Price and Quantity Transmission of Monetary Policy: Complements or Substitutes for Small Firms?

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Abstract

This paper examines the relationship between price and quantity sensitivities in the transmission of monetary policy to small firms. We exploit India's External Benchmark Lending Rate reform, which mandated banks to link loan rates for small firms directly to policy rates. Using loan-level data, we find that mandatory pass-through increased price sensitivity but reduced credit expansion and firm investment in response to policy rate cuts. Analysis of unsecured loans, relationship lending, competitive banking markets, and proxies for monitoring costs shows that upward-sloping marginal costs of lending drive this substitution between price and quantity sensitivities, weakening monetary policy's real effects.

Keywords: Monetary Policy Transmission, Interest Rate, Benchmarking
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1 Introduction

Central banks around the world routinely attempt to strengthen the transmission of monetary policy by encouraging banks to pass on policy rate cuts, particularly to small firms and especially in times of crisis. In China, the People's Bank of China has long relied on window guidance - informal directives to commercial banks - to ensure that reductions in the Loan Prime Rate are reflected in cheaper credit for micro and small enterprises (Chen et al. (2020)). In advanced economies, central banks have used targeted facilities to reinforce pass-through. Japan and South Korea have Special Funds-Supplying Operations and Bank-Intermediated Lending Support Facility, respectively, that aim to provide a stable and low-cost source of financing for banks to fund businesses (Nunn, 2022; Rhee, 2025). Implicit in these interventions is the assumption that stronger price transmission, i.e., lower lending rates, will be accompanied by stronger quantity transmission, i.e., an expansion in loan volumes. Whether this assumption holds depends on the relationship between the sensitivities of interest rates (price) and loan amount (quantities) to monetary policy. Yet empirically identifying this relationship is difficult in most settings, because both are simultaneously determined in equilibrium.

We study the relationship between price and quantity transmission of monetary policy to small firms by exploiting a unique policy reform in India where the central bank exogenously altered the pass-through of policy rates by requiring banks to benchmark new small-firm loans to an external reference rate directly linked to the policy rate. This regulatory change provides a setting to identify how the sensitivity of loan prices translates into the sensitivity of loan quantities. Our analysis focuses on policy rate cuts, as the period immediately before and after the reform was characterized exclusively by rate reductions rather than increases.

An exogenous increase in price sensitivity may reduce quantity sensitivity to a policy rate cut when banks face upward-sloping marginal costs of lending to constrained firms (Ottonello and Winberry (2020), Mahoney and Weyl (2017), Agarwal et al. (2015)). These costs may arise from higher screening and monitoring requirements or from the increased default risk associated with larger loan sizes (Bryan et al. (2024)). In such settings, a bank that passes on the rate cut less fully preserves margin to absorb these costs and can therefore expand lending more than a bank that aligns its lending rate closely with the policy rate. Put differently, mandating full pass-through limits banks' ability to increase lending while maintaining profitability. Likewise, if banks use central bank liquidity during crises for balance sheet repair or "stealth recapitalization" (Acharya et al. (2020), Brunnermeier et al. (2020)), then forcing a reduction in lending rates may tighten capital constraints further, resulting in even less credit expansion than in the absence of the regulatory push.

Conversely, there is evidence that surviving banks in a crisis gain market power, widen margins, and do not expand lending commensurately with policy rate cuts. For instance, surviving banks in the U.S. and Europe were found to charge higher spreads and restrict credit supply during crises (Berger and Bouwman (2013), Beck et al. (2013), Acharya et al. (2019), Schivardi et al. (2017)). In such settings, a mandate to pass on policy rate cuts may primarily reduce banks' markups. Banks with market power may then find it more profitable to expand lending once constrained to pass on policy rate cuts and thus no longer able to optimize on price margins. These contrasting mechanisms highlight the theoretical ambiguity and underscore the need for empirical evidence on the relationship between price and quantity sensitivities to monetary policy.

Our main findings are easily summarized. First, we show that for small firms, the price and quantity sensitivities to policy rate cuts are negatively related, so that credit expansion in response to a rate cut is smaller under a regime of mandated price pass-through than under one without such a mandate. Second, this reduced expansion of credit also limits incremental investment by borrowers. Third, by examining lending costs, monitoring intensity, and loan defaults, we find evidence consistent with an upward-sloping marginal cost of lending to small firms as the underlying mechanism (Ottonello and Winberry (2020)).

The economic setting for our study is provided by an order issued by the Reserve Bank of India (RBI) mandating new benchmarks for pricing loans to small firms (SMEs) beginning October 1, 2019. The RBI argued that under the existing regime, where loan pricing was tied to banks' marginal cost of funds, policy rate changes were not being fully transmitted to lending rates, especially for small borrowers (Acharya (2017)). To address this, the RBI introduced the External Benchmark Lending Rate (EBLR), which required banks to link loan rates for SMEs and retail borrowers directly to an external benchmark—either the policy rate or the yield on short-term government securities. Moreover, the spreads over the benchmark were restricted to reflect only borrower-specific credit assessments, limiting banks' ability to offset benchmark changes by adjusting margins. This feature makes the EBLR distinct from voluntary benchmarking practices, as it eliminates banks' discretion to mute policy-rate pass-through through spread adjustments.

Under the EBLR regime, changes in the policy rate were therefore transmitted directly and almost mechanically to lending rates for small firms. This regulatory shift exogenously altered the price sensitivity of loan contracts, creating a setting in which the corresponding adjustment in loan quantities can be observed directly. In particular, it enables us to assess whether stronger price pass-through is accompanied by stronger or weaker credit expansion.

We begin our empirical analysis with a first-stage test of whether the EBLR regulation in fact altered the pass-through of policy rates to small firms. Using impulse response functions, we find that prior to the EBLR reform, banks did not fully transmit policy rate changes to SME borrowers even after 12 months of a rate cut. By contrast, under the EBLR regime, policy rate changes were almost completely transmitted within a single quarter. As a comparison, we examine the behavior of shadow banks, to which the EBLR did not apply. For SME borrowers served by shadow banks, we observe no significant change in the speed or extent of policy rate transmission across the pre- and post-EBLR periods. Likewise, for large firms—also outside the scope of the regulation—the transmission of policy rates by both banks and shadow banks remains largely unchanged.

For identification, we exploit the fact that the EBLR reform mandated only banks—and not nonbank lenders—to benchmark new loan rates to an external policy rate. Our main strategy compares the sensitivity of loan rates and quantities to changes in the policy rate for the same small firm across bank and nonbank lenders, before and after the reform. The resulting triple-difference design combines (i) the response of credit outcomes to policy-rate changes, (ii) the shift in this response around the reform, and (iii) the differential exposure of treated (banks) versus control (nonbanks) lenders. We further reinforce identification with a within-borrower, across-bank design that exploits heterogeneity in pre-EBLR pass-through: banks with weaker transmission prior to the reform were more constrained by the mandate, generating an additional source of quasi-experimental variation.

A natural concern is that the lack of identical lenders differentially subject to regulation, or the overlap of the post-reform period with Covid-19, might confound inference. In our context, however, threats arise only from shocks that simultaneously (i) increase the bank–nonbank difference in price transmission and (ii) reduce the corresponding difference in quantity transmission precisely at the reform cutoff. The existence of such shocks is unlikely. Nonetheless, we conduct a battery of tests to ensure that the identification is robust.

First, standard diagnostics reveal no pre-trends in differential transmission, the results are robust to borrower $\times$ time fixed effects à la Khwaja and Mian (2008), and placebo tests using EBLR-exempt borrowers show no differential effects. Second, to address potential compositional differences between banks and nonbanks, we flexibly control for lender fundamentals interacted with monetary-policy changes and also the intensive margin of borrower-lender relationships interacted with monetary policy changes, restrict the nonbank sample to systemically important institutions subject to similar supervisory oversight as banks, account for differences in loan types, and verify that balance-sheet structures do not diverge differentially after the reform. Finally, we document that Covid-related regulatory measures applied symmetrically to all lenders, the EBLR reform clearly predates the pandemic, our estimates are not driven by lender-specific shifts in credit demand during Covid, and all main results remain unchanged when the Covid year is excluded entirely.

Using both identification strategies, we first formally verify the first-stage results. We find that affected lenders pass on policy rate changes almost one-for-one within the first month of a policy rate change. Before the EBLR, the baseline first-quarter transmission by both banks and shadow banks to SMEs was close to zero. After the EBLR, however, a 1 percentage point change in policy rates led to an approximately 82 basis points in bank lending rates, while the transmission by shadow banks remained unchanged relative to the pre-EBLR period. The estimated coefficient for banks is not statistically different from unity, confirming that the regulation effectively enforced full pass-through.

Our main result is that mandating the transmission of interest rates reduces credit expansion in response to policy rate cuts. Specifically, we find that for a one percentage point decrease in policy rates, loan amounts expand by 20.4% less in the post-EBLR period than in the pre-EBLR period when comparing banks with shadow banks. In our within-bank specification, the decline in loan expansion in response to a percentage decline in policy rates is about 17.1%. These effects are not driven by a continuation of pre-existing trends. Taken together, the results indicate that for small firms, price and quantity sensitivities are substitutes: greater price sensitivity reduces quantity sensitivity.

In the second part of the paper, we examine the mechanism behind our findings. We argue that forced pass-through of policy rate cuts crowds out banks' resources for screening and monitoring, which rise with loan size and are essential to expanding credit (Ottonello and Winberry, 2020; Mahoney and Weyl, 2017; Agarwal et al., 2015). Three patterns support this view. First, because screening and monitoring are more important for unsecured loans, we expect and find a stronger negative relationship between price and quantity sensitivity in this segment. Second, bank–firm pairs engaged in relationship lending are more affected. Relationship lending relies on superior screening and monitoring, and such lenders are known to shield firms from frictions by intertemporally smoothing prices and quantities (Petersen and Rajan, 1995; Bolton et al., 2016). Restricting price transmission undermines this smoothing ability, and we indeed observe weaker quantity responses for relationship lenders. Third, the effect is more pronounced in regions with higher banking competition, where limited markups leave lenders with fewer margins to fund screening and monitoring, forcing them to scale back lending in response to rate cuts. Finally, we show that banks were constrained in their ability to offset the decline in benchmark rates by increasing margins in the post-EBLR period.

Additional evidence highlights the upward-sloping marginal cost of lending. Defaults and interest rates increase with loan size even within the same firm, suggesting rising costs of default as lending expands. Moreover, the negative relationship between price and quantity sensitivities is weaker for healthier firms, consistent with the idea that the marginal cost of lending rises less steeply for stronger borrowers (Ottonello and Winberry, 2020). Taken together, these findings point to upward-sloping lending costs as the central mechanism driving our main result.

Together, these findings support the interpretation that upward-sloping marginal costs of lending—driven by screening, monitoring, and risk management—constitute the mechanism behind the substitution between price and quantity sensitivities.

Next, we examine the effect of the exogenous increase in interest rate sensitivity of bank loans on investment by borrowing firms. We find that firms more reliant on bank loans expand investment less in response to a policy rate cut under the EBLR regime than before. Specifically, we classify treated firms as those with a pre-EBLR share of bank loans above the median, and show that a one percentage point decline in policy rates is associated with an 80 percent differentially lower increase in investment for treated firms relative to others. This suggests that while mandatory pass-through strengthens price transmission, it may simultaneously weaken the investment channel of monetary policy.

In the final part of the paper, we conduct a series of robustness tests and address residual concerns. First, a potential concern is that we use the policy rate itself rather than its unanticipated component. Under the EBLR, however, lending rates for treated borrowers were mechanically tied to the policy rate and could adjust only after a policy change, regardless of expectations. Hence, anticipated and unanticipated cuts have identical effects on loan rates. Anticipatory adjustments in loan quantities are also unlikely, as lenders could not reprice before the policy move and both treated and control lenders faced the same expectations.

Second, as an external validation, we exploit the fact that the EBLR mandate also applied to retail borrowers. Using loan-level data from a large Indian credit bureau, we show that the sensitivity of lending to policy rates likewise declined differentially for banks relative to nonbanks in the post-EBLR period. Third, we analyze district-level lending data from the RBI to test if the lower sensitivity of lending to monetary policy scales up to the aggregate level. We find that the sensitivity of credit growth to policy rate changes is lower under the EBLR regime in districts with a relatively higher presence of small and medium firms subject to the reform. Finally, we implement additional placebo exercises to verify that our results are not driven by chance. Together, these robustness checks reinforce the conclusion that the observed substitution between price and quantity sensitivities is a consequence of the EBLR mandate rather than confounding factors.

This paper contributes to the literature on financial frictions in the transmission of monetary policy and their real effects (Bemanke and Gertler (1989); Bernanke and Gertler (1995); Bernanke et al. (1996)). Prior work has emphasized several frictions that shape the strength of monetary transmission: imperfect substitutability between deposits and bonds (Kashyap et al. (1993); Stein (1998); Kashyap and Stein (2000); Jiménez et al. (2012)), lending dependence on bank and firm net worth due to information asymmetry and agency problems (Kiyotaki and Moore (1997); Holmstrom and Tirole (1997); Gertler and Gilchrist (1994); Bernanke et al. (1996); Jiménez et al. (2014); Bolton and Freixas (2006); Bhardwaj et al. (2022)), and banks' market power that creates deposit franchises (Drechsler et al. (2017, 2021)). Most of these frictions are found to amplify the effects of monetary policy. More recent studies show that such amplification persists despite financial market development and is evident even among large U.S. firms (Ottonello and Winberry (2020); Gürkaynak et al. (2022); Cloyne et al. (2023); Jeenas (2023)).

While the extant literature examines how financial frictions shape the overall transmission of monetary policy, we focus on their role in governing the relationship between price and quantity sensitivities to policy rate cuts. Our setting is unique in that price sensitivity was exogenously altered, allowing us to identify this relationship directly rather than observing it as an equilibrium outcome. The evidence suggests that central bank efforts to mandate stronger pass-through of policy rate cuts can paradoxically result in weaker credit expansion. In doing so, we provide new evidence that financial frictions can dampen, rather than amplify, the real effects of monetary policy.

We also contribute to the literature on relationship banking, which examines how banks finance informationally opaque small firms (Boot (2000)). This literature emphasizes that banks incur costs associated with lending, which may be upward-sloping with loan amounts when collateral is scarce (Rajan (1992); Boot and Thakor (2000); Bharath et al. (2011); López-Espinosa et al. (2017); Li and Strahan (2021); Paravisini et al. (2023)). For the same reason, loan defaults and loss-given-default may also rise with loan size. To mitigate these risks and moral hazard problems induced by high interest rates, banks typically recover costs by intertemporally adjusting loan terms, smoothing interest rates and margins (Petersen and Rajan (1995), Bolton et al. (2016)). We add to this literature by showing that pricing flexibility is essential for sustaining relationship lending, and that curtailing such flexibility through regulatory mandates can constrain banks' willingness to extend relationship-based credit.

2 Institutional Details

2.1 Monetary Policy Implementation in India

Monetary policy in India is set by the Monetary Policy Committee (MPC) of the Reserve Bank of India (RBI). The MPC's primary mandate is to maintain price stability, and its main policy instrument is the repo rate, the fixed rate at which commercial banks can borrow reserves from the RBI against eligible collateral. Repo transactions are short-term, ranging from overnight to thirty days, after which banks repurchase the pledged securities.

On a day-to-day basis, the RBI seeks to keep the interbank call money rate within a narrow corridor around the repo rate. To achieve this, it conducts liquidity management operations. Variable rate repo auctions are used to infuse liquidity into the banking system, while variable rate reverse repo auctions are used to absorb excess liquidity. Together, these tools help maintain the call rate close to the policy rate.

The RBI has repeatedly expressed concerns about slow and incomplete monetary transmission, particularly with respect to rate cuts for small firms.[1] In response, it introduced a series of reforms to strengthen pass-through. On July 1, 2010, the RBI required banks to adopt a base rate system, linking loan pricing to their average funding costs (Tantri and Vishen (2025)). On April 1, 2016, the system was modified to use the marginal cost of funds–based lending rate (MCLR). Despite these reforms, however, internal RBI reports indicated that pass-through to lending rates—especially for small borrowers—remained sluggish and incomplete.[2]

2.2 The External Benchmarks Lending Rate (EBLR) Framework

To improve the transmission of monetary policy, the Reserve Bank of India introduced the External Benchmark Lending Rate (EBLR) framework on October 1, 2019.[3] Under this regime, commercial banks were required to link all new floating-rate retail loans and floating-rate loans to micro and small, and medium enterprises enterprises to one of three prescribed external benchmarks: the RBI's repo rate, the Government of India's three-month Treasury bill yield, or the six-month Treasury bill yield.[4] Effective April 1, 2020, the scope of the mandate was expanded to include medium enterprises.

