The Loan Renegotiation Channel of Quantitative Easing

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Abstract

We study a channel of quantitative easing (QE) that operates through the renegotiation of existing bank loans. Matching syndicated-loan amendment histories to bank balance sheets, we find that larger purchases of agency mortgage-backed securities (MBS) are associated with more renegotiation at exposed banks, with the increase concentrated in amendments that favor lenders. Relative to other loans of the same firm in the same quarter, a doubling of quarterly purchases is associated with committed amounts 2.5 percent lower, spreads 10.6 basis points higher, and maturities 3.1 percent shorter on exposed banks' amendments. The response is present among borrowers in covenant compliance and no stronger among those in violation, distinguishing it from renegotiation driven by creditor control rights in technical default. It is concentrated among well-capitalized, well-provisioned, and profitable banks and among less profitable, lower-valuation, but not highly leveraged borrowers, consistent with a bargaining channel in which financially strong lenders extract larger concessions from borrowers with weaker outside options. A shift-share design indicates that a ten-percentage-point increase in lender-favorable renegotiation lowers firms' annual investment rate by 6 percent of its mean. QE thus tightened existing corporate credit for borrowers of exposed banks, potentially offsetting part of its expansionary effects on new lending.

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1 Introduction

Large-scale asset purchases have been a central instrument of monetary policy since 2008, and banks play an important role in transmitting them to the real economy. A large literature shows that banks more exposed to purchased assets change their lending, expanding mortgage credit and, in some settings, reallocating credit away from commercial borrowers (Rodnyansky & Darmouni, 2017; Di Maggio et al., 2020; Chakraborty et al., 2020). This evidence focuses on the origination of new loans. Yet most corporate borrowing at any point in time is already outstanding, and the terms of outstanding bank loans can change before maturity. Renegotiation is pervasive, more than 90 percent of long-term credit agreements are amended before maturity, with material changes to loan amounts, pricing, and maturity (Roberts & Sufi, 2009; Roberts, 2015). The stock of outstanding credit therefore provides another margin through which monetary policy can reach firms, including those that do not seek new financing. Whether quantitative easing (QE) operates through this margin, and in which direction, remains an open question. If purchases relax exposed banks' lending constraints, they may accommodate existing borrowers on easier terms. If purchases instead raise the opportunity cost of the balance sheet committed to corporate lending, exposed banks may have an incentive to reprice or reduce existing credit. This paper examines how QE affects the terms of outstanding bank loans through renegotiation, why the response takes its observed form, and whether it affects firms' investment.

We study syndicated loans around the Federal Reserve's purchases of agency mortgage-backed securities (MBS) from 2006 to 2024. Combining DealScan amendment histories for loan tranches with the balance sheets of their lead lenders and borrower fundamentals, we exploit variation in quarterly MBS purchases and banks' predetermined holdings of these securities. Figure 1 shows that MBS purchases varied substantially across several episodes and did not move in lockstep with Treasury purchases, motivating our focus on bank exposure to MBS rather than to asset purchases more generally. Our baseline design compares renegotiation outcomes across loans associated with banks of different levels MBS exposure as purchase intensity changes. For contractual terms, we compare amendments to loans of the same firm in the same quarter across lenders with different exposure, absorbing borrower-specific shocks to credit demand and credit quality (Khwaja & Mian, 2008). Bank fixed effects absorb persistent differences in lenders' renegotiation practices, and exposure is measured before the relevant loan events. The design thus isolates the differential response of exposed lenders rather than attributing common changes in borrower conditions to QE.

We find that QE tightened the terms of existing corporate credit. As Federal Reserve purchases increase, renegotiation at exposed banks becomes more frequent, with

the additional amendments concentrated in lender-favorable changes. A doubling of quarterly MBS purchases raises the probability of a lender-favorable amendment by approximately 2.2 percentage points relative to other banks' loans to the same firm, with no statistically significant increase in amendments favorable to the borrower. The contractual terms move in the same direction. Relative to other banks' amendments for the same firm in the same quarter, a doubling of purchases is associated with committed amounts that are 2.5 percent lower, spreads that are 10.6 basis points higher, and maturities that are 3.1 percent shorter. The joint movement of quantities and prices is consistent with a contraction in the supply of existing credit by exposed lenders, rather than a decline in borrower demand alone.

A natural alternative explanation is that these changes reflect covenant enforcement rather than a distinct response to QE. Covenant violations can activate creditor control rights, allowing lenders to reduce commitments and impose less favorable terms on borrowers in technical default (Chava & Roberts, 2008; Nini et al., 2012). This possibility is especially relevant given evidence that lender health affects the exercise of covenant rights (Chodorow-Reich & Falato, 2022) and that banks with greater monetary policy exposure write stricter covenants ex ante to preserve the option to cut commitments during future tightening (Degerli & Wang, 2026). We test this alternative by allowing the QE response to differ between borrowers in violation and those in compliance. Among compliant borrowers, the estimated response is statistically significant and larger than the full-sample estimate. The additional response among borrowers in violation is negative and statistically insignificant, and its confidence interval rules out a materially stronger response among violating borrowers. The findings therefore distinguish our renegotiation channel from one in which changes in lender conditions are transmitted primarily through the exercise of contractual control rights following a breach.

The cross-sectional evidence further clarifies the economic mechanism. On the lender side, the response is significantly stronger among banks with higher capital ratios, larger loan-loss provisions, and greater profitability. This pattern runs counter to an account in which financially constrained banks are forced to contract existing credit, and is consistent with financially stronger banks being better positioned to act on the incentives created by purchases. Pressing borrowers to accept smaller commitments, higher spreads, or shorter maturities can jeopardize the lending relationship or reduce the value of the lender's claim; a well-capitalized and profitable bank is better positioned to bear these costs (Peek & Rosengren, 2005). On the borrower side, the response is stronger among firms with lower profitability and market valuations, consistent with exposed banks obtaining larger concessions from firms with fewer in-

ternal resources and less attractive financing alternatives (Rajan, 1992). The response also declines with book leverage. Although highly leveraged firms may have weak outside options, further tightening can jeopardize repayment and reduce the value of the lender's claim. Taken together, these patterns support a bargaining interpretation, purchases change the incentives of exposed banks to renegotiate, financial strength enables them to pursue concessions, and borrower outside options and repayment risk shape the terms they can obtain. The evidence is consistent with this mechanism but does not identify bargaining power as the unique explanation for the observed heterogeneity.

We next examine whether this renegotiation margin affects firms' real decisions. Lender-favorable amendments may redistribute surplus without changing investment if borrowers can readily replace the credit through other banks, capital markets, or internal funds. We therefore aggregate the loan-level data to the firm-quarter level and instrument lender-favorable renegotiation using a shift-share measure that combines predetermined borrowing relationships with the exposure of each lender to MBS purchases, following the relationship-based approach of Chodorow-Reich (2014). The instrument predicts renegotiation strongly, with a first-stage F-statistic of 18.4. The estimated investment response is negative and statistically significant, a ten-percentage-point increase in a firm's lender-favorable renegotiation rate reduces its investment rate in the following quarter by approximately 0.34 percentage points of installed capital, about six percent of the sample mean quarterly investment rate. The estimate remains stable when we control for a firm-level measure of new credit supply from exposed lenders, which itself enters insignificantly, and is robust to alternative relationship weights and additional controls. These results support the view that renegotiation of outstanding credit is an economically important margin of QE transmission.

Our paper contributes to three strands of literature. First, it contributes to the literature on the transmission of QE through bank lending (Rodnyansky & Darmouni, 2017; Di Maggio et al., 2020; Chakraborty et al., 2020; Luck & Zimmermann, 2020; Kandrac & Schlusche, 2021). This work shows that asset purchases affect the volume and terms of new credit, including by reallocating lending between mortgage and commercial borrowers. We identify an additional margin, purchases also change the terms of credit already extended. Our results complement Chakraborty et al. (2020). Exposed banks not only reallocate new lending away from commercial credit, but also shrink, reprice, and shorten corporate loans they have already made. They do so outside covenant violations, and the response is strongest among financially stronger banks. These findings do not overturn the evidence that QE can stimulate new lending; they show that originations alone provide an incomplete account of how asset purchases affect firms'

financing conditions. Using a structurally estimated model of bank intermediation, Diamond et al. (2024) show that reserves created by QE can raise banks' marginal lending costs and crowd out corporate credit. We complement their analysis by documenting a contractual margin through which QE-related tightening reaches firms. Exposed banks renegotiate the terms of outstanding corporate loans in their favor. Our evidence shows that this adjustment occurs even among covenant-compliant borrowers and reduces borrower investment, connecting changes in existing credit agreements to the real effects of asset purchases.

Second, our paper contributes to research on monetary policy transmission through outstanding debt and on private-credit renegotiation. Changes in policy rates pass through to floating-rate loans and affect firms' cash flows (Ippolito et al., 2018), while covenant violations transmit lender-side conditions through the exercise of creditor control rights (Chodorow-Reich & Falato, 2022). Banks may also design stricter covenants ex ante to preserve flexibility during future monetary tightening (Degerli & Wang, 2026). In related work, Lee (2023) shows that expansionary monetary policy increases corporate borrowing through loan renegotiation and that renegotiating firms account for a substantial share of the subsequent investment response. We complement this evidence by exploiting banks' predetermined MBS holdings and variation in Federal Reserve MBS purchases to document a contractionary response of outstanding corporate credit, more exposed lenders reduce commitments, raise spreads, and shorten maturities when renegotiating existing loans. This tightening occurs even among covenant-compliant borrowers, is concentrated among financially stronger lenders, and reduces borrower investment, revealing a lender-side renegotiation channel through which asset purchases can work against their intended stimulus for affected firms.

