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# Central bank collateral policy and credit pricing: evidence from Finland\*

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## Abstract

We study the effect of collateral eligibility of corporate loans on the pricing of these loans by banks in Finland. Specifically, we investigate whether loans that are pledgeable as collateral for central bank borrowing have lower liquidity premia and thus lower interest rates. For identification, we utilize two unanticipated changes in the collateral framework of the Bank of Finland after the COVID-19 pandemic in 2020 and loan-level corporate credit data from the Finnish implementation of Anacredit. Our main result is that we do not find evidence that collateral pool expansions by the central bank significantly affected interest rates paid by borrowers. The result contrasts with recent findings that imply significant effects of similar collateral pool expansions on credit supply. We hypothesize that differences in the institutional setting and economic environment between countries may explain the contradictory results. Our findings show that collateral policies may not have similar effects on credit pricing in all circumstances.

**Keywords:** monetary policy, collateral framework, credit pricing, interest rates, eligibility

**JEL Codes:** E43, E52, G21, G28

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# 1 Introduction

Financial institutions are required to pledge collateral against any borrowing from the central bank. Therefore, in addition to the traditional key tool of monetary policy—the central bank policy rates—the pool of eligible collateral may affect credit conditions and asset prices, and thus the monetary policy stance (e.g., [Ashcraft, Garleanu, and Pedersen, 2011](#); [Nyborg, 2017a](#)). If a larger share of loans granted by banks are eligible as central bank collateral, they can more easily obtain secured funding and free up balance sheet space elsewhere (e.g., [Loutskina, 2011](#)). Thus, the liquidity premium associated with these collateralizable loans may be smaller, reflected in lower interest rate paid by borrowers: a discount for eligibility.

During the COVID-19 pandemic, the Bank of Finland, like some other Eurosystem National Central Banks (NCBs), adopted domestic policy measures to support monetary policy transmission by tweaking its collateral framework twice. In this paper, we utilize these changes as quasi-natural experiments to provide new evidence on central bank collateral pool expansions. We study the effect of collateral eligibility of corporate loans—granted by banks in Finland—on the price of these loans. Specifically, we investigate whether liquidity premia and thus interest rates decrease on corporate loans that become eligible as collateral for central bank borrowing.<sup>1</sup>

Our main result is that we do not find evidence that collateral pool expansions by the central bank significantly affected interest rates paid by borrowers. The result contrasts with recent findings by [Mésonnier, O'Donnell, and Toutain \(2022\)](#) and [Van Bakkum, Gabarro, and Irani \(2018\)](#) who find that changes in collateral frameworks had significant effects on credit pricing (and also more broadly on credit supply) in France and the Netherlands in 2011–2012. Our results imply that collateral policy may not have similar effects on credit pricing

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<sup>1</sup>In this paper, we assess whether the liquidity premia decline in newly-eligible loans relative to other loans. This hypothesis assumes that the liquidity premia are driven by heterogeneous liquidity of different assets, i.e., a liquidity of an asset affects its price ([Longstaff, 1995](#)). Alternatively, if the aggregate liquidity of bank balance sheet drives the liquidity premia for all assets ([Holmström and Tirole, 1998](#)), the extension of eligible assets may reduce the liquidity premium on *all* loans, not just those that become eligible ([Mésonnier, O'Donnell, and Toutain, 2022](#)). We assess only the heterogeneous liquidity channel.

in all circumstances. Indeed, the Finnish banking system and its' operating environment during the COVID-19 crisis were clearly different than those in other countries during the eurozone crisis. For example, Finnish banks were not collateral-constrained and likely faced higher fixed costs in pledging credit claims as collateral than French banks.

We utilize two recent changes in the collateral framework of the Bank of Finland (BoF) as quasi-natural experiments in a differences-in-differences (DiD) setting. First, in August 2020, the BoF announced that it will adopt the so-called Additional Credit Claims (ACC) framework from September 2020 and thus accept a broader set of corporate loans as collateral. Specifically, due to the ACC introduction, loans of firms with an estimated probability of default (PD) during the next year between 0.4–1.0 % became eligible collateral. To estimate the impact of collateral eligibility on bank loan pricing, we compare the evolution of interest rates between newly-eligible (PD 0.4–1.0 %) with always-eligible (PD below 0.4 %) or never-eligible (PD above 1.0 %) loans using a DiD approach. Second, earlier in April 2020, as a response to the weakened economic conditions due to the COVID-19 pandemic, the BoF lowered the minimum nominal amount threshold for pledgeable corporate loans from EUR 500,000 to EUR 25,000. Thus, otherwise similar loans with different sizes were affected differently by the policy change. We use this policy change to get an alternative estimate for the impact of collateral eligibility on bank credit pricing by comparing the interest rates of loans with nominal sizes between EUR 25,000–500,00 to the rates of already-eligible loans (sizes EUR 500,000 or above) or never-eligible loans (sizes below EUR 25,000) before and after the policy change. That said, we see the ACC policy change as our primary event of interest because existing literature has studied similar ACC policy changes and the magnitude of the ACC intervention in terms of loan volume is higher (and thus the potential amount of collateral affected is higher).

We verify in several ways that our results are not affected by other drivers than the collateral policy changes. First, and importantly, both policy changes were announced by the BoF with practically immediate effect and by surprise as a response to the pandemic,

so anticipation effects are very unlikely to confound the results. Second, we employ various fixed effects as well as firm- and loan-level controls to ensure that unobserved shocks in credit demand or supply do not interrupt our analysis. Third, we utilize multiple control groups and narrow down the data sample to assess the robustness of our results.

Our data consist of a proprietary and confidential credit register—Finnish implementation of Anacredit—of all loans granted by Finnish credit institutions to corporations and public entities at a monthly frequency. An advantage of our database is that it includes information on all loans; there is no minimum size threshold of EUR 25,000, as typically in Anacredit data. We complement the loan data with confidential information on how much Finnish banks’ have borrowed from the BoF and what collateral they have pledged against those loans.

We put forward two sets of results, one for each collateral pool expansion. First, the baseline results imply that after the ACC introduction, interest rates on the newly-eligible treated loans increased 4 basis points compared to safer always-eligible loans and decreased 1 basis point compared to the riskier never-eligible loans. However, neither result is statistically significant, and the standard errors are quite large. Second, the baseline results imply that after expanding the collateral pool to loans between EUR 25,000—500,000, interest rates on these newly-eligible treated loans increased 5 basis points compared to larger always-eligible loans and decreased 3 basis points compared to smaller never-eligible loans. Neither these estimates are statistically significant. Both sets of results are qualitatively similar across different model specifications.

Taken together, the low and insignificant estimates with opposing signs versus different control groups suggest that the collateral rule changes did not significantly affect the pricing of newly-eligible loans. Thus, we find no evidence of an eligibility discount. That said, the confidence intervals of the estimates are quite wide, and we can confidently rule out only effects being larger than roughly 10 basis points.<sup>2</sup> Based on our results, however, we can state

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<sup>2</sup>In our baseline specification the widths of the 95 % confidence intervals are from 13 to 25 basis points, with zero never being outside the interval.

that the ACC introduction did not lower rates paid by newly-eligible borrowers by 8 (12) basis points compared to already-eligible (never-eligible) borrowers, as found by [Mésonnier, O'Donnell, and Toutain \(2022\)](#).<sup>3</sup> There are various potential reasons why our results are different. First and foremost, the differences in institutional settings between Finland and France are extensive. Specifically, while Finnish banks were not short of liquidity or collateral at the time of the policy changes, French banks arguably were in 2012. Further, the use of corporate loans as collateral is much more widespread in France than in Finland, likely due to 1) the nationally established practice as well as 2) the higher operational fixed costs related to pledging corporate loans as collateral in Finland compared to France, due to, for example, the automated reporting and pledging platform provided by the Banque de France. If banks are not collateral-constrained and prefer other assets than corporate loans as collateral, it is no surprise that the collateral pool expansion to additional corporate loans does not heavily affect their pricing.

Even though the policy changes did not significantly affect interest rate differentials between loans, we cannot deem them ineffective as policies. First, we cannot rule out an effect on credit pricing overall through increased liquidity of bank balance sheets or reduced uncertainty, as we do not observe aggregate credit supply in the absence of the collateral rule changes. Second, uncertainty related to the development of the economy was unusually high during the collateral rule changes in 2020, and it could have turned out that Finnish banks would have experienced a collateral shortage going forward if the economic situation had deteriorated. Such development did not materialize as the economy recovered from the COVID-19 slump more strongly than initially anticipated. Thus, the policy change may have been wise *ex ante* while proving unnecessary *ex post*.

**Related literature.** Our main contribution is to assess if changes in the liquidity of bank balance sheets due to the central bank collateral policy affect the pricing of loans that are

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<sup>3</sup>Estimates of this magnitude are outside our 95 % confidence intervals. Our baseline specification is somewhat different, but column *M6* in our result tables replicates the statistical analysis of [Mésonnier, O'Donnell, and Toutain](#) as closely as possible with our data.



affected by the policy. As already discussed above, the closest paper to ours is [Mésonnier, O'Donnell, and Toutain \(2022\)](#) who study a similar intervention in France in 2012. In their baseline analysis, they show that the adoption of the ACC framework decreased the price of affected loans by 8 basis points compared to already-eligible ones.<sup>4</sup> Our study differs from theirs in two important dimensions. First, we focus on a different country and economic environment by studying the effect of pandemic-induced policy interventions in Finland (compared to policies motivated by the eurozone debt crisis in France). From a data perspective, our data has more observations than [Mésonnier, O'Donnell, and Toutain](#) and includes a wider sample of loans, such as those below 25,000 EUR in size.

Additionally, [Van Bakkum, Gabarro, and Irani \(2018\)](#) conclude that the expansion of the collateral framework decreased rates on newly-eligible (securitizable and riskier) mortgages by at least 7 basis points, possibly more, in the Netherlands.<sup>5</sup> In contrast to their results, we argue that there are economic circumstances and banking systems—such as the Finnish case in 2020—in which collateral eligibility seems not to be a major driver of credit pricing. Importantly, [Van Bakkum, Gabarro, and Irani](#) show that in the Netherlands, an important mechanism driving an increase in credit supply and risk taking was that newly-eligible loans were securitized as residential mortgage-backed securities (RMBS). In contrast, there is no active market that Finnish banks could use to securitize (and potentially sell and thus offload from their own balance sheet) the newly-eligible corporate loans.

Our paper also relates to other studies of Eurosystem collateral framework. [Pelizzon, Riedel, Simon, and Subrahmanyam \(2024\)](#) shows that eligibility of corporate bonds as Eurosystem collateral lowers their yields and affects functioning of the securities lending market. We focus on a different type of collateral, non-marketable credit claims, instead of actively-traded corporate bonds. [Hüttel and Kaldorf \(2022\)](#) study the effect of broadening

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<sup>4</sup>Also studying the French ACC introduction in 2012, [Cahn, Duquerroy, and Mullins \(2022\)](#) show that the growth rate of the newly-eligible loans increased more than that of the already-eligible loans, while paying special attention to firm-bank relationships.

<sup>5</sup>Others have also studied the effect of more liquid bank balance sheets through securitization on bank lending; while in the U.S. collateralizability seems to lower the liquidity premia ([Loutskina, 2011](#); [Nadauld and Weisbach, 2012](#)), in Europe the evidence is less clear ([Kara, Marques-Ibanez, and Ongena, 2016](#)).

the Eurosystem collateral framework (by harmonizing rules across national central banks) on bank lending. While they show that banks that held the most newly-eligible assets increased their overall lending to risky and unproductive firms, our focus is on the change in pricing of newly-eligible loans compared to loans that are unaffected by the policy change. However, exploring whether the exposure to the collateral rule changes affected the riskiness of lending across banks in Finland would be an interesting question for future research.

Finally, we also contribute to the strand of literature that examines the role of central bank collateral frameworks more generally. Contributions in this area include [Ashcraft, Garleanu, and Pedersen \(2011\)](#), [Koulischer and Struyven \(2014\)](#) and [Cassola and Koulischer \(2019\)](#), who all study the effects of central bank collateral policy on financial markets and the economy. [Nissinen and Sihvonen \(2022\)](#) shows that exogenous variation in central bank collateral haircuts of specific bonds affects their yields (via their effect on funding costs). Also, [Nyborg \(2017b\)](#) and [Nyborg \(2017a\)](#) argue that central bank collateral frameworks can distort financial market pricing, while [Choi, Santos, and Yorulmazer \(2021\)](#) reason that accepting lower-quality (riskier) collateral may improve money market functioning by freeing up high-quality collateral to markets. In the case of the Eurosystem, [Bindseil and Papadia \(2006\)](#) argue that the value of collateral eligibility is at most a few basis points. Unlike these papers, our analysis focuses on a specific type of collateral—credit claims—and focuses on the link between collateral and credit pricing, where we do not find evidence of a significant eligibility discount in Finland in 2020.

The rest of the paper is organized as follows: Sections [2](#) and [3](#) describe the institutional setting and our data, respectively. Sections [4](#) and [5](#) present our empirical analyses. We discuss our results and potential policy implications and conclude in Section [6](#).

## 2 Institutional setting

### 2.1 Liquidity provision and collateral policy at the Bank of Finland

The Bank of Finland is a member of the Eurosystem. In practice, this means that the BoF implements monetary policy of the Eurosystem among its' own counterparties, namely the credit institutions operating in Finland. Thus, monetary policy implementation at the BoF consists mainly of executing the policy decisions taken by the Governing Council of the ECB. However, the BoF can make some choices independently, regarding, for example, collateral policy and emergency liquidity assistance. Specifically, while the ECB sets general requirements for collateral frameworks of NCBs, they may use some individual discretion in determining their own collateral rules for non-marketable instruments. For example, the BoF can specify the requirements for a specific subset of credit claims—Additional Credit Claims (ACC)—to be eligible as collateral (subject to approval by the ECB Governing Council).<sup>6</sup>

Unlike some central banks that implement monetary policy by buying and selling assets in the open market, the Eurosystem actively conducts repo operations with the financial institutions. Effectively, banks borrow reserves from the Eurosystem against collateral. Since 2015, an overwhelming majority of credit operations of the Eurosystem have been targeted longer-term refinancing operations (TLTROs). TLTROs are attractively priced long-term loans with built-in rules to incentivize bank lending to the real economy. The latest series of 3-year loans (TLTRO-III) was available for banks quarterly between September 2019 and December 2021.