Because the EBLR applied specifically to micro, small, and medium enterprises (SMEs), our analysis is restricted to this group, which we refer to as small firms throughout the paper. We follow the SME definition under the MSME Development Act of 2006. As per the Act, manufacturing (service) firms with investment up to INR 100 million (INR 50 million) in plant and machinery (equipment) were classified as SMEs.[5] The thresholds were revised in 2020 to INR 250 million for both manufacturing and service firms, with an additional turnover criterion of INR 2,500 million. Thus, the classification of SMEs is time-varying in our sample period. Table A1 of the Online Appendix tabulates the definition.

The EBLR marked a significant departure from earlier regimes by mandating the use

[1] See: `https://www.rbi.org.in/commonman/English/scripts/Notification.aspx?Id=786`

[2] See: `https://rbi.org.in/scripts/PublicationReportDetails.aspx?ID=878`. Also see: `https://rbidocs.rbi.org.in/rdocs/Bulletin/PDFs/01SP111217CBEF9077A25C48329B484120A3EF9B2F.PDF`

[3] See: `https://www.rbi.org.in/Scripts/NotificationUser.aspx?Id=11677&Mode=0`

[4] Note that a large proportion of loans (about 70%) issued in India are floating rate loans (Acharya (2017), Das (2015)).

[5] INR is an abbreviation for Indian Rupee.

of external reference rates in place of internally determined benchmarks.[6] It effectively constrained all three components of loan pricing: (i) the cost of funds had to be linked to an external benchmark, (ii) the credit risk premium was tied to the borrower's credit profile and could only be revised following a material change in credit assessment, and (iii) operating expenses could be revised no more than once every three years. Thus, banks could not modify loan pricing by adjusting the spread freely.

Our preliminary analysis of the MCA data shows widespread adoption of the EBLR. The share of new loans benchmarked to the repo rate rose immediately after the reform and reached about 80 percent of new loans within a few quarters.[7] By contrast, the regulation was not binding on nonbank financial institutions: only 4.17 percent of MSME loans from nonbanks were linked to external benchmarks during the same period.[8] This sharp and exogenous change in loan pricing rules creates a natural experiment to identify how mandatory price pass-through affects the quantity response of credit.

3 Hypothesis

This paper examines the impact of mandated transmission of policy rate cuts on the responsiveness of loan amounts to monetary policy. As discussed in the introduction, the existing literature on monetary transmission under financial frictions offers no clear ex-ante prediction: higher price sensitivity may be associated with either greater or weaker quantity sensitivity.

The theory of relationship banking (Rajan (1992); Boot and Thakor (2000); Petersen and Rajan (1995)) emphasizes that banks incur substantial fixed and variable costs to screen and monitor opaque borrowers. Continuous monitoring ensures borrower effort and appropriate project choice, while investments in expertise enable banks to lend in environments characterized by agency and information frictions. If larger loans require proportionally greater monitoring, then banks face an upward-sloping marginal cost of lending (Ottonello and Winberry (2020)).

In this framework, the lending rate charged by a bank can be decomposed into three

[6]We have reproduced the RBI circular that mandated EBLR in section A of the online appendix.

[7]The fraction is not close to 100% because there could be a difference between the methodology used in our study and the methodology used by banks for classifying MSME firms.

[8]We have presented the detailed break up in Table A2 of the online appendix.

components: (i) the cost of funds, largely determined by the policy rate; (ii) operational expenses related to screening, monitoring and cost of default, which increase with lending volume; and (iii) a markup, which depends on the elasticity of demand the bank faces. When policy rates decline, banks re-optimize by adjusting both quantities and markups. Some banks may expand lending and increase monitoring, while others may primarily use the additional margin to raise markups, with limited credit expansion.

Mandating full pass-through of policy rate cuts alters this trade-off. If such a mandate disproportionately compresses banks' ability to fund screening and monitoring relative to markups, then the responsiveness of lending to policy rate cuts will be lower than in the absence of regulation. By contrast, if the mandate primarily reduces markups without constraining monitoring expenditures (Dobbs (2004), Deneckere et al. (1997)), then the credit response to rate cuts may remain unchanged or even increase. Which outcome dominates depends on the structure of the banking market and the extent of financial frictions, making this an empirical question. Accordingly, our empirical tests evaluate whether mandatory pass-through of policy rate cuts reduces, increases, or leaves unchanged the sensitivity of loan quantities to monetary policy.

4 Data

Our analysis draws on three primary datasets: (i) loan-level data from the Ministry of Corporate Affairs (MCA), (ii) firm-level financial data from the Prowess database maintained by the Center for Monitoring Indian Economy (CMIE), and (iii) regulatory and macroeconomic data from the Reserve Bank of India (RBI). We describe each source below.

The MCA dataset provides loan-level information, including interest rates, and is limited to secured loans. We use these data in our tests relating to loan pricing. The dataset is constructed from loan contract filings obtained directly from the MCA. The Companies Act (2013) requires that all secured loans be registered with the MCA, making this a particularly comprehensive source.[9] We procured raw loan contract files for a random sample of 6,000 firms, paying the requisite per-firm fees. These contracts report borrower and lender

[9]The registration involves the creation of a charge, which is a right created by the borrower on its assets and properties in favor of a lender (Chopra et al. (2021)). The charge date denotes the date of loan filing to the government to create a charge on the loan.

identities, loan disbursement dates, amounts, interest rates, repayment schedules, and any subsequent modifications.[10]

The MCA data do not contain borrower or lender financials. To address this, we merge the loan-level dataset with firm-level balance sheet and income statement data from Prowess, using the Corporate Identification Number (CIN) to link firms across databases. This yields a matched sample of 5,053 unique firms. Because the MCA dataset does not contain standardized lender identifiers, we use a fuzzy-matching algorithm on lender names, followed by manual verification, to merge lenders with the Prowess dataset. We then cross-reference with the RBI's official list of financial institutions to classify lenders as banks or nonbanks.

We present the details of sample construction in Table 1. The sample period spans financial years 2017 to 2023, where the Indian financial year runs from April to March.12 We define the pre-EBLR period as the time before October 1, 2019, and the post-EBLR period as the time thereafter. There are 5,053 unique borrowers in the merged dataset. Of these, 2,142 are small firms.[11] The loan-level data corresponding to these 5,053 borrowers contain 30,369 observations in total, of which 6,205 loans were extended to small firms. The sample includes 63 unique banks and 134 unique nonbanks lending to small firms.

We use MCA data for tests related to interest rates and Prowess data for tests related to lending quantities. This distinction reflects the structure of the datasets: the MCA database reports loan-level interest rates but only for secured loans, whereas Prowess provides firm-level borrowing totals across both secured and unsecured credit. Accordingly, we organize the MCA data at the loan level for the interest rate analysis. Although we do not observe interest rates for unsecured loans, restricting the first-stage analysis to secured loans does not raise generalizability concerns, as the EBLR regulation mandated full pass-through regardless of loan type and did not distinguish between secured and unsecured lending.

For lending quantities, we rely on Prowess, which aggregates firms' total borrowings separately from banks and nonbanks. Including unsecured loans is crucial for assessing how monetary policy affects loan amounts. As discussed in Section 3, screening and monitoring costs drive the relationship between policy rate changes and lending behavior, and these

[10] We drop observations where the interest rates are less than the benchmark repo rates since RBI does not allow the banks to charge interest rates below the repo rate and where the interest rate is larger than 100%. Such cases are likely to contain incorrect information about the interest rate. However, we test and find that our results are robust to including the above cases in the sample.

[11] These 5,053 borrowers contribute about 30% of the total sales of firms in CMIE prowess database.

frictions are particularly salient for unsecured credit. Because such informational frictions matter far less when loans are fully collateralized, an analysis based solely on MCA secured loans would not provide a reliable measure of the sensitivity of loan quantities to monetary policy. However, Prowess identifies only a firm's lenders, not the allocation of borrowing across them. We therefore structure the Prowess data at the firm–lender-type–year level for our loan-amount tests.

To structure the data at firm–lender type–year level, we need the borrowings from banks as well as from non-banks. The total borrowings data for each firm, obtained from the Prowess database, mainly aggregate total borrowings from banks, non-banks, and related parties. Borrowings from banks are directly available in Prowess, while borrowings from related parties can be obtained from its related-party transactions section. Using these two components, we estimate borrowings from non-banks by subtracting borrowings from banks and borrowings from related parties from the total borrowings.

We begin with 38,697 unique firms. To identify small firms under the official definition, we require data on investment in plant and machinery as well as revenue. These variables are available for 29,168 firms, of which 16,133 qualify as small firms. Among these, 3,900 firms report positive new borrowings from both banks and nonbanks in at least one year. This sample yields 10,848 observations in our firm–lender type–year panel.

As discussed in Section 2.2, the EBLR framework was also applicable to retail loans. To externally validate our findings in this market, we obtain loan-level data from a large credit bureau in India. These data are organized at the customer–lender type level and include unique borrower identifiers, the lender type (bank or nonbank), loan origination dates, and repayment dates.

We obtain regulatory and monetary policy information from the RBI. Daily repo rates are obtained directly from the RBI's website; we compute monthly and annual averages by weighting daily rates according to the number of days for which each rate persisted. In addition, we use two key regulatory datasets from the RBI: (i) the official registry of banks and nonbanks, and (ii) aggregate lending data at the district–year level.

4.1 Summary Statistics

Table 2 reports summary statistics for all dependent and explanatory variables used in the analysis. For each variable, we present the number of observations, mean, median, and the 1st and 99th percentiles. The sample is restricted to small firms. Panel A summarizes the loan-level data. The median interest rate is 9.1 percent, and the median loan amount is INR 17 million. Panel B reports firm–year level variables used as controls in our firm-level tests. On average, firms are profitable, with an average leverage ratio of around 40 percent and positive investment rates. The median annual borrowing from banks (INR 16 million) exceeds the median borrowing from nonbanks (INR 8 million).

Panels C and D present lender–year and lender–firm–year variables. Banks report an average return on assets of 5 percent and a gross NPA ratio of 5 percent. Nonbanks appear somewhat weaker, with an average return on assets of 4.5 percent and a gross NPA ratio of 5.2 percent. Panel D further reports time-varying measures of the strength of bank–firm relationships. Panel E summarizes the monetary policy variable, the repo rate. Definitions of all variables are provided in Table A3 in the Online Appendix.

5 Empirical Strategy

As described in Section 2.2, the EBLR regulation applied specifically to bank lending to small firms by mandating that interest rates on these loans be tied to an external benchmark. A natural identification strategy would be to compare monetary policy transmission for small firms relative to large firms. However, this approach faces an important endogeneity concern.

The post-EBLR period coincides with the COVID-19 pandemic, during which the government and regulators implemented several interventions targeted directly at small firms, including the Emergency Credit Line Guarantee Scheme (ECLGS) and a moratorium on loan repayments.[12] These measures provided additional liquidity and risk buffers for small firms, potentially affecting their cost of credit independent of the EBLR. Thus, any comparison between small and large firms may confound the effect of EBLR with contemporaneous policy support. Even a sharp cutoff at the definition of small firms is unhelpful, since COVID-era interventions also varied at these thresholds.

[12] ECLGS was a scheme launched by the government of India to incentivize lenders to provide credit of up to Rs. 3 lakh crore to small firms at low cost during the COVID crisis.

We therefore employ an alternative strategy that exploits the fact that the EBLR mandate applied only to banks and not to nonbanks, allowing us to compare bank and nonbank lending within the sample of small firms. This design is appropriate for two reasons. First, our review of regulatory measures during the COVID period shows that while small firm-specific relief was extensive, none of the interventions created differential treatment between banks and nonbanks in lending practices or rate-setting. In particular, schemes such as the ECLGS and repayment moratoriums were uniformly applicable across all types of lenders.[13] Second, the data confirm that banks adopted the EBLR extensively for lending to small firms, whereas nonbanks continued to rely on their pre-existing benchmarking frameworks.

A potential concern is that we use the policy rate itself, rather than its unanticipated component, as the measure of monetary policy stance. In most settings, policy rates respond to macroeconomic conditions or expectations thereof, raising endogeneity concerns when used to study firm outcomes. Our context, however, differs fundamentally. The EBLR reform mechanically linked banks' benchmark lending rates for small firms to the policy rate and restricted their ability to offset benchmark changes through spread adjustments. Consequently, lending rates for treated borrowers could adjust only after a policy rate change—regardless of whether the change was anticipated or not—and by the same amount in both cases. For example, a one–percentage point cut in the policy rate, whether expected or unexpected, would reduce loan rates to small firms by exactly one percentage point once the change occurred. Hence, isolating the "unexpected" component of the policy rate would miss the very mechanism of interest: the mandated pass-through under the EBLR.

A related concern is that loan quantities might adjust in anticipation of policy changes. This would require lenders to expand lending before cutting rates, which is implausible under a regime where rate adjustments can occur only after the policy rate changes. Moreover, any anticipatory behavior would likely be common to both treated and control lenders, as both face similar information about the policy rate path. In our within-borrower design, even the control group comprises banks, making differential anticipation even less likely. Nonetheless, to account for any residual concerns, we control for the lender's prior-year loan amount to the borrower and its interaction with the current policy rate in all specifications.

Another concern with our approach is that firms borrowing from banks and nonbanks may differ systematically. To address this, we include firm-by-time fixed effects in our panel

[13] We list the series of measures in section D of online appendix.

specifications, following Khwaja and Mian (2008). These absorb both time-invariant and time-varying shocks at the firm level, ensuring that identification relies on within-firm comparisons across bank and nonbank loans.

A reader may also worry that systematic differences between banks and nonbanks, such as size, capitalization, or asset quality, could interact with monetary policy transmission in a time-varying way. To mitigate this, we follow Acharya et al. (2020) and control for observable lender characteristics: size, the ratio of nonperforming assets to total assets, capital adequacy, and return on assets (ROA). We begin by identifying the lenders associated with each firm from the Prowess database. In our firm–year–lender type panels, we then construct lender-type level controls by taking the simple average of these characteristics across all banks and, separately, across all nonbanks associated with a firm in a given year. For example, if a firm is served by three banks and two nonbanks, we compute the average return on assets (ROA) of the three banks and separately the average ROA of the two nonbanks for that year, and use these as control variables. Because loan-specific weights are unavailable, we rely on unweighted averages. In addition, to account for the time-varying bank-firm relationships, we also include the outstanding and new loans between the lender-firm pair in the previous year as control variables.

Finally, to allow for heterogeneous transmission conditional on lender characteristics, we interact these controls with $Repo_{t-1}$. This captures any endogenous changes in lender fundamentals after EBLR. For instance, if the ROA of the banks systematically reduces after the EBLR, any endogenous effect on the transmission due to a decline in ROA will be captured by the interaction between ROA and the policy rate. Additionally, we show that our results are robust to an alternative identification that exploits variation in the effect of EBLR mandate within banks, subsequently in section 5.4.

5.1 Baseline Transmission pre-EBLR: Dynamic Responses

We start by estimating the base-level transmission in the pre-EBLR period for banks. We employ a distributed lag framework to assess the dynamic response of interest rates to policy rates during the pre-EBLR period. This approach helps estimate the speed and magnitude of monetary policy transmission. We use the following specification for the test.

$$Y_{ijt} = \beta_0 + \sum_{n=1}^{n=12} \beta_n \cdot Repo_{m-n} + \gamma_i + \delta_j + \epsilon_{ijt} \tag{1}$$

Where Y_{ijt} is the interest rate charged by lender j to firm i on a loan issued on date t. The variable $Repo_{m-n}$ represents the repo rate n months before the current month. The coefficient β_n represents the sensitivity of the current period's interest rate to the policy rate changes n months before the current month. Panel A of Figure 1 plots the transmission of policy rate changes to small-firm lending rates for banks in the pre-EBLR period. Transmission was sluggish: lending rates did not respond significantly to contemporaneous or lagged policy rate changes, and the cumulative impact up to twelve months was indistinguishable from zero. In contrast, Panel B shows that under the EBLR regime, a one-percentage-point policy rate cut led to an immediate 97-basis-point reduction in lending rates in the following month, with cumulative responses over three, six, and twelve months statistically indistinguishable from one. We also find that the cumulative one-year transmission is significantly higher in the post-EBLR period than in the pre-EBLR period. Consistent with the regulatory design, Panels C and D show that nonbank pass-through remained unchanged: coefficients on lagged policy rates are statistically insignificant in both the pre- and post-EBLR periods.

