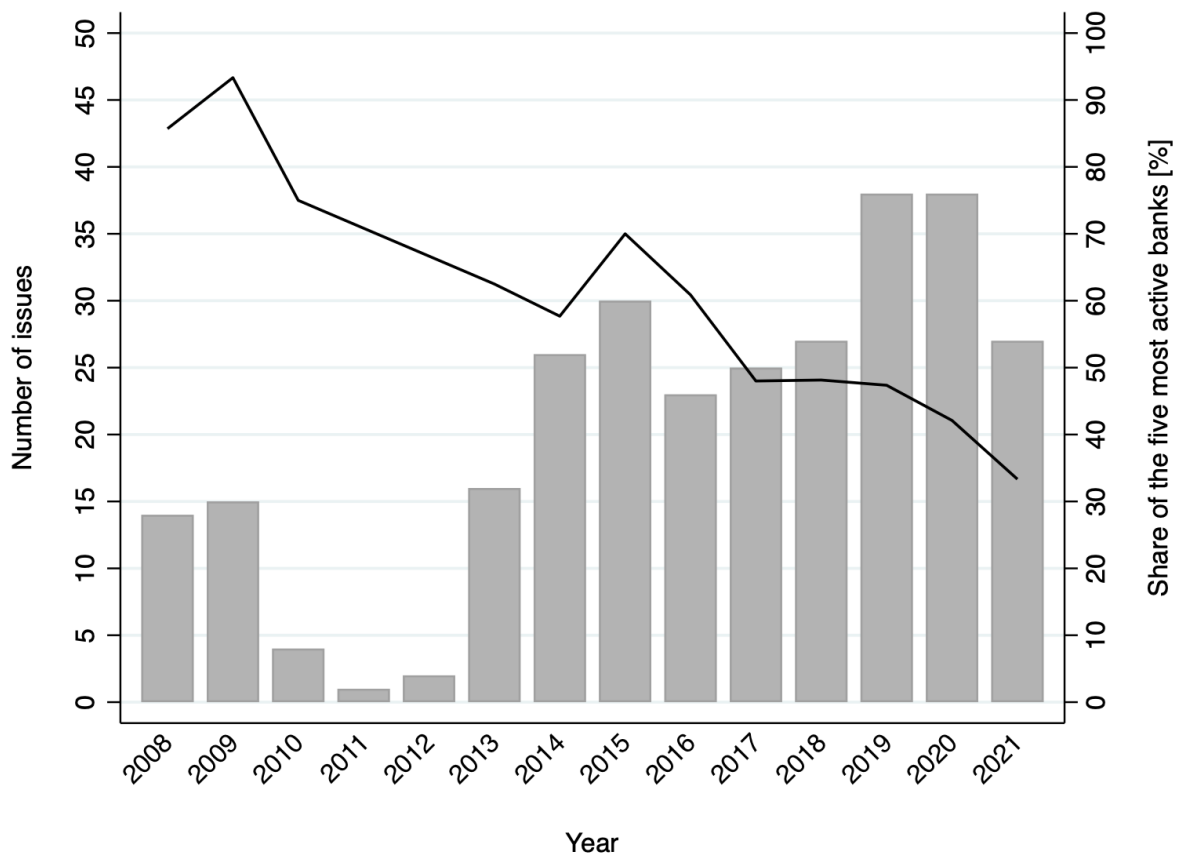
Figures

Figure 1: Aggregated outstanding issue volume of CoCo bonds



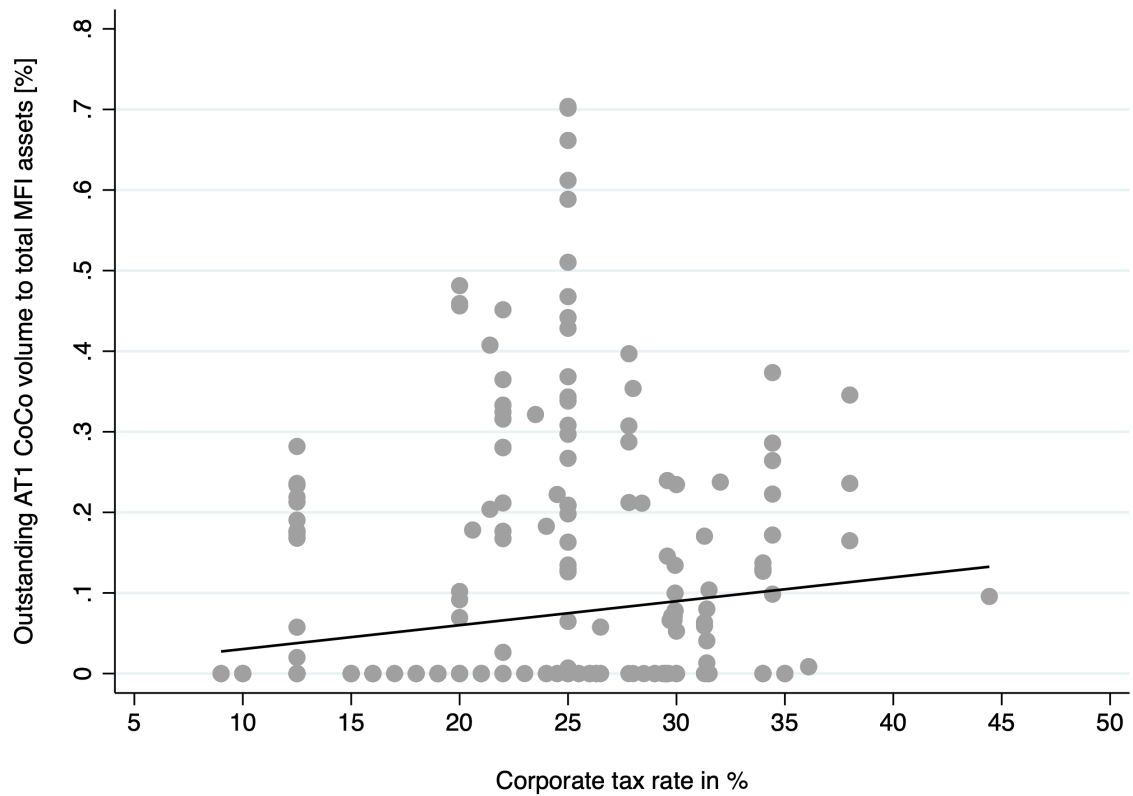
This figure shows the aggregated annual outstanding issue volume of CoCo bonds issued by publicly traded banks in the EU over the 2008 - 2021 sample period in billion euros that qualify as regulatory capital. CoCo bonds can either qualify as Additional Tier 1 (AT1) or Tier 2 (T2) capital and differ with respect to their loss absorption mechanism, i.e., write-down or equity conversion.