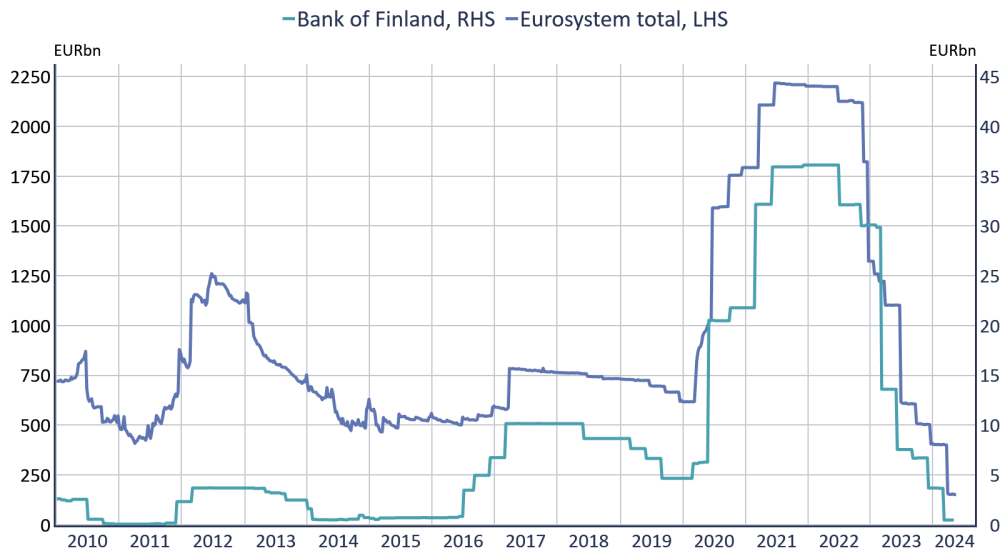
Eurosystem counterparty banks utilized TLTRO-III extensively. They increased their borrowing from around EUR 600bn in end-2019 to EUR 2,200bn in end-2021 (Figure 1). The 16 counterparty banks (or bank groups) of the BoF increased their central bank borrowing

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<sup>6</sup>The list of accepted collateral is fully harmonized across the Eurosystem for marketable assets. Also, requirements for non-marketable assets (credit claims such as corporate loans) are harmonized except for the minimum nominal amount. However, there might be operational differences in pledging credit claims across NCBs due to differences in legislation or IT systems. Finally, NCBs have leeway in setting the requirements for the ACC collateral (if they choose to adopt the ACC framework).

even more heavily between 2019 and 2021, from around EUR 5bn to EUR 36bn. In total, over 10 banks or bank groups borrowed from the BoF in the first half of 2020. However, banks did not borrow from the BoF very actively before the Eurosystem announced, in March–April 2020, the so-called special interest rate periods for TLTROs that made arbitrage-like income possible for banks.<sup>7</sup> This suggests that most counterparty banks of the BoF most likely did not *need* central bank credit at the time, but instead decided to borrow due to the below-market interest rates and profit considerations. The low central bank funding needs are also explained by the relatively stable capital and liquidity positions among counterparty banks of the BoF, which guaranteed them low rates in the funding markets. If banks do not need much central bank credit, they may not find a large pool of eligible central bank collateral as necessary.

**Figure 1:** Bank borrowing from the Eurosystem and the Bank of Finland



*Notes:* The graph plots the aggregate borrowing from refinancing operations of the Eurosystem and the Bank of Finland by their respective counterparty banks in EUR billion. Sources: ECB, Bank of Finland and Macrobond.

The Eurosystem manages risk related to credit operations by requiring banks to pledge collateral against any central bank borrowing. Due to the major role of loans to banks as

<sup>7</sup>In March 2020, the ECB announced that the interest rate on TLTROs may be as low as minus one percent until June 2021 and lengthened the low rate until June 2022 in December 2020. Simultaneously, banks' deposit rate at the ECB was higher at minus half a percent, making the loans particularly attractive.

a policy tool, rules for eligible collateral are of particular importance to the Eurosystem. Typically, collateral consists of marketable financial instruments such as government and corporate bonds.<sup>8</sup> These assets are priced daily in the market, and their collateral value depends on their price and haircut. Haircuts (and some other rules, such as concentration limits) depend on the collateral type to be pledged.

In addition to marketable assets, also non-marketable debt, in particular credit claims, are eligible as collateral. These are mostly loans to private corporates or public entities, such as municipalities. From banks' perspective, credit claims are attractive as collateral since they (unlike regular bonds) are illiquid and cannot easily be used to obtain secured funding from private financial markets. Thus, by pledging credit claims as collateral banks can use marketable liquid assets in their balance sheets more efficiently, for example, as high-quality liquid assets (HQLA) to fulfill their regulatory Liquidity Coverage Ratio (LCR). The opportunity cost of pledging credit claims as collateral is mostly the operational costs related to the pledging process.

The share of credit claims in the Eurosystem post-haircut collateral pools has been around 20–25 % since 2010, increasing to slightly above 30 % since the start of the COVID-19 pandemic in 2020 (Figure 2). However, the use of credit claims as collateral varies heavily by bank and national central bank. There are many countries in which credit claims are not used as collateral at all. In contrast, for example in France, the use of credit claims has traditionally been common. At the BoF, the share has been stable at around 30 % between 2017–2020 and decreased slightly since then (Figure 2, lower panel).<sup>9</sup> Notwithstanding the relatively high share, only a few counterparties utilize credit claims as collateral, and the use of ACC collateral is even less common (the exact number of banks using a specific collateral type is confidential). This suggests that most banks either 1) had enough of collateral to cover their (low) central bank funding needs without pledging non-marketable debt or 2)

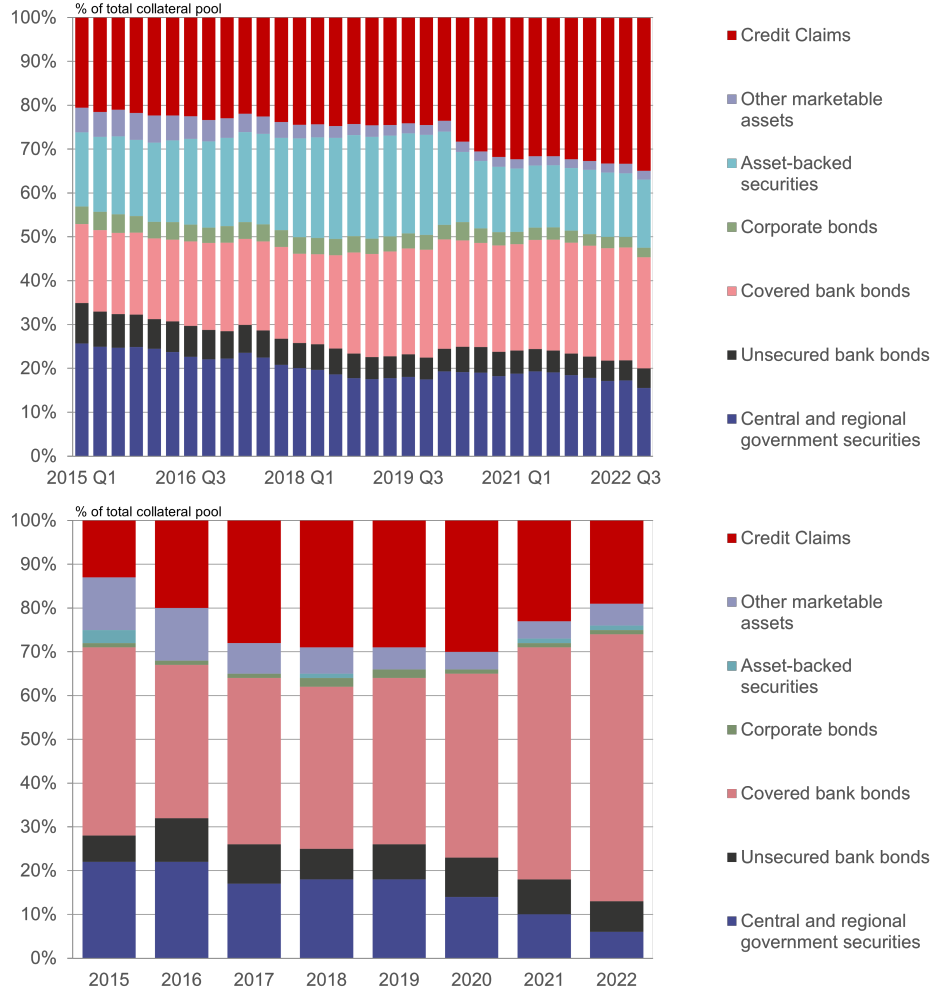
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<sup>8</sup>A list of marketable instruments acceptable as collateral is updated daily on the ECB's [website](#).

<sup>9</sup>Based on the BoF annual reports, the average (post-haircut) value of the collateral pledged at the BoF was around EUR 17bn in 2019, EUR 27bn in 2020, and EUR 40bn in both 2021 and 2022.

found the use of credit claims as collateral costly.

**Figure 2:** Central bank collateral pools



*Notes:* The figure plots the relative share of each asset class in the total (after haircut) collateral pool, pledged by counterparty banks of the Eurosystem (upper panel) and the Bank of Finland (lower panel). Eurosystem: data are quarterly month-end averages between Q1/2015–Q4/2022. Bank of Finland: data are annual averages between 2015–2022. Sources: ECB and Bank of Finland.

In Finland, an additional obstacle to more widespread pledging of credit claims is operational complexity.<sup>10</sup> To pledge credit claims as collateral, banks must get an approval from the BoF for their internal ratings-based (IRB) model for assessing credit risk that has also been approved by the prudential regulation authority. Banks might also need to update their

<sup>10</sup>Unlike in some other eurozone national central banks, an in-house credit assessment system, or ICAS, is not available at the BoF. With ICAS, the credit assessment of credit claims is done by the central bank, significantly reducing the operational costs for banks. For more information, see [Auria, Bingmer, Graciano, Charavel, Gavilá, Iannamorelli, Levy, Vignolo Maldonado, Resch, Rossi, et al. \(2021\)](#).

internal systems and participate in regular audits of their credit claim collateral and pledging process. The delivery of the credit claim collateral may also involve some operational costs. Furthermore, the use of ACC-eligible collateral requires an additional round of risk model approvals and testing. All in all, it may be a lengthy and operationally costly process for a bank to begin pledging credit claims as collateral, and even more so for ACC collateral. Thus, if banks do not plan to borrow from the central bank extensively or have plenty of marketable assets to cover their collateral needs, they may not have the incentives to become operationally capable of pledging credit claims as collateral.

ACC frameworks may also vary across NCBs in several dimensions, which may affect the usage of the frameworks or their potential to expand the aggregate collateral pool. For example, Additional Credit Claims may have different requirements for credit risk, types of debtor or loan guarantees, and pools of loans (versus individual loans) might be accepted in some NCBs but not others.

## 2.2 Changes in collateral rules

The BoF changed its collateral rules twice in 2020. First, in April 2020, credit claims with nominal size of EUR 25,000–500,000 became eligible collateral (in addition to claims of at least EUR 500,000 in nominal size).<sup>11</sup> There were no other changes in BoF’s collateral rules at the time. We call this the *LS (loan size) intervention*. Moreover, in August 2020, the BoF announced that it will adopt the Additional Credit Claims framework from September 2020.<sup>12</sup> This means that credit claims of firms with an estimated probability of default (PD) between 0.4–1.0 % during the next year became eligible as collateral (with higher haircuts than already-eligible credit claims); before the change only credit claims with a PD below 0.4

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<sup>11</sup>See the press release on the BoF’s [website](#).

<sup>12</sup>See the press release on the BoF’s [website](#). The ACC framework was introduced by the Eurosystem in December 2011, when the first National Central Banks adopted it in their collateral rules. As of 2022, ACC frameworks have been adopted by 17 National Central Banks (see the ECB [website](#)). As a response to the pandemic in 2020, also some other NCBs adopted or modified their ACC frameworks. National ACC frameworks can be altered to account for country-specific collateral policy needs, but need to be approved by the Governing Council of the ECB.

% were eligible. We call this the *ACC intervention*.<sup>13</sup> Both of the collateral policy changes in 2020 were announced with practically an immediate effect and were unanticipated by the market participants.

As a response to the pandemic in April 2020, also the ECB made (temporary) changes to the collateral rules that also have an impact on the rules set by the BoF (see [the ECB website](#) for details). For example, haircuts were lowered for all collateral and minimum requirements for the ACC programs were altered to accept a wider set of loans, such as publicly guaranteed pandemic-related corporate loans (a rule that was adopted by the BoF, as discussed above). Importantly for our analysis, these other changes in collateral rules should not have a varying effect on credit claims of different nominal amounts or PDs (although the rules may have changed the relative attractiveness between different types of collateral). Thus, the other changes in collateral rules should not compromise our identification strategy (see Sections [4.1](#) and [5.1](#) for details).

## 2.3 Finland in 2020 vs. France in 2012

To facilitate the comparison to our closest reference paper [Mésonnier, O'Donnell, and Toutain \(2022\)](#), we point out some key differences (and similarities) in the institutional setting between the collateral policy changes in Finland in 2020 and in France in 2012.

First, the operational cost of pledging credit claim collateral is different in Finland and France. In Finland, pledging non-marketable collateral requires several steps of verifying risk assessment models and updating internal processes. The fixed costs of pledging collateral are lower in France, because instead of getting their own risk models approved, banks can utilize credit rating system provided by the Banque de France. Moreover, the actual pledging process requires more manual work from banks in Finland, whereas in France banks can utilize the automated reporting and pledging platform of the Banque de France that has

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<sup>13</sup>Simultaneously, corporate loans that had a public guarantee related to COVID-19 became eligible collateral within the ACC framework. However, we do not consider this change in our analysis. Changes in the ACC frameworks related to COVID-19, such as the acceptance of loans with a pandemic-specific public guarantee, were more temporary in nature than other changes and initially valid only until fall 2021.



been in place since 2002 ([Mésonnier, O'Donnell, and Toutain, 2022](#)).