Third, our paper contributes to the literature on the real effects of bank credit supply (Khwaja & Mian, 2008; Chodorow-Reich, 2014; Huber, 2018). This literature shows that shocks to lenders affect borrowers' investment and employment when external finance is costly to replace. Such shocks typically impair lenders and reach firms through reduced new lending; impaired banks may also extend credit to weak borrowers to avoid recognizing losses (Peek & Rosengren, 2005; Caballero et al., 2008). Our setting differs in that the response is concentrated among financially stronger banks and operates through the renegotiation of existing loans. The investment results show that this contractual margin has real consequences, particularly for borrowers with limited financing alternatives. The estimates identify a contractionary effect associated with differential exposure to QE, not the aggregate net effect of asset purchases, which also includes broader effects on interest rates, asset prices, and new credit.

The remainder of the paper is organized as follows. Section 2 describes the in-

stitutional setting, data, and measurement. Section 3 presents the empirical strategies for the loan-level and firm-level analyses. Section 4 presents the loan-level results, evaluates alternative explanations, and examines the mechanism through bank- and borrower-side heterogeneity. Section 5 studies the investment effects of lender-favorable renegotiation. Section 6 concludes.

2 Institutional Background, Data, and Measurement

2.1 Institutional Background

Syndicated loans are credit facilities arranged by a lead bank and funded by a syndicate of lenders. Once originated, these contracts are frequently renegotiated before their scheduled maturity, with changes recorded in DealScan as amendments. Roberts & Sufi (2009) document that the majority of syndicated loans are amended at least once over their lives, often within the first year after origination. Amendments can change the committed amount, interest rate spread, maturity, and other contractual terms, making renegotiation an important margin through which the terms of existing credit adjust over time.

Amendments are economically distinct from new originations. Whereas a new loan establishes contractual terms at the outset of a lending relationship, an amendment revises an existing agreement after the lender has committed to the relationship and the parties' circumstances may have changed. The borrower's financial condition and the lender's alternative uses of capital can evolve independently of the conditions that prevailed at origination. Renegotiation therefore provides an opportunity to redistribute value between lenders and borrowers, potentially in either direction, without requiring the origination of a new loan.

This margin is particularly relevant for understanding the transmission of quantitative easing (QE). Through large-scale purchases of agency mortgage-backed securities (MBS) and Treasury securities, the Federal Reserve sought to ease financial conditions and influence the prices and yields of longer-term assets. While research on QE has emphasized its effects on financial markets and the supply of new credit, its implications for the contractual terms of *outstanding credit* are less well understood. Because banks entered QE with different MBS holdings, the scale of Federal Reserve purchases generated differential exposure across lenders. We examine whether this exposure affected the terms banks obtained when renegotiating existing syndicated loans.

Our outcomes capture changes in committed amount, spread, and maturity relative to the preceding contractual terms. We classify an amendment as lender-favorable

when it reduces the committed amount or maturity, or increases the interest rate spread. These changes provide a direct measure of how the terms of existing credit shift between lenders and borrowers, allowing us to distinguish the renegotiation channel from changes operating through new loan originations.

Two features of the amendment process are important for identification. First, amendments are active contractual changes, ordinarily requiring the consent prescribed by the loan agreement, rather than passive non-renewal at maturity. We therefore restrict the baseline sample to loans at least twelve months from scheduled maturity.[1] Second, amendments are distinct from covenant violations, which may activate separately specified lender rights and trigger renegotiation for reasons related to borrower distress. An amendment does not, by itself, imply that a covenant has been violated. We examine this alternative explanation directly in Section 4.2.1.

2.2 Data and Sample Construction

We combine five data sources. Loan-level information comes from DealScan, which records syndicated loan tranches at origination and at each subsequent amendment, enabling us to trace the full lifecycle of each loan. We follow Chava & Roberts (2008) to identify lead lenders. Bank balance sheets and income statements come from the quarterly Reports of Condition and Income (Call Reports), with banks matched to DealScan lenders via their RSSD identifier. Firm fundamentals come from the Compustat quarterly file; we exclude financial firms (SIC 6000–6999) and regulated utilities (SIC 4900–4999). We construct the data of covenant violations following Nini et al. (2012) and extend the data to 2023. The fifth source is the Federal Reserve's own operation records, from which we construct quarterly gross MBS purchase volumes following Chakraborty et al. (2020); we describe this measure in detail in Section 3.1.

Sample construction. Our loan-level sample spans 2002 to 2023 and is constructed at the loan-tranche level, with one observation per tranche per amendment round. We impose three restrictions. First, we retain only loans to U.S. non-financial, non-utility borrowers that can be matched to Compustat. Second, we exclude loans denominated in currencies other than U.S. dollars. Third, and most importantly for identification, we restrict the sample to loans with at least twelve months remaining until scheduled maturity, so that any change in contractual terms must originate from an active amendment rather than passive non-renewal; we verify that commitment changes in the retained sample cluster at recorded DealScan amendment dates rather than at scheduled

[1] We verify that commitment changes cluster at recorded amendment dates in DealScan rather than at scheduled maturity dates.

maturity dates. The final loan-level sample comprises 23,472 loan-tranche observations, covering 12,620 unique tranches, 3,088 borrowing firms, and 120 lending banks.

For the investment analysis we collapse the loan-level data to a firm-quarter panel. The firm-level sample is further restricted to firms with at least two distinct pre-QE lending relationships, which is required to construct the shift-share instrument. The firm-quarter panel comprises 13,278 observations covering 2,953 unique firms.

Renegotiation outcomes. Within each loan tranche, we identify amendment rounds as observations in which at least one of the committed amount, the all-in-drawn spread, or the maturity differs from the preceding round. Our outcome variables measure the direction and magnitude of each change, $\Delta Amount$ is the change in log committed amount, $\Delta Spread$ is the change in the all-in-drawn spread in basis points, and $\Delta Maturity$ is the change in log remaining maturity. We sign these outcomes so that a positive value corresponds to a lender-favorable change, a reduction in committed amount, an increase in spread, or a reduction in maturity. At the firm level, $LenderFav_{f,t}$ aggregates these outcomes across the firm's lenders using predetermined relationship weights, as described in Section 3.2.

Control variables. We include a rich set of controls in all specifications. Bank-level controls, measured four quarters prior to the loan event, include the logarithm of total assets, the capital ratio, the loan-to-deposit ratio, deposit cost, and cash and income shares. Loan-level controls include indicators for collateral, covenants, and seniority, and the logarithm of the original loan amount. Firm-level controls, lagged one quarter, include the logarithm of total assets, asset tangibility, book leverage, profitability, and Tobin's q.

QE purchase series. We measure quantitative easing using quarterly gross Federal Reserve purchases of agency MBS. We construct the series using the Federal Reserve Board's Dodd-Frank transaction disclosures for the first purchase program, the New York Fed's quarterly transaction files for 2011–2014, and records of outright TBA purchases thereafter. We validate the series against Federal Reserve holdings data. We use gross rather than net purchases to capture the Maturity Extension Program, in which the Federal Reserve simultaneously purchased long-term Treasuries and sold short-term Treasuries; gross purchases reflect the asset-side expansion relevant to bank balance sheets regardless of the liability-side operations conducted alongside them. Over our sample period the series has 50 active purchase quarters, with a mean volume of approximately $145 billion and a peak of $496 billion in 2020Q2, spanning the 2008–10,

2012–14, and 2020–22 purchase waves and the reinvestment flows between them. Figure 1 plots the full series. We measure banks' QE exposure through their pre-existing MBS holdings rather than Treasury holdings because MBS purchases dominate the variation in total purchases and the two series do not move together; the construction of the exposure variable is described in Section 3.1.

Summary statistics Tables 1 and 2 report summary statistics for the loan-level and firm-level samples. Figure 1 plots the time series of quarterly MBS purchases over the sample period, illustrating the three main purchase waves and the reinvestment flows between them.

3 Empirical Strategy

We estimate how QE affects loan renegotiation and how renegotiation affects firm investment. The first is the effect of QE on the terms of outstanding syndicated loans, does a bank's MBS exposure cause it to renegotiate existing loans in its own favor when the Federal Reserve purchases MBS? The second is the real consequence of that renegotiation, does lender-favorable amendment of existing credit reduce firm investment? Each question requires a distinct design. Section 3.1 describes the loan-level difference-in-differences for the first; Section 3.2 describes the firm-level shift-share IV for the second.

3.1 QE Exposure and Loan Renegotiation

We exploit variation across banks in their pre-existing MBS holdings and over time in the volume of Federal Reserve purchases. The baseline specification is

$$\begin{aligned} y_{i,r,b,f,t} &= \beta_1 \, HighMBS_{b,t-4} + \beta_2 \, HighMBS_{b,t-4} \times MBSPurchases_{t-1} \\ &\quad + \Gamma' X_{i,b,f,t} + \mu_{f,t} + \delta_b + \gamma_t + \varepsilon_{i,r,b,f,t}, \end{aligned} \tag{1}$$

where $y_{i,r,b,f,t}$ denotes a renegotiation outcome for loan tranche i of firm f, at amendment round r, with lead lender b, in quarter t. The indicator $HighMBS_{b,t-4}$ equals one for banks in the top tercile of the MBS-to-assets distribution, measured four quarters before the loan event so that exposure is predetermined with respect to both the amendment and the purchases. $MBSPurchases_{t-1}$ is the logarithm of gross Federal Reserve MBS purchases in the preceding quarter. The vector $X_{i,b,f,t}$ includes bank, loan, and firm controls, including measures of bank capital adequacy, liquidity, and funding conditions, all as defined in Section 2.2. The specification includes firm-by-quarter

fixed effects $\mu_{f,t}$, bank fixed effects δ_b, and quarter fixed effects γ_t, which absorb the main effect of $MBSPurchases_t$. Standard errors are clustered at the bank level, the level at which exposure varies.