We next examine the dynamic effects on loan volumes using the same framework, except that we aggregate at the firm–year level and present results at annual frequency. Figure 2 reports the results. Panels A and B (C and D) show the responses for banks (nonbanks). For banks, borrowing volumes in the pre-EBLR period exhibit a negative response to policy rates in the year following a policy change—that is, rate cuts lead to significant credit expansion. In contrast, under the EBLR regime, loan volumes show no significant response to policy rate movements. The difference between the pre- and post-EBLR coefficients is statistically significant, indicating a decline in the transmission of monetary policy to loan quantities after the reform. For nonbanks, we find no significant transmission either before or after the EBLR implementation, nor any meaningful difference between the two periods.

5.2 First Stage: Policy Rate Transmission to Interest Rate

We next formally test the effectiveness of the EBLR regime in changing the transmission of monetary policy rates to lending rates for small firms using a triple differences regression framework. We ask whether banks show a differentially higher transmission of policy rates to lending rates on loans issued to small firms compared to non-banks after the EBLR regime is implemented. To examine the thesis, we estimate the following specification using loan-level panel data limited to small firms. The sample period is 2017 to 2022.

$$\begin{aligned} Y_{ijt} =& \beta_0 + \beta_1 \cdot (Post_t \times Bank_j \times Repo_{m-1}) + \beta_2 \cdot (Post_t \times Bank_j) + \beta_3 \cdot (Bank_j \times Repo_{m-1}) \\ &+ \beta_4 \cdot (Post_t \times Repo_{m-1}) + \beta_5 Repo_{m-1} + \beta_6 Post_t + \beta_7 X_{jt} + \gamma_{it} + \delta_j + \epsilon_{ijt} \end{aligned} \tag{2}$$

The dependent variable Y_{ijt} is the interest rate charged by lender j to firm i on a loan issued on date t. $Repo_{m-1}$ is the weighted average repo rate for the previous month as defined in section 4. The variable $Post_t$ takes a value of 1 after the RBI implemented the EBLR framework, which became effective on October 1, 2019, and 0 for the period before that. The bank indicator $Bank_j$ is set to 1 if the lender is a bank and zero if it is a non-bank. We use firm $\times$ year (γ_{it}) and lender (δ_j) fixed effects, as described in Section 5. We double-cluster the standard errors at the firm and time levels. The control variables included in X_{jt} are as listed in section 5 above. We also include the interaction of the control variables with $Repo_{m-1}$.

The key interaction term of our interest $Post_t \times Bank_j \times Repo_{m-1}$ assesses whether there is a differential transmission of the repo rate to the interest rate between banks and non-bank lenders in the post-regulation period. We hypothesize the coefficient to be positive and statistically significant, indicating that banks exhibit a stronger transmission in the post-regulation period than non-bank lenders.

Table 3 presents the regression estimates. Columns (1) and (2) show the results with a sample limited to small firms. We include bank-year level control variables described in Section 5 and their respective interactions with $Repo_{m-1}$ in column (2). We also include firm $\times$ year and lender-level fixed effects in all columns.

The coefficients on $Repo_{m-1}$ and $Repo_{m-1} \times Bank$ are statistically indistinguishable from zero, which is in line with RBI's findings that the transmission of policy rate was weak for banks in the pre-EBLR period. Importantly, the coefficient on the triple interaction term is positive and statistically significant. The magnitude of the coefficient in column (1) implies that a one percentage point decline in repo rate is associated with about an 82 basis point decline in lending rate on loans issued by banks vis-a-vis non-banks in the post-EBLR period. We found in section 5.1 that the baseline transmission was statistically indistinguishable from zero in the pre-EBLR period for both banks and non-banks. Hence, the economic magnitude of 82 basis points implies that the EBLR mandate makes the transmission of policy rate in the immediate next month almost one-to-one, as expected by the EBLR regime.[14]

A potential concern is that the observed divergence in monetary transmission between banks and nonbanks during the EBLR period reflects other contemporaneous factors—such as the pandemic—rather than the reform itself. To address this, we exploit the fact that the EBLR mandate did not apply to large borrowers and conduct a placebo test. If monetary transmission dynamics changed for reasons unrelated to the reform, we should observe similar effects in this subsample. We re-estimate equation 2 excluding small firms and report the results in columns (3) and (4) of Table 3. Column (4) includes bank-year controls and their interactions with $Repo_{m-1}$. The estimated effects are substantially smaller, and the triple interaction term is statistically indistinguishable from zero.[15] These results suggest that the differential transmission we document is not driven by broader shifts in monetary policy transmission between banks and nonbanks.

5.2.1 Test for Pre-existing Trends

To verify that our results are not driven by pre-existing trends in the co-movement of lending and policy rates, we plot average interest rates for banks and nonbanks alongside the policy rate at a quarterly frequency in Figure 3. The black, blue, and red lines represent bank, nonbank, and policy rates, respectively. To maintain consistency with our baseline regressions, we restrict the sample to firms that borrow from both banks and nonbanks in a given year.[16] Panels A and B present the results for small and large firms, respectively, with the vertical line marking the introduction of the EBLR regime.

For small firms (Panel A), there is no evidence of systematic convergence or divergence in bank and nonbank interest rates prior to the reform. Both sets of rates were largely

[14] A t-test is unable to reject the hypothesis that the coefficient on the interaction term is equal to one.
[15] A t-test for difference in coefficient confirms that the decline in sensitivity is statistically significant.
[16] Figure A1 in the Online Appendix shows similar patterns using the full sample.

unresponsive to policy rate movements in the pre-EBLR period—for example, during the substantial policy rate cuts between the third quarter of 2018 and the second quarter of 2019, lending rates moved little. In contrast, after the EBLR's introduction, bank rates closely track policy rate declines starting in March 2020, while nonbank rates remain largely unresponsive. For large firms (Panel B), bank and nonbank interest rates exhibit similar responsiveness to policy rates both before and after the reform, with no sudden post-EBLR decline in bank rates as observed for small firms. Taken together, these patterns confirm the absence of pre-existing differential trends and support the interpretation that the sharp divergence in interest rate transmission between banks and nonbanks during the EBLR period reflects the impact of the reform rather than other contemporaneous factors.

5.3 Main Results: Policy Rate Transmission to Lending

Having established that the EBLR mandate led to a differential increase in the transmission of policy rates to bank lending rates, we now turn to our central question: how did this affect the sensitivity of lending quantities to a reduction in policy rate? As outlined in Section 3, the impact of forced pass-through on lending is theoretically ambiguous. To test the competing hypotheses, we examine the borrowing response of small firms to policy rates after the EBLR reform.

We obtain firm-level borrowing data from the Prowess database, which reports outstanding borrowing separately from banks and nonbanks. Using these data, we construct a firm–lender type–time panel of borrowing amounts, where lender type identifies banks versus nonbanks. We then estimate the following regression specification on the subsample of small firms, covering the period 2017–2023.

$$\begin{aligned} Y_{ijt} = {} & \beta_0 + \beta_1 \cdot (Repo_{t-1} \times Post_t \times Bank_j) + \beta_2 \cdot (Repo_{t-1} \times Post_t) + \beta_3 \cdot (Repo_{t-1} \times Bank_j) \\ & + \beta_4 \cdot (Post_t \times Bank_j) + \beta_5 \cdot Repo_{t-1} + \beta_6 \cdot Post_t + \beta_7 \cdot X_{jt} + \gamma_{it} + \alpha_j + \epsilon_{ijt} \end{aligned} \tag{3}$$

The dependent variable Y_{ijt} is the natural logarithm of borrowing amount by firm i from lender-type j in year t. $Repo_{t-1}$ is the weighted average repo rate in the previous year as defined in section 4. The rest of the variables are as defined in equation 2. The coefficient of interest is β_1, which represents the differential transmission of policy rates to lending amounts by banks compared to non-bank lenders in the post-regulation period.

We include firm–year fixed effects (γ_{it}) and lender-type fixed effects (α_j) in all specifications. Standard errors are double-clustered at the firm and year levels. The control variables in X_{jt} are as defined in Section 5.

Table 4 presents the estimates of the above regression. Columns (1) & (2) present the estimates for samples limited to small firms. In Column (2), we include lender type-year level control variables and their respective interactions with $Repo_{t-1}$ in the specifications. As expected, the coefficient on $Bank \times Repo_{t-1}$ is negative and statistically significant, indicating that banks were relatively more likely to transmit policy rates to borrowing in the pre-EBLR period. Importantly, the coefficient of $Post \times Bank \times Repo_{t-1}$ is positive and statistically significant. In column (1), the coefficient of 0.186 on the triple interaction term implies that in the post-mandate period, the transmission of policy rates to bank borrowing of small firms significantly weakened. When the repo rate decreases by one percentage point, expansion in borrowing by small firms through banks reduces by exp(0.186)-1 $\approx$ 20.4%, compared to non-bank borrowing. The results are even more pronounced when we include control variables in Column (2).[17] Thus, the mandatory pass-on of policy rates during periods of rate reductions results in a slower expansion of bank lending in the EBLR regime than before.

We do not expect a significant difference in monetary policy transmission between banks and non-banks for lending to large firms. To test the above, we estimate equation 3 using a sample that excludes small firms. We present the results in columns (3) and (4) of Table 4. The coefficient of the triple interaction term is statistically indistinguishable from zero. The result suggests that the policy rate transmission to borrowing did not decrease for large firms.[18] The above results are in line with the thesis that banks expand lending relatively less in response to cuts in policy rates.

5.4 Alternative Identification: Within Bank Heterogeneity

As noted in section 5, our baseline identification compares banks with nonbanks. While we control for interactions between observable lender characteristics and policy shocks in equations 2 and 3, residual concerns may remain if unobservable, time-varying shocks affected

[17]We also estimate the equation 3 using a sample restricted to firms with MCA interest rate data and present the results in Table A4 of the online appendix. Our results remain qualitatively similar.

[18]A t-test of the difference between the coefficients of the two samples reveals that the difference is statistically significant.

banks and nonbanks differently and coincided with the EBLR mandate. To address this concern, we develop an alternative identification strategy that restricts the sample to banks only.

The idea is to exploit cross-sectional heterogeneity in banks' pre-EBLR transmission of policy rates. Although all banks were subject to the EBLR mandate, treatment intensity varied according to banks' pre-reform speed of transmission under the earlier Marginal Cost of Funds–based Lending Rate (MCLR) regime, described in Section 2. While the MCLR formula was standardised by the central bank, banks retained considerable accounting discretion over key inputs such as operating expenses and overheads (Acharya, 2017). The RBI later acknowledged that this discretion undermined policy transmission—prompting the shift to the EBLR framework. Consequently, variation in transmission speed under MCLR reflected managerial and accounting choices rather than fundamental business differences. Consistent with this interpretation, Table A5 in the Online Appendix shows no significant differences between high- and low-transmission banks in either their asset or liability structures. Hence, it is unlikely that underlying business heterogeneity drives our results. Banks that transmitted policy changes more slowly before the reform were simply forced to transmit them more fully once the benchmark became externally determined under the EBLR.

We operationalise this approach as follows. For each bank, we compute the monthly ratio of the change in its benchmark rate (MCLR) to the change in the repo rate over the five years preceding the EBLR regime. We then average this ratio across months for each bank. Banks with an average ratio between 0.8 and 1.2 are classified as *control banks*, since their benchmarks already moved closely with the policy rate. Banks with ratios outside this range are classified as *treated banks*, since their benchmarks deviated more substantially from the policy rate and thus were more directly affected by the EBLR mandate.

5.4.1 Transmission to Interest Rate

Having identified the treated and control banks, we first test the differential change in transmission to interest rates in treated banks post-EBLR by estimating the following specification using loan-level data and a sample limited to banks.

$$\begin{aligned} Y_{ijt} =& \beta_0 + \beta_1 \cdot Post_t \times Treat\_Bank_j \times Repo_{m-1} + \beta_2 \cdot Post_t \times Treat\_Bank_j + \beta_3 \cdot Treat\_Bank_j \\ & \times Repo_{m-1} + \beta_4 \cdot Post_t \times Repo_{m-1} + \beta_5 Repo_{m-1} + \beta_6 Post_t + \beta_7 X_{jt} + \gamma_{it} + \delta_j + \epsilon_{ijt} \end{aligned} \tag{4}$$

Where the indicator variable *treat_bank* takes a value of one for treated banks (as defined above) and zero for control banks. The rest of the variables are the same as in equation 2.

We present the results in Table 5, organized in the same manner as Table 3. We include firm $\times$ year and lender fixed effects in all columns. We also include bank-year level control variables in the even-numbered columns.

Panel A shows the results for small firms. Consistent with the estimates in Table 3, the coefficient on the triple interaction term is positive and statistically significant. The banks with low sensitivity of benchmark rate to the policy rate in the pre-EBLR period are differentially more likely to transmit decreases in policy rates to interest rates in the post-EBLR period.

Panel B reports the corresponding results for large borrowers, who were not subject to the EBLR mandate. As expected, the triple interaction coefficient is small and statistically insignificant, confirming that the observed effect is specific to the mandated segment.

Taken together, this within-bank heterogeneity design reinforces that the stronger pass-through is driven by the EBLR mandate itself, rather than by concurrent shocks affecting banks and nonbanks differently.

5.4.2 Transmission to Borrowing

We next focus on loan quantity. We ask whether quantity transmission also changes post-EBLR. As per the hypothesis discussed in section 3, we expect the transmission of decreases in policy rates to lending quantity to decrease for the treated banks in the post-EBLR period. We can not conduct this test using a similar panel as in section 5.3, where we create a firm-lender type-year panel. This is because, while the Prowess database provides information about borrowings from banks and non-banks separately, it does not provide us with information about how much is borrowed from treated and control banks for our within-bank identification. Therefore, we define the exposure of borrowers to treated banks to conduct the test at the firm-year level.

The Prowess database reports the list of bankers for each firm but does not provide information on transactions or borrowing amounts. Using the banker information from the year prior to the implementation of the EBLR regime, we classify firms into treatment and control groups. A firm is assigned to the treatment group if it was associated with at least one bank that falls under the treated category. Other firms constitute the control group. We expect the sensitivity of borrowings to policy rates to decline significantly for the treated firms in the post-EBLR period. We test the thesis using the following specifications.

$$\begin{aligned} Y_{it} = &\beta_0 + \beta_1 \cdot (Repo_{t-1} \times Post_t \times Treat\_Firm_i) + \beta_2 \cdot (Repo_{t-1} \times Post_t) + \beta_3 \cdot (Repo_{t-1} \\ &\times Treat\_Firm_i) + \beta_4 \cdot (Post_t \times Treat\_Firm_{it}) + \beta_5 \cdot Repo_{t-1} + \beta_6 \cdot Post_t + \\ &\beta_7 \cdot X_{it} + \gamma_i + \alpha_t + \epsilon_{it} \end{aligned} \tag{5}$$

Where the indicator variable *treat* takes a value of one for treated firms and zero otherwise. The variables γ_i and α_t represent firm and year fixed effects, respectively. The variable X_{it} represents firm-year level control variables listed in equation 9 and their respective interactions with $Repo_{t-1}$. The rest of the variables are the same as in equation 3.

We present the findings in Table 6. The organization of the table mimics Table 4. We include firm and year fixed effects in all columns. We also include firm-year level control variables and their respective interactions with $Repo_{t-1}$ in the even-numbered columns. In line with the estimates in Table 4, we find that the coefficient on the interaction term $Repo_{t-1} \times Post \times Treat\_Firm$ is positive and statistically significant in panel A. Hence, the transmission of policy rate loosening to lending differentially declines significantly for firms with exclusive relations with treated banks in the post-EBLR period. We do not find any effect in panel B (large firms).[19]

The above results, through alternative identification, corroborate our main results and substantially allay the concern that the results could be due to unobservable differences between banks and non-banks.