Second, partly due to the differences in operational costs, a large number of French banks have been pledging credit claim collateral for years.<sup>14</sup> This has made them operationally more ready and lowered their hurdle to utilize newly-eligible credit claim collateral also in 2012. In contrast, most Finnish banks have not pledged credit claims very actively, and had to first incur fixed costs for setting up operational readiness to utilize ACC-eligible loans, as well as smaller denominated loans as collateral in 2020.

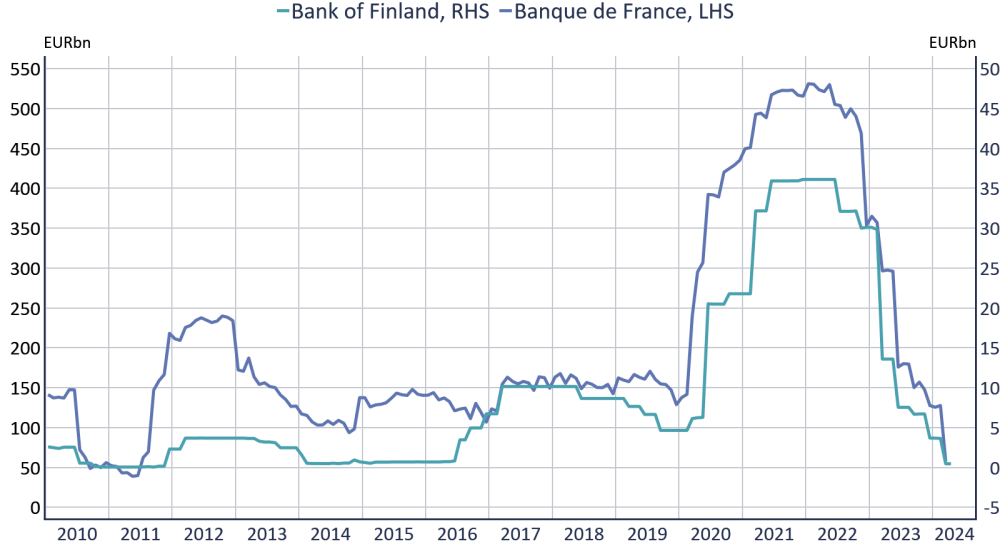
Third, the financial market environment was clearly different in 2012 and in 2020. In particular, most banks were in need of central bank funding and faced at least some degree of collateral constraints in 2011–2012 due to the turbulence created by the eurozone debt crisis. These banks included (at least some) French banks. However, most euro area banks—and in particular Finnish banks—faced no funding issues or collateral scarcity in 2020–2021 despite the pandemic, as the most gloomy economic scenarios did not realize. The effects of the different financial market environments on collateral and funding needs of banks were likely amplified by the difference in the relative strength of domestic banking sectors. Indeed, Finnish banks in 2020 were among the strongest banks in the euro area in terms of capital and liquidity position, while, at least some, French banks in 2012 faced funding issues.<sup>15</sup> The different development of central bank lending between the two countries underlines this observation. French banks borrowed quite heavily in 2012, whereas the Finnish banks did not. In 2020, *all* banks borrowed quite heavily from the Eurosystem due to the exceptionally attractive terms of the TLTRO loans (Figures 1 and 3). Notably, Finnish banks did not borrow very actively before the introduction of the arbitrage-like interest rate on TLTROs in spring 2020.

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<sup>14</sup>For example, [Mésonnier, O'Donnell, and Toutain](#) state that 16/56 of the French credit institutions posted ACC collateral in 2012, and these 16 institutions accounted for almost 75% of the collateral pledged at the Banque de France.

<sup>15</sup>Furthermore, Finnish banks may in general differ from French banks in their business models and liquidity management strategies, which can then be reflected in a lesser need of central bank credit, and therefore lower need of collateral (including non-marketable debt as collateral).

**Figure 3:** Bank borrowing from the Banque de France and the Bank of Finland



*Notes:* The graph plots the aggregate borrowing from refinancing operations of the Banque de France and the Bank of Finland by their respective counterparty banks in EUR billion. Sources: Banque de France, Bank of Finland and Macrobond.

## 3 Data

### 3.1 Sample selection and control group choices

We utilize two separate datasets. First, we use a proprietary and confidential credit register, namely the Finnish implementation of Anacredit<sup>16</sup>. The database contains data on all corporate loans granted by Finnish credit institutions at a monthly frequency. Second, we use confidential data from the BoF on banks' participation in Eurosystem operations to obtain information on banks' outstanding credit and pledged collateral with the central bank. Our data covers all counterparty banks of the BoF.

For the corporate loan data, our selection of sample is guided by the aim of capturing loans with characteristics that, excluding their nominal size and probability of default, would be eligible for the Eurosystem's collateral framework. We begin by extracting information on new loans at the creditor-debtor-loan level between April 2019 and September 2021. In out

<sup>16</sup>See the [BoF website](#) for reporting requirements, attribute definitions etc.

sample, creditors are Finnish credit institutions for which valid internal ratings-based PDs can be obtained (this is needed to determine the eligibility status of the loans). Debtors are Finland-domiciled entities operating in institutional sectors *S.11 non-financial corporations* or *S.13 general government*. Loan instruments are EUR denominated loans with instrument type *4791 other loans*. Loan observations are those having above-zero nominal outstanding amounts. For the response variable (loan interest rate), we trim large outliers in the 0.5 % percentile. Lastly, for our preferred specification with control variables (specified in detail below), we exclude observations with missing attribute values and clean observations with negative loan maturities and debtor ages.

We divide the remaining observations into six different groups. First, there are groups related to the *ACC regressions*, which are used to study the effect of accepting riskier loans as collateral around the intervention of September 2020. Second, there are groups related to the *LS regressions*, which are used to study the effect of lowering the minimum loan size threshold for collateral eligibility around the intervention of April 2020. In short, the loans are divided into groups based on their PD category, loan size category, as well as their reporting periods. The exact regression group definitions are as follows:

- *ACC Treatment group*: Contains observations that are equal to or above EUR 25,000 in size and have PDs in range (0.4%, 1.0%]. If not otherwise stated, the group includes observations from periods September 2019 – August 2020 (pre-event) and October 2020 – September 2021 (post-event).
- *ACC Always-Eligible control group*: Contains observations that are equal to or above EUR 500,000 in size and have PDs equal to or below 0.4%. If not otherwise stated, the time range is the same as in the ACC treatment group.
- *ACC Never-Eligible control group*: Contains observations that are either a) below EUR 25,000 in size and have PDs equal to or below 2.0%<sup>17</sup>, or b) above EUR 25,000 in size

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<sup>17</sup>Never-eligible control group both in ACC and LS intervention cases exclude observations with PDs 2.0

and have a PD between 1.0% and 2.0%. If not otherwise stated, the time range is the same as in the ACC treatment group.

- *LS Treatment group*: Contains observations that are in size range EUR [25 000, 500 000) and have PDs equal to or below 0.4%. If not otherwise stated, the group includes observations from periods April 2019 – March 2020 (pre-event) and May 2020 – April 2021 (post-event).
- *LS Always-Eligible control group*: Otherwise the same as the ACC always-eligible group, but the time range is as in the LS treatment group.
- *LS Never-Eligible control group*: Otherwise the same as the ACC never-eligible group, but the time range is as in the LS treatment group.

Table 1 summarizes the different groups. Further, Figure A1 in the Appendix contains a more detailed illustration with time dimension included.

**Table 1:** Illustration of the treatment and control groups

(a) ACC regression groups			
	PD $\leq$ 0.4%	0.4% < PD $\leq$ 1.0%	1.0% < PD $\leq$ 2.0%
LS < 25k	NE	NE	NE
25k $\leq$ LS < 500k		T	NE
LS > 500k	AE	T (strict)	NE (strict)

(b) LS regression groups			
	PD $\leq$ 0.4%	0.4% < PD $\leq$ 1.0%	1.0% < PD $\leq$ 2.0%
LS < 25k	NE (strict)	NE	NE
25k $\leq$ LS < 500k	T		NE
LS > 500k	AE		NE

*Notes:* The table illustrates how the treatment and control groups are formed from combinations of loan size and PD categories. The top (bottom) panel presents the case for the ACC (LS) regressions. 'T' stands for treatment, 'AE' for always-eligible, and 'NE' for never-eligible. Elements with the label 'strict' form the restricted treatment and control groups of model M7 (see text).

% or higher. The choice to limit the never-eligible sample is made to have the never-eligible control group match the treatment group better in terms of loan amounts and interest rates. If we included loans with PDs 2.0 % or higher, the number of loans and the average interest rate would be higher and further away from the treatment group than in the chosen never-eligible control group.

### 3.2 Descriptive evidence: ACC regression groups

Let us first scrutinize the ACC regression groups. Table 2 provides descriptive statistics for the treatment and control groups, separately for pre- and post-intervention periods. In turn, Figure 4 displays several metrics of credit supply across the treatment group (blue) as well as both control groups (yellow and orange).

The top subplot in Figure 4 shows that the average interest rates over groups remain broadly stable during the sample period. As expected, the average rate increases when moving from the control group with lower PD firms (always eligible; yellow) to the treatment group of medium PD firms (blue), and finally to the control group with higher PD firms (never eligible; orange). We don't observe any clear changes in the average rates across groups around the time of the policy change of September 2020.

The middle and bottom subplots in Figure 4 exhibit the development of the nominal amount (middle) and the total number (bottom) of new loans across treatment and control groups. Visual inspection reveals that both metrics develop quite similarly across groups. The changes from period to period around the intervention do not seem to be radically different in the treatment group compared to the control groups.

All in all, simple aggregate credit supply metrics do not exhibit any clear effects from the adoption of the ACC framework in September 2020.

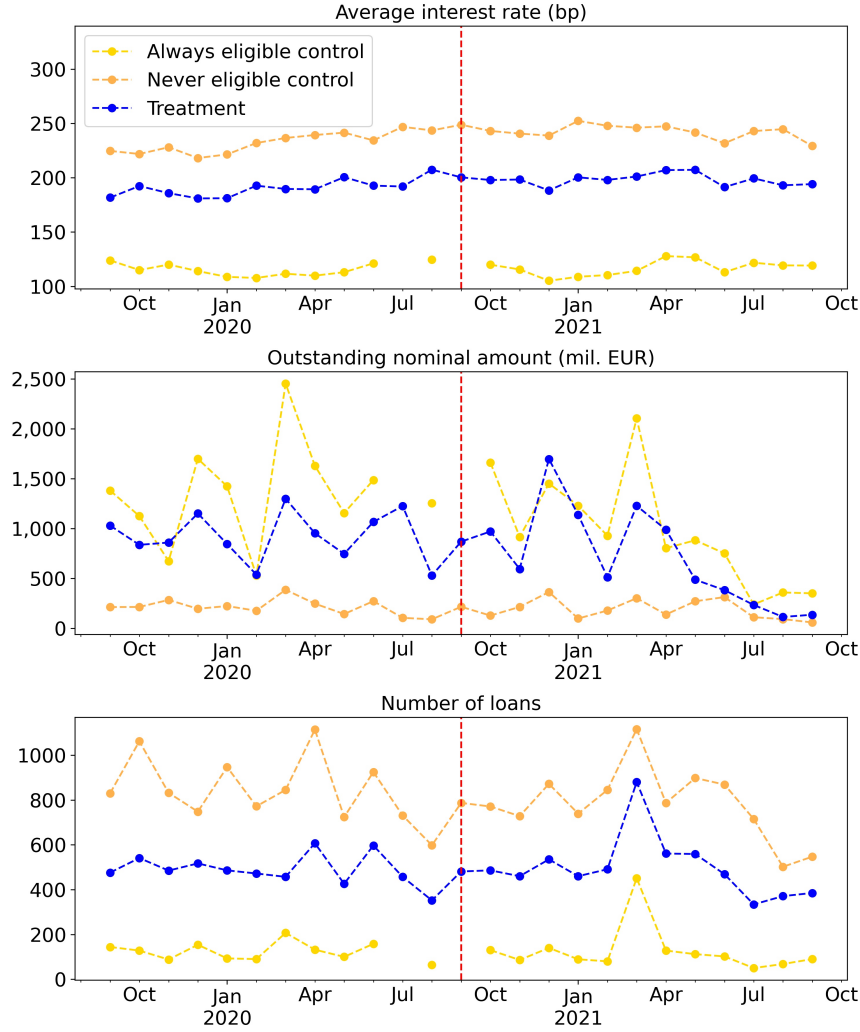
Figure 4 also helps to assess the magnitude of the collateral policy change. Around 35 % of the number of total loans in our sample belong to the treatment group, slightly below 10 % to the always-eligible control group, and around 55 % to the never-eligible control group. The relative shares are quite stable over time and in the ballpark of what could be expected for the whole stock of loans in Finland. In contrast, the relative share of outstanding nominal amount varies somewhat over time. On average, the treatment group covers 37 % of the total amount in our sample, while for control groups the shares are 53 % (always-eligible) and 9 % (never-eligible). From this perspective, the policy change is significant as a major share of banks' loan portfolios becomes eligible as collateral.