The coefficient of interest, β_2, measures how the difference in renegotiation outcomes between loans of exposed and unexposed banks varies with the volume of Federal Reserve purchases. Three features of the design address the main identification concerns. First, firm-by-quarter fixed effects absorb all time-varying borrower characteristics, credit demand, credit quality, investment opportunities, that are common across the firm's lending relationships (Khwaja & Mian, 2008). The within-firm, within-quarter comparison ensures that β_2 is identified from variation across lenders to the same borrower at the same point in time, so that borrower-side shocks cannot account for the estimated differences in contractual terms. Second, bank fixed effects absorb any time-invariant differences in renegotiation practices across lenders, so that β_2 does not simply reflect exposed banks being structurally more inclined to extract favorable terms. Third, the four-quarter lag on $HighMBS_{b,t-4}$ ensures that exposure reflects a portfolio position formed well before the purchase quarter, removing the possibility that banks adjust their MBS holdings in anticipation of renegotiation opportunities.

Two further threats to identification receive explicit attention in Section 4. The first is whether the estimated response reflects active amendments or passive non-renewal at maturity; the twelve-month sample restriction described in Section 2.2 addresses this by construction, and we verify that commitment changes cluster at recorded DealScan amendment dates. The second is whether the response reflects covenant enforcement triggered by borrower distress rather than lender- initiated renegotiation; we directly test this using pre-amendment compliance status, the conditional relationship between borrower performance and renegotiation, and the temporal sequence of events. We additionally examine pre-trends in renegotiation behavior between high- and low-exposure banks before QE episodes to verify the parallel-trends assumption.

3.2 Lender-Favorable Renegotiation and Firm Investment

To estimate real effects we aggregate the loan-level data to a firm- quarter panel and relate investment to the renegotiation of the firm's outstanding loans,

$$Inv_{f,t} \;=\; \theta\, LenderFav_{f,t-1} \;+\; \Phi' X_{f,t-1} \;+\; \alpha_f + \gamma_t + \varepsilon_{f,t}, \tag{2}$$

where $Inv_{f,t}$ is the investment rate of firm f in quarter t, $X_{f,t-1}$ collects lagged firm characteristics, and α_f and γ_t are firm and time fixed effects. The regressor of interest,

$$LenderFav_{f,t} = \sum_b \omega^{\text{pre}}_{f,b} LenderFav_{f,b,t},$$

is the share of the firm's loans amended in the lender's favor in quarter t, averaged across its lenders b with weights $\omega^{\text{pre}}_{f,b}$ equal to each bank's share of the firm's borrowing originated before 2008Q4. OLS does not identify θ. Loan terms are renegotiated as news about the borrower arrives (Roberts & Sufi, 2009), a firm whose prospects deteriorate may both invest less and accept less favorable loan terms; causality may also run in reverse, as a firm scaling back its investment plans may seek less committed credit or shorter maturities. Either force produces a negative association between lender-favorable renegotiation and investment that does not reflect a credit supply effect.

We therefore estimate equation (2) by two-stage least squares, using only the variation in renegotiation that originates on the lender's balance sheet. The instrument aggregates the bank-level treatment to the firm level via predetermined lending relationships,

$$Z_{f,t} = \sum_b \omega^{\text{pre}}_{f,b} HighMBS_{b,t} \times MBSPurchases_t, \tag{3}$$

following Chodorow-Reich (2014) in using pre-existing bank– firm relationships to carry bank-level shocks to the firm level. The instrument is relevant because syndicated lending relationships are persistent and because, as Section 4 documents, exposed banks renegotiate in their own favor when purchases are large. The weights $\omega^{\text{pre}}_{f,b}$ are fixed before November 2008, the date of the first purchase program announcement, so they cannot reflect lender choices made in anticipation of or in response to QE. Bank exposure is measured four quarters in advance, and purchases are set by the Federal Open Market Committee in response to aggregate conditions. The instrument is therefore orthogonal to firm- level investment demand by construction, and firm and time fixed effects remove, respectively, permanent differences between borrowers of MBS-heavy and other banks, and the macroeconomic conditions that prompt purchases. Identification comes from comparing, within a quarter, firms with more and less exposed pre-QE lenders as purchase intensity varies; Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2022) provide the formal conditions for shift- share instruments of this kind.

The exclusion restriction requires that the instrument affects investment only through the renegotiation of existing loans. The loan-level evidence in Section 4 supports this. The within-firm design shows that contractual tightening originates with exposed lenders rather than with borrower-side deterioration, and the response is no stronger for firms

in covenant violation, which rules out the possibility that the instrument captures lender responses to weak borrower performance. The main residual concern is that QE exposure may also affect exposed banks' supply of new credit to the same firms. The direction of this channel is not settled in the literature, QE raises overall lending by MBS-exposed banks (Rodnyansky & Darmouni, 2017) but may crowd out commercial lending (Chakraborty et al., 2020). If exposed banks expand new credit to these firms, the omitted channel raises investment and our estimate of θ is conservative; if they contract it, the estimate overstates the effect of renegotiation alone. We examine this directly in Section 5.3 and show that the investment response is concentrated among firms whose existing loan terms tighten, not among those receiving differential new credit from exposed banks. We therefore interpret θ as the effect of lender-induced tightening that reaches firms through the renegotiation of their outstanding loans, a local average treatment effect for borrowers whose terms respond to their lenders' QE exposure.

4 QE and the Renegotiation of Existing Credit

This section presents loan-level evidence that QE exposure changes the terms on which banks renegotiate outstanding syndicated loans. We first examine the incidence and direction of renegotiation, showing that the increase associated with Federal Reserve MBS purchases is concentrated in amendments favorable to the lender. We then examine the contractual terms set at amendment and show that they shift against borrowers along all three dimensions observed, committed amount, interest rate spread, and maturity. Section 4.2 examines alternative explanations for these findings, while Section 4.3 investigates the economic forces underlying the response.

4.1 Renegotiation Incidence and Loan Terms

Renegotiation incidence. Table 3 reports estimates of equation (1) for the incidence and direction of renegotiation. The dependent variable in column (1) is an indicator equal to one if an amendment round changes the committed amount, spread, or maturity of the tranche. Columns (2) and (3) distinguish renegotiations favorable and unfavorable to the lender, respectively. Each specification is a linear probability model.

The incidence of renegotiation at exposed banks increases with Federal Reserve purchases. The coefficient on the interaction between bank MBS exposure and purchase intensity in column (1) is 0.0184 ($t = 1.71$), significant at the ten-percent level. The coefficient on $HighMBS_{b,t-4}$ alone is statistically insignificant, providing no evi-

dence of a systematic difference in renegotiation incidence unrelated to purchase intensity. The differential response thus emerges as the volume of Federal Reserve purchases increases.

Distinguishing renegotiations by direction reveals that this increase is concentrated in lender-favorable amendments. The interaction coefficient in column (2) is 0.0320 ($t = 2.47$), significant at the five-percent level. By contrast, the corresponding coefficient for lender-unfavorable renegotiation in column (3) is 0.0096 ($t = 0.53$), less than one-third as large and statistically indistinguishable from zero. The additional renegotiation associated with QE purchases is therefore concentrated in amendments that shift contractual terms in the lender's favor, rather than in a symmetric increase in renegotiation benefiting either party. This asymmetry is consistent with a change in the terms lenders obtain from existing borrowers, rather than a response consisting simply of more frequent contractual adjustment.

The economic magnitude is meaningful. A doubling of quarterly purchases (0.69 log points) raises the probability of a lender-favorable amendment at an exposed bank by approximately 2.2 percentage points relative to loans of less-exposed banks, conditional on the specification's controls and fixed effects. Moving from the average active purchase quarter, with gross purchases of approximately $145 billion, to the peak of $496 billion in 2020Q2 (1.23 log points) raises this differential by approximately 3.9 percentage points. The incidence results establish that QE exposure is associated with a greater likelihood of lender-favorable renegotiation. We next examine whether this response is reflected in the contractual terms of amended loans.

[Table 3 here.]

Contractual terms at amendment. Table 4 examines the intensive margin of renegotiation. The dependent variables are changes relative to the preceding contractual round of the same tranche in the logarithm of committed amount, the all-in-drawn spread in basis points, and the logarithm of maturity. These regressions replace firm-by-year fixed effects with firm-by-quarter fixed effects, restricting the comparison to amendments of loans to the same firm in the same quarter across lenders with different QE exposure. Borrower-specific shocks to credit demand and credit quality common to the firm's lending relationships are therefore absorbed by the fixed effects (Khwaja & Mian, 2008).

All three contractual terms move in the lender's favor as Federal Reserve purchases increase. The interaction coefficient is -0.0365 ($t = -2.15$) for committed amount, 15.29 ($t = 3.05$) for the all-in-drawn spread, and -0.0447 ($t = -2.59$) for maturity. These estimates are statistically significant at the five-, one-, and five-percent levels,

respectively. Conditional on the same borrower and quarter, amendments at exposed banks thus involve smaller commitments, higher borrowing costs, and shorter maturities relative to amendments at other banks.

The economic magnitudes are substantial. A doubling of quarterly purchases (0.69 log points) is associated with committed amounts that are approximately 2.5 percent lower, spreads that are 10.6 basis points higher, and maturities that are approximately 3.1 percent shorter at exposed banks, relative to other banks' amendments for the same firm in the same quarter. Moving from the average active purchase quarter to the 2020Q2 peak (1.23 log points) implies corresponding changes of approximately -4.5 percent, $+18.8$ basis points, and -5.5 percent. For context, the mean change in the all-in-drawn spread at amendment is 6.8 basis points, with a standard deviation of 52 basis points. The spread response to a doubling of purchases is therefore approximately one and a half times the mean spread change and one-fifth of its standard deviation.