Figure 2: Issue activity in the EU CoCo bond market



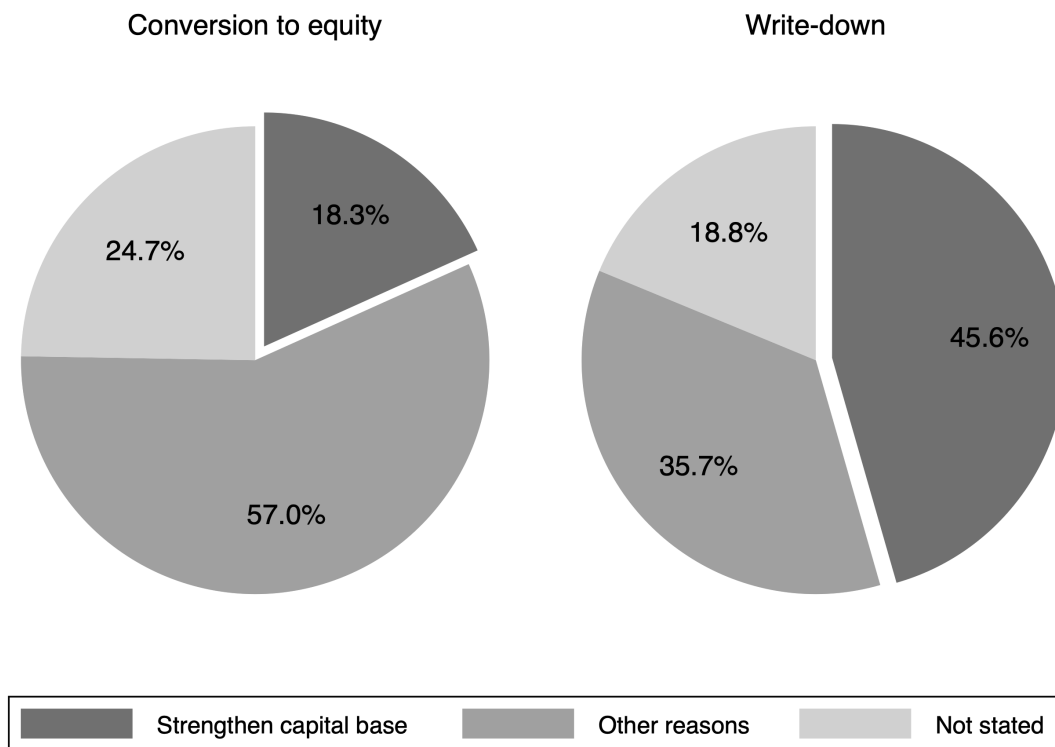
This figure shows the annual number of CoCo bond issues (bars, left-hand side) by publicly traded banks in the EU over the 2008 - 2021 sample period and the share of new issues attributable to the five most active banks over the entire period in % (line, right-hand side).

Figure 3: Annual outstanding AT1 CoCo bond volume and corporate income tax rates



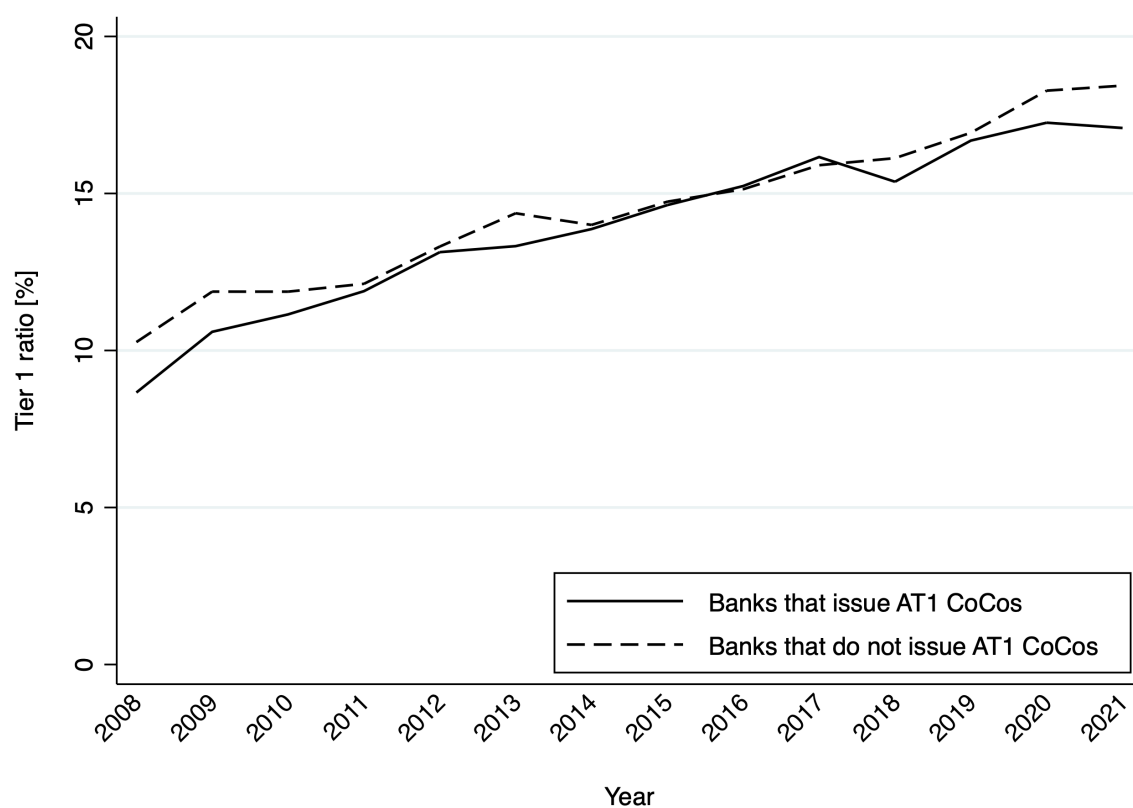
This figure displays the outstanding volume of AT1-qualifying CoCo bonds issued by listed banks in the EU from 2008 to 2021 per country-year scaled by the banking sector size (total MFI assets) in percent relative to the country- and year-specific corporate income tax rate.

Figure 4: Stated use of the proceeds from AT1 CoCo issues



The pie charts depict the different uses of proceeds specified in the prospectuses of AT1 CoCo bonds issued by publicly traded banks in the EU between 2008 and 2021. The left chart refers to the total issue volume of AT1 CoCos that convert to equity if triggered vs. loss absorption through write-down on the right-hand side. The category *strengthen capital base* includes all CoCos with stated use of proceeds like ‘strengthen regulatory capital base’, ‘included in Tier 1 capital base’, and ‘strengthen the leverage ratio and MREL’. If the prospectus does not explicitly refer to strengthening the capital base but indicates what the use of proceeds is, the bond is included in the category *other reasons*. That, for example, includes ‘general corporate purposes’ and ‘general financing purposes’.

Figure 5: Tier 1 ratio development of listed EU banks



This figure depicts the average Tier 1 ratio of listed banks in the EU that issued AT1 CoCos (solid line) over the 2008-2021 period vs. those that did not issue AT1 CoCos (dashed line).