6 Mechanism

We now turn to the mechanism underlying our main findings. As outlined in Section 3, our central explanation for the reduced sensitivity of loan quantities is that the EBLR mandate constrained banks' ability to expand screening and monitoring expenditures when policy rates declined. With reduced flexibility to preserve margins, banks were less able to fund the incremental costs required to expand lending. To test this mechanism, we first exploit

[19] The results are largely unchanged even if we define exposure based on exclusive relation with treated banks.

cross-sectional heterogeneities at the bank and firm levels. We then provide direct evidence through the relationship between loan performance and loan sizes, monitoring and screening-related expenses, and constraints on banks' margins.

6.1 Relationship Banking

A well-established result in the literature is that relationship lenders possess superior screening and monitoring ability and can mitigate borrower moral hazard by intertemporally smoothing loan terms (Petersen and Rajan (1995)). A mandatory pass-through of policy rates, however, may blunt this flexibility by constraining banks' ability to fund monitoring expenditures. If our proposed mechanism is correct, the decline in transmission of policy rate cuts to lending quantity should be more pronounced for firms engaged in relationship borrowing. To test the thesis, we create a measure of relationship lending in the spirit of Tantri and Vishen (2025).

Our measure captures three independent dimensions of the firm–bank relationship. The first is industry-specific lending expertise, defined following Boot and Thakor (2000), Paravisini et al. (2023). Using the MCA loan-level dataset, we calculate the proportion of each bank's share of lending in each industry relative to its total loan book as of the year 2020 (the year preceding the treatment year). Industry classification for a firm is determined using 2-digit National Industrial Classification(NIC) codes obtained from the Prowess database. A bank is classified as an "expert" in industries where its share of lending exceeds the median share of its portfolio. We create an indicator variable equal to one for bank–industry pairs where the bank is an expert and zero otherwise.

The second dimension is geographical proximity. Using RBI branch location data, we calculate each bank's branch density within a district as the ratio of the bank's branches to the total number of branches in that district (as of 2020). For each district, we then classify banks with density above the median as having high branch presence and those below as having low presence (Bolton et al. (2016), Agarwal and Hauswald (2010)). For each firm-bank pair, we designate them as being geographically proximate if the corresponding bank has high bank presence in the district in which the respective firm is headquartered. The corresponding indicator variable is one for such firm-bank pairs and zero otherwise.

The third dimension is lending intensity (Bharath et al. (2011)). We identify firm–bank pairs with more than two loan transactions in the five years preceding 2020 as having a stronger history of interaction. The corresponding indicator variable is one for such firm-bank pairs and zero otherwise.

We use the three indicator variables corresponding to these dimensions and sum them to form an overall 'relationship' score ranging from 0 to 3. Firm–bank pairs with scores above the median (equal to one) are classified as relationship pairs. We identify firms that have at least one relationship bank based on the above measure as relationship borrowers.

We then estimate regression equation 3 separately for firms with at least one relationship bank and firms with no relationship bank. We restrict the sample to small firms. Panel A of Table 7 presents the estimates. Column (1) shows that for relationship borrowers, the coefficient on $Post \times Bank \times Repo_{t-1}$ is positive and statistically significant, with a magnitude of 0.526. This implies that following a one-percentage decrease in the repo rate, banks expand lending by (exp(0.526) - 1) $\approx$ 69% less than non-banks in the post-EBLR period for firms with relationship-based borrowing.[20]

By contrast, for non-relationship borrowers, shown in column (2), the triple interaction coefficient ($Post \times Bank \times Repo_{t-1}$) is statistically indistinguishable from zero and low in magnitude. This suggests that the reduced transmission of monetary policy through bank lending during easing is substantially stronger for relationship borrowers in the post-EBLR period. A t-test of the difference between the coefficients in the two samples reveals that the difference is statistically significant and confirms that the decline in monetary transmission is concentrated among relationship borrowers.

These results are consistent with our proposed mechanism that when regulatory constraints limit banks' ability to adjust margins, relationship lenders—whose advantage relies on flexible pricing to recover monitoring costs—reduce lending most sharply. The results also indicate that the results are not due to borrowers switching from banks to non-banks during the EBLR regime. If the results were due to such borrowers, we should have found larger effects in borrowers with no banking relationship.

[20]We do not include the bank-year level control variables, because we are testing for the banking relationship channel. The inclusion of average characteristics of the firms' lenders will absorb some of the relationship effects. However, our results are qualitatively similar after including the control variables.

6.2 Bank competition

As outlined in Section 3, lenders face a trade-off between earning higher markups and incurring screening and monitoring expenses when setting interest rates. The trade-off exists only in cases where lenders have significant market power. In highly competitive markets, banks can afford not to pass on policy rate cuts only if they use the resulting margin to finance screening and monitoring expenses in order to expand lending. They cannot use the rate cuts to increase markups. By contrast, banks with significant market power may offset changes in expenses by adjusting markups while maintaining lending. Consequently, mandatory pass-through is more likely to constrain banks' ability to finance monitoring and expand credit in competitive markets than in concentrated markets. Thus, if reduced monitoring is the channel for weaker credit transmission under EBLR, the effect should be stronger in regions with ex-ante high banking competition.

To test the above thesis, we first identify regions with high banking competition. Using RBI data on bank branch locations for 2020, we compute each bank's branch share within a district, square these shares, and sum them to obtain a Herfindahl–Hirschman Index (HHI) of branch concentration. Districts above the median HHI are classified as low-competition (high concentration), and those below the median as high-competition (low concentration). We then estimate equation 3 separately for the two groups.

Panel B of Table 7 presents the results. Column (3) shows the estimates for high-competition districts. The coefficient on $Post_t \times Bank_j \times Repo_{t-1}$ is positive and statistically significant. The estimate of 0.5 in Column (3) implies that a one–percentage point decline in the repo rate is associated with a differential reduction in bank lending of (exp(0.5)-1) $\approx$ 65% relative to nonbanks in the post-EBLR period.

By contrast, in low-competition districts (Column (4)), the results are substantially weaker. The coefficient is negative but statistically indistinguishable from zero in Column (4). A t-test confirms that the difference in coefficients across the two groups is statistically significant. Consistent with our proposed mechanism, the decline in transmission is significantly stronger in regions with high banking competition.

6.3 Healthy versus Unhealthy Firms

Ottonello and Winberry (2020) show that financially healthy firms face relatively flat marginal cost curves, whereas financially constrained firms face steeper ones. As discussed in Section 3, our proposed mechanism for lower transmission of policy rate cuts to loan quantities under the EBLR requires upward-sloping marginal costs of lending. Consequently, if this channel is at work, we should observe a stronger decline in transmission for financially unhealthy firms and a weaker effect for healthy firms.

Following Ottonello and Winberry (2020), we measure firm health using the interest coverage ratio (ICR), defined as the ratio of profit before interest and tax to interest expense. This measure proxies for a firm's distance to default: firms with low ICRs do not generate sufficient profits to cover their interest liabilities and are more likely to default.[21] We classify small firms with an ICR above the median level of ICR among all firms as financially healthy and the rest as unhealthy.

We estimate equation 3 separately for healthy and unhealthy firms, with results reported in Panel C of Table 7. In column (5), we restrict the sample to unhealthy firms, while in column (6), we restrict the sample to healthy firms. Both specifications include firm–lender type and year fixed effects.

For unhealthy firms, the coefficient on the triple interaction term Post × Bank × Repo is positive and statistically significant. In Column (5), the estimate of 0.312 implies that a one–percentage–point decline in the repo rate is associated with a differential reduction in bank borrowing of approximately exp(0.312)-1 $approx$37% relative to nonbanks in the post-EBLR period. Thus, banks reduce the responsiveness of their lending to policy rate cuts when lending to financially weak firms.

By contrast, for healthy firms, the coefficient on the triple interaction term is smaller and statistically indistinguishable from zero. As expected, the decline in transmission under the EBLR is significantly weaker for firms with stronger balance sheets. These findings are consistent with our proposed channel that the reduced credit expansion arises from upward-sloping marginal costs of lending to financially constrained firms.

6.4 Unsecured versus Secured Borrowings

Our first test of the proposed mechanism exploits the difference between secured and unsecured loans in their reliance on screening and monitoring. Unlike secured lending, where

[21] See Ottonello and Winberry (2020) for a discussion of how distance to default shapes marginal lending costs.

collateral mitigates risk, unsecured lending depends more heavily on lenders' monitoring and screening efforts (Benmelech et al. (2024); Jackson and Kronman (1978)). As explained in section 3, the new regime restricts the ability of the banks to spend on screening and monitoring efforts. Therefore, we expect the reduction in sensitivity of lending to policy rates post-EBLR mandate to be stronger for unsecured lending.

To conduct the above test, we need measures of firms' secured and unsecured borrowings. The Prowess database reports total borrowings from banks and nonbanks but provides information on unsecured loans for less than 10 percent of observations. To overcome this limitation, we combine Prowess and MCA data. Specifically, we obtain total borrowings from banks and nonbanks from Prowess. We then compute secured borrowings by summing the secured loan amounts reported in the MCA dataset across all lenders for each firm. Unsecured borrowings are calculated as the difference between total borrowings and secured borrowings, separately for banks and nonbanks, at the firm–lender type–year level. We then estimate equation 3 separately for secured and unsecured borrowing.

Columns (1) and (2) of Table 8 report the results. Column (1) presents the estimates for unsecured loans. The coefficients on the triple interaction term are positive and statistically significant. The coefficient of 0.129 implies that a one-percentage-point decrease in the repo rate is associated with a differential reduction in bank borrowing of approximately $\exp(0.129)-1 \approx 13.7\%$ relative to nonbanks in the post-EBLR period.[22]

Column (2) shows the estimates for secured loans. Here, the triple interaction term is statistically insignificant across specifications, suggesting no differential change in monetary policy transmission through the bank channel for secured lending after the EBLR.[23]

6.5 Short Term versus Long Term Borrowings

As noted in Section 2, the EBLR regulation allows banks to benchmark loans either to the policy rate or to yields on comparable long-term government bonds. In practice, banks predominantly use the policy rate for short-term loans, which naturally align with short-term risk-free rates, and rely on long-term government bond yields for longer-maturity loans. This institutional feature implies that, if our results are indeed driven by the EBLR reform, the

[22]We need to make the exponential transformation since the dependent variable is in log scale.

[23]A test for the difference between the coefficients in the two regressions reveals that the difference is statistically significant.

effects should be more pronounced for short-term credit. Moreover, because screening and monitoring costs contain a substantial fixed component, their impact is mechanically larger for short-term loans, where such costs cannot be amortized over long horizons. For both reasons, we expect—and find—that the effects of the EBLR regime are stronger for short-term lending. To test the above thesis, we obtain the long- and short-term borrowings for banks and non-banks from the prowess database. Then, we create the firm-lender type-year level panel following the same algorithm as in section 5.3. We then estimate the equation 3 separately or short term and long term borrowings.

We present the result in columns (3) and (4) of Table 8. In column (3), the dependent variable is the natural logarithm of short-term borrowings. We find that the the coefficient on the triple interaction term is positive and statistically significant. In column (4), the coefficient is statistically indistinguishable from zero and economically low in magnitude. Hence, as expected, the decline in transmission to loan quantities is more pronounced for short-term loans.

Together, the above results in sections 6.4 and 6.5 also show that the results are less likely to be driven by a differential change in the type of lending by banks and non-banks during the COVID-19 pandemic, which coincides with the EBLR regime. The results hold even within the unsecured and short-term loan-types.

6.6 Relationship between Defaults and Loan Size

We then provide evidence on the relationship between loan size and the costs of lending. Our hypothesis posits that the marginal cost of lending is upward-sloping. While we do not directly observe marginal costs, larger loans may entail higher expected default risk, which can be interpreted as an additional cost to lenders. To test this, we examine whether larger loans are more likely to default. We use loan default data from CIBIL, India's largest credit bureau, and merge the bank–firm–year level default data with the panel of outstanding loans from the MCA database. We estimate the following specification:

$$Default_{ijt+1} = \beta_0 + \beta_1 \cdot (log\_outstanding_{ijt}) + \alpha_{it} + \lambda_j + \epsilon_{ijt} \tag{6}$$

Where the dependent variable $default_{ijt+1}$ is an indicator variable that takes a value of one if firm i defaults on lender j in year $t+1$. The variable $log\_outstanding_{ijt}$ is the natural logarithm of outstanding loan between firm i and lender j in year t. The variables α_{it} and λ_{jt} represent $firm \times year$ and $bank$ fixed effects, respectively. The inclusion of $firm \times year$ fixed effects makes the analysis within a firm across various sizes of outstanding loans with different lenders. Hence, β_1 captures the marginal increase in the likelihood of default of the same borrower with an increase in the size of the outstanding loan.

We present the result in column 1 of Table A6 in the online appendix. The coefficient β_1 is positive and statistically significant, showing that borrowers are indeed more likely to default on larger loans. Because this is an equilibrium outcome, we interpret the higher probability of default as an additional cost to banks over and above incremental monitoring and screening expenditures.

We next examine whether banks price this cost by adjusting interest rates. To do so, we replace the dependent variable with the average interest rate charged to a borrower–bank pair in year t, defined as the loan-amount–weighted average of rates across all contracts. Column (2) shows that the coefficient on log(outstanding) is positive and statistically significant. Thus, banks not only face higher default risk on larger loans but also compensate by charging higher interest rates to borrowers with larger outstanding exposures.

6.7 Direct Effect on Lending Related Expenses

We next provide direct evidence on the mechanism by examining whether the sensitivity of lending-related expenses to policy rates declined under the EBLR regime. While precise and comprehensive measures of screening and monitoring costs are not available in banks' public disclosures, we construct proxies using accounting heads that plausibly capture such activities. If the mechanism described in Section 3 is at work, we expect to observe a reduction in the responsiveness of these expenses to policy rates following the mandate.

Specifically, we create a proxy for screening and monitoring expenses based on categories likely to reflect lender activities in overseeing borrowers. Screening and monitoring typically involve (i) frequent visits by bank officers to borrowers' business locations and (ii) repeated communication with borrowers. Accordingly, we combine employee travel and communication expenses into a measure of lending-related costs.

The communication expenses component includes expenditures on telecommunications, postal services, internet connectivity, and IT costs such as software development and ITenabled services. This granularity allows us to capture both traditional monitoring activities (e.g., site visits, postal communication) and technology-driven monitoring (e.g., internet-based interactions, telephonic communication).

We then scale total travel and communication expenses by total operating expenses to construct our proxy measure of lending-related expenses. Using this proxy, we estimate the following regression specification on a bank–year panel:

$$\begin{aligned}\mathrm{Y}_{jt} = \beta_0 + \beta_1 \cdot (\mathrm{Bank}_j \times \mathrm{Post}_t \times \mathrm{Repo}_{t-1}) + \beta_2 \cdot (\mathrm{Bank}_j \times \mathrm{Post}_t) + \beta_3 \cdot (\mathrm{Bank}_j \times \mathrm{Repo}_{t-1}) \\ + \beta_4 \cdot (\mathrm{Post}_t \times \mathrm{Repo}_{t-1}) + \beta_5 \cdot \mathrm{Bank}_j + \beta_6 \cdot \mathrm{Post}_t + \beta_7 \cdot \mathrm{Repo}_{t-1} + \beta_8 \cdot X_{jt} + \alpha_j + \lambda_t + \epsilon_{jt}\end{aligned} \tag{7}$$

The dependent variable in our empirical model, Y_{jt}, represents the measure of lending-related expenses for lender j in year t. The rest of the variables are as defined in equation 3. The triple interaction term, $Bank \times Post \times Repo_{t-1}$, represents the differential sensitivity of lending-related expenses to policy rates. The model also includes bank fixed effects α_j and time fixed effects λ_t. We double-cluster the standard errors at the lender and year level. We expect banks' lending-related expenses and policy rates to be negatively related in the pre-EBLR period. Therefore, a positive coefficient on the triple interaction term represents a decline in sensitivity.