The coefficient on $HighMBS_{b,t-4}$ alone is negative and statistically significant for spreads (-12.35, $t = -2.71$), but statistically insignificant for committed amounts and maturities. The estimated tightening is therefore associated with the interaction between MBS exposure and purchase intensity, rather than a uniformly more lender-favorable pattern across all three terms at exposed banks. This distinction reinforces the importance of the QE-related variation in the results.

The joint movement of quantities and prices is also informative. A decline in borrower demand could reduce the amount of credit sought, but it does not readily explain why amendments simultaneously feature higher spreads and shorter maturities relative to other loans to the same firm. The combination of lower commitments, higher spreads, and shorter maturities is consistent with a contraction in the supply of credit by exposed lenders. Taken together, Tables 3 and 4 establish the central loan-level result, as Federal Reserve MBS purchases increase, exposed banks renegotiate outstanding syndicated loans on terms that are systematically more favorable to the lender.

[Table 4 here.]

4.2 Alternative Explanations

The baseline design relies on the assumption that, absent QE-induced variation in purchase intensity, loans of more and less exposed banks would not have followed systematically different renegotiation trends. We examine this prediction using a pre-period event-study specification that interacts a high-MBS-exposure indicator, measured four quarters before each QE episode and held fixed, with relative-quarter indi-

cators. The specification uses up to eight pre-episode quarters, excludes periods at or before the end of the preceding QE program, and takes the quarter immediately before each episode as the reference period. It includes episode-specific firm-year, bank, and quarter fixed effects, with standard errors clustered at the bank level. Table 9 presents the results. No individual pre-period coefficient is statistically significant at the five-percent level, although the lender-favorable coefficient six quarters before an episode is significant at the ten-percent level. Joint tests fail to reject that the pre-period coefficients are zero for overall renegotiation ($p = 0.258$) and lender-favorable amendments ($p = 0.240$), but reject this null for lender-unfavorable amendments ($p = 0.018$). Together with the baseline fixed effects, these results provide qualified support for the identifying assumption underlying our main lender-favorable outcome.

A second concern is that observed changes in loan commitments reflect passive non-renewal at scheduled maturity rather than active renegotiation. The baseline sample therefore excludes loans within twelve months of their scheduled maturity. Figure 2 plots the timing of recorded commitment changes relative to the previously scheduled maturity date. The bulk of these changes occurs well before maturity, with only 14.8% falling within the twelve months preceding scheduled expiration. Commitment changes are therefore not predominantly concentrated near loan expiration. This timing pattern, together with the baseline maturity restriction, reduces the concern that the estimated changes reflect passive non-renewal and supports their interpretation as amendments to existing credit agreements.

These checks address the timing and measurement of amendment activity. We next examine a more substantive alternative, whether the lender-favorable response reflects covenant enforcement following borrower breaches rather than a distinct response to QE exposure.

[Table 9 here.]

[Figure 2 here.]

4.2.1 A Comparison with the Covenant Enforcement Channel

A natural alternative interpretation of the preceding results is that they reflect the exercise of creditor control rights following covenant violations, rather than a distinct response to QE. A covenant breach can place a loan in technical default and strengthen the lender's bargaining position, allowing it to reduce commitments, raise borrowing costs, and constrain investment (Chava & Roberts, 2008; Nini et al., 2012). Because these outcomes resemble the contractual changes documented above, our estimates could instead capture renegotiation triggered by borrower breaches. A related concern

is that QE may affect renegotiation only when a violation provides the contractual basis for intervention.

This distinction connects our findings to two studies of the covenant channel in monetary policy transmission. Chodorow-Reich & Falato (2022) show that during the 2008–09 crisis, lenders in poor health were less likely to waive violations and more likely to cut commitments following a breach, transmitting lender distress to existing borrowers through covenant enforcement. Degerli & Wang (2026) document an anticipatory channel, banks more exposed to conventional monetary tightening write stricter covenants ex ante, preserving the option to reduce commitments if borrowers subsequently breach them. Though distinct, both mechanisms rely on covenants to transmit lender-side conditions to outstanding loans. The key question is whether QE-related tightening is similarly contingent on covenant violations or also occurs when borrowers remain in compliance.

We test this by augmenting equation (1) with an indicator for covenant violation, $Violation_{f,t}$, and its interactions with $HighMBS_{b,t-4}$, $MBSPurchases_{t-4}$, and their product. The coefficient on $HighMBS_{b,t-4} \times MBSPurchases_{t-4}$ measures the QE response among compliant borrowers; the triple interaction captures the additional response among borrowers in violation. If covenant enforcement accounts for the baseline results, the response should be absent or materially weaker among compliant borrowers.

Table 5 provides little support for this prediction. Among compliant borrowers, the interaction coefficient is 0.0478 ($t = 2.72$), significant at the one-percent level and larger than the full-sample estimate of 0.0320 in Table 3. The response is clearly present outside technical default. The triple interaction is -0.0410 ($t = -1.54$), negative but statistically insignificant, implying a point estimate of 0.0068 among borrowers in violation, compared with 0.0478 among compliant borrowers. Although the difference is not statistically significant, the confidence interval rules out a positive triple interaction larger than approximately 0.01, around one-fifth of the response among compliant borrowers. The estimates thus provide no evidence that violations amplify QE-related tightening and rule out a materially larger incremental response among borrowers in violation.

[Table 5 here.]

The results distinguish our channel from covenant enforcement as an explanation for the main findings. The tightening does not require a breach to activate lenders' contractual control rights, it also reaches borrowers whose loans remain in compliance. This is consistent with evidence that renegotiation is common outside technical default and can reflect changes in the parties' circumstances and bargaining positions (Roberts & Sufi, 2009). It contrasts with the covenant channel emphasized

by Chodorow-Reich & Falato (2022) and Degerli & Wang (2026), in which breach-contingent control rights are central to transmitting lender-side conditions. Our evidence does not imply that covenant enforcement is unimportant or that violations never affect renegotiation. Rather, covenant enforcement alone cannot account for the QE-related pattern, exposed banks obtain more favorable terms even from compliant borrowers. The findings support a distinct renegotiation channel through which QE affects existing lending relationships without requiring a covenant breach.

4.3 Mechanism and Cross-Sectional Heterogeneity

The preceding results establish that QE-exposed banks renegotiated their outstanding loans on more favorable terms, including when borrowers remained in compliance with their covenants. The remaining questions are why purchases induced exposed banks to tighten existing credit and which borrowers bore the adjustment. Outside technical default, an amendment requires the consent of both parties. Its terms therefore depend on the lender's incentive to seek concessions, its capacity to withstand a failed negotiation, and the borrower's ability to resist. The mechanism must consequently be examined on both sides of the lending relationship.

We distinguish two accounts of the lender's response. Under a balance-sheet-constraint account, exposed banks tighten because their own financial positions are under pressure, leading them to conserve capital or reduce risky exposures, as financially impaired lenders did during the 2008–09 crisis (Chodorow-Reich & Falato, 2022). Under a relative-return account, MBS purchases change the relative attractiveness of alternative uses of bank balance sheets, potentially raising the opportunity cost of maintaining existing corporate loans on their previous terms (Chakraborty et al., 2020). Exposed banks may then have stronger incentives to renegotiate, with financially stronger lenders better positioned to pursue concessions. The two accounts yield different predictions for the bank-side response. On the borrower side, the relative-return account predicts that tightening should be strongest where borrowers have the weakest outside options, provided that the concessions sought do not jeopardize repayment.

To examine these predictions, we augment equation (1) for lender-favorable renegotiation with the interaction of $HighMBS_{b,t-4} \times MBSPurchases_{t-1}$ and a bank or firm characteristic, including all lower-order terms. The coefficient on the triple interaction measures how the response of lender-favorable renegotiation to QE purchases varies with that characteristic. Because these characteristics are not randomly assigned, the estimates do not identify the causal effects of changing bank or borrower characteristics themselves. Instead, they reveal where the QE-related response is concentrated and whether its cross-sectional pattern is consistent with the competing explanations.

4.3.1 Bank-Side Evidence: Incentives and Capacity

We first examine whether the lender-favorable response of exposed banks depends on their financial strength. We consider three characteristics: the capital ratio, which measures loss-absorbing capacity; loan-loss provisions, which capture reserves set aside against expected credit losses; and net income, which measures current profitability and the ability to generate capital internally. The competing accounts make different predictions. If exposed banks tighten because their balance sheets are under pressure, the response should be stronger among banks with less capital, thinner provisions, and lower earnings. If QE instead raises the opportunity cost of maintaining existing corporate credit and financially stronger banks are better positioned to act on that incentive, the response should increase with each characteristic.

Table 6 reports the estimates. The triple-interaction coefficients are positive and statistically significant in all three columns, 0.0735 ($t = 2.02$) for the capital ratio, 0.0904 ($t = 2.23$) for loan-loss provisions, and 0.0478 ($t = 4.06$) for net income. The lender-favorable response of exposed banks to Federal Reserve purchases is therefore stronger among banks that are better capitalized, hold larger provisions against loan losses, and are more profitable. This pattern runs counter to the prediction that the response is driven primarily by financially constrained banks contracting existing credit to conserve scarce balance-sheet capacity.