Tables

Table 1: Summary statistics

| Panel A: Bank-level variables | Mean | SD | P25 | Med | P75 | N |
|---|--------|--------|--------|--------|----------|-------|
| Outcome variables: | | | | | | |
| $\ln(\text{z-score})$ | 3.47 | 1.14 | 2.73 | 3.57 | 4.31 | 1,376 |
| $\ln(\text{asset risk})$ | 0.38 | 1.38 | -0.20 | 0.55 | 1.19 | 1,702 |
| RWA/Assets [%] | 52.38 | 18.73 | 38.18 | 52.30 | 66.71 | 1,649 |
| NPL ratio [%] | 9.15 | 10.44 | 2.70 | 5.63 | 11.91 | 1,978 |
| $sd(\text{ROAA})$ | 0.60 | 0.96 | 0.13 | 0.26 | 0.62 | 1,394 |
| $sd(\text{net inc.})$ | 0.42 | 1.00 | 0.01 | 0.03 | 0.23 | 1,543 |
| $sd(\text{AA})$ | 5.33 | 13.49 | 0.14 | 0.46 | 2.40 | 1,356 |
| EO ¹ | -0.89 | 1.16 | -1.50 | -0.75 | -0.15 | 1,321 |
| EO ² | -1.30 | 1.14 | -1.90 | -1.21 | -0.61 | 1,321 |
| EO ³ | -0.90 | 1.18 | -1.49 | -0.75 | -0.15 | 1,321 |
| EO ⁴ | -0.90 | 1.18 | -1.50 | -0.72 | -0.13 | 918 |
| Additional bank-level variables: | | | | | | |
| $\ln(\text{assets})$ | 16.36 | 2.31 | 14.77 | 16.33 | 17.83 | 2,003 |
| Capital/Assets [%] | 10.35 | 8.12 | 6.37 | 8.96 | 12.37 | 1,993 |
| Llp/Int. inc. [%] | 31.40 | 59.90 | 7.70 | 18.78 | 36.31 | 1,952 |
| Cost/Income [%] | 64.09 | 33.19 | 53.25 | 61.58 | 71.74 | 2,003 |
| ROAA [%] | 0.46 | 2.22 | 0.21 | 0.56 | 0.99 | 1,939 |
| Loans/Assets [%] | 60.19 | 16.87 | 52.47 | 62.70 | 72.54 | 1,997 |
| Tier 1/RWA [%] | 14.54 | 4.80 | 11.30 | 14.00 | 17.20 | 1,493 |
| Tier 1/Tot. capital [%] | 87.13 | 10.47 | 80.56 | 88.15 | 96.67 | 1,439 |
| LP/Loans [%] | 1.34 | 5.27 | 0.24 | 0.64 | 1.34 | 1,989 |
| LP/Assets [%] | 0.75 | 1.47 | 0.14 | 0.39 | 0.82 | 1,995 |
| ΔNPA [%] | 0.41 | 20.64 | -0.68 | 0.02 | 1.06 | 1,838 |
| ΔLoans [%] | 12.76 | 280.19 | -1.67 | 3.32 | 8.78 | 1,997 |
| Panel B: Bond-level variables | | | | | | |
| Issue vol. [mn EUR] | 661.41 | 465.48 | 300.00 | 659.14 | 1,000.00 | 220 |
| Write-down CoCos | 603.08 | 437.64 | 185.07 | 544.03 | 1,000.00 | 162 |
| Equity-conversion CoCos | 824.36 | 504.76 | 485.99 | 750.00 | 1,093.69 | 58 |
| Issue vol./Assets [%] | 0.27 | 0.35 | 0.07 | 0.13 | 0.31 | 218 |
| Write-down CoCos | 0.28 | 0.37 | 0.07 | 0.12 | 0.32 | 160 |
| Equity-conversion CoCos | 0.25 | 0.31 | 0.10 | 0.14 | 0.26 | 58 |
| Issue vol./Tot. capital [%] | 3.75 | 3.79 | 1.56 | 2.39 | 4.09 | 205 |
| Write-down CoCos | 3.64 | 3.59 | 1.55 | 2.28 | 4.09 | 151 |
| Equity-conversion CoCos | 4.04 | 4.32 | 1.65 | 2.66 | 5.03 | 54 |
| Issue vol. /AT1 capital [%] | 62.38 | 114.67 | 15.64 | 29.72 | 56.35 | 148 |
| Write-down CoCos | 64.60 | 127.57 | 14.87 | 25.13 | 54.30 | 111 |
| Equity-conversion CoCos | 55.72 | 62.50 | 18.43 | 32.70 | 58.33 | 37 |
| Panel C: Country-level variables | | | | | | |
| BRRD dummy | 0.39 | 0.49 | 0.00 | 0.00 | 1.00 | 329 |
| CRD IV dummy | 0.48 | 0.50 | 0.00 | 0.00 | 1.00 | 329 |
| Tax rate [%] | 22.80 | 7.29 | 19.00 | 22.00 | 28.50 | 329 |
| ΔGDP [%] | 1.52 | 4.24 | -0.11 | 1.87 | 3.54 | 348 |
| ΔRPP [%] | 0.60 | 6.65 | -2.50 | 1.40 | 4.60 | 347 |
| ΔUnemp [p.p.] | -0.01 | 1.56 | -0.90 | -0.20 | 0.60 | 348 |
| General | 0.24 | 0.43 | 0.00 | 0.00 | 0.00 | 241 |

This table presents summary statistics for the pre-processed unbalanced panel from 2008 to 2021. Panel A displays outcome and control variables at the bank-level. Panel B depicts summary statistics for AT1 CoCos issued by the banks in the sample. Panel C shows summary statistics for country-level control variables. For details on how the variables are defined and on the data sources, see Table A.1 in the Appendix.

Table 2: Impact of AT1 CoCo issuances on banks' distance to insolvency

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) | (4) |
|--------------------------------------|---------------------|---------------------|---------------------|----------------------|
| ATT | -0.164** (0.074) | -0.185* (0.101) | -0.185* (0.101) | -0.261*** (0.093) |
| Panel B: Event $ATT_{(-2,1)}$ | | | | |
| ATT | -0.166** (0.082) | -0.192** (0.090) | -0.212** (0.099) | -0.245*** (0.094) |
| Panel C: Event $ATT_{(-2,2)}$ | | | | |
| ATT | -0.159* (0.090) | -0.217** (0.091) | -0.239** (0.108) | -0.292*** (0.110) |
| N | 1228 | 1198 | 1151 | 834 |
| Bank size control | Yes | Yes | Yes | Yes |
| Country controls | Yes | Yes | Yes | Yes |
| CAMEL controls | No | Yes | Yes | Yes |
| First issues only | No | No | Yes | Yes |
| Regulatory capital controls | No | No | No | Yes |

This table displays the average treatment effect on the treated from issuing CoCo bonds that qualify as AT1 capital on banks' distance to insolvency ($\ln(z\text{-score})$). Panels A, B, and C show the results for estimating the ATT based on the difference-in-differences estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#) for different event windows. I control for bank size ($\ln(\text{assets})$) in all regressions. I further include dummies to control for the country-time-variation in implementing BRRD and CRD IV into national law and the corporate tax rate. In column (2), I add a set of standard bank-level controls (CAMEL) for capital adequacy (Capital/Assets), asset quality (Llp/Int. inc.), management capabilities (Cost/Income), earnings (ROAA), and liquidity (Loans/Assets). In column (3), I limit the post-treatment period to years before a bank issues another AT1 CoCo bond. In column (4), I add bank-level controls for regulatory capitalization (Tier 1/RWA) and capital quality (Tier 1/Tot. capital). All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Pre-treatment trends

| Panel A: Pre-treatment ATTs | | (1) |
|---|---------|---------|
| τ_{-1} | -0.319* | (0.187) |
| τ_{-2} | -0.333 | (0.247) |
| τ_{-3} | 0.218 | (0.264) |
| τ_{-4} | 0.149 | (0.260) |
| Panel B: Average pre-treatment ATT | | |
| τ_{avg} | -0.071 | (0.119) |
| N | 842 | |