**Table 2:** Descriptive statistics for ACC regression groups

	Mean	SD	5 perc	25 perc	50 perc	75 perc	95 perc
Treatment, pre. N = 5,873.							
IR	190	79	75	140	185	235	329
Loan size	1,888	7,462	30	50	117	500	9,200
Debtor age	22	18	2	8	19	32	56
Loan maturity	5.7	4.6	0.5	3.0	5.0	6.0	15.0
PD	0.6%	0.2%	0.5%	0.5%	0.6%	0.8%	0.8%
Treatment, post. N = 5,991.							
IR	199	80	84	150	192	240	350
Loan size	1,417	6,497	29	51	116	450	5,781
Debtor age	21	18	2	7	16	31	53
Loan maturity	5.5	4.5	0.6	3.0	5.0	6.0	15.0
PD	0.6%	0.2%	0.5%	0.5%	0.6%	0.8%	0.8%
Always eligible control, pre. N = 1,456.							
IR	115	54	45	75	105	146	210
Loan size	11,235	28,793	500	1,000	2,186	10,000	48,083
Debtor age	27	26	1	7	19	38	84
Loan maturity	6.0	6.2	0.2	1.8	4.5	7.2	22.3
PD	0.2%	0.1%	0.0%	0.1%	0.2%	0.3%	0.4%
Always eligible control, post. N = 1,526.							
IR	116	54	45	77	110	150	202
Loan size	7,655	20,565	500	823	1,800	5,000	31,736
Debtor age	28	25	1	8	23	42	80
Loan maturity	6.8	6.8	0.2	3.0	5.0	9.0	24.2
PD	0.2%	0.1%	0.0%	0.1%	0.2%	0.3%	0.4%
Never eligible control, pre. N = 10,128.							
IR	232	109	90	161	222	294	399
Loan size	252	1,754	8	20	38	100	671
Debtor age	20	17	1	7	15	29	49
Loan maturity	4.9	3.6	0.7	3.0	4.9	5.1	10.0
PD	1.3%	0.5%	0.3%	1.2%	1.2%	1.9%	1.9%
Never eligible control, post. N = 9,389.							
IR	242	114	90	176	227	295	406
Loan size	242	1,431	8	20	38	100	564
Debtor age	19	16	1	6	14	30	49
Loan maturity	5.0	3.6	0.8	3.0	4.9	5.9	10.0
PD	1.3%	0.5%	0.3%	1.2%	1.2%	1.6%	1.9%

*Notes:* The table presents descriptive statistics for ACC regression groups as in the baseline model M5. 'SD' refers to standard deviation, 'X perc' to Xth percentile, and 'IR' to loan interest rate. The time interval is from 2019-09 to 2021-09. Intervention period 2020-09 is excluded from the sample. Interest rate is given in basis points, loan size in thousand euros, and debtor age as well as loan maturity in years. Sources: Finnish implementation of Anacredit and authors' calculations.

**Figure 4:** Credit supply metrics for ACC regression groups



*Notes:* The three panels present, respectively, the evolution of the average new loan interest rate, the outstanding new loan amount, and the number of new loan contracts in each ACC regression group over time. The data sample corresponds to the baseline model M5. July 2020 and September 2020 observations in the always-eligible control group have been omitted due to confidentiality concerns. Sources: Finnish implementation of Anacredit and authors' calculations.

### 3.3 Descriptive evidence: LS regression groups

Let's now turn to the LS regression groups. Table 3 and Figure 5 provide similar descriptive statistics as above with the ACC regression groups.

In the top subplot of Figure 5, we observe that after the policy change the average interest rate trends higher slightly more in the control group of never-eligible loans (orange line)

than in other groups. The never-eligible group consists mostly of small loans with nominal amounts below EUR 25,000. Theoretically, this observation could result from smaller loans becoming more expensive for borrowers as only larger loans are pledgeable as collateral. However, such a change in average rates could also result from, for example, changes in the composition of borrowers in the control group. If firms borrowing in small amounts started to be more risky (for example, due to the economic effects of the pandemic), average rates charged on small loans would also increase. Alternatively, small loans may in general include more firms with higher credit risk, and thus changes in the pricing of credit risk premia may affect treatment and control groups differently. We investigate these conjectures further below.

The middle and bottom subplots of Figure 5 show the development of other credit supply metrics in the treatment and control groups: the outstanding amount and the total number of new loans each month. We cannot find a clear change in the cross-group relationships between these measures during the time of intervention.

Similarly as above with the ACC regression groups, Figure 5 helps to assess the magnitude of the collateral policy change in the case of LS intervention. Compared to the ACC intervention, the magnitude of the LS intervention is much smaller. In particular, a relatively small fraction of loans—25 % in terms of loan numbers and 3 % in terms of nominal amounts—belongs to the treatment group. For the always-eligible and never-eligible control groups, the corresponding numbers are 10 % & 84 % and 65 % & 13 %, respectively. From this perspective, one might not expect large effects from the LS intervention, as merely 3 % of the previously ineligible bank loan volume becomes eligible as collateral due to the policy change.

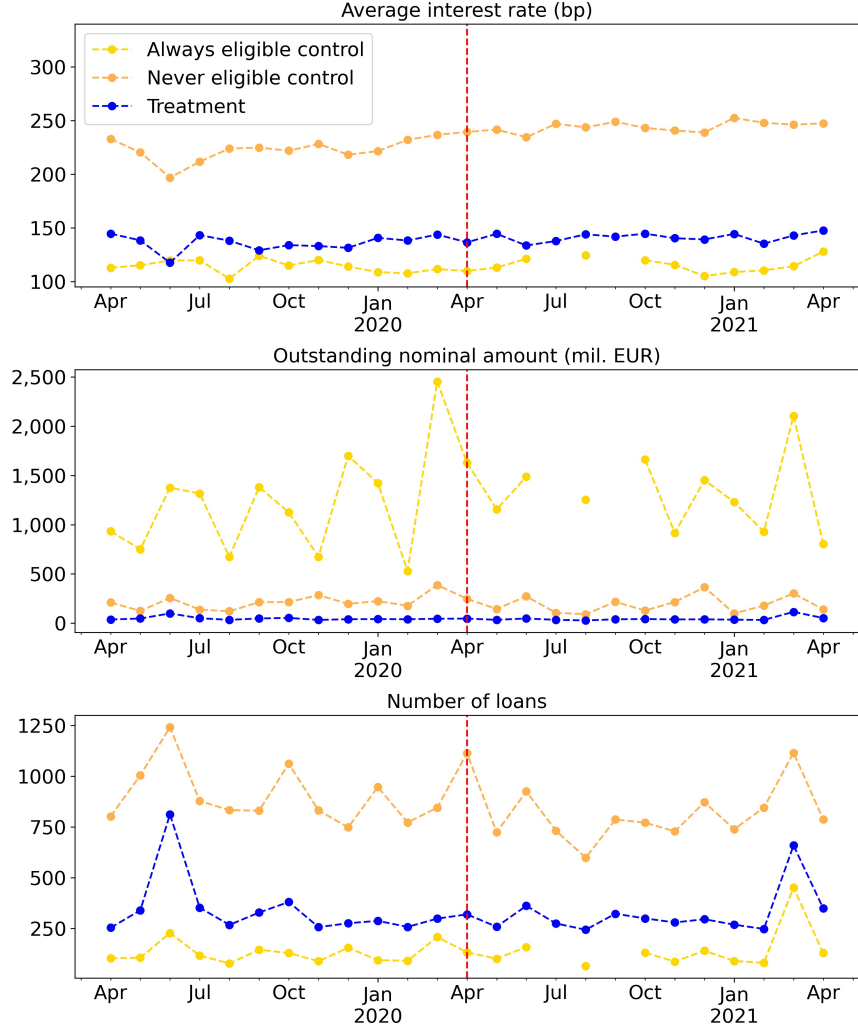


**Table 3:** Descriptive statistics for LS regression groups

	Mean	SD	5 perc	25 perc	50 perc	75 perc	95 perc
Treatment, pre. N = 4,104.							
IR	134	63	60	94	121	162	246
Loan size	138	110	30	50	100	200	380
Debtor age	33	21	5	17	31	44	66
Loan maturity	7.2	5.4	0.5	4.0	5.0	10.0	19.7
PD	0.2%	0.1%	0.1%	0.2%	0.3%	0.3%	0.4%
Treatment, post. N = 3,853.							
IR	141	64	64	95	136	179	250
Loan size	139	112	30	50	100	200	382
Debtor age	31	20	4	15	28	42	64
Loan maturity	6.4	5.0	0.5	3.0	5.0	9.3	17.2
PD	0.2%	0.1%	0.1%	0.1%	0.3%	0.3%	0.4%
Always eligible control, pre. N = 1,532.							
IR	115	55	47	78	100	146	204
Loan size	9,357	24,270	500	945	1,860	7,500	39,582
Debtor age	27	26	2	6	20	42	82
Loan maturity	7.0	7.0	0.2	2.0	5.0	9.9	24.5
PD	0.2%	0.1%	0.0%	0.1%	0.2%	0.3%	0.4%
Always eligible control, post. N = 1,627.							
IR	115	52	45	80	105	150	206
Loan size	9,671	26,233	504	934	1,981	6,940	39,428
Debtor age	28	26	1	7	22	38	84
Loan maturity	6.1	6.2	0.2	2.5	4.7	7.5	20.0
PD	0.2%	0.1%	0.0%	0.1%	0.1%	0.3%	0.4%
Never eligible control, pre. N = 10,796.							
IR	221	111	85	151	202	278	400
Loan size	236	1,749	7	18	36	100	600
Debtor age	20	17	1	7	15	30	51
Loan maturity	4.9	3.6	0.6	3.0	4.9	5.4	10.1
PD	1.3%	0.5%	0.3%	1.2%	1.2%	1.9%	2.0%
Never eligible control, post. N = 9,623.							
IR	244	113	95	176	232	295	402
Loan size	234	1,474	8	20	37	100	600
Debtor age	19	16	1	6	14	29	48
Loan maturity	4.8	3.5	0.8	3.0	4.9	5.1	10.0
PD	1.3%	0.5%	0.3%	1.2%	1.2%	1.8%	1.9%

*Notes:* The table presents descriptive statistics for LS regression groups as in the baseline model M5/M5a. 'SD' refers to standard deviation, 'X perc' to Xth percentile, and 'IR' to loan interest rate. The time interval is from 2019-04 to 2021-04. Intervention period 2020-04 is excluded from the sample. Interest rate is given in basis points, loan size in thousand euros, and debtor age as well as loan maturity in years. Sources: Finnish implementation of Anacredit and authors' calculations.

**Figure 5:** Credit supply metrics for LS regression groups



*Notes:* The three panels present, respectively, the evolution of the average new loan interest rate, the outstanding new loan amount, and the number of new loan contracts in each LS regression group over time. The data sample corresponds to the baseline model M5. July and September 2020 observations in the always-eligible control group have been omitted due to confidentiality concerns. Sources: Finnish implementation of Anacredit and authors' calculations.

## 4 Results: Accepting riskier corporate loans as collateral

In this section, we analyze the main event of interest, that is, the September 2020 ACC intervention. We investigate whether accepting riskier corporate loans as collateral affected their pricing. The policy intervention is similar to the one assessed by [Mésonnier, O'Donnell, and Toutain \(2022\)](#), providing us a natural reference study.

## 4.1 Identification and econometric strategy

Our goal is to assess whether corporate loans are priced more favourably by banks if the loans are eligible as central bank collateral. To do this, we utilize a collateral policy change by the BoF as a quasi-natural experiment. Specifically, in September 2020, the BoF introduced the ACC program and began to accept riskier credit claims with PD of 0.4–1.0 % as collateral. Previously, the BoF accepted only credit claims with PD of 0.4 % or below as collateral. We utilize both "static" and "dynamic" difference-in-differences (DiD) specifications<sup>18</sup> in a repeated cross-sample setting to compare the interest rate development of newly-eligible loans in the ACC treatment group (defined in Section 3.1) and loans in the always-eligible and never-eligible control groups.

### 4.1.1 Static difference-in-differences

To estimate the effect of becoming eligible as central bank collateral on loan pricing, we utilize the following static DiD regression specification:

$$\begin{aligned} Rate_{lfbt} = & \alpha_1 \mathbb{1}_{ACC-T} + \alpha_2 Post_{t \geq 9/2020} + \beta \mathbb{1}_{ACC-T} \times Post_{t \geq 9/2020} \\ & + \Phi_{xt} + \gamma_1 X_{lt} + \gamma_2 Z_{ft} + \varepsilon_{lfbt}, \end{aligned} \quad (1)$$

where  $Rate_{lfbt}$  is the interest rate of loan  $l$  of firm  $f$  at bank group  $b$  at month  $t$ . Dummy  $\mathbb{1}_{ACC-T}$  is equal to 1 if the loan is in the ACC treatment group (otherwise zero) and thus eligible as collateral only in the ACC framework after September 2020.  $Post_{t \geq 9/2020}$  is equal to 1 from September 2020 onwards (this term might be redundant depending on the chosen time fixed effects).  $\Phi_{xt}$  is a vector of fixed effects, nesting both *period* fixed effects as well as interacted *entity-period* fixed effects, where "entity" ( $x$ ) refers to an arbitrary entity (such as a bank group or debtor industry). Vectors  $X_{lt}$  and  $Z_{ft}$  denote other loan- and firm-specific control variables, respectively. Below we specify elements in vectors  $\Phi_{xt}$ ,  $X_{lt}$  and  $Z_{ft}$

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<sup>18</sup>See, e.g., [Roth, Sant'Anna, Bilinski, and Poe \(2023\)](#) for the distinction between "static" and "dynamic" difference-in-differences.

separately for each regression model.<sup>19</sup> Standard errors are clustered at *bank group*  $\times$  *month* level. We exclude the intervention month from the data sample.<sup>20</sup>

In Equation (1), our coefficient of interest is the DiD estimate  $\beta$  that measures the differential loan pricing for ACC (riskier) and non-ACC (less risky) firms, and therefore the differential pricing of loans due to becoming eligible as central bank collateral in September 2020. If collateral eligibility lowers the interest rate that banks require firms to pay for their loans, we would expect  $\beta$  to be negative.

We make three assumptions: 1) the policy change is not anticipated, 2) the policy change does not affect the control group, and 3) groups have parallel trends pre-treatment. If all of the three assumptions hold, the difference in average interest rates between the treatment and control groups is affected only by the change in collateral eligibility—and  $\beta$  then measures the causal effect of eligibility on loan pricing. Regarding the first assumption, the ACC program adoption was announced with an immediate effect and was unanticipated by markets participants. Hence, we argue that the assumption of non-anticipation is satisfied. The second assumption should also hold as our control group consists of loans whose collateral eligibility is unchanged.<sup>21</sup>

Our main worry about the validity of the parallel trends assumption is possible time-varying shocks to the pricing of credit risk, which would confound the effect of becoming eligible on the pricing of the loans.<sup>22</sup> We aim to alleviate this issue with our careful selection

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<sup>19</sup>For example, in Table 4 model *M1* has  $\Phi_{xt} = [1]^T$  (that is, no fixed effects and only an intercept) and model *M5* has  $\Phi_{xt} = [\phi_{bt}, \phi_{it}, \phi_{rt}]^T$ , where  $\phi_{bt}$  denotes *bank group-period* FEs,  $\phi_{it}$  *debtor industry-period* FEs, and  $\phi_{rt}$  *debtor region-period* FEs.