The coefficient on $HighMBS_{b,t-4} \times MBSPurchases_{t-1}$ remains positive in all three specifications. It is significant at the ten-percent level in the capital and provisions specifications (0.0254 and 0.0277), while in the net-income specification it is positive but statistically insignificant (0.0121, $t = 0.82$). The positive triple interaction in that specification indicates that the response increases with bank profitability, even though the coefficient at the reference value of profitability is imprecisely estimated. By contrast, the interaction between each characteristic and purchase intensity alone is statistically insignificant throughout. The cross-sectional pattern is therefore associated with the combination of QE exposure and purchase intensity, rather than a general tendency for banks with stronger balance sheets to renegotiate more aggressively during purchase periods.

These findings suggest a distinction between the incentive to tighten and the capacity to act on it. MBS purchases may increase the opportunity cost of maintaining existing corporate loans on their previous terms, creating an incentive for exposed banks to seek more favorable contractual terms. But extracting concessions is not costless. Demanding a smaller commitment, a higher spread, or a shorter maturity may jeopardize the lending relationship or raise the probability of default on the lender's own claim. A well-capitalized and profitable bank is better positioned to absorb the consequences of

a failed negotiation and to redeploy its balance sheet elsewhere. A financially weaker bank has greater reason to accommodate the borrower when further tightening could impair repayment or force the recognition of losses (Peek & Rosengren, 2005). The positive interactions with capital and profitability are consistent with exposed banks pursuing concessions more aggressively when they have the financial capacity to bear these risks.

The provisions result points in the same direction, although it warrants a more qualified interpretation. Larger provisions may provide a buffer against credit losses, but they can also reflect greater underlying risk in a bank's loan portfolio. The positive interaction with provisions is therefore not decisive in isolation. Its consistency with the capital and profitability results, however, makes an account based primarily on financially distressed banks contracting existing credit less plausible. Taken together, the bank-side evidence favors a relative-return and bargaining interpretation over a simple balance-sheet-constraint account, the QE-related response is concentrated among exposed lenders that appear better equipped to pursue concessions, rather than among those under the greatest observable financial pressure.

[Table 6 here.]

4.3.2 Firm-Side Evidence: Outside Options and Bargaining Power

The bank-side evidence explains why some exposed lenders are better positioned to seek concessions, but not why the response varies across their borrowers. A bank's QE-induced incentive to renegotiate may extend across its lending relationships, while the terms it can obtain depend on each borrower's bargaining position. A lender should be able to extract larger concessions from borrowers with fewer credible alternatives and smaller concessions from those better able to refuse or walk away. The within-firm, within-quarter design identifies the lender-driven nature of the adjustment; the cross-sectional evidence asks which borrowers of the same exposed bank bear the largest adjustment. We examine three dimensions of the borrower's bargaining position, profitability, Tobin's q, and book leverage.

The profitability and valuation results are consistent with differences in borrowers' ability to resist and hence their bargaining power relative to incumbent lenders. Profitability determines the internal funds available to finance investment and debt service, while also conveying information about creditworthiness to outside creditors. More profitable firms can rely more heavily on internal finance and are better positioned to attract external funding, allowing them to resist unfavorable amendments more credibly. Less profitable firms have less financial slack, may present a weaker signal to

alternative lenders, and face a higher cost of refusing an amendment or moving their business elsewhere. Tobin's q reflects the market's assessment of a firm's assets and growth opportunities and, in turn, its potential access to equity and other sources of external finance. Firms with low profitability or low q are therefore more dependent on their incumbent lending relationships, the threat to reject an amendment or switch lenders is less credible, giving the incumbent greater bargaining power. If QE-related tightening operates through shifts in relative bargaining power, the response should be strongest where these alternatives are most limited.

Book leverage introduces a competing consideration. More highly leveraged firms may have weaker outside options because other lenders are less willing to extend credit to them, potentially strengthening the incumbent's bargaining position. At the same time, high leverage increases the risk that tighter credit terms will undermine repayment. A lender must balance the concessions it can extract against the possibility that further tightening impairs the borrower's ability to service its debt and reduces the value of the lender's own claim. The bargaining account therefore does not necessarily predict the strongest response among the most indebted firms, at sufficiently high leverage, the cost of jeopardizing repayment may outweigh the benefit of extracting further concessions. This distinction also separates the bargaining interpretation from a simpler distress-driven account, which would more naturally predict a stronger response among borrowers with greater financial fragility.

Table 7 reports the estimates. The triple-interaction coefficients are negative and statistically significant at the five-percent level in all three columns, -0.5423 ($t = -2.20$) for profitability, -0.0408 ($t = -2.55$) for Tobin's q, and -0.1705 ($t = -2.26$) for book leverage. The lender-favorable response of exposed banks to Federal Reserve purchases therefore declines with borrower profitability, market valuation, and leverage. The coefficient on $HighMBS_{b,t-4} \times MBSPurchases_{t-1}$ is positive in every specification, 0.0330 and 0.0277, significant at the five-percent level, for profitability and leverage, respectively, and 0.0256, significant at the ten-percent level, for Tobin's q. By contrast, the interaction between each borrower characteristic and purchase intensity alone is statistically insignificant throughout. The results suggest that borrower characteristics shape how exposed lenders adjust loan terms in response to Fed purchases, rather than simply capturing differences in how firms with different characteristics are repriced during purchase periods. This mirrors the logic of the within-firm design, just as the loan-level estimates point to a lender-driven adjustment, the cross-sectional patterns indicate that the adjustment is disproportionately borne by borrowers with weaker bargaining positions.

The profitability and valuation results are consistent with differences in borrowers'

ability to resist an incumbent lender's demands. Less profitable firms have fewer internal resources with which to absorb tighter credit conditions and may appear less creditworthy to alternative creditors, particularly when those creditors lack the incumbent's borrower-specific information (Rajan, 1992). Firms with lower q have weaker market valuations and may find it more difficult to replace bank finance with equity or other external funding. Both types of firms are more dependent on their existing lending relationships, refusing an amendment or moving elsewhere is more costly, weakening their bargaining position. A given exposed lender, facing the same QE-induced shift in incentives across its portfolio, can therefore obtain larger concessions from borrowers with weaker outside options than from those with stronger ones. The statistically insignificant interactions between borrower characteristics and purchase intensity alone provide no evidence that weaker firms simply receive worse terms from all lenders during purchase periods. Instead, the differential response is associated with lender exposure to Federal Reserve purchases, consistent with the same lender-side shock having larger effects where borrowers are less able to resist it.

The leverage result adds an important qualification. If exposed lenders extracted the most from borrowers solely because they had the weakest outside options, one might expect the response to increase with leverage, since highly indebted firms may have fewer alternative sources of credit. Instead, the response declines with leverage. This pattern is consistent with the lender's concern for repayment limiting its willingness to press already heavily indebted borrowers. Bargaining power alone does not determine the concessions a lender will seek; the lender must also consider the consequences of obtaining them. For a highly leveraged firm, a smaller commitment, higher spread, or shorter maturity may further impair repayment and destroy value for the lender as well as the borrower. The negative leverage interaction suggests that this repayment-risk constraint dominates the opposing effect of leverage on outside options over the range captured by the estimates. Thus, although weaker outside options may strengthen a lender's bargaining position, the lender's willingness to exploit that advantage depends on the risk that further tightening poses to repayment.

Taken together, the bank- and firm-side results provide a coherent bargaining interpretation for Section 4.2.1. The QE-related response does not require a covenant breach, and it is not concentrated among the most highly leveraged borrowers. Together, the results are difficult to explain solely as the exercise of contractual control rights against financially distressed firms. They are instead consistent with exposed lenders obtaining concessions from borrowers whose outside options are weak, while tempering their demands where further tightening poses greater repayment risk. Because the three borrower characteristics enter separately, the estimates do not establish

that the same firms jointly exhibit low profitability, low q, and low leverage. Rather, each captures a distinct dimension of the mechanism, internal financial capacity, access to alternative financing, and the lender's own exposure to repayment risk.

Taken together, the bank- and firm-side results provide a coherent bargaining interpretation of the renegotiation channel. The response is stronger among exposed banks with greater capital and profitability, consistent with these lenders having both the incentive and the capacity to seek concessions. It is also stronger for borrowers with lower profitability and lower market valuations, whose weaker outside options limit their ability to resist an incumbent lender. But the response is attenuated among highly leveraged firms, for which further tightening can jeopardize repayment. The same QE-induced shift in lender incentives can therefore have different consequences for borrowers of the same exposed bank, those with stronger outside options are better able to resist, while those more dependent on their incumbent relationship have less bargaining power. This pattern is consistent with a discretionary adjustment of existing credit through bargaining, in which relatively strong lenders obtain larger concessions from borrowers with limited alternatives, rather than a contraction driven primarily by lender distress or the exercise of contractual control rights following covenant violations.

[Table 7 here.]

5 Real Effects of Lender-Favorable Renegotiation

The preceding sections establish that QE-exposed banks renegotiated their outstanding loans on more favorable terms, including when borrowers remained in compliance with their covenants. Whether these contractual changes affect firms' real decisions is a separate question. If borrowers can replace the credit withdrawn by an exposed lender at little cost, by turning to other banks, capital markets, or internal funds, lender-favorable renegotiation may redistribute surplus without changing investment. The loan-level estimates cannot settle this question because the within-firm comparison identifies differences across lenders while holding common borrower conditions fixed, and is therefore silent about the firm's overall access to credit (Khwaja & Mian, 2008). This section moves from the loan to the firm to examine whether lender-favorable renegotiation reduces investment.

5.1 First Stage and Identification

We estimate equation (2) by two-stage least squares on the firm-quarter panel, following the design in Section 3.2. The endogenous regressor, $LenderFav_{f,t-1}$, is the share of the firm's loans amended in the lender's favor in the preceding quarter. The instrument, $Z_{f,t-1}$, aggregates the loan-level QE treatment to the firm level using predetermined lending relationships, as defined in equation (3). It combines pre-QE bank-firm borrowing weights with variation in banks' exposure to Federal Reserve MBS purchases.