This table presents the result for the dynamic (Panel A) and average (Panel B) pre-treatment ATTs. The dependent variable is $\ln(z\text{-score})$. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Placebo test

| Panel A: Event $ATT_{(-2,0)}$ | | (1) |
|--------------------------------------|--------|---------|
| ATT | 0.140 | (0.235) |
| Panel B: Event $ATT_{(-2,1)}$ | | |
| ATT | -0.050 | (0.258) |
| Panel C: Event $ATT_{(-2,2)}$ | | |
| ATT | -0.289 | (0.288) |
| N | 651 | |

This table presents the result for a placebo test for different event windows. The dependent variable is $\ln(z\text{-score})$. I only keep never-treated and not-yet-treated observations in my sample. I randomly choose a set of banks to issue a placebo CoCo in a randomly assigned year, such that the share of treated observation is approximately similar to the baseline case, i.e., 10%. In line with my baseline estimations, the placebo treatment is non-reversible. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Robustness of the baseline result to the sample selection

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) | (4) | (5) | (6) |
|--------------------------------------|----------------------|------------------------|------------------------|----------------------|---------------------|----------------------------|
| ATT | -0.261*** (0.093) | -0.280*** (0.096) | -0.310** (0.130) | -0.230** (0.103) | -0.181** (0.086) | -0.217** (0.104) |
| Panel B: Event $ATT_{(-2,1)}$ | | | | | | |
| ATT | -0.245*** (0.094) | -0.272*** (0.097) | -0.241* (0.136) | -0.249*** (0.095) | -0.160* (0.084) | -0.216** (0.102) |
| Panel C: Event $ATT_{(-2,2)}$ | | | | | | |
| ATT | -0.292*** (0.110) | -0.306*** (0.113) | -0.238* (0.132) | -0.254*** (0.090) | -0.203** (0.090) | -0.216** (0.102) |
| N | 834 | 805 | 731 | 792 | 715 | 650 |
| Sample selection: | Baseline | Issues in 2014-2021 | Issues in 2016-2021 | Const. amount out | Not delisted | Strengthen capital base |

This table presents robustness tests for the baseline results (column (1)) concerning the sample selection. The outcome variable is $\ln(z\text{-score})$ in all regressions. In column (2) and (3), I restrict the sample to AT1 CoCo issues from 2014-2021 and 2016-2021, respectively. In column (4), I drop all post-treatment observations once the outstanding bond volume changes. In column (5), I drop all banks delisted at some point in the 2008-2021 period. Column (6) presents the result for the subset of CoCos for which the prospectuses explicitly state that the proceeds will be used to strengthen the capital base. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). Panels A-C present the results for different event windows. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Robustness of the baseline result to additional bank-level confounders

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) | (4) |
|---|----------------------|----------------------|----------------------|---------------------|
| ATT | -0.261*** (0.093) | -0.243*** (0.094) | -0.315*** (0.117) | -0.695** (0.353) |
| Panel B: Event $ATT_{(-2,1)}$ | | | | |
| ATT | -0.245*** (0.094) | -0.218** (0.094) | -0.307*** (0.110) | -0.517* (0.306) |
| Panel C: Event $ATT_{(-2,2)}$ | | | | |
| ATT | -0.292*** (0.110) | -0.222** (0.091) | -0.374** (0.150) | -0.445 (0.281) |
| N | 842 | 797 | 734 | 329 |
| Panel D: Summary statistics of <i>confounder</i> | | | | |
| Mean | . | 53.34 | 1.27 | 7.60 |
| SD | . | 17.23 | 6.03 | 2.32 |
| P25 | . | 40.41 | 0.38 | 5.89 |
| Med | . | 53.06 | 0.67 | 7.24 |
| P75 | . | 66.51 | 1.11 | 9.20 |
| Additional control: | Baseline | RWA/ Assets | Charter value | Leverage ratio |

This table presents robustness tests for the baseline results (column (1)) concerning additional bank-level confounders. The outcome variable is $\ln(\text{z-score})$ in all regressions. In column (2), I replace the regulatory capital control variables with RWA/Assets. In column (3), I add an additional control for the banks' charter value to my baseline controls. In column (4), I add the regulatory leverage ratio to my baseline set of controls. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). Panels A-C present the results for different event windows. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Baseline results for differently estimated z-scores

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) |
|---|----------------------|----------------------|---------------------|
| ATT | -0.261*** (0.093) | -0.249*** (0.092) | -0.286 (0.176) |
| Panel B: Event $ATT_{(-2,1)}$ | | | |
| ATT | -0.245*** (0.094) | -0.228** (0.095) | -0.319* (0.188) |
| Panel C: Event $ATT_{(-2,2)}$ | | | |
| ATT | -0.292*** (0.110) | -0.272** (0.113) | -0.381** (0.191) |
| N | 834 | 834 | 810 |
| Panel D: Summary statistics of $\ln(z\text{-score})$ | | | |
| Mean | 3.426 | 3.408 | 3.710 |
| SD | 1.103 | 1.112 | 1.340 |
| P25 | 2.708 | 2.680 | 2.825 |
| Med | 3.504 | 3.482 | 3.696 |
| P75 | 4.215 | 4.215 | 4.531 |
| Capital ratio numerator: Estimation of $sd(ROAA)$: | eq+hy rw | eq rw | eq+hy reg |

This table presents robustness tests for the baseline results (column (1)) concerning the estimation choices related to the outcome variable $\ln(z\text{-score})$. In columns (2), the numerator of the capital ratio used to calculate the z-score (cf. equation (1)) does not include hybrid capital. In columns (3), the estimation of $sd(ROAA)$ is based on the regression specified in equation (5) vs. 5-year rolling windows in columns (1) and (2). All results are based on the DiD estimator for staggered treatment adoption by Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020). Panels A-C present the results for different event windows. Panel D displays the summary statistics for the differently estimated z-scores in the regression samples. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Decomposition of the baseline effect on banks' distance to insolvency

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) | (4) | (5) | (6) |
|--------------------------------------|-----------------------|-------------------|------------------|--------------------|--------------------|-------------------|
| ATT | -0.261*** (0.093) | -0.037 (0.113) | 0.161 (0.152) | 0.150* (0.080) | 0.264** (0.125) | -0.134 (1.433) |
| Panel B: Event $ATT_{(-2,1)}$ | | | | | | |
| ATT | -0.245*** (0.094) | -0.026 (0.114) | 0.176 (0.182) | 0.159** (0.071) | 0.288** (0.120) | -0.145 (1.273) |
| Panel C: Event $ATT_{(-2,2)}$ | | | | | | |
| ATT | -0.292*** (0.110) | -0.023 (0.117) | 0.229 (0.210) | 0.174** (0.072) | 0.317** (0.130) | -0.331 (1.251) |
| N | 842 | 842 | 842 | 842 | 842 | 808 |
| Outcome variable: | $\ln(z\text{-score})$ | ROAA | CA ratio | $sd(ROAA)$ | $sd(\text{net I})$ | $sd(AA)$ |

This table presents the treatment effects of issuing AT1 CoCo bonds on the different components of banks' z-score (see column (1) for the baseline results). Columns (2) and (3) display the results for the components of the z-score numerator, i.e., ROAA and capital-to-assets ratio. Column (4) shows the treatment effects for the z-score denominator, i.e., the standard deviation of ROAA. The dependent variable in columns (5) and (6) are the standard deviation of net income and average assets, respectively. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). Panels A-C present the results for different event windows. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: Impact on the volatility of interest income and expenses