<sup>20</sup>Intervention announcement took place within the intervention month, with new loans being granted within the month before and after the announcement. If banks' lending policy was to react immediately to the intervention, some observations from the intervention month might be "untreated" while some are "treated". In order to avoid ambiguous observations in our static difference-in-differences regressions, the intervention period is excluded. In our auxiliary dynamic difference-in-differences specifications we include the treatment month in the sample.

<sup>21</sup>Thus, we explicitly rule out the possibility that the policy change lowers the liquidity premia on *all* loans (see footnote 1).

<sup>22</sup>In particular, a specific threat to the parallel trends assumption may be that riskier firms (as measured by their PD) experience different shocks in loan demand and/or supply in the months following the COVID-19 shock in March 2020 compared to their less risky peers. As a result, it would seem as if the collateral policy change had an effect on loan pricing, even though the effect would be due to differential shocks to credit demand and/or supply across firm PD groups.

of the control groups. We compare the loans that became eligible (treatment group) to both 1) those that were already eligible (always-eligible control group, safer firms), and 2) those that did not become eligible (never-eligible control group, riskier firms). Both provide an estimate for the effect of being eligible as collateral on loan pricing, with different focuses. If there is a shock to credit demand or supply explained only by firm creditworthiness (instead of the collateral policy change), the DiD estimates in our regressions should be different across analyses with different control groups.<sup>23</sup> Vice versa, if the policy change indeed affected the pricing of credit, we would expect to see similar DiD estimates across both control groups. Further, as noted in footnote 17, by filtering out very high PD observations from the control groups we attempt to ensure they provide a valid comparison for the treatment group.

In addition to carefully selecting the control groups, we aim to minimize the risk of confounding by using varying sets of control variables and fixed effects. In Tables 4 and 5, we estimate seven different regression models (M1–M7). The first model *M1* presents results of the vanilla DiD model (that is, no fixed effects nor covariates). The second model *M2* adds *bank group-treatment period*<sup>24</sup> fixed effects. While we assume that the relative composition of banks across treatment and control groups remains the same, there might be bank-specific idiosyncrasies with time-varying effects (shocks that affect the bank’s credit supply, such as varying risk premia), which we want to control for with the interacted fixed effects<sup>25</sup>. Relatedly, model *M3* adds *debtor industry-treatment period* and *debtor region-treatment period* fixed effects, with which we aim to control for debtor industry and region-specific factors with time-variant effects (e.g., shocks to credit demand or supply).

Our treatment and control groups have loans of different sizes, potentially introducing bias. In our treatment group, there are loans with loans size above EUR 25,000 (and PDs in

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<sup>23</sup>Typically in DiD settings, the ‘never-treated’ group is the most natural comparison group. However, as pointed out by [Mésonnier, O’Donnell, and Toutain \(2022\)](#), if banks value collateral eligibility differently over time and the COVID-19 crisis or the TLTRO-III program affected the value of eligibility, using the always-eligible control group may provide a better estimate of the eligibility discount than the never-eligible control group.

<sup>24</sup>"Treatment period" refers to pre-/post-intervention period.

<sup>25</sup>[Zeldow and Hatfield \(2021\)](#) discuss in length how time-invariant/time-variant control variables with time-invariant/time-variant effects should be included in a DiD setting.

the range of (0.4%, 1.0%]). The ACC always-eligible group has only loans with sizes above EUR 500,000, and the ACC never-eligible group has loans of all sizes. In order to rule out the possible interference of the earlier April 2020 LS intervention via differently-sized loans, we add a loan-level control variable, the loan size category, in model *M4*.<sup>26</sup> Additionally, we consider a more restricted sample to deal with the issue of differently-sized loans in different control groups (see discussion about *M7* below).

Our preferred specification is *M5*. In addition to the already mentioned covariates, it adds two debtor-level covariates: debtor size category and firm age. The aim is to capture the time-variant evolution of firm riskiness within the treatment and control groups (namely, in different PD categories) that could confound the effect of higher PD loans becoming eligible on loan pricing. We argue that firm size and firm age together form a decent proxy of the riskiness of a firm.<sup>27</sup> In addition to the preferred specification, in specification *M6* we add two loan-level covariates—loan maturity and a 'protected' dummy (indicating whether the loans have collateral allocated)—as well as month fixed-effects. Specification *M6* matches, as closely as possible, the baseline specification of our closest reference paper [Mésonnier, O'Donnell, and Toutain \(2022\)](#).<sup>28</sup>

Finally, as a robustness check (with the aim of having as similar as possible treatment and control groups) we consider a stricter definition of the regression groups by restricting the sample to only loans larger than EUR 500,000 (model *M7*).<sup>29</sup> Panel (a) in Table 1 illustrates this (combinations labeled '*strict*'). As a result, the treatment group now consists

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<sup>26</sup>Strictly speaking, loans with sizes in the range of EUR [25 000, 500 000) and PDs in the range of (0.4%, 1.0%] did not become eligible in the April 2020 LS intervention due to their PDs being too high. However, if the April intervention did have a large effect, there could be some spillover to loans that were eligible in terms of their loans size and almost eligible in terms of their PD. We want to account for any such possible spillover by adding loan size as a control variable.

<sup>27</sup>Figure A1 in the Appendix demonstrates our preferred specification M5 as a Directed Acyclic Graph (DAG).

<sup>28</sup>[Mésonnier, O'Donnell, and Toutain \(2022\)](#) also include investment loan dummy, debtor debt-to-assets and sales-to-assets as well as part-of-group dummy as control variables. We do not have reliable data on these at our disposal. Further, their model had data in quartely frequency and used quarter fixed effects in their baseline model. Our static DiD models are in monthly frequency and we employ monthly fixed effects.

<sup>29</sup>We also considered a regression discontinuity design-like approach, where, in order to enhance identification, we focused on observations close to the threshold determining collateral eligibility. However, this approach did not provide clear advantages.

only of observations with loan sizes above EUR 500,000 and PDs in range (0.4%, 1.0%]. The always-eligible control group remains the same. The never-eligible control group now consists of observations that are above EUR 500,000 in size and have PDs in range (1.0%, 2.0%]. Thus, the regression groups differ only with respect to their PD categories but not size categories. Beside the treatment and control group definitions, M7 is equivalent to M5. However, the loan size category control drops from the regression as treatment and control groups only consist of loans of the same size category.

We present evidence for the validity of the parallel trends assumption in Section 4.2, where we discuss results from the dynamic DiD regressions.<sup>30</sup>

#### 4.1.2 Dynamic difference-in-differences

To provide an alternative view with an ability to analyze the time dimension more properly as well as to investigate the plausibility of the parallel trends assumption in the pre-intervention period, we also run a quarterly-frequency dynamic DiD specification akin to model *M5*:

$$Rate_{lft} = \alpha_1 \mathbb{1}_{ACC-T} + \Phi_{xt} + \sum_{r=-4, r \neq -1}^4 \beta_r \mathbb{1}_{ACC-T} \times [R_{l,t} = r] + \gamma_1 X_{lt} + \gamma_2 Z_{ft} + \varepsilon_{lft}, \quad (2)$$

where  $r$  denotes the relative quarter to the first treatment quarter 2020-Q3 ( $r = 0$ )<sup>31</sup>,  $R_{l,t}$  is the relative quarter of the loan, and  $\mathbb{1}_{ACC-T} \times [R_{l,t} = r]$  are the dynamic interaction dummy terms that equal 1 for the treatment group observations reported at the relative quarter  $r$ . The last full pre-treatment quarter 2020-Q2 is selected as the reference quarter, and thus the corresponding dummy is excluded from the regression. Fixed effects  $\Phi_{xt}$  as well as loan-specific covariates  $X_{lt}$  and firm-specific covariates  $Z_{ft}$  are as in specification *M5* of Equation (1). Standard errors are clustered at the *bank group*  $\times$  *quarter* level. Similar to

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<sup>30</sup>We note that a visual inspection of the pre-trends in the aggregate data makes it plausible that the parallel trends assumption holds (see Section 3.2).

<sup>31</sup>2020-Q3 is selected as the first post-treatment quarter as it contains the month of the treatment. It also contains two pre-treatment months, and thus we should not pay too much attention to the estimate obtained for this particular quarter as it may be hard to interpret.

the static DiD, the estimates of interest are  $\{\beta_r\}_{r=-4, r \neq -1}^4$ , for which negative values indicate an eligibility discount.

## 4.2 Regression results

**Always-Eligible control group.** Results from regression (1) with the always-eligible control group are in Table 4. Our baseline model (column *M5*) suggests that interest rates in the treatment group increase, on average, 3.8 basis points more than in the control group due to the collateral policy change, but the standard error is large (3.3 bps), so the estimate is not statistically significant. The 95 % confidence interval for the estimate is (-2.9, 10.4) bps, meaning that we can confidently rule out only effects larger than that—and cannot reject the null hypothesis that the intervention does not affect credit pricing ( $\beta = 0$ ).

Figure 6 shows the results from the dynamic DiD (Equation (2) with control variables as in *M5*). We can draw two takeaways from it. First, conditional on our control variables, the parallel trend assumption seems to hold, as the pre-intervention trend of the coefficient estimate is quite stable and close to zero (both the estimate and zero are within the confidence intervals) in each pre-period quarter.<sup>32</sup> Second, there is a message similar to Table 4: the ACC intervention in September 2020 did not significantly affect the interest rate differential between newly-eligible and already-eligible loans (zero is not outside the 95 % confidence interval in any post-period quarter).

Other columns than *M5* in Table 4 present results from the other regression models as outlined above. Irrespective of the model, coefficient estimates are quite similar to each other and statistically insignificant. The DiD estimate in the final column (restricted sample model *M7*) is slightly higher but again not statistically significant (despite the more similar treatment and control groups, standard errors increase as the number of observations decreases).

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<sup>32</sup>Indeed, the pre-trends are more volatile if we do not condition on the control variables. The estimates are also somewhat more volatile if we estimate the dynamic DiD at monthly frequency instead of quarterly frequency.



**Never-Eligible control group.** Results with the alternative control group of never-eligible loans are presented in Table 5. The message is similar to the always-eligible control group: the DiD estimate in the baseline model  $M5$  is close to zero (-1.3 bps) and not statistically significant. The corresponding 95 % confidence interval is wide: (-9.7, 7.0) bps. Estimates in columns  $M1-M4$  and  $M6$  vary slightly around zero but are each statistically non-different from zero. As with the always-eligible control group, the restricted sample ( $M7$ ) has again a more positive estimate, albeit a higher standard error and thus no statistical significance.

The dynamic DiD results (Equation (2) with controls as in  $M5$ ) for the never-eligible control group are shown in Figure 6. We make analogous observations as with the always-eligible control group: the parallel trend assumption seems to hold, and the intervention in September 2020 did not significantly affect the interest rate differential between newly-eligible and never-eligible loans.

It is possible that only banks with more experience in, or need of, pledging credit claims are the ones who respond to the policy change, but the effect does not show up in the aggregate data (Mésonnier, O'Donnell, and Toutain, 2022). Indeed, the rules concerning credit claim collateral mainly affect the banks that actually use credit claim collateral. To see whether this is the case, we would like to restrict our sample to only such banks. Unfortunately, we cannot publish the results for such a sample due to confidentiality concerns.<sup>33</sup>

In summary, we find no evidence for the hypothesis that accepting riskier loans as collateral would have reduced the interest rate charged on these loans. However, we do not

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<sup>33</sup>We ran the baseline regressions of this section using a restricted bank sample. In these unpublished results we did not find evidence of a significant collateral eligibility discount. The DiD estimate does not show any clear pattern and the corresponding standard error increases compared to the baseline results. All in all, like our baseline results, the unpublished ones do not suggest that the collateral policy change would have had a significant effect on loan pricing. Instead, we find that some banks increased the credit risk premia they charge from firms around the intervention, possibly due to worsening economic prospects due to the COVID-19 pandemic. For those banks, the DiD estimate is positive with the always-eligible group (cf. Table 4) and negative in the never-eligible sample (cf. Table 5). Note that in Table 4 the control group contains lower PD firms, while in Table 5 the control group contains higher PD firms. The opposite signs of the estimates suggest that those banks did not react to the collateral rule change; if the effect was caused by the collateral rule change, we would expect similarly-signed DiD estimates with both control groups.

find strong evidence of a zero effect either (the standard errors are fairly large). This means we can confidently rule out only effects that are outside the 95 % confidence interval—(-2.9, 10.4) bps for the always-eligible control group and (-9.7, 7.0) bps for the never-eligible control group.