The first stage is strong. The coefficient on $Z_{f,t-1}$ is positive and statistically significant, and the first-stage F-statistic is 18.4. The instrument therefore predicts meaningful variation in lender-favorable renegotiation across firms. This relationship follows directly from the loan-level evidence, exposed banks renegotiate outstanding loans in their own favor when Federal Reserve purchases increase, so firms whose pre-QE lending relationships were concentrated among these banks experience greater exposure to the renegotiation response.

The predetermined weights prevent subsequent changes in firms' lender composition from mechanically determining their measured exposure. Firm fixed effects absorb persistent differences across firms with different pre-QE lender portfolios, while time fixed effects absorb aggregate purchase intensity and common macroeconomic conditions. We assess the identifying variation using two additional tests.

First, we find no statistically significant evidence of differential investment trends across firms with different pre-QE lender-portfolio exposures. Table 11 reports investment regressions interacting predetermined portfolio exposure with relative-quarter indicators, using the quarter immediately preceding each formal purchase episode as the reference period. The specifications include firm controls, episode-specific firm and quarter fixed effects, and firm-clustered standard errors. In the pooled sample, the seven pre-period interactions are individually insignificant and jointly fail to reject the null of zero coefficients ($F = 1.24$, $p = 0.280$). Restricting the analysis to QE1 yields similar results ($F = 1.30$, $p = 0.249$). These findings are consistent with the absence of differential pre-trends, although the pooled evidence is dominated by QE1 and provides limited information about later episodes.

Second, Table 12 reports an artificial-onset placebo test in purchase-free quarters before QE1. Because the actual purchase-interacted instrument is identically zero in these quarters, we instead interact predetermined lender-portfolio exposure with an indicator beginning in 2007Q4, four quarters before formal QE1. The sample covers 2006Q4–2008Q2 and requires zero purchases in both the outcome quarter and the purchase-source quarter underlying the actual instrument. Including firm controls,

firm and quarter fixed effects, and firm-clustered standard errors, the estimated investment coefficient is -0.0030 ($p = 0.736$), with a cluster-robust Wald F-statistic of 0.11. The corresponding coefficient for lagged lender-favorable renegotiation is 0.1051 ($p = 0.211$), with a Wald F-statistic of 1.57. Neither regression therefore detects a statistically significant exposure- related change around the artificial onset. Together with the pre-trend evidence, these results provide limited support against the concern that the main findings reflect pre-existing differences in investment dynamics.

The exclusion restriction is a separate consideration. Conditional on the controls and fixed effects, the instrument must affect investment through lender-favorable renegotiation rather than through other channels of QE exposure. Predetermined weights and the absence of differential pre-trends address important concerns about endogenous lender matching and borrower-specific trends, but do not by themselves exclude effects on new lending or other dimensions of credit supply. We examine this alternative directly below.

[Table 11 here.]

[Table 12 here.]

5.2 Investment Effects

Table 8 reports the second-stage estimates. The coefficient on instrumented lender-favorable renegotiation is -0.0343 ($t = -2.09$), significant at the five-percent level. A ten-percentage-point increase in a firm's lender-favorable renegotiation rate reduces its investment rate in the following quarter by approximately 0.34 percentage points of installed capital, equivalent to about six percent of the sample mean quarterly investment rate. The estimate indicates that changes in the terms of outstanding credit have economically meaningful consequences for firms' real decisions.

The loan-level results suggest several margins through which lender-favorable renegotiation can affect investment. A reduction in committed credit limits the liquidity available to finance capital expenditure. A higher spread raises the cost of external finance and diverts cash flow from investment. A shorter maturity brings forward refinancing needs and exposes the firm to rollover risk, making long-lived capital commitments less attractive. The IV estimate captures the overall investment response associated with lender-favorable renegotiation; it does not separately identify the contribution of each contractual margin.

The response is consistent with incomplete substitution across sources of finance. Firms cannot necessarily replace credit on comparable terms when an incumbent lender tightens loan terms. Previous research documents that creditor-imposed changes to

loan terms can depress investment (Chava & Roberts, 2008; Nini et al., 2012) and that lending relationships are costly to replace (Chodorow-Reich, 2014). The firm-side heterogeneity in Section 4.3.2 reinforces this interpretation, QE-related increases in lender-favorable renegotiation are larger among firms with lower profitability and market valuations, whose internal resources and alternative financing opportunities are more limited. These borrowers are less able to offset unfavorable amendments by switching lenders or accessing capital markets. The investment result thus suggests that renegotiation is more than a redistribution of surplus within a lending relationship, changes in the terms of existing credit can affect the borrower's investment decisions.

[Table 8 here.]

5.3 Exclusion Restriction and Robustness

The main threat to the exclusion restriction is that QE exposure may affect investment through changes in new credit supply to the same firms. The direction of this alternative channel is not unambiguous. Existing evidence suggests that QE can expand lending by MBS-exposed banks (Rodnyansky & Darmouni, 2017), while other work documents a potential crowding out of commercial lending (Chakraborty et al., 2020). If the instrument affects investment through new originations independently of renegotiation, the IV coefficient cannot be interpreted as isolating the causal effect of lender-favorable renegotiation.

To assess this alternative, we augment the investment specification with a firm-level measure of new credit supply from exposed lenders, constructed analogously to $Z_{f,t}$ but using origination volumes rather than amendment outcomes. The estimated coefficient on instrumented lender-favorable renegotiation, in Table 13, remains stable, while the new-credit-supply measure enters insignificantly. This evidence suggests that the investment response is not readily explained by the observed variation in new lending from exposed banks alone. Although this test does not rule out every possible channel through which QE exposure could affect investment, it strengthens the interpretation that the renegotiation of outstanding loans is an economically important part of the response.

We conduct two additional robustness checks. First, in Table 14, we replace the baseline relationship weights $\omega_{f,b}^{\text{pre}}$ with alternative predetermined measures, including equal weights across lenders and weights based on the number of prior loans rather than borrowing volume. The results remain similar, indicating that the findings do not depend on a particular weighting scheme. Second, in Table 15, we exclude the 2020–22 purchase wave, which coincided with unusually large macroeconomic shocks, and

control for industry-by-quarter fixed effects to absorb sectoral variation in investment opportunities. The findings remain robust to these specifications.

Taken together, the first-stage, pre-trend, placebo, and robustness evidence supports the design against several important alternative explanations. The analysis of new credit supply further suggests that the investment response is not simply a reflection of observed changes in originations. The causal interpretation of the IV estimate nevertheless rests on the exclusion restriction, conditional on the controls and fixed effects, other effects of instrument-induced QE exposure must not independently account for the estimated investment response.

[Table 13 here.]

[Table 15 here.]

5.4 Implications for the Transmission of Quantitative Easing

The results identify renegotiation of outstanding loans as an additional margin through which QE can reach the real economy. The literature on the bank lending channel of QE has primarily examined the volume and terms of new lending (Rodnyansky & Darmouni, 2017; Chakraborty et al., 2020). The channel documented here operates instead through the stock of existing corporate credit. Because a firm need not originate a new loan for its outstanding agreements to be amended, this margin can affect firms that do not approach the credit market for new financing in a given quarter.

The direction of this response is contractionary for the borrowers of exposed banks. When the Federal Reserve purchases MBS, exposed banks renegotiate outstanding corporate loans toward smaller commitments, higher spreads, and shorter maturities. The resulting investment response works against the intended stimulus for the firms affected. The findings therefore suggest that evaluations of QE based solely on new loan originations can overlook adjustments to outstanding credit and provide an incomplete picture of the financing conditions facing existing borrowers.

The estimates do not establish the aggregate net effect of QE on corporate investment. Identification comes from cross-sectional differences in firms' predetermined exposure to affected lenders, while time fixed effects absorb policy effects and macroeconomic shocks common across firms. The results instead identify a countervailing margin, for firms whose existing lenders respond to MBS purchases by tightening contractual terms, renegotiation can transmit a contractionary impulse through outstanding credit.

This distinction has implications for the assessment of asset-purchase programs. The composition of purchases may affect not only the assets banks hold and the new

loans they originate, but also the incentives governing their existing corporate lending relationships. The incidence of the renegotiation response is also uneven, tightening is concentrated among well-capitalized and profitable banks and falls disproportionately on borrowers with weak prospects and limited financing alternatives. A strong banking sector is therefore not sufficient to ensure that monetary easing improves the terms faced by every existing borrower. Understanding the transmission of QE requires attention not only to new credit, but also to how lenders renegotiate the stock of credit already outstanding.

6 Conclusion

This paper documents a renegotiation channel through which quantitative easing affects the terms of existing corporate credit. When the Federal Reserve purchases agency MBS, banks with large pre-existing MBS holdings renegotiate their outstanding syndicated loans in their own favor. Relative to other banks' amendments for the same firm in the same quarter, a doubling of purchases is associated with committed amounts that are 2.5 percent lower, spreads that are 10.6 basis points higher, and maturities that are 3.1 percent shorter. The response operates outside covenant violations and is concentrated among well-capitalized, well-provisioned, and profitable banks. It falls disproportionately on borrowers with low profitability and market valuations, but is weaker among highly leveraged firms. This pattern is consistent with exposed banks acting on the incentives created by purchases, with their financial strength enabling them to pursue concessions from borrowers with limited outside options, subject to the risk of impairing repayment. The firm-level results further indicate that a ten-percentage-point increase in lender-favorable renegotiation reduces the following-quarter investment rate by approximately 0.34 percentage points of installed capital, or about six percent of its sample mean.