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) |
|--|--------------------|-------------------|-------------------|
| ATT | 0.264** (0.125) | -0.098 (0.084) | -0.142 (0.107) |
| Panel B: Event $ATT_{(-2,1)}$ | | | |
| ATT | 0.288** (0.120) | -0.070 (0.098) | -0.107 (0.101) |
| Panel C: Event $ATT_{(-2,2)}$ | | | |
| ATT | 0.317** (0.130) | -0.125 (0.100) | -0.144 (0.091) |
| N | 842 | 842 | 837 |
| Panel D: Summary statistics of <i>outcome</i> | | | |
| Mean | 0.314 | 0.411 | 0.371 |
| SD | 0.779 | 1.302 | 1.291 |
| P25 | 0.011 | 0.012 | 0.007 |
| Med | 0.036 | 0.053 | 0.038 |
| P75 | 0.178 | 0.231 | 0.180 |
| Outcome variable: | <i>sd</i> (net I) | <i>sd</i> (int I) | <i>sd</i> (int E) |

This table presents the treatment effects of issuing AT1 CoCo bonds on the standard deviations of the interest subcomponents of net income (column (1)). Columns (2) and (3) display the results for the standard deviation of interest income and expenses, respectively. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). Panels A-C present the results for different event windows. Panel D displays the summary statistics for the outcome variables in the regression samples. Panels A-C present the results for different event windows. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: Impact on the volatility of operating income, expenses, and loss provisions

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) | (4) | (5) | (6) |
|--|--------------------|-------------------|-------------------|------------------|--------------------|--------------------|
| ATT | 0.264** (0.125) | -0.103 (0.076) | -0.023 (0.018) | 0.011 (0.015) | 0.039* (0.022) | 0.183** (0.088) |
| Panel B: Event $ATT_{(-2,1)}$ | | | | | | |
| ATT | 0.288** (0.120) | -0.031 (0.064) | -0.016 (0.018) | 0.021 (0.023) | 0.039** (0.020) | 0.150* (0.077) |
| Panel C: Event $ATT_{(-2,2)}$ | | | | | | |
| ATT | 0.317** (0.130) | 0.010 (0.058) | -0.021 (0.023) | 0.027 (0.023) | 0.052** (0.021) | 0.144** (0.070) |
| N | 842 | 842 | 835 | 755 | 830 | 830 |
| Panel D: Summary statistics of <i>outcome</i> | | | | | | |
| Mean | 0.314 | 0.263 | 0.057 | 0.057 | 0.088 | 0.222 |
| SD | 0.779 | 0.769 | 0.154 | 0.164 | 0.287 | 0.513 |
| P25 | 0.011 | 0.013 | 0.003 | 0.003 | 0.003 | 0.006 |
| Med | 0.036 | 0.044 | 0.009 | 0.010 | 0.010 | 0.025 |
| P75 | 0.178 | 0.172 | 0.036 | 0.039 | 0.049 | 0.139 |
| Outcome variable: | <i>sd(net I)</i> | <i>sd(op I)</i> | <i>sd(st E)</i> | <i>sd(adm E)</i> | <i>sd(oth E)</i> | <i>sd(LP)</i> |

This table presents the treatment effects of issuing AT1 CoCo bonds on the standard deviations of the operating subcomponents of net income (column (1)). Column (2) displays the results for the standard deviation of operating income and Columns (3) - (5) for the standard deviations of different operating expenses, i.e., staff, administrative, and other operating expenses. Column (6) presents the results for the standard deviation of loss provisions. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). Panels A-C present the results for different event windows. Panel D displays the summary statistics for the outcome variables in the regression samples. Panels A-C present the results for different event windows. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: Impact of AT1 CoCo issuances on earnings opacity

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) | (4) |
|---|--------------------|-------------------|-------------------|--------------------|
| ATT | 0.398** (0.197) | 0.326* (0.172) | 0.439* (0.229) | 1.116* (0.637) |
| Panel B: Event $ATT_{(-2,1)}$ | | | | |
| ATT | 0.345* (0.202) | 0.312* (0.161) | 0.379 (0.234) | 1.248** (0.566) |
| Panel C: Event $ATT_{(-2,2)}$ | | | | |
| ATT | 0.364* (0.204) | 0.333* (0.172) | 0.395* (0.232) | 1.176** (0.513) |
| N | 659 | 659 | 659 | 382 |
| Panel D: Summary statistics of EO | | | | |
| Mean | -0.814 | -1.253 | -0.826 | -0.793 |
| SD | 1.080 | 1.085 | 1.100 | 1.083 |
| P25 | -1.329 | -1.821 | -1.324 | -1.357 |
| Med | -0.691 | -1.188 | -0.676 | -0.644 |
| P75 | -0.105 | -0.555 | -0.132 | -0.012 |
| Outcome variable: | EO^1 | EO^2 | EO^3 | EO^4 |
| LP model outcome: | LP/Loans | LP/Assets | LP/Loans | LP/Loans |
| LP model control IFRS9 x ΔNPA_{t+1} : | No | No | Yes | No |
| LP model control General x Tax : | No | No | No | Yes |

This table presents the treatment effects of issuing AT1 CoCo bonds on earnings opacity. The outcome variable EO^1 is the natural logarithm of the absolute values of the residuals from estimating the loss provision model specified in equation (2) and presented in column (1) of Table A.3. Higher values indicate greater earnings opacity. EO^2 in column (2) is based on the loss provision model where the outcome variable is loss provisions scaled by lagged total assets (see column (2) of Table A.3). EO^3 in column (3) is based on the loss provision model that includes a control for the impact of IFRS 9 on future changes in non-performing assets (see column (3) of Table A.3). EO^4 in column (4) is based on the loss provision model that includes a control for the impact of the corporate tax rate if general loan loss provisions are tax deductible (see column (4) of Table A.3). In column (5), the dependent variable is EO^1 and the sample excludes AT1 CoCo bonds issued in 2015 following the ECB's Asset Quality Review at the end of 2014 that required most banks in the sample to increase loss provisions. For more details on the loss provision model and the proxy for earnings opacity, see Section 3.3. All results are based on the DiD estimator for staggered treatment adoption by Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020). Panels A-C present the results for different event windows. Panel D displays the summary statistics for the outcome variables in the regression samples. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 12: Sensitivity of $ATT_{(-2,0)}$ on earnings opacity to CoCo characteristics

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) | (4) | (5) |
|--|--------------------|-----------------------|-------------------------|--------------------------------------|------------------|
| ATT | 0.398** (0.197) | 0.387* (0.200) | 0.378 (0.253) | 0.433* (0.232) | 0.314 (0.319) |
| N | 659 | 643 | 616 | 621 | 585 |
| Panel B: CoCo issues (<i>means</i>) | | | | | |
| Issue vol./Assets | 0.683 | 0.701 | 0.638 | 0.738 | 0.414 |
| Issue vol./Tot. cap. | 8.443 | 8.586 | 8.169 | 8.964 | 6.216 |
| Panel C: Issuer (<i>means</i>) | | | | | |
| $\ln(\text{assets})$ | 17.29 | 17.15 | 17.47 | 16.70 | 18.20 |
| Tier 1/RWA | 12.04 | 12.02 | 12.03 | 12.70 | 12.28 |
| Sample selection criteria: | Baseline | Loss absorption WD | Equity conversion EC | Issue vol./Assets <25%ile >75%ile | |