**Table 4:** ACC regression, treatment vs. always-eligible control group

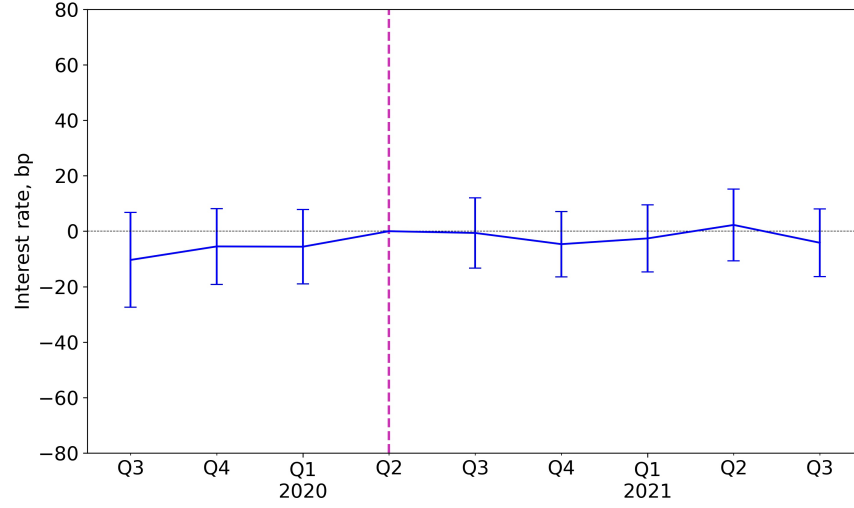
	<i>Dependent variable: interestrate</i>						
	M1	M2	M3	M4	M5	M6	M7
Intercept	115.3*** (2.0)						
Post	1.0 (3.0)						
Treatment	76.4*** (3.1)	83.7*** (2.8)	73.9*** (2.9)	71.4*** (3.8)	64.1*** (3.4)	61.1*** (3.4)	62.3*** (4.4)
Post x Treatment	6.9* (3.8)	4.1 (3.5)	4.3 (4.2)	4.2 (4.2)	3.8 (3.3)	3.7 (3.4)	8.6 (5.9)
Maturity						-1.8*** (0.1)	
Protected						4.2* (2.4)	
Debtor age					-0.8*** (0.1)	-0.7*** (0.1)	-0.6*** (0.1)
Medium debtor					-24.0*** (5.6)	-20.7*** (5.2)	-6.7 (4.9)
Small debtor					-9.9** (3.8)	-5.8 (3.7)	0.1 (3.9)
Micro debtor					3.4 (4.7)	9.6** (4.4)	-1.6 (5.7)
Large loan				-4.1 (3.4)	-3.1 (3.1)	-1.4 (2.7)	
Bank group x treatment period FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry x treatment period FEs	No	No	Yes	Yes	Yes	Yes	Yes
Region x treatment period FEs	No	No	Yes	Yes	Yes	Yes	Yes
Month FEs	No	No	No	No	No	Yes	No
Observations	14,997	14,997	14,997	14,997	14,846	14,846	5,905
$R^2$	0.14	0.16	0.23	0.23	0.28	0.29	0.40
Adjusted $R^2$	0.14	0.16	0.22	0.22	0.27	0.28	0.40

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

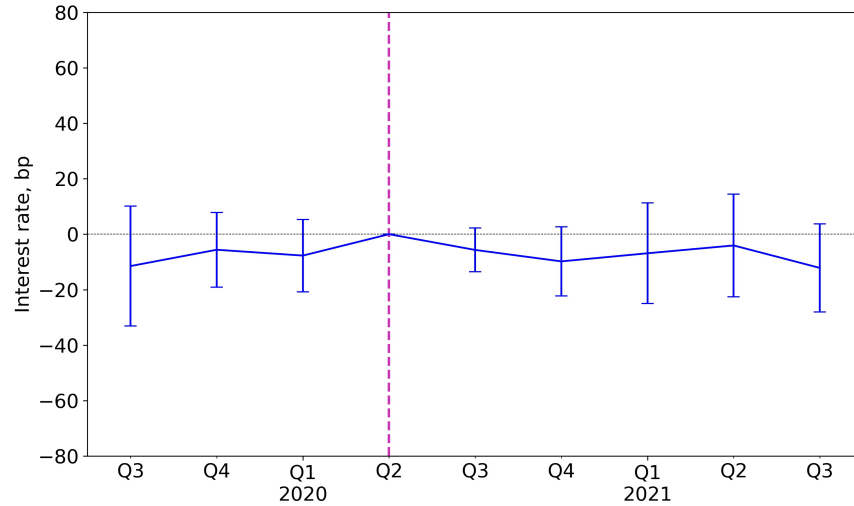
*Notes:* In models M1-M6, the treatment group consists of newly eligible loans with PDs between 0.4 % and 1.0 % and loan sizes equal to or above EUR 25,000, and the control group of always-eligible loans with PDs less than 0.4 % and loan sizes equal to or above EUR 500,000. Model M7 is otherwise as M5, but the treatment group is narrowed down: it consists of newly eligible loans with PDs between 0.4 % and 1.0 % and loan sizes equal to or above EUR 500,000. Dependent variable is interest rate of loan (in basis points). Intervention period is September 2020, which is excluded from the sample. Pre-period consists of a time range from 2019-09 to 2020-08. Post-period consists of a time range from 2020-10 to 2021-09. Standard errors are clustered at bank group x month level. In fixed effects, industry is given at TOL 2008 character level, region at NUTS 2 level, and treatment period indicates pre/post periods. Firm size classification (micro, small, medium, large) is in accordance with the Annex to Commission Recommendation 2003/361/EC. Loan size categories are small ( $x < \text{EUR } 25,000$ ), medium ( $\text{EUR } 25,000 \leq x < \text{EUR } 500,000$ ), and large ( $x \geq 500,000$ ). Loan maturity is given in months. Protected-dummy refers to whether loan observation has allocated protection. Sources: Finnish implementation of Anacredit and authors' calculations.

**Figure 6:** ACC dynamic DiD, treatment vs. always-eligible control group



*Notes:* The figure displays results from the dynamic DiD model at a quarterly frequency. The model specification is similar to the static DiD model M5, except that standard error clustering is at the bank group x quarter level. The purple vertical line denotes the last full quarter before the intervention quarter. The first post-quarter 2020-Q3 contains both months prior to the intervention (July, August) as well as the intervention month (September). Vertical bars denote the 95 % confidence intervals. Sources: Finnish implementation of Anacredit and authors' calculations.

**Figure 7:** ACC dynamic DiD, treatment vs. never-eligible control group



*Notes:* The figure displays results from the dynamic DiD model at a quarterly frequency. The model specification is similar to the static DiD regression M5, except that standard error clustering is at the bank group x quarter level. The purple vertical line denotes the last full quarter before the intervention quarter. The first post-quarter 2020-Q3 contains both months prior to the intervention (July, August) as well as the intervention month (September). Vertical bars denote the 95 % confidence intervals. Sources: Finnish implementation of Anacredit and authors' calculations.

**Table 5:** ACC regression, treatment vs. never-eligible control group

	<i>Dependent variable: interstrate</i>						
	M1	M2	M3	M4	M5	M6	M7
Intercept	249.6*** (5.2)						
Post	6.4 (5.6)						
Treatment	-57.9*** (3.1)	-63.2*** (4.0)	-51.0*** (3.6)	-25.5*** (3.0)	-17.3*** (2.4)	-17.7*** (2.2)	-34.4*** (4.5)
Post x Treatment	1.5 (4.0)	5.7 (4.6)	1.8 (4.2)	1.2 (4.3)	-1.3 (4.2)	-1.3 (3.9)	8.7 (6.5)
Maturity						-2.2*** (0.2)	
Protected						5.8** (2.8)	
Debtor age					-1.1*** (0.0)	-1.0*** (0.0)	-0.8*** (0.1)
Medium debtor					-42.2*** (6.1)	-38.8*** (5.7)	-27.6*** (4.3)
Small debtor					-20.8*** (5.5)	-16.7*** (5.2)	-19.3*** (4.3)
Micro debtor					-3.1 (5.9)	2.8 (5.4)	-25.2*** (4.5)
Small loan				63.7*** (4.0)	35.0*** (3.6)	30.0*** (3.8)	
Large loan				0.1 (3.9)	3.6 (2.6)	4.8** (2.4)	
Bank group x treatment period FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry x treatment period FEs	No	No	Yes	Yes	Yes	Yes	Yes
Region x treatment period FEs	No	No	Yes	Yes	Yes	Yes	Yes
Month FEs	No	No	No	No	No	Yes	No
Observations	32,352	32,352	32,352	32,352	31,381	31,381	4,165
$R^2$	0.05	0.07	0.14	0.18	0.19	0.20	0.32
Adjusted $R^2$	0.05	0.06	0.14	0.18	0.19	0.20	0.31

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

*Notes:* In models M1-M6, the treatment group consists of newly eligible loans with PDs between 0.4 % and 1.0 % (inclusive) and loan sizes equal to or above EUR 25,000, and the control group of never-eligible loans (many combinations of LS and PD categories, see text). Model M7 is otherwise as M5, but the treatment and control groups are narrowed down: the treatment group consists of newly eligible loans with PDs between 0.4 % and 1.0 % (inclusive) and loan sizes equal to or above EUR 500,000, and the control group of never-eligible loans with PDs between 1.0 % and 2.0 % (inclusive) and loan sizes equal to or above EUR 500,000. Intervention period is September 2020, which is excluded from the sample. Pre-period consists of a time range from 2019-09 to 2020-08. Post-period consists of a time range from 2020-10 to 2021-09. See Table 4 for more details. Sources: Finnish implementation of Anacredit and authors' calculations.

## 5 Results: Lowering the minimum loan size threshold for collateral eligibility

In this section, we focus on the LS intervention of April 2020. We investigate whether accepting smaller corporate loans as collateral affected their pricing. This provides additional evidence on whether collateral eligibility is important for the pricing of credit.

### 5.1 Identification and econometric strategy

Since April 2020, the Bank of Finland began to accept credit claims with nominal amounts in the range of EUR [25 000, 500 000) as collateral. Before the change, the BoF accepted as collateral only credit claims with nominal amounts of at least EUR 500,000. In similar vein as in Section 4, we utilize static and dynamic DiD specifications to compare the interest rate development of newly-eligible loans to always-eligible and never-eligible loans (as defined in Section 3.1).

#### 5.1.1 Static difference-in-differences

To estimate the effect of becoming eligible on loan pricing, we utilize a very similar static DiD specification as in Equation 1:

$$\begin{aligned} Rate_{lfbt} = & \alpha_1 \mathbb{1}_{LS-T} + \alpha_2 Post_{t \geq 4/2020} + \beta \mathbb{1}_{LS-T} \times Post_{t \geq 4/2020} \\ & + \Phi_{xt} + \gamma_1 X_{lt} + \gamma_2 Z_{ft} + \varepsilon_{lfbt}, \end{aligned} \tag{3}$$

where dummy  $\mathbb{1}_{LS-T}$  is equal to 1 if the loan is in the LS treatment group (otherwise zero). Dummy  $Post_{t \geq 4/2020}$  is equal to 1 from April 2020 onwards (this term might be redundant depending on the chosen time fixed effects). Other variables, clustering of standard errors, and intervention month exclusion are as described in relation to Equation (1).

As in the previous section, our coefficient of interest in Equation 3 is the DiD estimate  $\beta$  that measures the differential loan pricing between loans of different nominal amounts,

and therefore the differential pricing due to becoming eligible as central bank collateral. We expect a negative  $\beta$  if a eligibility discount in loan pricing exists.

The main assumptions behind the DiD design are again 1) no-anticipation, 2) non-effect of the treatment on control groups, and 3) parallel trends. Regarding the first assumption, and similarly to the ACC intervention, the LS intervention became as a surprise to market participants, so we argue that the assumption is fulfilled.<sup>34</sup> Regarding the second assumption, we again assume no effect from the treatment on the liquidity premia of *all* loans (see footnote 1).

Regarding the parallel trends assumption, a particular worry for its validity is that the demand and supply for differently sized loans may evolve differently if their typical riskiness is different and there are changes in the risk premia due to, for example, a COVID-19 shock. As a result, it could seem as if a loan pricing change across loan size groups would be driven by the collateral rule change, even though the actual driver would be risk premia changes induced by the COVID-19 shock. Our use of two control groups—both smaller (never-eligible) as well as larger (always-eligible) loans—should eliminate this concern. If the collateral rule change had an effect on interest rates, we would expect similar effect estimates across both control groups.

We further aim to alleviate the risk of confounding by using varying sets of control variables and fixed effects. In Table 6 (treatment vs. always-eligible) we estimate six models ( $M1-M3$  and  $M5-M7$ ). In Table 7 (treatment vs. never-eligible) we estimate eight models ( $M1-M4$ ,  $M5a$ ,  $M5b$  and  $M6-M7$ ). Models  $M1-M3$  are the same in both tables and are analogous to models with the same names in Section 4.1. We include various fixed effects essentially for the same reason as with ACC regressions. Bank group-treatment period fixed effects aim to control for bank-specific idiosyncrasies with time-varying effects (shocks that affect bank’s credit supply). In turn, debtor industry-treatment period and debtor region-treatment period fixed effects aim to control for debtor specific time-variant shock to credit

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<sup>34</sup>Simultaneously, the other changes in collateral rules (see Section 2.2) should not have heterogeneous effects on loans of different sizes.

demand or supply.

In the next table columns we add more controls and/or restrict the sample. Loan size categories and PD categories differ across the treatment and control groups. The treatment group only has loans with PDs less or equal to 0.4% (and loan sizes in the range of EUR [25 000, 500 000)). On the other hand, the never-eligible control group has loans with PDs in the range of (0.4%, 1.0%] (and sizes below EUR 25,000). These loans in the never-eligible group would have become eligible in September 2020 (ACC intervention) had it been for their PD category alone. In order to control for the possible interference of the ACC intervention via loans in this particular sub-group that were close to being eligible also by their size (see Footnote 26), we add a loan-level control variable of PD category (interacted with treatment period) in column  $M_4$  of Table 7. In contrast, Table 6 does not include the PD category as a control (and hence no  $M_4$  at all) as the treated and always-eligible groups have loans only from the same PD category ( $\leq 0.4\%$ ). We interact the PD category control with the treatment period because it became evident in our analysis that some banks have changed the interest rates they charge for loans of similar PDs over pre/post periods (i.e., altered their required risk premia). Specifically, the risk premia for loans with higher PDs have increased more than risk premia for loans with lower PDs, due to which this effect is not captured simply by using bank group  $\times$  treatment period fixed effects.<sup>35</sup>

Our preferred model is again  $M5$ <sup>36</sup>, where we additionally control for firm riskiness (proxied by debtor size and age).<sup>37</sup> In particular, we expect firm size to affect loan sizes (larger firms take larger loans), so that firm riskiness can act as a confounder. In Table 7, we present two versions of  $M5$ :  $M5a$  and  $M5b$ . The former is our preferred model, whereas the latter showcases how omitting the treatment period interaction from the PD category fixed effects would result in a very different outcome (see above the discussion related to  $M_4$ ).

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<sup>35</sup>That is, we have a confounder with a time-varying effect.