These findings have three broader implications. First, the composition of asset purchases matters for corporate borrowers. Purchases directed at the mortgage market change the incentives of the banks most active in that market, which respond in part by repricing and reducing the corporate credit they have already extended. This renegotiation margin is distinct from the effects of QE on new lending and can work against the intended easing for existing borrowers. Second, evaluations of monetary policy transmission based only on loan originations provide an incomplete picture of firms' financing conditions. Outstanding credit remains subject to renegotiation as lenders' incentives change, even before loans mature or covenant violations activate contractual control rights. Monitoring amendments alongside new lending would therefore

provide a more complete assessment of how asset purchases reach firms. Third, the incidence of this channel is uneven. The tightening is carried out by financially stronger banks and falls disproportionately on borrowers with weak prospects and limited financing alternatives, rather than being confined to financially distressed lenders or borrowers in technical default. Balance-sheet strength, which supports banks' capacity to lend, can also enable exposed lenders to pursue more favorable terms on existing credit when their incentives change.

Our estimates identify a contractionary response associated with differential exposure to QE, not the aggregate net effect of asset purchases on corporate investment. That net effect also reflects changes in interest rates, asset prices, and the supply of new credit common across firms. It therefore depends on the balance between the expansionary effects of QE on new lending and the tightening of outstanding credit documented here, as well as on the composition of banks' balance sheets and firms' dependence on existing lending relationships. Quantifying the net contribution of these opposing margins is an important question for future research. Our results nevertheless establish that understanding QE transmission requires looking beyond the flow of new credit to the terms of credit already outstanding.

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Tables

Table 1: Summary Statistics: Loan-Level Sample

Variable	Mean	SD	p25	p50	p75
Loan Spread (bps)	219.620	139.504	125.000	200.000	275.000
Loan Amount ($ million)	468.284	895.018	60.000	195.000	500.000
Loan Maturity (months)	43.472	22.172	27.000	48.000	60.000
Covenant	0.713	0.452	0.000	1.000	1.000
Renegotiation	0.593	0.491	0.000	1.000	1.000

The sample contains 23,472 tranche-round-bank observations. Statistics use all non-missing observations for each variable. Loan Spread is the all-in spread drawn over LIBOR, Loan Amount is measured in millions of dollars, and Loan Maturity is measured in months. Covenant equals one if the DealScan covenant indicator is set. Renegotiation equals one if DealScan amendment information contains "Amendment."

Table 2: Summary Statistics: Firm-Level Sample

Variable	Mean	SD	p25	p50	p75
Firm Size	7.168	1.835	6.022	7.161	8.293
Leverage	0.347	0.648	0.163	0.294	0.452
Tangibility	0.320	0.268	0.113	0.238	0.461
ROA	0.022	0.425	0.019	0.032	0.047
Tobin's Q	0.974	0.886	0.432	0.830	1.312

The sample contains 13,278 unique firm-quarter observations for 2,953 firms and is restricted to firm-quarters with non-missing investment. Statistics use all non-missing observations for each variable. Firm Size is the log of total assets; Leverage is short- plus long-term debt divided by total assets; Tangibility is net property, plant, and equipment divided by assets; ROA is operating income before depreciation divided by lagged assets; and Tobin's Q is the market-value-of-assets measure used in the regressions.

Table 3: Quantitative Easing and Renegotiation

Dependent Variable	Renegotiation	Lender Fav.	Lender Unfav.
High MBS Holdings	0.0364	0.0478	-0.0079
	(1.34)	(1.59)	(-0.37)
High MBS $\times$ MBS Purchases	0.0184*	0.0320**	0.0096
	(1.71)	(2.47)	(0.53)
No. of Obs	16,780	16,670	16,677
Controls	Yes	Yes	Yes
Firm-Y FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Cluster	Bank	Bank	Bank

*p$<$0.10, **p$<$0.05, ***p$<$0.01. *t*-statistics in parentheses.

Table 4: Quantitative Easing and Loan Terms

Dependent Variable	Δ Loan Amount	Δ Loan Spread	Δ Loan Maturity
High MBS Holdings	0.0222	-12.35***	-0.0003
	(0.65)	(-2.71)	(-0.01)
High MBS × MBS Purchases	-0.0365**	15.29***	-0.0447**
	(-2.15)	(3.05)	(-2.59)
No. of Obs	8,080	7,575	7,422
Controls	Yes	Yes	Yes
Firm-YQ FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Cluster	Bank	Bank	Bank

*p<0.10, **p<0.05, ***p<0.01. *t*-statistics in parentheses.

Table 5: Quantitative Easing, Renegotiation, and Covenant Violation

Dependent Variable	Renegotiation (Lender Fav.)
High MBS Holdings	0.0380
	(1.01)
1(Covenant Violation)	-0.0156
	(-0.42)
High MBS $\times$ MBS Purchases	0.0478***
	(2.72)
High MBS $\times$ 1(Covenant Violation)	0.0235
	(0.42)
1(Covenant Violation) $\times$ MBS Purchases	0.0228
	(1.56)
High MBS $\times$ MBS Purchases $\times$ 1(Covenant Violation)	-0.0410
	(-1.54)
No. of Obs	16,670
Controls	Yes
Firm-Y FE	Yes
Bank FE	Yes
Time FE	Yes
Cluster	Bank

*p<0.10, **p<0.05, ***p<0.01. *t*-statistics in parentheses.

Table 6: Quantitative Easing, Renegotiation, and Bank Characteristics

Dependent Variable	Renegotiation (Lender Fav.)		
Bank Char.	Capital Ratio	Loan Loss Reserves	Net Income
High MBS Holdings	0.0548	0.0485	0.0969***
	(1.56)	(1.51)	(2.76)
Bank Char.	0.0157	-0.0723*	0.1057**
	(0.92)	(-1.68)	(2.07)
High MBS × MBS Purchases	0.0254*	0.0277*	0.0121
	(1.79)	(1.83)	(0.82)
High MBS × Bank Char.	0.0290	-0.0526	-0.0936*
	(0.32)	(-0.51)	(-1.81)
Bank Char. × MBS Purchases	-0.0100	0.0122	-0.0238
	(-0.64)	(1.13)	(-1.59)
High MBS × MBS Purchases × Bank Char.	0.0735**	0.0904**	0.0478***
	(2.02)	(2.23)	(4.06)
No. of Obs	16,670	16,670	16,670
Controls	Yes	Yes	Yes
Firm-Y FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Cluster	Bank	Bank	Bank

*p<0.10, **p<0.05, ***p<0.01. *t*-statistics in parentheses.

Table 7: Quantitative Easing, Renegotiation, and Firm Characteristics

Dependent Variable	Renegotiation (Lender Fav.)		
Firm Char.	Profitability	Tobin's q	Book Leverage
High MBS Holdings	0.0492	0.0503*	0.0553*
	(1.64)	(1.71)	(1.81)
Firm Char.	0.7017	0.0633	0.0033
	(0.88)	(0.86)	(1.53)
High MBS × MBS Purchases	0.0330**	0.0256*	0.0277**
	(2.37)	(1.93)	(2.32)
High MBS × Firm Char.	-0.1101	0.0105	0.1277
	(-0.11)	(0.19)	(1.15)
Firm Char. × MBS Purchases	-0.0930	0.0001	0.0710
	(-0.38)	(0.00)	(1.16)
High MBS × MBS Purchases × Firm Char.	-0.5423**	-0.0408**	-0.1705**
	(-2.20)	(-2.55)	(-2.26)
No. of Obs	16,670	16,670	16,670
Controls	Yes	Yes	Yes
Firm-Y FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Cluster	Bank	Bank	Bank

*p<0.10, **p<0.05, ***p<0.01. t-statistics in parentheses.

Table 8: Renegotiation and Real Effects

Dependent Variable	Investment Rate
Lender Fav. Renegotiation	-0.0343**
	(-2.09)
1st-stage *F*-stat	18.4
No. of Obs	6,159
Controls	Yes
Firm FE	Yes
Time FE	Yes
Cluster	Firm

*p<0.10, **p<0.05, ***p<0.01. *t*-statistic in parentheses. *F*-stat is the Kleibergen-Paap *F*-stat.

Table 9: Robustness: Pre-QE Trends

Dependent Variable	Renegotiation	Lender Fav.	Lender Unfav.
High pre-episode MBS exposure × relative-quarter indicators			
$k = -8$	0.0000	-0.2333	0.1697
	(0.00)	(-1.40)	(0.92)
$k = -7$	-0.0589	-0.1115	-0.0440
	(-0.43)	(-0.93)	(-0.41)
$k = -6$	-0.1845	-0.1802*	-0.1220
	(-1.51)	(-1.73)	(-1.00)
$k = -5$	-0.1006	-0.2319	0.0415
	(-0.55)	(-1.48)	(0.39)
$k = -4$	-0.1067	-0.1530	0.0255
	(-0.49)	(-0.73)	(0.24)
$k = -3$	-0.1352	-0.1358	-0.1070
	(-1.15)	(-0.90)	(-1.49)
$k = -2$	-0.0257	-0.0944	0.0352
	(-0.25)	(-1.17)	(0.44)
Joint pre-period F	1.34	1.39	2.85
Joint pre-period p-value	0.258	0.240	0.018
No. of Obs	3,028	3,018	3,018
Controls	Yes	Yes	Yes
Episode × Firm-Year FE	Yes	Yes	Yes
Episode × Bank FE	Yes	Yes	Yes
Episode × Quarter FE	Yes	Yes	Yes
Cluster	Bank	Bank	Bank

*p<0.10, **p<0.05, ***p<0.01. Bank-clustered t-statistics in parentheses. Pre-windows include at most eight quarters and start strictly after the preceding QE program ends. The reference is $k = -1$ in each episode. QE start quarters follow the sample pipeline: 2008Q4, 2010Q4, 2012Q3, and 2020Q1. Prior-program end quarters (inclusive) are 2010Q1, 2011Q2, and 2014Q4, based on FOMC statements dated March 16, 2010; June 22, 2011; and October 29, 2014, respectively. High exposure is the top tercile of MBS-to-assets four quarters before each episode, ranked across distinct banks in its original eight-quarter candidate window and held fixed. QE4 has no usable observations at $k = -6$ and $k = -5$ in any column. QE2 has no high-exposure reference-quarter observations in any column. Outcomes and controls follow Table 3. The joint null is $\beta_{-8} = \cdots = \beta_{-2} = 0$, tested using the bank-clustered covariance matrix.