This table presents the treatment effects of issuing AT1 CoCo bonds on bank opacity (EO^1) for subsamples of the treated dependent on characteristics of the issued bond. Column (1) presents the baseline results for the entire sample. In columns (2) and (3), I differentiate between write-down and equity conversion CoCos. In columns (4) and (5), I exclude the bottom and top quartile of CoCo issues with respect to issue volume to total lagged total assets. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). Panel A presents the results for the (-2,0)-event window. Panel B displays the means of the characteristics of the selected CoCos. Panel C displays the means of the issuer's size and capitalization measured as the average over the years before an issuance. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 13: Sensitivity of $ATT_{(-2,0)}$ on earnings opacity to bank characteristics

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) | (4) | (5) |
|--|--------------------|---|-------------------|-------------------------------|------------------|
| ATT | 0.398** (0.197) | 0.296 (0.255) | 0.528* (0.305) | 0.338 (0.215) | 0.418 (0.315) |
| N | 659 | 641 | 588 | 625 | 581 |
| Panel B: CoCo issues (<i>means</i>) | | | | | |
| Issue vol./Assets | 0.683 | 0.487 | 0.744 | 0.663 | 0.616 |
| Issue vol./Tot. cap. | 8.443 | 7.090 | 8.705 | 8.050 | 8.855 |
| Panel C: Issuer (<i>means</i>) | | | | | |
| $\ln(\text{assets})$ | 17.29 | 18.41 | 16.57 | 17.07 | 17.99 |
| Tier 1/RWA | 12.04 | 11.63 | 12.94 | 13.12 | 11.50 |
| Sample selection criteria: | Baseline | $\ln(\text{assets})$ <25%ile >75%ile | | Tier 1/RWA <25%ile >75%ile | |

This table presents the treatment effects of issuing AT1 CoCo bonds on bank opacity (EO^1) for subsamples of the treated dependent on characteristics of the issuing bank. Column (1) presents the baseline results for the entire sample. In columns (2) and (3), I exclude the bottom and top quartile of treated banks with respect to the issuer's size ($\ln(\text{assets})$) measured as the average over the years before the issuance. In columns (4) and (5), I exclude the bottom and top quartile of treated banks with respect to the issuer's capitalization (Tier1/RWA) measured as the average over the years before the issuance. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). Panel A presents the results for the (-2,0)-event window. Panel B displays the means of the characteristics of the selected CoCos. Panel C displays the means of the issuer's size and capitalization measured as the average over the years before an issuance. I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix

A.1 Write-down of AT1-qualifying CoCos issued by Credit Suisse

Credit Suisse reported a decline in customer deposits by 67 billion Swiss francs and a net asset outflow of 61.2 billion in Q1'23, which amounts to 11% of the bank's total assets at the end of 2022 ([Credit Suisse, 2023](#)). After this significant outflow of deposits, the Swiss Financial Market Supervisory Authority (FINMA) identified Credit Suisse as at risk of becoming illiquid. To safeguard depositors and stabilize financial markets, [FINMA \(2023a\)](#) approved a takeover by UBS and ordered the write-down of all AT1 contingent convertible bonds to increase the bank's core capital. The trigger events for all outstanding Credit Suisse AT1 CoCo bonds at the time of the merger were varying minimum thresholds for the bank's CET ratio, ranging from 3% to 9.75% ([FINMA, 2023b](#)). Given that Credit Suisse reported a CET1 ratio of 14.1% at the end of 2022 and of 20.3% after the write-down ([Credit Suisse, 2022, 2023](#)), the regulator's decision to order a write-down without a shareholders bail-in resulted in a public outcry of CoCo investors and reports about their plans to file lawsuits against FINMA (e.g., [CNBC, 2023](#)). However, according to the Swiss capital regulation (Article 29 of the Swiss Capital Adequacy Ordinance ([Swiss Federal Council, 2012](#))), AT1 capital is subject to a write-down before a bank can receive public sector assistance. That is also in line with the recommendations of the [Basel Committee on Banking Supervision \(2011\)](#). [FINMA \(2023b\)](#) considered the government-backed loan and liquidity guarantees Credit Suisse obtained in conjunction with the takeover to be public sector assistance.

All this prompted EU bank regulators to issue a joint statement emphasizing their commitment to the seniority principle, according to which equity shareholders must absorb losses

before AT1 investors are subject to a bail-in ([ECB, 2023](#)). However, that is only true under recovery and resolution rules. Since CRR requires the trigger event for AT1 CoCos to be the CET1 ratio falling below 5.125%, bond investors are subject to a bail-in before the entire CET1 capital, which includes share capital, gets wiped out.

Table A.1: Variable definitions

| Variable | Definition | Source |
|---|--|---|
| Outcome variables: | | |
| $\ln(\text{z-score})$ | Logarithm of the z-score, i.e., the distance to insolvency defined as the sum of ROAA and the capital to asset ratio divided by the standard deviation of ROAA | Bankfocus & own calculations |
| $\ln(\text{asset risk})$ | Logarithm of the product of the annualized stock volatility and the market leverage ratio, i.e., market capitalization divided by the sum of total liabilities and market capitalization | Bankfocus, Refinitiv & own calculations |
| RWA/Assets [%] | Ratio of risk-weighted assets to total assets | Bankfocus & own calculations |
| NPL ratio [%] | Ratio of non-performing loans to gross loans | Bankfocus |
| $sd(\text{ROAA})$ | Standard deviation of ROA over a five-year rolling window | Bankfocus & own calculations |
| $sd(\text{net I})$ | Standard deviation of net income over a five-year rolling window | Bankfocus & own calculations |
| $sd(\text{AA})$ | Standard deviation of average assets over a five-year rolling window | Bankfocus & own calculations |
| $sd(\text{int I})$ | Standard deviation of total interest income over a five-year rolling window | Bankfocus & own calculations |
| $sd(\text{int E})$ | Standard deviation of total interest expense over a five-year rolling window | Bankfocus & own calculations |
| $sd(\text{op I})$ | Standard deviation of operating revenues over a five-year rolling window | Bankfocus & own calculations |
| $sd(\text{LP})$ | Standard deviation of total loss provisions over a five-year rolling window | Bankfocus & own calculations |
| $sd(\text{st E})$ | Standard deviation of staff expenses over a five-year rolling window | Bankfocus & own calculations |
| $sd(\text{adm E})$ | Standard deviation of other administrative expenses over a five-year rolling window | Bankfocus & own calculations |
| $sd(\text{oth E})$ | Standard deviation of other operating expenses over a five-year rolling window | Bankfocus & own calculations |
| EO ¹ | The natural logarithm of the absolute value of the residuals calculated based on the loss provision model specified in equation (2) and estimated in column (1) of Table A.3 | Bankfocus & own calculations |
| EO ² | The natural logarithm of the absolute value of the residuals calculated based on the loss provision model specified in equation (2) and estimated in column (2) of Table A.3 | Bankfocus & own calculations |
| EO ³ | The natural logarithm of the absolute value of the residuals calculated based on the loss provision model specified in equation (2) and estimated in column (3) of Table A.3 | Bankfocus & own calculations |
| EO ⁴ | The natural logarithm of the absolute value of the residuals calculated based on the loss provision model specified in equation (2) and estimated in column (4) of Table A.3 | Bankfocus & own calculations |
| Additional bank-level variables: | | |
| $\ln(\text{assets})$ | Logarithm of total assets | Bankfocus & own calculations |