Columns (1) and (2) of Table 9 present the estimates of the above regression. In column (1), the ratio of travel and communication expenses to total expenses is the dependent variable. The coefficient on the double interaction term $Bank \times Repo_{t-1}$ is negative and statistically significant, indicating a negative relation between lending-related expenses and benchmark rate for banks in the pre-EBLR period. The above result shows that the banks do indeed spend more on screening and monitoring in response to a decline in policy rates. Hence, the result is consistent with our assumption that a significant proportion of these expenses is related to screening and monitoring.

In contrast, the coefficient on the triple interaction term $Post \times Bank \times Repo_{t-1}$ is positive and statistically significant. This implies that following the introduction of the EBLR, banks expanded their lending-related expenses less than nonbanks in response to policy rate cuts. In other words, the mandate weakened the sensitivity of banks' monitoring expenditures to policy rates.

As a robustness test, we broaden the definition of lending-related expenses to include consulting fees, on the premise that banks may also rely on external consultants for screening and monitoring. We get this information from the prowess database. Thus, as a robustness test of the current measure, we construct a revised measure by adding consulting fees to travel and communication expenses and dividing the total by overall expenses. We present the results in column (2) with this revised measure. The results are qualitatively the same as above. The results support the thesis that the sensitivity of lending-related expenses to policy rates reduces significantly for banks in the post-EBLR period.

Taken together, these findings provide direct evidence that mandatory pass-through constrained banks' ability to adjust monitoring costs in response to monetary policy, thereby weakening the transmission of policy rate cuts to loan quantities.

6.8 Response of Margins to Benchmark Rates

As noted in Section 2, banks could not mechanically adjust spreads under the EBLR and continued to face screening and monitoring expenditures. Banks could sustain these expenditures only if their net interest margin (NIM), calculated before expenses and provisions, remained unaffected by policy rate changes. In that case, the sensitivity of NIM to policy rates would be unchanged. By contrast, if banks were forced to cut lending rates without being able to adjust spreads, NIM would fall and its sensitivity to policy rates would decline.

Since we have very limited access to data on NIMs for NBFCs, we rely on the within-bank identification in section 5.4.1 to test the above thesis. We estimate the following bank-year level equation for the test.

$$\begin{aligned} Y_{jt} = &\beta_0 + \beta_1 \cdot Post_t \times Treat\_Bank_j \times Repo_{t-1} + \beta_2 \cdot Post_t \times Treat\_Bank_j + \beta_3 \cdot Treat\_Bank_j \\ &\times Repo_{t-1} + \beta_4 \cdot Post_t \times Repo_{t-1} + \beta_5 Repo_{t-1} + \beta_6 Post_t + \beta_7 X_{jt} + \gamma_j + \delta_t + \epsilon_{jt} \end{aligned} \tag{8}$$

Where Y_{jt} represents the NIM of bank j in year t. The variable $repo_{t-1}$ is the previous year's repo rate. The variables γ_j and δ_t represent bank and year fixed effects, respectively. The rest of the variables are the same as in equation 4. The coefficient β_2 represents the differential response of NIM to changes in benchmark rates for treated banks in the pre-EBLR period, and β_1 represents the differential change in transmission of benchmark rates to the NIM of the treated banks in the EBLR regime.

We present the results in column (3) of Table 9. We find that the coefficient β_2 is negative and statistically significant, indicating that treated banks widened spreads after a decline in repo rates, which is expected since treated banks were less likely to transmit policy rates in the pre-EBLR period. Importantly, we find that the coefficient β_1 on the triple interaction term is positive and statistically significant. Hence, the tendency to increase spreads in response to a decline in interest rates declined for treated banks during the EBLR regime. The result is consistent with our thesis.

7 Real Effects: Sensitivity of Firms' Investment

We now turn to the real effects of the change in quantity sensitivity to monetary policy. As shown in Section 5.3, the transmission of policy rates to borrowing by small firms weakened through banks relative to nonbanks in the post-EBLR period during monetary easing. A natural question is whether this reduced credit response translated into weaker investment by firms more dependent on banks.

We measure firm investment as the annual addition to property, plant, and equipment (PPE). To proxy for firms' dependence on banks, we calculate the share of bank borrowing in total borrowing in the financial year 2020. Firms with above-median shares of bank borrowing are classified as high bank-exposure firms, and those below the median as low bank-exposure firms. We the estimate the following specification:

$$\begin{aligned} Y_{it} = \beta_0 &+ \beta_1 \cdot (\text{Repo}_{t-1} \times \text{Post}_t \times \text{Bank Exp}_i) + \beta_2 \cdot (\text{Repo}_{t-1} \times \text{Bank Exp}_i) \\ &+ \beta_3 \cdot (\text{Repo}_{t-1} \times \text{Post}_t) + \beta_4 \cdot (\text{Post}_t \times \text{Bank Exp}_i) \\ &+ \beta_5 \cdot Post_t + \beta_6 \cdot \text{Repo}_{t-1} + \beta_7 \cdot \text{Bank Exp}_i + \beta_8 \cdot X_{it} + \alpha_i + \lambda_t + \epsilon_{it} \end{aligned} \tag{9}$$

The dependent variable here is the natural logarithm of the gross addition in PPE value for a firm i in a given year t. As the panel is at the firm-year level, we calculate the lagged repo $Repo_{t-1}$ as the weighted average repo rate for the previous year. The variable $Post_t$ is as defined in equation 3. The bank dependence indicator $BankExp_i$ is set to 1 if the firm has high bank exposure as per the definition stated above and 0 otherwise. The three-way interaction term ($\text{Post}_t \times BankExp_i \times \text{Repo}_{t-1}$) assesses whether there is a differential effect on transmission of policy rate cuts to investments for bank-dependent firms in the post-regulation period. The regression includes firm fixed effects (α_i) and time fixed effects (λ_t).

We include a vector of time-varying firm-level control variables X_{it} and their interactions with $Repo_{t-1}$ in the model. We follow Ottonello and Winberry (2020) and include the following control variables: firm size (measured by the natural logarithm of lagged total assets), firm profitability (measured by return on assets), firm liquidity (measured by current ratio), and firm leverage (measured by the debt-to-assets ratio). We double-cluster the standard errors at the firm and year levels.

Table 10 presents the estimates of the above regression. Columns (1) and (2) present results for small firms, while Columns (3) and (4) report results for the sample that excludes small firms. Columns (2) and (4) include the full set of control variables and their interactions with lagged policy rates.

For small firms, the coefficient on the triple interaction term $PostBankExposureRepo_{t-1}$ is positive and statistically significant with a magnitude of 0.588 implies that a one–percentage point decline in the policy rate is associated with an approximately (exp(0.588)-1) $\approx$ 80% lower increase in investment for high bank-exposure firms relative to low bank-exposure firms in the post-EBLR period.

By contrast, for large firms (Columns (3) and (4)), the corresponding coefficient is small and statistically insignificant, indicating no differential change in investment sensitivity to policy rates across bank-dependent and less bank-dependent firms.[24]

Taken together, these results show that the weaker transmission of policy rate cuts to credit supply under the EBLR regime had real effects: small firms, more reliant on bank financing, expanded investment significantly less in response to monetary easing

8 Robustness Tests

8.1 Machine Learning Method

We complement the DiD identification with a machine learning (ML) approach that flexibly models the mapping between borrower, loan, and lender characteristics and interest rates. By training an ML model exclusively on the pre-EBLR period and applying it to the post-EBLR period, we can generate loan-level counterfactual interest rates. We elaborate on the details of implementation and results in section B of the online appendix. The results are

[24]A t-test of the difference between the coefficients reveals that the difference is statistically significant.

qualitatively similar.

8.2 External Validation on Retail Loans

As noted in Section 5, the EBLR framework also applies to retail loans.[25] Our main analysis has focused on commercial loans, where detailed information on interest rates and borrower identity is available. To externally validate our findings in the retail market, we use loan-level data from a large credit bureau.

We construct a panel of outstanding loans at the borrower–lender type–year level. The panel begins in 2017 with all borrower–lender type pairs with an outstanding relationship. A new pair is added when a borrower initiates lending with a new lender type and dropped once the loan is fully repaid. Using this panel, we estimate the regression equation 3, restricting the sample to 2017–2022 (the period covered by the data). The dependent variable is the new loan amount between a borrower and a lender type in a given year. Because many pairs record zero new borrowing in some years, we estimate a Poisson model following (Chen and Roth (2024)). Since the dataset lacks interest rate information, the test focuses only on the sensitivity of loan quantities.

Table 11 in the Online Appendix reports the results. Column (1) shows that the coefficient on the triple interaction term $Post \times Repo_{t-1} \times Bank$ is positive and statistically significant, consistent with our main finding that policy rate transmission to lending declined for banks relative to nonbanks in the post-EBLR period. The result is robust to estimating an OLS model, as shown in Column (2). The magnitude of the coefficient in column (1) is 0.392. This implies that a one percentage point decline in repo rate is associated with a (exp(0.392)-1) $\approx$ 48% lower increase in lending to retail borrowers.

8.3 External Validation on District-level Credit Growth

If the decline in transmission of rate cuts to small firms is economically meaningful, it should aggregate up to the regional level credit patterns. We therefore turn to an external validation exercise using district-level borrowing data from the RBI to test whether the

[25] According to RBI data, 60.2 % of personal loans across scheduled commercial banks were floating, while an overwhelming 94.8 % of home loans were floating-rate as of March 2023. For more, see: https://www.rbi.org.in/Scripts/PublicationReportDetails.aspx?UrlPage=&ID=1248

weaker transmission of policy rate cuts to small firms is visible in regional credit growth. We use the following specifications to test whether the transmission of policy rates to lending is lower in districts that are more exposed to small firms.

$$\begin{aligned} Y_{kt} =& \beta_0 + \beta_1 Repo_{t-1} \times Post_t \times MSME\_Exp_k + \beta_2 Repo_{t-1} \times Post_t + \beta_3 Post_t \times MSME\_Exp_k \\ &+ \beta_4 Repo_{t-1} \times MSME\_Exp_k + \beta_5 Post_t + \beta_6 MSME\_Exp_k + \beta_6 Repo_{t-1} + \alpha_k + \delta_t + \varepsilon_{kt} \end{aligned} \tag{10}$$

Where the dependent variable Y_{kt} is the year-on-year growth in borrowing in district k in year t. We include district-level fixed effects, α_k, and year-level fixed effects, δ_t. The variable $MSME\_exp$ measures the districts' exposure to small firms. To create the measure of districts' exposure to small firms, we obtain the number of small firms at the district level from the Government of India's Udyam database.[26] The database provides the counts of small firms as of the year 2024. Therefore, the inherent assumption is that the current distribution of small firms across the districts is representative of their distribution during our sample period. We collect the district-level GDP data from the website of the Ministry of Statistics and Program Implementation (MoSPI). We normalize the number of small firms by district GDP. Districts are then classified into two groups: those with above-median small firms-to-GDP ratios and those with below-median ratios. We define the former group as districts with high exposure to small firms and the latter group as districts with low exposure to small firms. We then estimate the coefficients using district-year-level panel data. The rest of the variables are as in equation 3.

We present the estimates in column (1) of Table A7 of the online appendix. We find that the coefficient on the triple interaction term is positive and statistically significant at the 10% level with a magnitude of 0.580. It indicates that there is a 58 basis points lower increase in credit growth in response to a one percentage point decline in the policy rate.

8.4 Endogeneity due to Differences between Banks and Non-Banks

As highlighted in section 5, a potential concern related to our identification is that observable and unobservable differences between banks and non-banks may confound our inferences. Specifically, the argument could be that some other factor precisely coinciding with the EBLR regime affects banks and non-banks differentially. Such a factor should increase the

[26] `https://dashboard.msme.gov.in/udyam_dist_wise.aspx?stid=36`

transmission of benchmark rates to the loan prices, and, simultaneously, decrease transmission to the loan quantities for banks relative to non-banks. The likelihood of the existence of such a factor is remote. Nevertheless, we conduct several diagnostic tests to control for such factors, as highlighted in sections 5. We provide three additional pieces of evidence to rule out the confounding effects of differences between banks and non-banks.

First, we limit the sample of NBFCs to large NBFCs that are identified as systemically important.[27] These NBFCs are plausibly comparable to banks due to their larger size. They are also primarily into the lending business like banks. We first estimate equation 2 using the above sample of NBFCs. We present the result in Table A8 of the online appendix. We find that the results are consistent with Table 3. Further, using the firm-lender relationship data as of 2020, we identify firms with an exclusive relationship with systemically important NBFCs. We then restrict the data to these firms and estimate equation 3 using the above sample of firms. We present the results in Table A9 of the online appendix. We find that the results are consistent with Table 4.

Second, we limit the sample to borrowers who borrow from both banks and non-banks in both pre- and post-EBLR periods. Hence, the average quality of borrowers is largely similar in the pre- and post-EBLR periods. We estimate equation 3 using the above sample and present the results in Table A10 of the online appendix. We find that our results are qualitatively similar to Table 4. To check whether limiting to the above sample of firms indeed balances the firm quality in the pre- and post-EBLR periods, we also compare sample of firms that borrow from both banks and non-banks in both pre- and post-EBLR periods across pre- and post-EBLR periods based on sale growth, profit margin, leverage ratio (debt to asset ratio), and liquidity (cash to asset ratio). We present the results in Table A12 of the online appendix and find that the differences are statistically indistinguishable from zero.

Finally, we have already shown the robustness of our results within banks. We have also shown that in the within-bank identification, the treated and control banks do not differ in section 5.4.1. Although the balance sheet structure of banks and non-banks differ significantly, test whether the structure with banks and within non-banks change in the post-EBLR period. We compare the balance sheet structure of banks (non-banks) in the pre- and post-EBLR periods and present the comparison in Panel A (B) of Table Table A11 of the

[27]NBFCs with asset size larger than INR 5 billion are identified as systemically important. See: `https://rbi.org.in/Scripts/BS_NBFCList.aspx` for the list of systemically important NBFCs.

online appendix. Specifically, on the asset side, we compare the proportion of total loans that are long-term and short-term. On the liability side, we compare the proportion of banks' liabilities funded by demand deposits, savings deposits, and long-term deposits. For NBFCs, we compare the proportion of total liabilities funded by banks, bonds, and commercial papers. On the asset side, differences between the variables are statistically indistinguishable from zero. On the liability side, the differences between demand deposits and savings deposits for banks, and commercial papers for non-banks are statistically significant. However, the differences are economically low in magnitude. Hence, we do not see a significant change in the balance sheet structure of banks and non-banks during the EBLR regime.

8.5 Placebo Tets

We also conduct placebo tests using false definitions of small firms and false treatment years. We present the results in section C of the online appendix. The placebo exercise confirms that our findings are not the result of spurious correlations or random sampling variation, but are specific to the mandated small firms segment and EBLR treatment.

8.6 COVID interventions

A potential concern is that our results may be confounded by COVID-19 interventions. Our review of regulatory announcements shows that none of the major COVID-19 interventions created differential treatment between banks and nonbanks. Section D of the Online Appendix provides a detailed list of the COVID-related interventions of the government. Nevertheless, we estimate equations 2 and 3 after dropping 2020-21, the year in which the economy dipped sharply. We present the results related to loan prices in Tables A13 of the online appendix and results related to loan quantities in A14 of the online appendix. The results remain qualitatively unchanged. We show a chart of GDP growth around COVID-19 pandemic in Figure A2 of the online appendix.

9 Conclusion

This paper provides new evidence on the relationship between price and quantity sensitivities in the transmission of monetary policy to small firms. Exploiting India's 2019 External

Benchmark Lending Rate reform, we show that mandatory pass-through of policy rate cuts strengthens price sensitivity but dampens credit expansion and investment. Our analysis points to upward-sloping marginal costs of lending—arising from screening, monitoring, and risk management—as the mechanism behind this substitution.

These findings have important policy implications. Central banks often view stronger and faster pass-through of policy rates as desirable, especially during downturns when supporting small firms is critical. Our results suggest, however, that enforcing pass-through may come at the cost of weaker credit and investment channels. In economies where small and medium enterprises play a central role in output and employment, monetary frameworks must explicitly account for this trade-off. Policies that focus narrowly on improving price transmission may inadvertently restrict the flow of credit to constrained borrowers.

This insight also speaks to programs such as the European Central Bank's Targeted Longer-Term Refinancing Operations (Andrade et al., 2019) and the Bank of England's Funding for Lending Scheme (Churm et al., 2015), which condition the provision of cheap funding to banks on significant credit expansion. Such measures are unlikely to achieve their objectives if regulators do not recognize that expanding credit requires higher operating and risk-management costs—costs that inherently limit the extent to which lenders can transmit policy rates to borrowers. Without accommodating these resource requirements, policies that mandate stronger pass-through may unintentionally weaken the very credit channels they aim to stimulate.