<sup>36</sup>Note that  $M5$  in Table 6 does not have PD category fixed effects since both the treatment and control group have loans only from the same PD category.

<sup>37</sup>Figure A3 in the Appendix demonstrates our preferred specifications  $M5/M5a$  as a directed acyclical graph (DAG).

As in Section 4.1, we include additional control variables in model *M6* to match more closely the baseline specification of our reference paper [Mésonnier, O'Donnell, and Toutain \(2022\)](#). Further, *M7* uses a 'strict' sample where only loans with PDs less or equal to 0.4% are kept (see panel (b) in Table 2 for an illustration).<sup>38</sup>

We present evidence supporting the validity of the parallel trends assumption in Section 5.2, where we discuss results from the dynamic DiD regression.<sup>39</sup>

### 5.1.2 Dynamic difference-in-differences

As with the ACC regressions in Section 4.1.2, we also run a dynamic DiD specification akin to model *M5/M5a* at a quarterly frequency:

$$Rate_{lft} = \alpha_1 \mathbb{1}_{ACC-T} + \Phi_{xt} + \sum_{r=-4, r \neq -1}^4 \beta_r \mathbb{1}_{LS-T} \times [R_{l,t} = r] + \gamma_1 X_{lt} + \gamma_2 Z_{ft} + \varepsilon_{lft}, \quad (4)$$

where 2020-Q2 is the first treatment quarter and  $\mathbb{1}_{LS-T} \times [R_{l,t} = r]$  are the dynamic interaction dummies that equal 1 for the treatment group observations reported at the relative quarter  $r$ . The last full pre-treatment quarter 2020-Q1 is selected as the reference quarter. Further details are discussed in conjunction with Equation (2).

## 5.2 Regression results

**Always-Eligible control group.** Results from regression (3) with the always-eligible control group are in Table 6. Our baseline model (column *M5*) suggests that interest rates in the treatment group increase 5.0 basis points more than in the control group due to the collateral policy change, but the standard error is large (3.3 bps), so the estimate is not statistically significant. The 95 % confidence interval for the estimate is (-1.5, 11.5) bps,

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<sup>38</sup>Beside the treatment and control group definitions, *M7* is equivalent to *M5*. However, the PD category control drops from the regression as the treatment group and the never-eligible control group consist of loans of the same PD category. The always-eligible control group stays intact. Hence, *M7* does not appear in Table 6.

<sup>39</sup>We note that visual inspection of pre-trends in the aggregate data makes it plausible that the parallel trends assumption holds (see Section 3.3).



meaning that we can confidently rule out only effects larger than that—and cannot reject the null hypothesis that the intervention does not affect credit pricing ( $\beta = 0$ ).

Figure 8 shows the results from the dynamic DiD (Equation (2) with control variables as in *M5*). We make similar observations as in the ACC case. First, conditional on our control variables, the parallel trends assumption seems to hold.<sup>40</sup> Second, the LS intervention in April 2020 did not significantly affect the interest rate differential between newly-eligible and already-eligible loans (zero is not outside the 95 % confidence interval in any post-period quarter). Thus, the static and dynamic DiD support the same conclusion.

Other columns than *M5* in Table 6 present results from the other regression models. The estimates of  $\beta$  are positive but statistically insignificant in specifications *M1–M3*. In column *M6*, the positive estimate turns statistically significant at 10 % risk level. If becoming eligible lowered the liquidity premia and thus the interest rates, we would expect a negative estimate. Our takeaway is that there is no evidence of collateral eligibility having a significant effect on loan pricing.

**Never-Eligible control group.** Results with the alternative control group of never-eligible loans are presented in Table 7. In our baseline model *M5a*, the estimate of the eligibility discount is 2.9 basis points, but is far from being statistically non-different from zero as standard errors are large (with the 95 % confidence interval from -15.3 to 9.4 bps).

The dynamic DiD results for the never-eligible control group are shown in Figure 9 (Equation (2) with controls as in *M5a*). Albeit the estimates being somewhat more volatile, our take is the same as with the always-eligible control group: the parallel trend assumption seems to hold, and the intervention in April 2020 did not significantly affect interest rates between newly-eligible and never-eligible loans.

Other columns than *M5* in Table 7 show results of alternative models for robustness. We want to specifically point out column *M5b*. It shows that if we did not interact the PD category control with the treatment period, the results would indicate a negative DiD

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<sup>40</sup>Again, the pre-trends are more volatile if we do not include controls, and the estimates are more volatile if we estimate the dynamic DiD at monthly frequency instead of quarterly frequency.

estimate and thus provide evidence for an eligibility discount. However, as discussed in the previous subsection, this observation is due to the increase in the risk premia charged by some banks in our sample. As loan sizes are negatively correlated with the risk premia, interest rates in the control group (with smaller loans and thus higher PD values) increase more than in the treatment group when risk premia increase. The estimate is close to zero and insignificant when we control for the change in risk premia over time (as in model *M5a*). Additional controls (*M6*) and restricting the sample (*M7*) result in slightly more negative estimates but without statistical significance.

As with the ACC regressions in Section 4, we would like to restrict our sample to the banks that actually use credit claims as collateral, but cannot publish the results due to confidentiality concerns.<sup>41</sup>

All in all, we again find no evidence that accepting smaller loans as collateral has significantly affected the pricing of newly-eligible loans. As in Section 4, standard errors are quite high and we can confidently rule out only effects that are outside the 95 % confidence interval—(-1.5, 11.5) bps for the always-eligible control group and (-15.3, 9.4) bps for the never-eligible control group.

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<sup>41</sup>We again ran the baseline model of this section also using the restricted bank sample. In these unpublished results, we did not find evidence of a significant collateral eligibility discount.

**Table 6:** LS regression, treatment vs. always-eligible control group

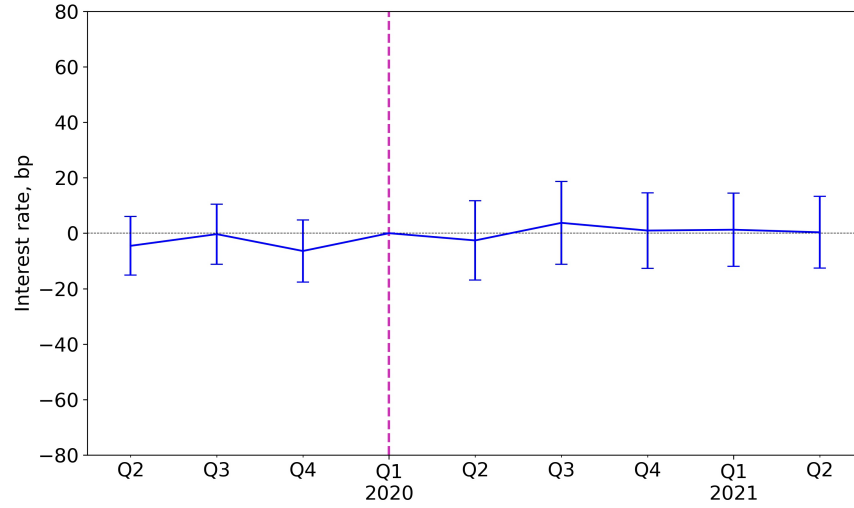
	<i>Dependent variable: interestrate</i>				
	M1	M2	M3	M5	M6
Intercept	115.4*** (2.3)				
Post	0.1 (2.8)				
Treatment	18.3*** (4.2)	21.5*** (5.3)	15.3*** (3.6)	4.7* (2.6)	3.2 (2.6)
Post x Treatment	7.6 (4.7)	4.3 (5.7)	5.6 (4.0)	5.0 (3.3)	5.8* (3.2)
Maturity					-0.8*** (0.1)
Protected					10.8*** (3.2)
Debtor age				-0.4*** (0.0)	-0.4*** (0.0)
Medium debtor				33.4*** (4.2)	31.8*** (4.5)
Small debtor				48.0*** (4.5)	46.5*** (4.7)
Micro debtor				55.0*** (3.9)	55.0*** (4.3)
Bank group x treatment period FEs	No	Yes	Yes	Yes	Yes
Industry x treatment period FEs	No	No	Yes	Yes	Yes
Region x treatment period FEs	No	No	Yes	Yes	Yes
Month FEs	No	No	No	No	Yes
Observations	11,181	11,181	11,181	11,116	11,116
$R^2$	0.03	0.05	0.14	0.23	0.24
Adjusted $R^2$	0.03	0.05	0.14	0.22	0.23

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

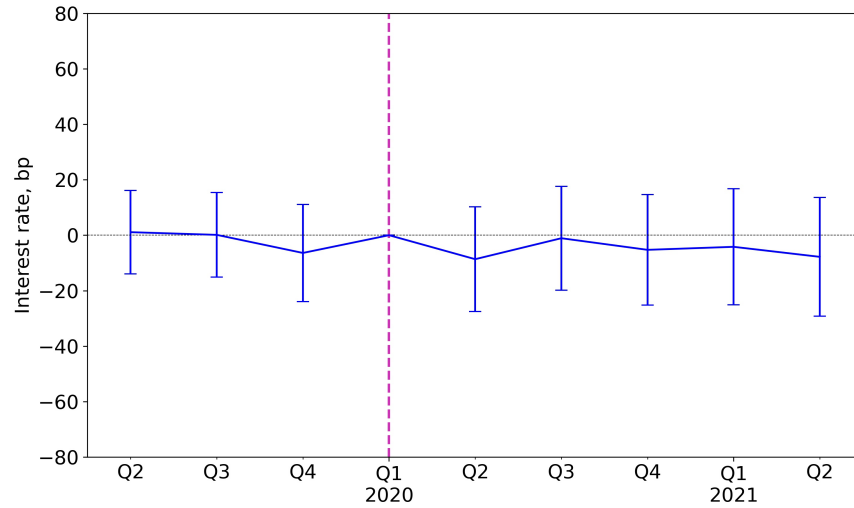
*Notes:* In models M1-M6, the treatment group consists of newly eligible loans with PDs equal to or less than 0.4 and loan sizes between EUR 25,000 (inclusive) and 500,000, and the control group of always-eligible loans with PDs equal to or less than 0.4 % and loan sizes equal to or above EUR 500,000. Intervention period is April 2020, which is excluded from the sample. Pre-period consists of a time range from 2019-04 to 2020-03. Post-period consists of a time range from 2020-05 to 2021-04. In this specification, M4 coincides with M3 and is thus left out. See Table 4 for more details. Sources: Luoti/Anacredit and authors' calculations.

**Figure 8:** LS dynamic DiD, treatment vs. always-eligible control group



*Notes:* The figure displays results from the dynamic DiD model at a quarterly frequency. The model specification is similar to the static DiD regression M5, except that standard error clustering is at the bank group x quarter level. The purple vertical line denotes the last full quarter before the intervention quarter. Vertical bars denote the 95 % confidence intervals. Sources: Finnish implementation of Anacredit and authors' calculations.

**Figure 9:** LS dynamic DiD, treatment vs. never-eligible control group



*Notes:* The figure displays results from the dynamic DiD model at a quarterly frequency. The model specification is similar to the static DiD regression M5a, except that standard error clustering is at the bank group x quarter level. The purple vertical line denotes the last full quarter before the intervention quarter. Vertical bars denote the 95 % confidence intervals. Sources: Finnish implementation of Anacredit and authors' calculations.

**Table 7:** LS regression, treatment vs. never-eligible control group

	<i>Dependent variable: interestrate</i>							
	M1	M2	M3	M4	M5a	M5b	M6	M7
Intercept	238.2*** (6.2)							
Post	19.3*** (6.5)							
Treatment	-104.5*** (4.9)	-113.0*** (6.1)	-87.0*** (7.4)	-43.2*** (6.3)	-33.1*** (4.2)	-30.9*** (3.6)	-24.0*** (4.2)	-23.0*** (3.6)
Post x Treatment	-11.6** (5.4)	-3.4 (6.6)	-8.7 (7.9)	-2.8 (7.7)	-2.9 (6.2)	-8.2** (3.3)	-4.7 (6.4)	-8.3 (5.3)
Maturity							-2.3*** (0.2)	
Protected							8.9*** (2.1)	
Debtor age					-0.6*** (0.0)	-0.6*** (0.0)	-0.6*** (0.0)	-0.5*** (0.0)
Medium debtor					13.8** (6.7)	13.7** (6.7)	13.9** (6.6)	54.7*** (7.0)
Small debtor					38.5*** (6.7)	38.5*** (6.7)	38.9*** (6.5)	75.7*** (6.7)
Micro debtor					60.9*** (7.0)	61.0*** (7.0)	62.7*** (6.8)	83.1*** (6.3)
Low PD						-39.5*** (3.8)		
High PD						-10.6*** (3.8)		
Low PD x Pre				-58.5*** (7.7)	-33.1*** (4.8)		-35.4*** (4.9)	
Low PD x Post				75.3*** (5.6)	69.7*** (5.0)		57.3*** (4.9)	
Med PD x Post				150.9*** (4.3)	116.8*** (4.0)		106.3*** (4.0)	
High PD x Pre				-8.5 (6.3)	-6.0 (5.4)		-1.3 (5.4)	
High PD x Post				127.2*** (3.0)	101.4*** (4.0)		94.9*** (4.0)	
Bank group x treatment period FEs	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x treatment period FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Region x treatment period FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Month FEs	No	No	No	No	No	No	Yes	No
Observations	29,350	29,350	29,350	29,350	28,376	28,376	28,376	9,530
$R^2$	0.14	0.16	0.24	0.25	0.29	0.29	0.30	0.25
Adjusted $R^2$	0.14	0.16	0.23	0.24	0.29	0.29	0.30	0.25

*Note:*

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

*Notes:* In models M1-M6, the treatment group consists of newly eligible loans with PDs equal to or less than 0.4 % and loan sizes between EUR 25,000 (inclusive) and 500,000, and the control group of never-eligible loans (many combinations of LS and PD categories, see text). Model M7 is otherwise as M5a, but the control group is narrowed down: it consists of never-eligible loans with PDs equal to or less than 0.4 % and loan sizes less than EUR 25,000. Intervention period is April 2020, which is excluded from the sample. Pre-period consists of a time range from 2019-04 to 2020-03. Post-period consists of a time range from 2020-05 to 2021-04. See Table 4 for more details. Sources: Finnish implementation of Anacredit and authors' calculations.