Continued on next page –

Table A.1 – continued from previous page

| Variable | Definition | Source |
|-----------------------------------|--|---|
| Capital/Assets [%] | Ratio of the difference between assets and liabilities to assets | Bankfocus & own calculations |
| Llp/Int. inc. [%] | Ratio of loan loss provisions to net interest revenues | Bankfocus |
| Cost/Income [%] | Ratio of total operating expenses to total operating income | Bankfocus |
| ROAA [%] | Ratio of net income to average assets | Bankfocus |
| Loans/Assets [%] | Ratio of loans to assets | Bankfocus |
| Tier 1/RWA [%] | Ratio of Tier 1 capital to risk-weighted assets | Bankfocus |
| Tier 1/Tot. capital [%] | Ratio of Tier 1 capital to total regulatory capital | Bankfocus & own calculations |
| Charter value | Ratio of the book value of assets minus equity plus the market value of equity to the book value of assets | Bankfocus, Refinitiv & own calculations |
| Leverage ratio [%] | Ratio of Tier 1 capital to total exposure | Bankfocus |
| LP/Loans [%] | Total loss provisions scaled by lagged total loans | Bankfocus & own calculations |
| LP/Assets [%] | Total loss provisions scaled by lagged total assets | Bankfocus & own calculations |
| Δ NPA [%] | Change in non-performing assets scaled by lagged total loans | Bankfocus & own calculations |
| Δ Loans [%] | Change in total loans scaled by lagged total loans | Bankfocus & own calculations |
| CoCo bond-level variables: | | |
| Issue vol. [mn EUR] | Issue amount in mn EUR | Refinitiv |
| Issue vol./Assets [%] | Ratio of issue amount to lagged total assets | Bankfocus, Refinitiv & own calculations |
| Issue vol./Tot. capital [%] | Ratio of issue amount to lagged total regulatory capital | Bankfocus, Refinitiv & own calculations |
| Issue vol. /AT1 capital [%] | Ratio of issue amount to lagged AT1 capital | Bankfocus, Refinitiv & own calculations |
| Country-level variables: | | |
| BRRD dummy | Dummy = 1 if the key law implementing the Bank Recovery and Resolution Directive in a country has been published in the second half of the previous year or the first half of the current year | Koetter et al. (2022b) |
| CRD IV dummy | Dummy = 1 if the key law implementing the Capital Requirements Directive IV in a country has been published in the second half of the previous year or the first half of the current year | Koetter et al. (2022b) |
| Tax | Country-specific corporate tax rate in % | OECD Statistics & Tradingeconomics |
| General | Dummy = 1 if general loan loss provisions are tax deductible | Andries et al. (2017) & World Bank Regulation and Supervision Survey (2019) |
| Δ GDP [%] | GDP growth | ECB Data Portal |
| Δ RPP [%] | Growth in residential property prices | ECB Data Portal |

Continued on next page –

Table A.1 – continued from previous page

| Variable | Definition | Source |
|------------------------------|-----------------------------|-----------------|
| ΔUnemp [p.p.] | Change in unemployment rate | ECB Data Portal |

Table A.2: Summary statistics for the final baseline estimation sample

| Panel A: Bank-level variables | Mean | SD | P25 | Med | P75 | N |
|---|-------------|-----------|------------|------------|------------|----------|
| Outcome variables: | | | | | | |
| $\ln(\text{z-score})$ | 3.43 | 1.10 | 2.71 | 3.50 | 4.22 | 834 |
| $\ln(\text{asset risk})$ | 0.42 | 1.30 | -0.04 | 0.60 | 1.12 | 739 |
| RWA/Assets [%] | 51.94 | 17.20 | 38.93 | 51.56 | 65.46 | 793 |
| NPL ratio [%] | 9.49 | 10.87 | 2.59 | 5.57 | 12.05 | 817 |
| $sd(\text{ROAA})$ | 0.55 | 0.80 | 0.14 | 0.27 | 0.60 | 834 |
| $sd(\text{net inc.})$ | 0.32 | 0.78 | 0.01 | 0.04 | 0.18 | 834 |
| $sd(\text{AA})$ | 3.48 | 10.61 | 0.13 | 0.47 | 1.61 | 818 |
| EO ¹ | -0.81 | 1.08 | -1.33 | -0.69 | -0.09 | 670 |
| EO ² | -1.26 | 1.09 | -1.83 | -1.19 | -0.55 | 670 |
| EO ³ | -0.83 | 1.11 | -1.32 | -0.68 | -0.10 | 670 |
| EO ⁴ | -0.80 | 1.12 | -1.34 | -0.64 | -0.01 | 415 |
| Additional bank-level variables: | | | | | | |
| $\ln(\text{assets})$ | 16.50 | 1.93 | 15.13 | 16.60 | 17.69 | 834 |
| Capital/Assets [%] | 9.63 | 3.82 | 6.70 | 9.05 | 12.46 | 834 |
| Llp/Int. inc. [%] | 30.90 | 58.13 | 7.10 | 17.39 | 37.97 | 834 |
| Cost/Income [%] | 63.11 | 22.19 | 53.37 | 61.06 | 72.13 | 834 |
| ROAA [%] | 0.54 | 1.44 | 0.21 | 0.55 | 1.02 | 834 |
| Loans/Assets [%] | 60.45 | 15.26 | 53.47 | 62.97 | 72.05 | 834 |
| Tier 1/RWA [%] | 15.04 | 4.53 | 11.80 | 14.60 | 17.50 | 834 |
| Tier 1/Tot. capital [%] | 88.17 | 10.11 | 80.89 | 89.04 | 98.31 | 834 |
| LP/Loans [%] | 0.93 | 1.41 | 0.18 | 0.52 | 1.22 | 834 |
| LP/Assets [%] | 0.62 | 1.05 | 0.10 | 0.32 | 0.75 | 834 |
| ΔNPA [%] | -0.54 | 15.43 | -0.85 | -0.08 | 0.73 | 805 |
| ΔLoans [%] | 5.40 | 16.70 | -1.70 | 3.64 | 8.26 | 834 |
| Panel B: Bond-level variables | | | | | | |
| Issue vol. [mn EUR] | 549.40 | 467.77 | 112.50 | 500.00 | 925.09 | 56 |
| <i>Write-down CoCos</i> | 507.76 | 479.10 | 40.00 | 500.00 | 907.70 | 38 |
| <i>Equity-conversion CoCos</i> | 637.32 | 443.01 | 200.00 | 579.32 | 940.65 | 18 |
| Issue vol./Assets [%] | 0.47 | 0.46 | 0.13 | 0.31 | 0.57 | 56 |
| <i>Write-down CoCos</i> | 0.49 | 0.46 | 0.12 | 0.36 | 0.59 | 38 |
| <i>Equity-conversion CoCos</i> | 0.42 | 0.48 | 0.13 | 0.24 | 0.35 | 18 |
| Issue vol./Tot. capital [%] | 6.20 | 4.91 | 2.66 | 4.34 | 9.03 | 56 |
| <i>Write-down CoCos</i> | 6.24 | 4.67 | 2.65 | 4.96 | 9.30 | 38 |
| <i>Equity-conversion CoCos</i> | 6.12 | 5.52 | 2.66 | 3.67 | 6.35 | 18 |
| Issue vol. /AT1 capital [%] | 91.81 | 101.63 | 34.64 | 69.35 | 93.87 | 32 |
| <i>Write-down CoCos</i> | 92.91 | 117.05 | 18.02 | 67.80 | 87.66 | 23 |
| <i>Equity-conversion CoCos</i> | 88.99 | 48.34 | 50.05 | 88.03 | 123.78 | 9 |
| Panel C: Country-level variables | | | | | | |
| BRRD dummy | 0.48 | 0.50 | 0.00 | 0.00 | 1.00 | 243 |
| CRD IV dummy | 0.60 | 0.49 | 0.00 | 1.00 | 1.00 | 243 |
| Tax rate [%] | 22.88 | 7.54 | 19.00 | 22.00 | 29.50 | 243 |
| ΔGDP [%] | 2.08 | 3.99 | 0.66 | 2.07 | 3.84 | 243 |
| ΔRPP [%] | 2.16 | 4.82 | -0.40 | 2.50 | 4.80 | 243 |
| ΔUnemp [p.p.] | -0.31 | 1.28 | -1.10 | -0.40 | 0.30 | 243 |
| General | 0.27 | 0.45 | 0.00 | 0.00 | 1.00 | 157 |