While our empirical setting is India, the implications are broader. As noted in the introduction, central bank nudges to ensure rate pass-through—particularly during crises—are not unique to India. The Indian context is especially well-suited for studying this question because of the explicit nature of the mandate. Yet the mechanism we highlight relies on financial frictions that rise with loan size and on the monitoring and screening costs faced by lenders—phenomena that are not specific to India. Although the magnitude of such frictions and the share of firms affected will vary across countries, the direction of our results is likely to extend to other settings, including advanced economies.

At the same time, our study has limitations. First, while the reform provides a unique quasi-experimental setting, it coincides with the COVID-19 period, which may have interacted with lending behavior in ways we cannot fully disentangle despite robustness checks. Second, we cannot provide a complete picture of the relationship between price and quantity transmissions, as we observe only rate cuts and not rate increases before and after the event. Finally, our measures of screening and monitoring are indirect: we rely on cross-sectional variation in travel, communication, and consulting expenses, which serve as proxies rather than direct measures.

Overall, the results underscore that monetary policy transmission cannot be evaluated solely through the lens of price pass-through. The interaction between price and quantity sensitivities, shaped by financial frictions, is crucial for understanding how monetary policy ultimately affects the real economy.

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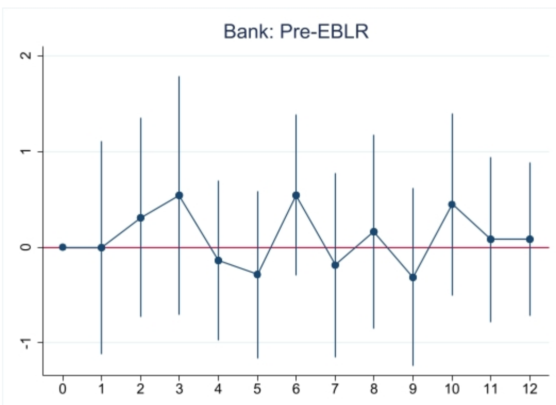
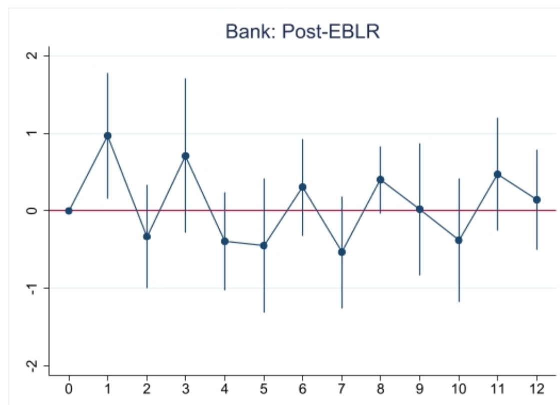
Figure 1: Interest Rate Pass-Through to small firms by Lender Type

This figure illustrates the impulse response of interest rates for small firms to changes in the repo rate, categorized by lender type (banks and non-banks) and regulatory regime (pre- and post-EBLR implementation). The analysis is based on data from loans to small firms issued after April 1, 2015. In Panel A (B), the coefficients from regressions estimating the impact of lagged repo rate changes on interest rates charged by banks (non-banks) during the pre-MCLR period are plotted. Panels C (D) depict the corresponding coefficients for banks (non-banks) in the post-MCLR period. The vertical axis in each panel represents the estimated impact of repo rate changes, while the horizontal axis displays the n^{th} months relative to the month in which we measure the interest rate. We report the coefficients up to 12 months of lag.

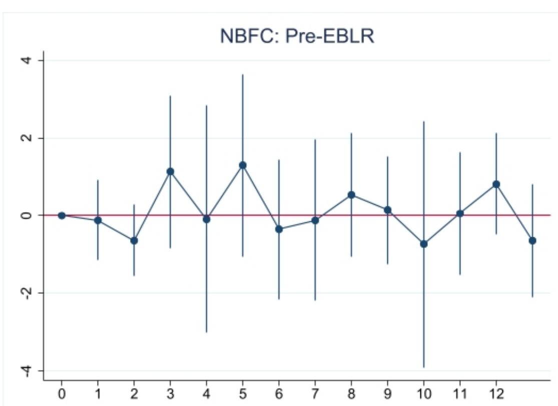
(a) Panel A



(b) Panel B



(c) Panel C



(d) Panel D

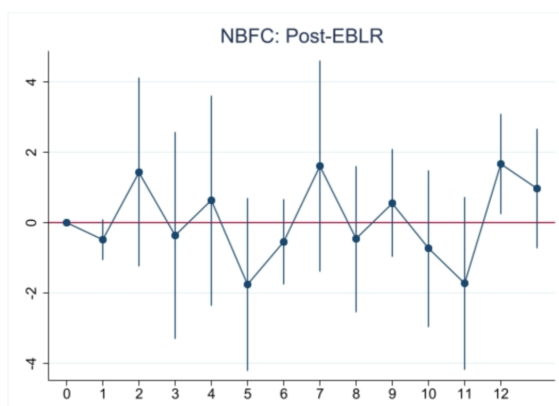
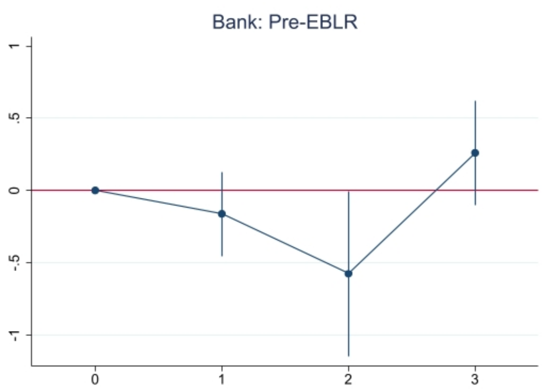


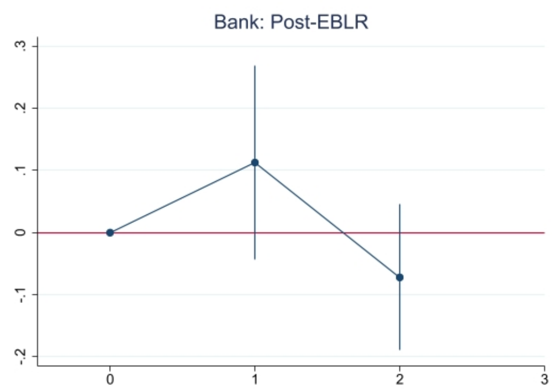
Figure 2: Small firm Borrowing Response to Monetary Policy by Banks and Non-Banks

This figure illustrates the impulse response of borrowing amount for small firms to changes in the repo rate, categorized by lender type (banks and non-banks) and regulatory regime (pre- and post-EBLR implementation). The analysis uses data from loans to small firms issued after April 1, 2016. In Panel A (B), we plot the coefficients from regressions estimating the impact of lagged repo rate changes on borrowing from banks (non-banks) during the pre-MCLR period. In Panel C (D), we depict the corresponding coefficients for banks (non-banks) in the post-MCLR period. For each panel, the vertical axis represents the estimated impact of repo rate changes, while the horizontal axis displays the n^{th} year relative to the year in which we measure the natural logarithm of borrowing. We report the coefficients up to 3 years of lag.

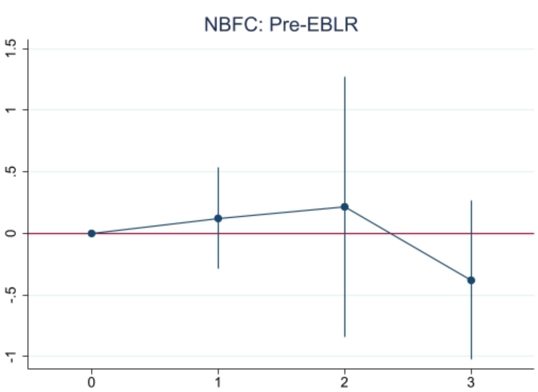
(a) Panel A



(b) Panel B



(c) Panel C



(d) Panel D

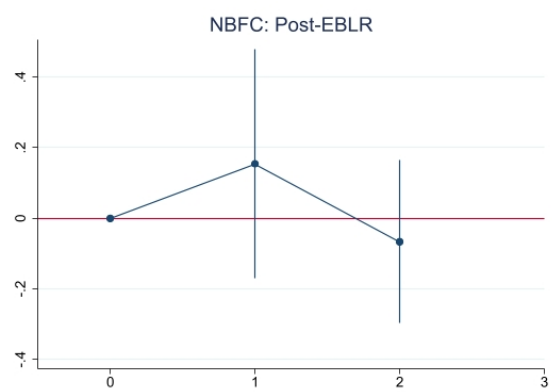
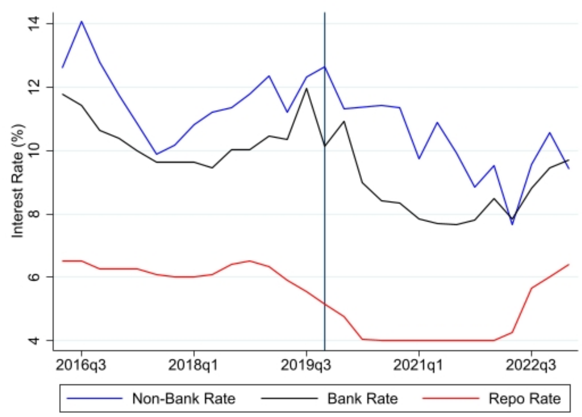


Figure 3: Pre-Trend for small and large Firms

In this figure, we show the evolution of interest rates charged to small and large firms in our sample period. Panel A presents the interest rate trends for small firms, while Panel B shows the corresponding trends for large firms. We limit the sample to firms that borrow from both banks and non-banks in the respective year. For each panel, we plot the average interest rates charged by banks (black line) and non-banks (blue line) alongside the RBI's repo rate (red line) as a benchmark. The data are plotted at a quarterly frequency. The x-axis represents the corresponding quarter. The vertical line indicates the implementation of the External Benchmark Lending Rate (EBLR) framework in October 2019. This visualization allows us to compare lending rates across different types of financial institutions and examine how they respond to changes in the policy rate. From the figure, it is evident that for small firms, the decline in lending rates materialized almost immediately following the policy rate cut and subsequently exhibited close co-movement with the repo rate, as illustrated by the black line in Figure (a). In contrast, such a decline is absent in the case of nonbank lenders.Furthermore, prior to the EBLR intervention, bank lending rates showed little systematic co-movement with the repo rate cuts.

(a) Panel A: Small Firms



(b) Panel B: Large Firms

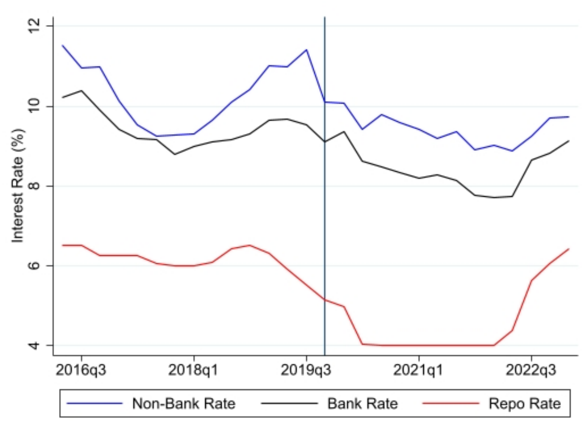


Table 1: Sample Construction

In this table, we present the sample construction. The sample period spans the financial years (FY) 2017-2023. In India, a financial year runs from April 1 of the preceding calendar year to March 31 of the stated calendar year. Accordingly, our sample spans April 1, 2016 to March 31, 2023. In panel A, we present the sample construction for the loan-level panel using the MCA database. In panel B, we present the sample construction for the firm-lender type-year level panel using the Prowess database.

Panel A: Loan Level Data	
Sample Period	2017 to 2023
Number of Unique Firms with Interest Rate Data	5,053
Number of small Firms in the MCA Dataset	2,142
Number of large Firms in the MCA Dataset	3,373
Number of banks in the MCA Dataset	63
Number of non-banks in the MCA Dataset	134
Number of unique observations in the MCA Dataset	30,369
Number of unique observations for small firms	6,205
Number of unique observations for large firms	24,164

Panel B: Borrowing Data	
Sample Period	2017 to 2023
Number of Firms in Initial Sample from the Prowess Database in the sample period	38,697
Number of unique small firms in the sample period	16,133
Number of unique small firms with positive new borrowings from both banks and non-banks	3,900
Number of unique firm-year observations corresponding to small firms	5,424
Number of unique firm-lender type-year observations for small firms in the sample period	10,848

Table 2: Descriptive Statistics

In this table, we report the descriptive statistics related to key variables. The sample spans the years 2017 to 2023 and is restricted to small firms in panels A and B. Panel A is for loan-level variables. Panel B is for firm-year level variables. Panels C and D are for bank and NBFC-year level variables, respectively. Panel E is for firm-lender-year level variables. Finally, Panel F is for the time series variable. We define the variables in Table A3 of the online appendix.

Variables	N	Mean	Median	SD	1%ile	99%ile
Panel A: Loan level: small firms						
Interest Rate	6,205	9.52	9.10	2.17	5.75	16.01
Loan Amount (INR mn)	6,205	270	17	1681	0.4	3500
Panel B: Firm-year level: small firms						
Borrowing - bank (INR mn)	5,424	268	16	4,445	0.1	2190
Borrowing - NBFC (INR mn)	5,424	337	8	7,266	0.1	2782
Unsec Borrowing (Bank) (INR mn)	5,424	191	6	3,883	0	1313
Unsec Borrowing (NBFC) (INR mn)	5,424	310	6	7,248	0	2337
Total Assets (INR mn)	5,424	4,832	418	83,489	10	41891
ROA	5,418	0.059	0.067	0.249	-0.337	0.275
Leverage	5,424	0.414	0.352	0.660	0.006	1.649
Addition to PPE	4,503	133	9	2,233	0.1	1132
Panel C: Bank-year level						
Monitoring exp/Total expense %	239	2.147	1.055	3.12	0.196	13.686
GNPA/Assets %	196	5.034	2.777	6.534	0.005	41.55
CRAR	230	27.755	15.680	32.793	9.500	133.590
ROA	239	0.028	0.055	0.093	-0.344	0.093
Total Assets (INR mn)	239	312,985	53,726	752,450	720	3,623,715
Panel D: NBFC-year level						
Monitoring exp/Total expense %	3,968	3.338	2.114	4.209	0.037	20.833
GNPA/Assets %	1,321	5.234	2.132	9.774	0.000	58.370
CRAR	1,788	42.200	28.560	31.420	8.880	133.940
ROA	3,954	0.045	0.069	0.858	-0.226	0.373
Total Assets (INR mn)	3,954	66,015	1,623	349,668	26	1,294,490
Panel E: Firm-Lender Relationship						
Outstanding (banks) (INR mn)	17,788	340	51	1986	0	6392
Outstanding (NBFCs) (INR mn)	11,665	486	21	3455	0	11143
Panel F: Time Series - Monthly						
Repo rate	84	5.398	5.95	1.014	4	6.75