## 6 Discussion and conclusion

In this paper, we study if expansions of the central bank collateral framework affect credit pricing. We utilize two changes in the Bank of Finland’s collateral policy in 2020 for identification in a difference-in-differences setting.

Our main result is that while the changes allowed banks to pledge both riskier and smaller corporate loans as collateral for central bank borrowing, neither of these changes significantly affected the banks’ pricing of such loans compared to loans that were 1) already eligible or 2) never became eligible. Therefore, we do not find significant statistical evidence that supports the presence of an eligibility discount on corporate loans. We can confidently rule out effects being larger than around 10 basis points. Our result contrasts with earlier findings in the literature that have found collateral eligibility to have a decreasing effect on bank credit pricing. In particular, effects of the magnitude of 8 and 12 basis points—depending on the control group choice—found by [Mésonnier, O’Donnell, and Toutain \(2022\)](#) are outside the 95 % confidence intervals in our respective specifications. There are several possible reasons for our contradicting findings (discussed also in [Section 2](#)).

First, focusing on [Mésonnier, O’Donnell, and Toutain \(2022\)](#) and [Cahn, Duquerroy, and Mullins \(2022\)](#), an important difference between Finland in 2020 and France in 2012 is the state of the economy and the banking system. Finnish banks were overall in relatively good shape in 2020 in terms of their capital and liquidity position and did not actively borrow from the central bank. In contrast, during the eurozone crisis in 2011–2012, French bank lending was somewhat sluggish and at least somewhat constrained by liquidity and collateral needs of the banking system, which likely contributed to the presence of an eligibility discount at that time. There are also several important differences in the practical implementation of collateral policies between the Bank of Finland and Banque de France, as discussed in [Section 2](#). These make the use of credit claims as collateral less common and more costly (in terms of both time and money) in Finland than in France. As a result of these reasons, compared to the Finnish case in 2020, the French banking system in 2012 was 1) in more

need of central bank borrowing and collateral and 2) in a better position to fully utilize the newly-adopted ACC collateral framework. This may explain why our results contrast with findings from France.

Second, compared to [Van Bakkum, Gabarro, and Irani \(2018\)](#) who find an increase in the supply of credit in the Netherlands after lower-rated RMBS became eligible collateral, our paper focuses on whether the pricing of corporate loans was affected by the change in eligibility. Importantly, the main mechanism behind the additional risk taking by banks found in [Van Bakkum, Gabarro, and Irani](#) was driven by the subsequent securitization of the newly-granted mortgage loans. Thus, banks were able to either 1) use RMBS as collateral with lower fixed costs, if RMBS were retained on their balance sheet, or 2) offload the risk from their balance sheet by selling RMBS. In our case, the eligibility change affected Finnish corporate loans for which no active securitization market exist.

We note that even though we find no significant effect of the collateral rule changes on the pricing of newly-eligible loans, we cannot conclude that the policy actions were necessarily ineffective. First, it is theoretically possible that the larger collateral pool increased the liquidity of bank balance sheets and eased credit pricing equally to *all* firms and loan types. However, this cannot be identified easily due to the absence of a control group. Second, as a part of a broader policy package, the rule changes may have eased uncertainty around the economic outlook as well as banks' funding prospects and therefore supporting credit supply overall.

Even if the collateral policy changes had no effect on credit pricing, they were not necessarily bad policy decisions. When the rule changes were introduced in April and September 2020, there was considerable uncertainty around the economic outlook due to the pandemic. Thus, it was probably wise to look for various alternative and ex ante not very costly policies to prevent bank lending from restricting firm activity, even if the policy would turn out to be not necessary ex post. If the economy had collapsed or bank collateral needs increased substantially, the policy changes might have had more significant effects. From this perspective,

the collateral policy change can be seen as a risk management measure.



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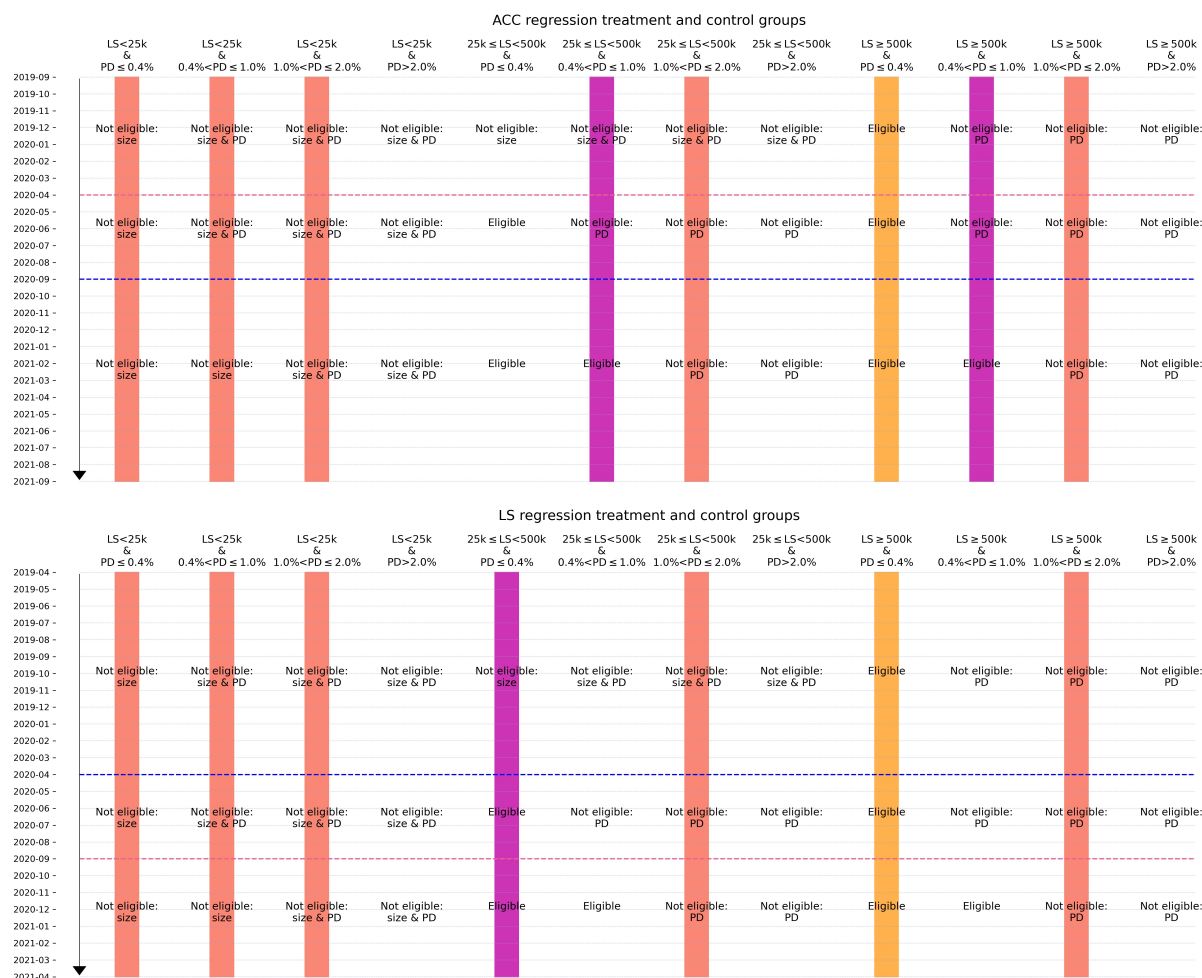
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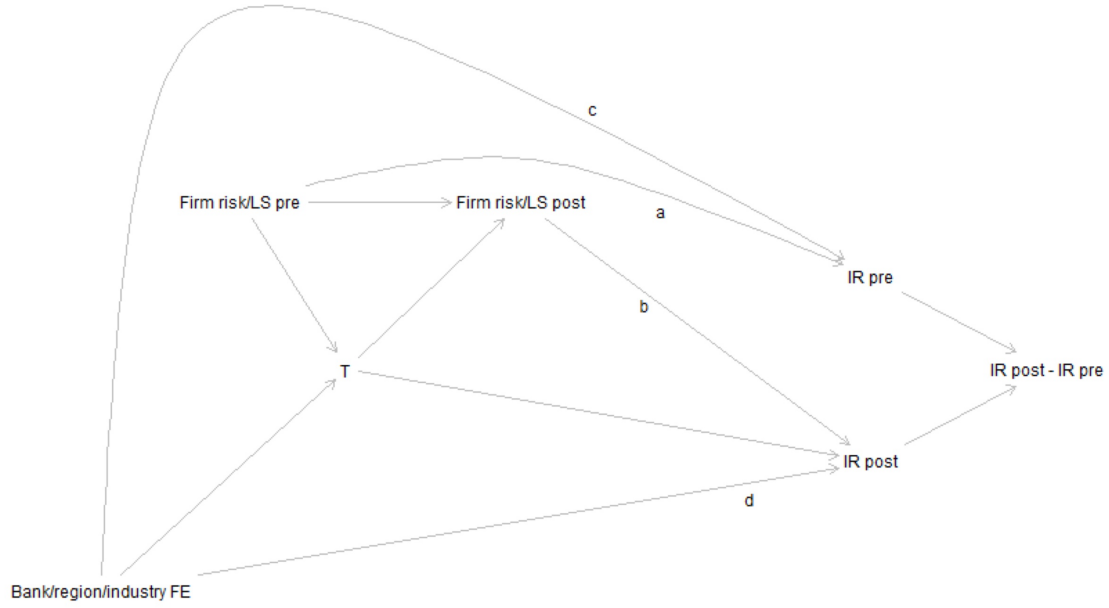
## Appendix A Additional Figures and Tables

**Figure A1:** Detailed illustration of the treatment and control groups



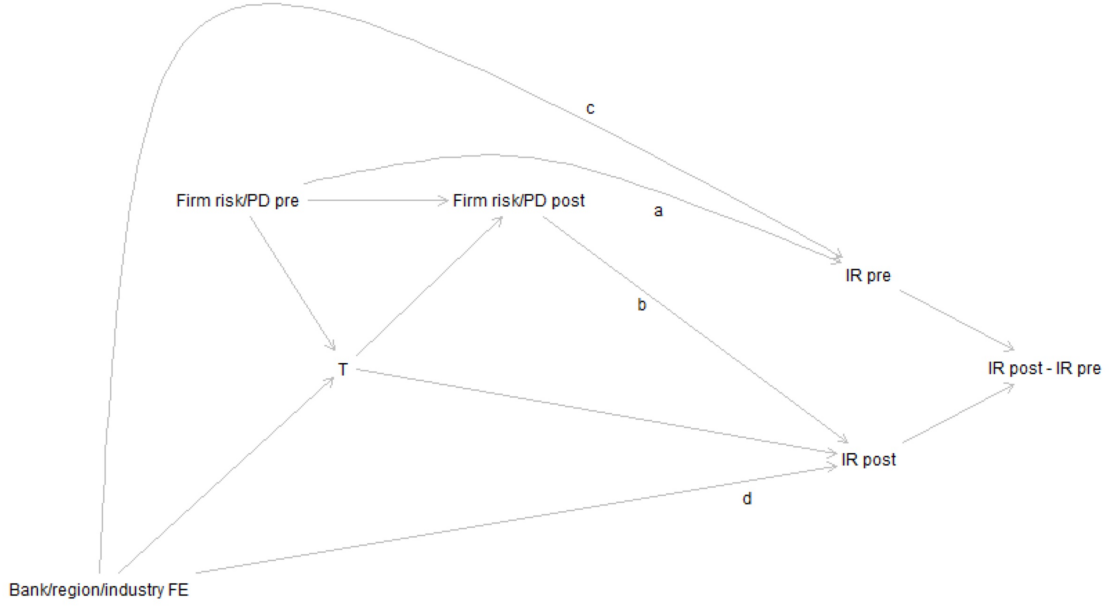
*Notes:* A detailed illustration of how the treatment and control groups are formed from combinations of loan size and PD categories, as well as over which months the samples span. The top (bottom) panel presents the case for ACC (LS) regressions. The blue horizontal dashed line indicates the corresponding intervention period, whereas the pink dashed line indicates the intervention period for the other set of regressions. Labels designate whether a given combination of loan size and PD categories was eligible before the LS intervention of April 2020, between the LS and ACC interventions, and after the ACC intervention of September 2020. Colored rectangles represent regression groups: treatment group (purple), always-eligible control group (orange), and never-eligible control group (peach).

**Figure A2:** Directed Acyclic Graph for the baseline ACC regression model



*Notes:* The figure illustrates posited causal effects between variables in the ACC regression case as a Directed Acyclic Graph (DAG). Covariate selection in the baseline model M5 is consistent with the DAG (see the main text). The specification is the same for both always-eligible and never-eligible control group cases. Treatment assignment 'T' is to be understood to encompass the relationship 'PD group' → 'Eligibility'. We allow fixed effects to have varying effects ( $c \neq d$ , separately for each fixed effect). For the time-varying covariates – firm riskiness as proxied via debtor size category and firm age, as well as loan size (LS) – we assume time-invariant effects ( $a = b$ ). 'IR' refers to interest rate and 'pre' and 'post' to pre- and post-intervention periods, respectively.

**Figure A3:** Directed Acyclic Graph for the baseline LS regression model



*Notes:* The figure illustrates posited causal effects between variables in the LS regression case as a Directed Acyclic Graph (DAG). Covariate selections in the baseline models M5 (always-eligible control group) and M5a (never-eligible control group) are consistent with the DAG (see the main text). Treatment assignment 'T' is to be understood to encompass the relationship 'LS group'  $\rightarrow$  'Eligibility'. We allow fixed effects to have varying effects ( $c \neq d$ , separately for each fixed effect). For firm riskiness, we assume time-constant effects ( $a = b$ ). For PD category, we assume time-invariant effects ( $a = b$ ) with the always-eligible control group and time-varying effects ( $a \neq b$ ) with the never-eligible control group. 'IR' refers to interest rate and 'pre' and 'post' to pre- and post-intervention periods, respectively.

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