This table presents summary statistics for the pre-processed unbalanced panel from 2008 to 2021. The table depicts the summary statistics for the bank-level observations (Panel A), AT1 CoCo issues (Panel B), and country-level control variables (Panel C) used in the final baseline estimation, i.e., column (4) of Table 2, and all subsequent analyses. For details on how the variables are defined and on the data sources, see Table A.1 in the Appendix.

Table A.3: Loss provision models to generate bank opacity proxies

| | (1) LP/Loans | (2) LP/Assets | (3) LP/Loans | (4) LP/Loans |
|--|----------------------|---------------------|----------------------|----------------------|
| CoCo=1 | -0.235 (1.386) | 0.306 (1.016) | -0.218 (1.374) | -1.326 (1.989) |
| ΔNPA_t | 0.012** (0.006) | 0.011** (0.005) | 0.011* (0.006) | 0.009 (0.006) |
| ΔNPA_{t+1} | 0.014** (0.006) | 0.013** (0.005) | 0.013* (0.007) | 0.011 (0.007) |
| ΔNPA_{t-1} | 0.004 (0.003) | 0.004 (0.003) | 0.004 (0.003) | 0.003 (0.003) |
| ΔNPA_{t-2} | 0.003* (0.002) | 0.002 (0.001) | 0.003* (0.002) | 0.003 (0.002) |
| $\ln(\text{assets})_{t-1}$ | -0.059 (0.048) | -0.060 (0.037) | -0.058 (0.048) | -0.036 (0.065) |
| ΔLoans_t | 0.014*** (0.000) | -0.000 (0.000) | 0.014*** (0.000) | 0.014*** (0.000) |
| CoCo=1 \times ΔNPA_t | 0.033 (0.033) | 0.013 (0.026) | 0.034 (0.033) | 0.053 (0.051) |
| CoCo=1 \times ΔNPA_{t+1} | 0.025 (0.050) | 0.013 (0.035) | 0.015 (0.046) | 0.020 (0.072) |
| CoCo=1 \times ΔNPA_{t-1} | -0.032 (0.023) | -0.029* (0.016) | -0.032 (0.023) | 0.014 (0.029) |
| CoCo=1 \times ΔNPA_{t-2} | 0.022 (0.021) | 0.018 (0.017) | 0.021 (0.021) | -0.003 (0.024) |
| CoCo=1 \times $\ln(\text{assets})_{t-1}$ | 0.033 (0.074) | -0.007 (0.054) | 0.032 (0.074) | 0.089 (0.106) |
| CoCo=1 \times ΔLoans_t | -0.015*** (0.005) | -0.002 (0.004) | -0.016*** (0.005) | 0.002 (0.015) |
| ΔGDP_t | -0.104** (0.045) | -0.081** (0.037) | -0.105** (0.045) | -0.253*** (0.087) |
| ΔRPP_t | -0.005 (0.011) | -0.003 (0.009) | -0.005 (0.011) | 0.009 (0.013) |
| ΔUnemp_t | 0.110 (0.070) | 0.095 (0.061) | 0.108 (0.069) | 0.124 (0.083) |
| IFRS9=1 \times ΔNPA_{t+1} | | | 0.022 (0.026) | |
| General=1 \times Tax | | | | 0.013 (0.009) |
| N | 1,336 | 1,336 | 1,336 | 932 |
| R ² | 0.94 | 0.41 | 0.94 | 0.96 |

This table presents the results of estimating the determinants of loss provisioning according to the model specified in equation (2). The dependent variable in column (1), (3), and (4) is loss provisions scaled by lagged total loans in percent. The dependent variable in column (2) is scaled by lagged total assets. In column (3), I add the interaction of a dummy indicating the applicability of IFRS 9 and the lead of the change in non-performing assets divided by lagged total loans (ΔNPA_{t+1}). In column (4), I add the interaction of the applicable corporate tax rate (Tax_t) with a dummy indicating if general loan loss provisions are tax deductible (General_t). Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.4: Baseline results for varying treatment effects aggregations

| | | |
|--------------------------------|-----------|---------|
| Panel A: $ATT_{(-2,3)}$ | | (1) |
| ATT | -0.302*** | (0.111) |
| Panel B: $ATT_{(-3,2)}$ | | |
| ATT | -0.220** | (0.102) |
| Panel C: $ATT_{(-3,3)}$ | | |
| ATT | -0.231** | (0.103) |
| Panel D: Calendar ATT | | |
| ATT | -0.410** | (0.208) |
| N | 842 | |
| Bank size control | Yes | |
| Country controls | Yes | |
| CAMEL controls | Yes | |
| First issues only | Yes | |
| Regulatory capital controls | Yes | |

This table displays the average treatment effect on the treated from issuing CoCo bonds that qualify as AT1 capital on banks' $\ln(z\text{-score})$ for different event windows lengths (Panels A-C) and for aggregating the treatment effect by calendar time (Panel D). All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.5: Impact of AT1 CoCo issuances on other bank-risk measures

| Panel A: Event $ATT_{(-2,0)}$ | (1) | (2) | (3) |
|--------------------------------------|--------------------------|-------------------|-------------------|
| ATT | 0.059 (0.091) | -0.297 (0.566) | -0.742 (0.653) |
| Panel B: Event $ATT_{(-2,1)}$ | | | |
| ATT | 0.051 (0.086) | -0.628 (0.607) | -0.456 (0.814) |
| Panel C: Event $ATT_{(-2,2)}$ | | | |
| ATT | 0.060 (0.093) | -0.732 (0.665) | -0.299 (1.074) |
| N | 744 | 789 | 821 |
| Outcome variable: | $\ln(\text{asset risk})$ | RWA/Assets | NPL ratio |

This table displays the average treatment effect on the treated from issuing CoCo bonds that qualify AT1 capital on different bank-risk measures, i.e., (1) $\ln(\text{asset risk})$, (2) RWA/Assets, and (3) NPL ratio. All results are based on the DiD estimator for staggered treatment adoption by [Callaway and Sant'Anna \(2021\)](#) and [Sant'Anna and Zhao \(2020\)](#). I include the full set of controls (bank size, country controls, CAMEL, regulatory capital controls) and limit the sample to first issues only. All control variables are lagged. For details on how the variables are defined, see Table A.1 in the Appendix. All regressions include bank- and year-fixed effects. Standard errors are clustered at the bank level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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