Figures

Figure 1: Quantitative Easing

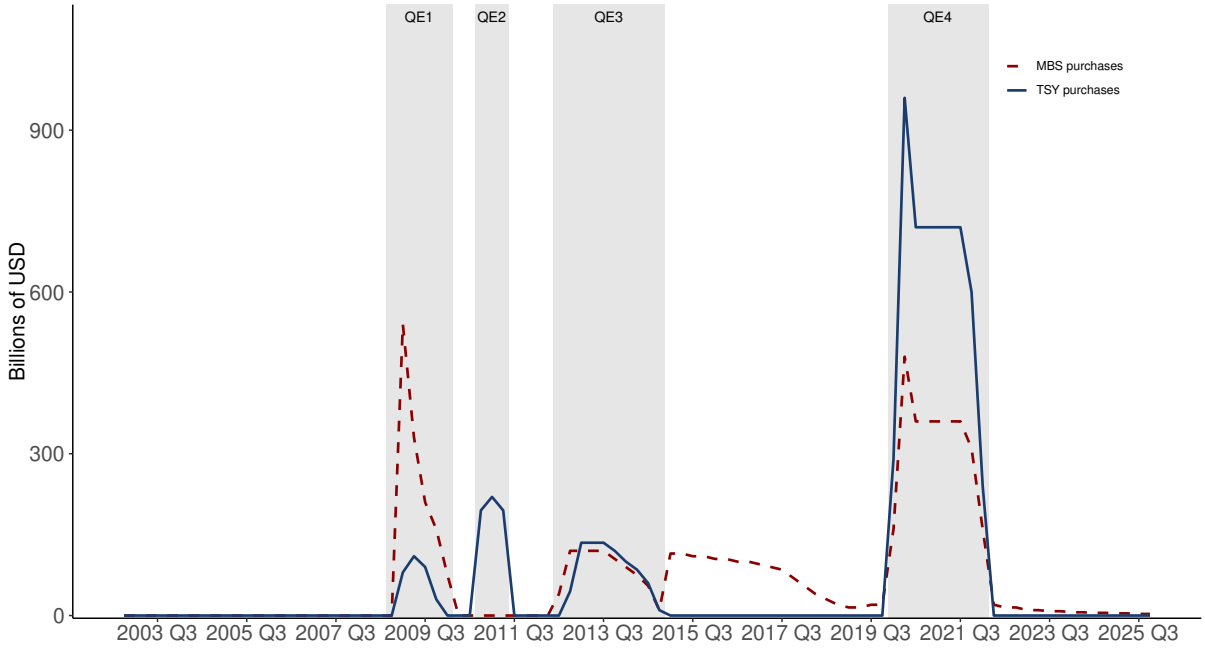
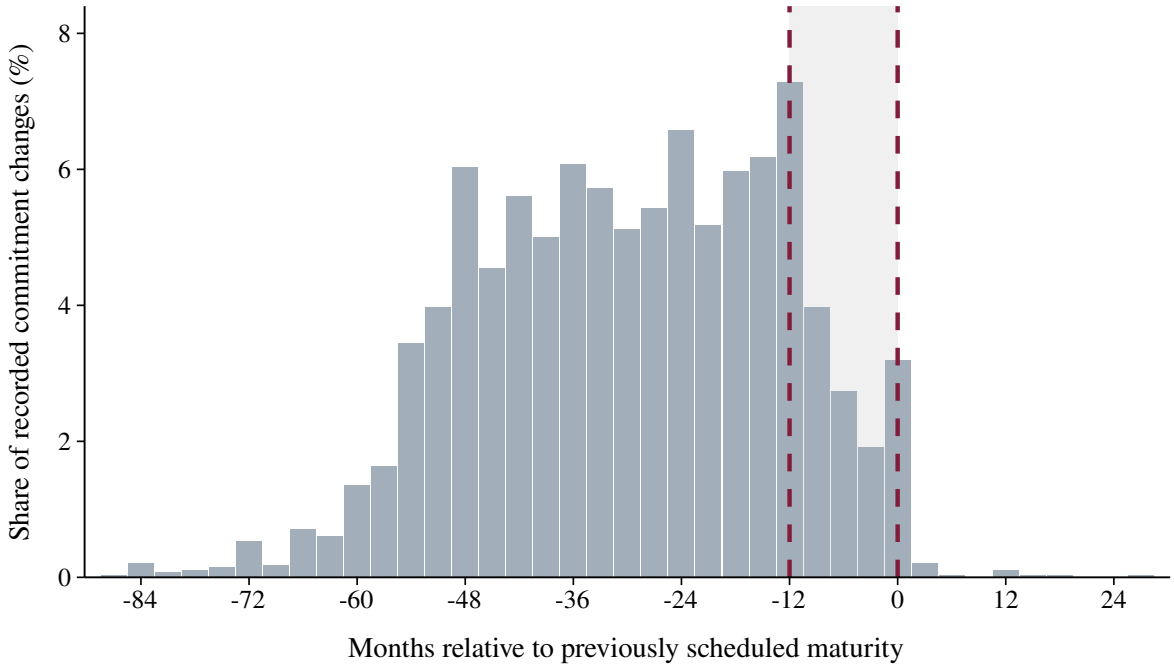


Figure 2: Commitment Change Clustering



A Additional Empirical Results

A.1 First-Stage Regression

Table 10: First Stage of the IV Regression

Dependent variable	Lender-favorable renegotiation
$Z_{f,t-1}$	0.0346***
	(4.29)
First-stage F	18.4
Controls	Yes
Firm FE	Yes
Time FE	Yes
Cluster	Firm

*p<0.10, **p<0.05, ***p<0.01. Firm-clustered t-statistic in parentheses.

A.2 Robustness Checks

Table 11: Firm Investment Pre-Trends by Fixed Lender-Portfolio Exposure

Dependent variable: Investment	Pooled QE1–QE4	QE1 only
Fixed portfolio exposure $E_{f,e}$ × relative-quarter indicators		
$k = -8$	-0.0226	-0.0222
	(-1.02)	(-0.99)
$k = -7$	-0.0193	-0.0203
	(-0.81)	(-0.85)
$k = -6$	0.0043	0.0049
	(0.19)	(0.21)
$k = -5$	-0.0226	-0.0233
	(-1.05)	(-1.08)
$k = -4$	-0.0220	-0.0223
	(-1.03)	(-1.04)
$k = -3$	-0.0259	-0.0277
	(-0.88)	(-0.93)
$k = -2$	-0.0010	-0.0008
	(-0.05)	(-0.04)
$k = -1$	Reference	Reference
Joint pre-period F	1.24	1.30
Joint pre-period p-value	0.280	0.249
Controls	Yes	Yes
Episode × Firm	Yes	Yes
Time FE	Yes	Yes
Cluster	Firm	Firm

*p<0.10, **p<0.05, ***p<0.01. Firm-clustered t-statistics in parentheses.

Table 12: Pre-QE Artificial Onset Placebo Test

Dependent variable	Lender-favorable reneg.	Investment
$E_{f,\text{QE1}} \times \mathbf{1}[t \geq \text{2007Q4}]$	0.1051	-0.0030
	(1.25)	(-0.34)
p-value	0.211	0.736
Cluster-robust Wald F	1.57	0.11
Controls	Yes	Yes
Firm FE	Yes	Yes
Time FE	Yes	Yes

*p<0.10, **p<0.05, ***p<0.01. Firm-clustered t-statistics in parentheses.

Table 13: IV Regression Controlling for New Lending from Exposed Lenders

	Baseline	+New credit
Instrumented lender-favorable reneg.	-0.0343**	-0.0207*
	(-2.09)	(-1.70)
First-stage F	18.36	13.32
Firm FE	Yes	Yes
Time FE	Yes	Yes
Cluster	Firm	Firm

*p<0.10, **p<0.05, ***p<0.01. Firm-clustered t-statistics in parentheses.

Table 14: Investment IV with Alternative Relationship Weights: Same Sample

	Baseline	Equal lender weights	Prior-loan-count weights
Instrumented lender-favorable reneg.	-0.0343**	-0.0331**	-0.0341**
	(-2.09)	(-2.06)	(-2.06)
First-stage F	18.36	18.89	18.09
Observations	6,159	6,159	6,159
Firm FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Cluster	Firm	Firm	Firm

*p<0.10, **p<0.05, ***p<0.01. Firm-clustered t-statistics in parentheses.

Table 15: IV Regression Excluding 2020–22

	Baseline	Exclude 2020–22
Instrumented lender-favorable reneg.	-0.0343**	-0.0598*
	(-2.09)	(-1.74)
First-stage F	18.36	17.13
Firm FE	Yes	Yes
Time FE	Yes	Yes
Cluster	Firm	Firm

*p<0.10, **p<0.05, ***p<0.01. Firm-clustered t-statistics in parentheses.