Table 3: Interest Rates

This table presents results for the differential transmission of the repo rate to the interest rate for firms in the RBI-mandated post-repo benchmarking period. The dependent variable is the loan level interest rate of a firm as captured from the MCA database. The explanatory variable, $Repo_{m-1}$, is the average repo rate in the previous month. Post is an indicator variable; it is 1 after RBI mandated the repo benchmarking, applicable from 1st October 2019. Bank is an indicator variable; it is 1 if the lender is a bank and 0 for non-bank financial institutions. The data are organized at a loan level for the sample period FY 2017 to FY 2023. Columns (1) & (2) show the results for all the small firms in the sample. Columns (3) & (4) show the results for all the large firms. Small firms are defined according to the Government of India's guidelines as presented in Table A1 & Section 2.2. We include Firm × Year and Lender fixed effects in all the models and columns. Columns (2) and (4) have the lender-level control variables - asset size, the ratio of nonperforming assets to total assets, capital adequacy, return on assets (ROA), and outstanding loans between the respective firm-lender pair - included in the model. The interactions of the respective control variables with $repo_{t-1}$ are also included in the model. The standard errors reported in parentheses are robust, clustered at the firm & year levels and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Interest rate			
	Small firms		Large firms	
	(1)	(2)	(3)	(4)
Post × Bank × $Repo_{m-1}$	0.821**	0.558**	0.282	0.394
	(0.250)	(0.190)	(0.467)	(0.380)
Bank × $Repo_{m-1}$	-0.153	-0.037	0.062	-0.063
	(0.215)	(0.100)	(0.460)	(0.366)
Post × $Repo_{m-1}$	-0.378	-0.102	-0.283	-0.377
	(0.370)	(0.346)	(0.708)	(0.624)
Post × Bank	-4.804**	-4.061**	-1.533	-2.376
	(1.518)	(1.307)	(2.983)	(2.315)
$Repo_{m-1}$	0.587	0.449	0.533	0.625
	(0.353)	(0.347)	(0.701)	(0.612)
Post	2.505	2.690	1.492	2.521
	(2.129)	(1.887)	(4.389)	(3.737)
Observations	3,667	3,360	19,968	18,574
R-squared	0.800	0.789	0.779	0.720
Control Variables	No	Yes	No	Yes
Firm × Year and Lender F.E.	Yes	Yes	Yes	Yes

Table 4: Transmission of Monetary Policy to Firm Borrowing

This table presents results for the differential transmission of the repo rate to borrowing amounts in the RBI-mandated post-repo benchmark period. The data are organized at the firm–lender type–year level and span the sample period from 2017 to 2023. The dependent variable is the natural logarithm of the total borrowing amount. $Repo_{t-1}$ is the weighted average yearly repo rate for the previous year. The weighted average is calculated by weighting each daily repo rate by the number of days it persisted during the year. Post is a binary variable equal to 1 for financial years after 2020, corresponding to the period following the RBI's external benchmarking mandate (effective October 1, 2019), and 0 for earlier years. Bank is a binary variable equal to 1 if the lender is a bank and 0 otherwise. Columns (1) and (2) show results for total borrowing by small firms, and columns (3) and (4) for large firms. Small firms are defined according to the Government of India's guidelines as presented in Table A1 & Section 2.2. We include Firm × Year and lender-type fixed effects in all the models and columns. We include the following lender-type level control variables in Columns (2) and (4) - asset size, the ratio of nonperforming assets to total assets, capital adequacy, return on assets (ROA), and outstanding loans to the respective lender type - averaged at the firm-lender-type level for their bank and non-bank lenders separately. The interactions of the respective control variables with $repo_{t-1}$ are also included in the model. The standard errors reported in parentheses are robust, double-clustered at the firm and year levels, and adjusted for heteroskedasticity. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Log Borrowings			
	MSME		Non-MSME	
	(1)	(2)	(3)	(4)
Post × Bank × $Repo_{t-1}$	0.186**	0.228***	-0.074	-0.001
	(0.076)	(0.008)	(0.081)	(0.015)
Bank × $Repo_{t-1}$	-0.175**	-0.075***	-0.080	-0.215***
	(0.065)	(0.012)	(0.078)	(0.009)
Post × Bank	-1.025*	-1.165***	0.486	-0.118
	(0.524)	(0.038)	(0.562)	(0.077)
Observations	10,848	9,684	14,168	12,918
R-squared	0.732	0.762	0.746	0.787
Control variables	No	Yes	No	Yes
Firm × Year F.E.	Yes	Yes	Yes	Yes
Lender type F.E.	Yes	Yes	Yes	Yes

Table 5: Alternative Identification: Interest Rates

This table presents results for the differential transmission of the repo rate to the interest rate for firms in the RBI-mandated post-repo benchmark period. The dependent variable is the loan-level interest rate of a firm as captured from the MCA database. The explanatory variable, $Repo_{m-1}$, is the average repo rate in the previous month. Post is an indicator variable; it is 1 after RBI mandated the repo benchmarking, applicable from 1st October 2019. Treat Bank is an indicator variable; it is 1 if the banks' sensitivity of MCLR to the policy rate in the five years before EBLR is low and zero otherwise. The sensitivity is defined in section 5.4. The data are organized at a loan level for the sample period 2017-2023. Columns (1) & (2) show the results for all the small firms in the sample. Columns (3) & (4) show the results for all the large firms. Small firms are defined according to the Government of India's guidelines as presented in Table A1 & Section 2.2. We include Firm × Year and Lender fixed effects in all the models and columns. Columns (2) and (4) have the control variables listed in Table 3 included in the model. The standard errors reported in parentheses are robust, clustered at the firm & year levels and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Interest rate			
	Small firms		Large firms	
	(1)	(2)	(3)	(4)
Post × Treat Bank × $Repo_{m-1}$	0.608***	0.683**	-0.277	-0.268
	(0.138)	(0.245)	(0.436)	(0.436)
Treat Bank × $Repo_{m-1}$	-0.061	-0.361	0.237	0.174
	(0.122)	(0.275)	(0.432)	(0.455)
Post × $Repo_{m-1}$	0.005	0.163	0.125	0.137
	(0.462)	(0.462)	(0.306)	(0.316)
Post × Treat Bank	-2.919***	-3.433**	1.253	1.137
	(0.763)	(1.356)	(2.672)	(2.724)
$Repo_{m-1}$	0.616	1.325	0.438	0.738
	(0.450)	(1.056)	(0.310)	(0.000)
Post	-0.279	-1.105	-0.611	-0.654
	(2.769)	(2.782)	(1.872)	(1.935)
Observations	2,510	2,506	13,536	13,520
R-squared	0.732	0.734	0.732	0.733
Control variables	No	Yes	No	Yes
Firm × Year F.E.	Yes	Yes	Yes	Yes
Lender F.E.	Yes	Yes	Yes	Yes

Table 6: Alternative Identification: Borrowing

This table presents results for the differential transmission of the repo rate to the total borrowing amount of firms in the RBI-mandated post-repo benchmark period. The data are organized at firm-lender type-year level and span the sample period 2017 to 2023. The dependent variable is the natural logarithm of the total borrowing amount of a firm in the given financial year. The explanatory variable, $Repo_{t-1}$, is the weighted average yearly repo rate for the previous year. The weighted average repo rate is calculated by weighing the daily repo rate by the number of days for which that repo rate persisted. Post is an indicator variable. It is 1 after the financial year 2020, corresponding to the period after the RBI mandated external benchmarking of floating rate loans effective October 1, 2019, and 0 for years before that. Treat Firm is an indicator variable; it is 1 if at least one of the firm's bankers is a treated bank and zero for other firms. The treated banks are as defined in Table 5. Columns (1) & (2) show the results for all the small firms in the sample. Columns (3) & (4) show the results for all the large firms. Small firms are defined according to the Government of India's guidelines as presented in Table A1 & Section 2.2. We include Firm and Year fixed effects in all the models and columns. Columns (2) and (4) have the control variables listed in Table 4 included in the model. The standard errors reported in parentheses are robust, double-clustered at the firm & year levels and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Log(borrowing)			
	Small firms		Large firms	
	(1)	(2)	(3)	(4)
Post × Treat Firm × $Repo_{t-1}$	0.165**	0.158**	-0.132	-0.132
	(0.061)	(0.060)	(0.097)	(0.095)
Treat Firm × $Repo_{t-1}$	-0.144**	-0.130*	0.018	0.025
	(0.059)	(0.061)	(0.059)	(0.058)
Post × Treat Firm	-1.000**	-0.947**	0.573	0.614
	(0.357)	(0.361)	(0.554)	(0.531)
Observations	14,420	14,406	17,811	17,810
R-squared	0.733	0.736	0.722	0.722
Control variables	No	Yes	No	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes

Table 7: Borrowing Response: Lending Relationship, Banking Competition, and firm health

This table presents results on the differential transmission of the repo rate to the borrowing amounts of small firms with and without a lending relationship and firms located in districts with high and low banking competition during the RBI-mandated post-repo benchmark period. The data are organized at the firm-lender type-year level and span the sample period from 2017 to 2023. The dependent variable is the natural logarithm of the total borrowing amount of a firm in a given financial year. Column (1) reports results for firms with relationship borrowing, while column (2) presents results for firms in non-relationship borrowing. A firm is identified as having a relationship bank if at least one of its banks is above median based on the composite score of the firm-bank relationship, as elaborated in section 6.1. Column (3) shows estimates for firms in high bank competition districts, and column (4) for firms in low bank competition districts. High (Low) bank competition districts are the ones with below (above) Herfhinadahl Index (HHI) of bank branches as elaborated in section 6.2. Column (5) presents estimates for firms with an above median interest coverage ratio, and column (6) presents estimates for firms with a below median interest coverage ratio. The explanatory variable, $Repo_{t-1}$, is the weighted average yearly repo rate for the previous year. The weighted average is computed by weighing each daily repo rate by the number of days it prevailed during the year. *Post* is an indicator variable equal to 1 for financial years after 2020, corresponding to the period following the Reserve Bank of India's mandate requiring external benchmarking of floating rate loans effective October 1, 2019, and 0 for years prior. *Bank* is a binary indicator equal to 1 if the lender is a bank and 0 for non-bank financial institutions. The sample is restricted to small firms, defined in accordance with the Government of India's classification as presented in Table A1 & Section 2.2. We include Firm × Year and Lender type fixed effects in all the models. The standard errors reported in parentheses are robust, double-clustered at the firm and year levels, and adjusted for heteroskedasticity. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Log(borrowing)					
	Panel A: Relationship Heterogeneity		Panel B: Banking Competition		Panel C: Firm Health	
	Relationship	Non-Relationship	High Competition	Low Competition	ICR below median	ICR above median
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Bank × $Repo_{t-1}$	0.526*	0.013	0.500***	-0.086	0.312**	0.151
	(0.236)	(0.051)	(0.114)	(0.156)	(0.100)	(0.096)
Bank × $Repo_{t-1}$	-0.462**	-0.024	-0.397***	0.010	-0.227*	-0.213**
	(0.185)	(0.051)	(0.053)	(0.154)	(0.100)	(0.070)
Post × Bank	-3.014*	0.010	-2.972***	0.643	-1.647*	-0.941
	(1.502)	(0.371)	(0.655)	(1.079)	(0.726)	(0.606)
Observations	3,478	7,370	4,716	5,960	5,374	4,966
R-squared	0.738	0.730	0.738	0.730	0.748	0.704
Firm × Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Lender type F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table 8: Transmission of Monetary Policy based on Loan Type

This table presents results for the differential transmission of the repo rate based on loan type in the RBI-mandated post-repo benchmark period. The data are organized at the firm–lender type–year level and span the sample period from 2017 to 2023. The dependent variable is the natural logarithm of the borrowing amount—reported separately for different loan types. $Repo_{t-1}$ is the weighted average yearly repo rate for the previous year. The weighted average is calculated by weighting each daily repo rate by the number of days it persisted during the year. Post is a binary variable equal to 1 for financial years after 2020, corresponding to the period following the RBI's external benchmarking mandate (effective October 1, 2019), and 0 for earlier years. Bank is a binary variable equal to 1 if the lender is a bank and 0 for non-bank financial institutions. Column (1) shows the result for unsecured borrowing, and column (2) for secured borrowing. Column (3) shows the result for short-term borrowing, and column (4) for long-term borrowing. The sample is restricted to small firms only. Small firms are defined according to the Government of India's guidelines as presented in Table A1 & Section 2.2. We include Firm × Year and lender-type fixed effects in all the models and columns. The standard errors reported in parentheses are robust, double-clustered at the firm and year levels, and adjusted for heteroskedasticity. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Log Unsecured	Log Secured	Log Short Term	Log Long Term
	(1)	(2)	(3)	(4)
Post × Bank × $Repo_{t-1}$	0.129*	-0.031	0.022***	0.013
	(0.067)	(0.251)	(0.004)	(0.203)
Bank × $Repo_{t-1}$	-0.139*	0.398**	-0.143***	0.030
	(0.061)	(0.147)	(0.002)	(0.202)
Post × Bank	-0.721	1.037	0.028	-0.322
	(0.468)	(1.475)	(0.018)	(1.364)
Observations	7,986	1,104	6,022	3,634
R-squared	0.720	0.707	0.675	0.748
Control variables	No	No	No	Yes
Firm × Year F.E.	Yes	Yes	Yes	Yes
Lender type F.E.	Yes	Yes	Yes	Yes

Table 9: Monitoring by lenders and Net Interest Margin

This table presents the results for the differential transmission of repo rates on the monitoring expenses incurred by lending banks for firms in the RBI-mandated post-repo benchmark period. The data are organized at a lender-year level for the sample period 2017-2023. The dependent variable is the ratio between Monitoring Expenses and Total Expenses. The sum total of communication expenses and travel expenses is considered a proxy for monitoring expenses in column (1). We also add consulting fees to the above amount in columns (2). The dependent variable is Net Interest Margin (NIM) in column (3). The explanatory variable, $Repo_{t-1}$, is the weighted average yearly repo rate for the previous year. The weighted average repo rate is calculated by weighing the daily repo rate by the number of days for which that repo rate persisted. Post is an indicator variable. It is 1 after the financial year 2020, corresponding to the period after the RBI mandated external benchmarking of floating rate loans effective October 1, 2019, and 0 for years before that. Bank is an indicator variable; it is 1 if the lender is a bank and 0 for non-banks. We include lender and year-fixed effects in all the models and columns. The standard errors reported in parentheses are robust, double-clustered at the lender & year levels, and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Monitoring expense/Total expense		NIM
	(1)	(2)	(3)
Post × $Repot - 1$ × bank	0.930**	1.208**	0.342***
	(0.350)	(0.426)	(0.030)
Bank × $Repot - 1$	-0.418	-0.410	-0.104***
	(0.411)	(0.433)	(0.018)
Post × Bank	-4.544	-5.611*	-1.386***
	(2.362)	(2.714)	(0.144)
Observations	3,921	3,921	221
R-squared	0.757	0.599	0.843
Lender F.E.	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes

Table 10: Investments

This table presents results for the differential transmission of the repo rate to the firms' investments in the RBI-mandated post-repo benchmark period. The data are organized at a firm-year level for the sample period 2017-2023. The dependent variable is the natural logarithm of the change in value of property, plant, and equipment by a firm in the given financial year. The explanatory variable, $Repo_{t-1}$, is the weighted average yearly repo rate for the previous year. The weighted average repo rate is calculated by weighing the daily repo rate by the number of days for which that repo rate persisted. Post is an indicator variable. It is 1 after the financial year 2020, corresponding to the period after the RBI mandated external benchmarking of floating rate loans effective October 1, 2019, and 0 for years before that. Bank Exp is an indicator variable; it is 1 for firms with above median exposure to bank borrowing and 0 otherwise. Columns (1) & (2) show the results for all the small firms in the sample. Columns (3) & (4) show the results for all the large firms. Small firms are defined according to the Government of India's guidelines as presented in Table A1 & Section 2.2. We include Firm and Year fixed effects in all the models and columns. Columns (2) and (4) have the firm-year level control variables - the natural logarithm of lagged total assets, return on assets, current ratio, and the debt-to-assets ratio - and their respective interactions with $Repo_{t-1}$ included in the model. The standard errors reported in parentheses are robust, double-clustered at the firm and year levels, and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Log(Δproperty, plant, and equipment)			
	Small firms		Large firms	
	(1)	(2)	(3)	(4)
Post × Bank Exp × $Repo_{t-1}$	0.588**	0.582**	-0.172	-0.101
	(0.203)	(0.207)	(0.176)	(0.168)
Bank Exp × $Repo_{t-1}$	-3.665**	-3.650**	1.193	0.760
	(1.240)	(1.257)	(1.035)	(0.994)
Post × Bank Exp	-3.665**	-3.745**	1.193	0.806
	(1.240)	(1.254)	(1.035)	(0.998)
Observations	3,934	3,929	5,768	5,768
R-squared	0.729	0.731	0.826	0.830
Control variables	No	Yes	No	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes

Table 11: Retail Lending

This table presents results for the differential transmission of the repo rate to the retail lending amount in the RBI-mandated post-repo benchmark period. The data are organized at the borrower-lender type-year level for the sample period 2017 to 2022. The dependent variable is the loan amount and the natural logarithm of the new loan amount extended by the respective lender type to the respective borrower in the given financial year in columns 1 and 2, respectively. The explanatory variable, $Repo_{t-1}$, is the weighted average yearly repo rate for the previous year. The weighted average repo rate is calculated by weighing the daily repo rate with the number of days for which that repo rate persisted. Post is an indicator variable. It is 1 after the financial year 2020, corresponding to the period after the RBI mandated external benchmarking of floating rate loans effective October 1, 2019, and 0 for years before that. Bank is an indicator variable; it is 1 if the lender is a bank and 0 for non-banks. Column (1) shows the estimates for a Poisson model. Column (2) shows the estimates for the OLS model. We include borrower $\times$ Year and Lender type fixed effects in all the models and columns. The standard errors reported in parentheses are robust, double-clustered at the borrower & year levels and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

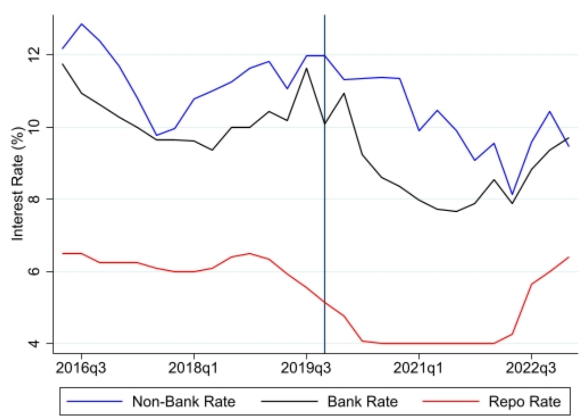
	Loan Amount	Log(Loan Amount)
	Poisson	OLS
	(1)	(2)
Post X Bank X $Repo_{t-1}$	0.392***	2.920***
	(0.094)	(0.601)
Bank X $Repo_{t-1}$	-0.231**	-1.362**
	(0.094)	(0.493)
Post X Bank	-1.925***	-14.982***
	(0.612)	(3.742)
Observations	81,918	156,686
R-squared		0.613
Borrower X Year F.E.	Yes	Yes
Lender type F.E.	Yes	Yes

Internet Appendix

Figure A1: Pre-Trend for small and large Firms- Robustness

In this figure, we show the evolution of interest rates charged to small and large firms in our sample period. Panel A presents the interest rate trends for small firms, while Panel B shows the corresponding trends for large firms. For each panel, we plot the average interest rates charged by banks (black line) and non-banks (blue line) alongside the RBI's repo rate (red line) as a benchmark. The data are plotted at a quarterly frequency. The x-axis represents the corresponding quarter. The vertical line indicates the implementation of the External Benchmark Lending Rate (EBLR) framework in October 2019. This visualization allows us to compare lending rates across different types of financial institutions and examine how they respond to changes in the policy rate. From the figure, it is evident that for small firms, the decline in lending rates materialized almost immediately following the policy rate cut and subsequently exhibited close co-movement with the repo rate, as illustrated by the black line in Figure (a). In contrast, such a decline is absent in the case of nonbank lenders.Furthermore, prior to the EBLR intervention, bank lending rates showed little systematic co-movement with the repo rate cuts.

(a) Small Firms



(b) Large Firms

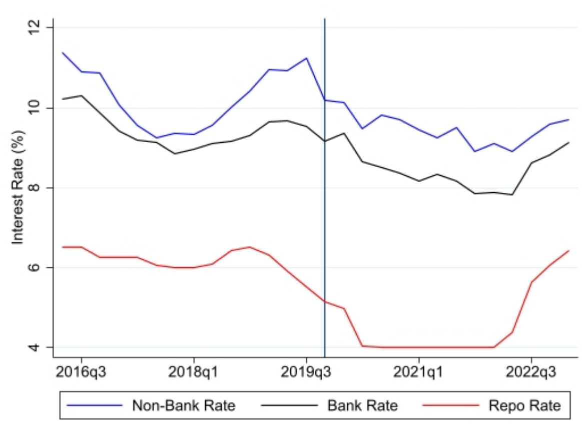


Figure A2: The figure plots the trend of GDP growth in India between 2017 and 2023..

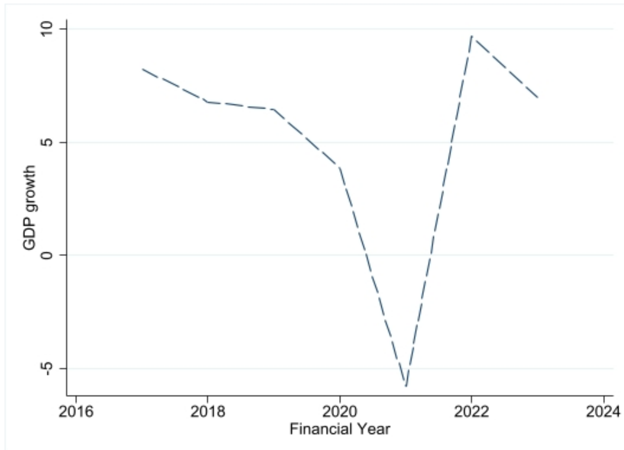


Table A1: Small firm Definition

MSME Classification - MSME Development Act of 2006			
Criteria based on investment in Plant & Machinery or Equipment (in Millions)			
Classification	**Micro**	**Small**	**Medium**
Manufacturing	Investment < INR 0.25 M	Investment < INR 50 M	Investment < INR 100 M
Services	Investment < INR 1 M	Investment < INR 20 M	Investment < INR 50 M
MSME Classification from 1st July 2020			
Manufacturing & Services	Investment < INR 10 M and Turnover < INR 50 M	Investment < INR 50 M and Turnover < INR 500 M	Investment < INR 250 M and Turnover < INR 2500 M

Table A2: Interest Rate Benchmark Distribution with Large vs Small Firms

	Post Period			
	Bank-MSME (932)	non-bank-MSME (33)	Bank-Large (1,847)	non-bank-Large (55)
Base Rate	6.22	3.03	2.54	25.45
MCLR	24.46	15.15	76.28	41.82
EBLR	68.24	18.18	20.14	1.82
Others	1.07	63.63	1.02	30.91
	Pre Period			
	Bank-MSME (531)	non-bank-MSME (95)	Bank-Large (1,744)	non-bank-Large (55)
Base Rate	5.27	9.47	4.3	15.79
MCLR	93.6	23.16	93.52	32.33
Others	1.13	67.37	2.18	51.88

Table A3: Variable Definition

Variable Name	Definition
Interest rate	It is the reported lending rate for a given loan from the MCA database.
Loan Amount	It is the value of the new borrowing amount of a firm in a given year from the Prowess database.
Borrowing from Banks	It is the value of the new borrowing amount of a firm from commercial banks in a given year from the Prowess database.
Borrowing from non-banks	It is the value of the new borrowing amount of a firm from non-banks in a given year from the Prowess database. It is estimated by deducting bank borrowings and related party borrowings from total borrowings.
Repo Rate	It is the policy rate at which the central bank lends short-term funds to commercial banks, typically against government securities.
Lag Repo	This represents the lagged value of the weighted average repo rate from the previous month, where each rate is weighted by the number of days it was in effect.
Monitoring Expenses	It is a proxy for the expenses incurred by lenders to monitor the loans. It is calculated as the total communication expenses and travel expenses by the lending bank in a given year.
Property, Plant and Equipment	It is the total value of a firm's gross investment in property, plant and equipment in a given year.
Sales	It is the sum total of all regular gross income generated by a firm from the sale of goods and non-financial services.
MSME Indicator	It is an indicator variable that identifies whether a given firm belongs to medium, small, and micro enterprises or not. The detailed time-varying MSME definition is given in the Table A1.

Table A3: Variable Definition (Continued)

Variable Name	Definition
Total Assets	It is the sum total of all current and non-current assets held by a firm as of the last day of a financial year.
ROA	Return on Assets is calculated by dividing a firm's net income by its total assets in a given year.
Leverage	It is the ratio between the total outstanding borrowing and total assets in a given year for a firm.
Addition to PPE	It is the gross addition (investment) to Property, Plant and Equipment by a firm in a given year.
Total Expenses	It is the sum total of all expenses incurred by a firm in a given year.
Employees	It is the total number of people employed by a given firm in a given year.
GNPA	It is the gross non-performing assets reported by a bank in a given year.
CRAR	Capital Adequacy Ratio is calculated as the ratio of Tier 1 Capital & Tier 2 Capital and Risk-Weighted Assets of a bank in a given year.

Table A4: Transmission to Lending: MCA sample

This table presents results for the differential transmission of the repo rate to the borrowing amount of firms in the RBI-mandated post-repo benchmark period. The data are organized at a firm-year-lender type level for the sample period 2017-2023. The sample is limited to MSME borrowers. The dependent variable is the natural logarithm of the total borrowing amount of a firm in the given financial year. The explanatory variable, $Repo_{t-1}$, is the weighted average yearly repo rate for the previous year. The weighted average repo rate is calculated by weighing the daily repo rate by the number of days for which that repo rate persisted. We include the interaction term - COVID *times* Repo *times* Bank - as a control variable in both columns. We restrict the sample to firms with interest rate data from MCA. Post is an indicator variable that takes a value of one after the implementation of EBLR and zero otherwise. Bank is an indicator variable; it is 1 if the lender is a bank and 0 for non-banks. We include firm × year fixed effects in all the models and columns. We include bank-year level control variables listed in Table 4 and their respective interactions with $Repo_{m-1}$ in column (2). The standard errors reported in parentheses are robust, double-clustered at the firm & year levels, and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Log(borrowing)	
	(1)	(2)
Post × Bank × $Repo_{m-1}$	0.522***	0.629***
	(0.044)	(0.017)
Bank × $Repo_{m-1}$	-0.328***	-0.366***
	(0.027)	(0.012)
Post × Bank	-2.858***	-3.692***
	(0.318)	(0.104)
Observations	3,574	3,234
R-squared	0.711	0.741
Control variables	No	Yes
Firm × Year F.E.	Yes	Yes
Lender type F.E.	Yes	Yes

Table A5: Comparison of Borrowers - Pre- and Post-EBLR

This table compares the banks with high and low sensitivity of MCLR to Repo rate in the pre-EBLR period. We limit the sample to before the EBLR (2020). The comparison is presented for the lending portfolio and the funding structure of the banks. The measures related to the lending portfolio are the proportion of banks' loans extended to the agriculture sector, industry, services, and MSME firms. The measures related to the funding structure are the proportion of banks' deposits funded by demand deposits, savings deposits, and term loans. We present the differences and the t-stat corresponding to the differences.

	Treated Banks	Control Banks	Difference	T-stat
Industry	0.319	0.263	-0.056	-1.349
Agriculture	0.139	0.121	-0.018	-0.599
Services	0.321	0.305	-0.016	-0.350
MSME	0.072	0.068	-0.004	-0.284
Demand Deposit	0.118	0.087	-0.031	-0.856
Savings Deposit	0.271	0.256	-0.015	-0.529
Term Deposit	0.611	0.656	0.045	1.373

Table A6: Loan Size and Default

This table presents the relationship between loan size and default. The data are organized at the bank-firm-year level and span the years 2017 to 2023. The dependent variable, $Default$, is an indicator variable that takes a value of 1 if the firm defaults on the respective lender in that year. The variable *Log_outstanding* is the natural logarithm of the outstanding loan between the firm and the bank in that year. We include $firm \times year$ and *bank* fixed effects in column (1), while we include $firm \times year$ and $bank \times year$ fixed effects in column (2). The standard errors reported in parentheses are robust, double-clustered at the firm & year levels, and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Default	Interest Rate
	(1)	(2)
Log_outstanding	0.006***	0.080**
	(0.000)	(0.024)
Observations	486,231	22,519
R-squared	0.325	0.805
Firm X Year F.E.	Yes	Yes
Lender F.E.	Yes	Yes

Table A7: District-level Lending

This table presents results for the differential transmission of the repo rate to district-level lending in the RBI-mandated post-repo benchmark period. The data are organized at a district-year level for the sample period 2017-2023. The dependent variable is the year-on-year growth in borrowing in the district in the given financial year. The explanatory variable, $Repo_{t-1}$, is the weighted average yearly repo rate for the previous year. The weighted average repo rate is calculated by weighing the daily repo rate by the number of days for which that repo rate persisted. Post is an indicator variable. It is 1 after the financial year 2020, corresponding to the period after the RBI mandated external benchmarking of floating rate loans effective October 1, 2019, and 0 for years before that. The district's exposure to small firms is defined as the ratio between the number of small firms and the GDP of the respective district as of the year 2020. Small firms are defined according to the Government of India's guidelines as presented in Table A1 & Section 2.2. We then define the variable $MSME\_exp$, which takes a value of one for districts with exposure above the median value of exposure to small firms as defined above and zero otherwise. Column (1) shows the results for the pooled sample with the triple interaction term as the main explanatory variable and includes district and year-fixed effects. Column (2) shows the estimates with the sample restricted to districts with exposure values above the median value of exposure to small firms. Column (3) shows the results for the sample restricted to districts with exposure values below the median value of exposure to small firms. We include district fixed effects in models of columns (2) and (3). The standard errors reported in parentheses are robust, double-clustered at the district and year levels, and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Loan Growth		
	Full Sample	MSME exposed	Non-MSME exposed
	(1)	(2)	(3)
Post × $Repo_{t-1}$ × MSME exp	0.580*		
	(0.295)		
$Repo_{t-1}$ × MSME exp	-3.555		
	(1.900)		
MSME exp × $Repo_{t-1}$	-0.482		
	(0.259)		
Post × $Repo_{t-1}$		0.570**	-0.004
		(0.213)	(0.160)
$Repo_{t-1}$		-0.513**	-0.034
		(0.198)	(0.143)
Post		-3.753**	-0.233
		(1.415)	(1.019)
Observations	758	383	375
R-squared	0.327	0.279	0.374
District F.E.	Yes	Yes	Yes
Year F.E.	Yes	No	No

Table A8: Interest Rates - Limited to systemically important NBFCs

This table presents results for the differential transmission of the repo rate to the interest rate for firms in the RBI-mandated post-repo benchmarking period. The data are organized at a loan level for the sample period FY 2017 to FY 2023. The NBFC sample is limited to systemically important NBFCs and the firm sample is limited to small firms. The dependent variable is the loan level interest rate of a firm as captured from the MCA database. The explanatory variable, $Repo_{m-1}$, is the average repo rate in the previous month. Post is an indicator variable; it is 1 after RBI mandated the repo benchmarking, applicable from 1st October 2019. Bank is an indicator variable; it is 1 if the lender is a bank and 0 for non-bank financial institutions. We include Firm × Year and Lender fixed effects in all the models and columns. We include lender-level control variables - asset size, the ratio of nonperforming assets to total assets, capital adequacy, return on assets (ROA), and outstanding loans between the respective firm-lender pair - in column 2. The interactions of the respective control variables with $repo_{m-1}$ are also included in the model. The standard errors reported in parentheses are robust, clustered at the firm & year levels and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Interest rate	
	(1)	(2)
Post × Bank × $Repo_{m-1}$	0.996**	1.085**
	(0.390)	(0.311)
Post × Bank	-6.239**	-7.069***
	(2.225)	(1.757)
$Repo_{m-1}$	0.884	1.050*
	(0.545)	(0.507)
Post	4.527	5.393
	(3.304)	(2.975)
Post × $Repo_{m-1}$	-0.707	-0.810
	(0.541)	(0.516)
Bank × $Repo_{m-1}$	-0.294	-0.459***
	(0.221)	(0.120)
Observations	3,094	3,031
R-squared	0.781	0.776
Control variables	No	Yes
Firm × Year F.E.	Yes	Yes
Lender F.E.	Yes	Yes

Table A9: Transmission of Monetary Policy to Firm Borrowing - Limited to systemically important NBFCs

This table presents results for the differential transmission of the repo rate to borrowing amounts in the RBI-mandated post-repo benchmark period. The data are organized at the firm–lender type–year level and span the sample period from 2017 to 2023. The data are limited to firms with an exclusive relationship with systemically important NBFCs. The dependent variable is the natural logarithm of the total borrowing amount. $Repo_{t-1}$ is the weighted average yearly repo rate for the previous year. The weighted average is calculated by weighting each daily repo rate by the number of days it persisted during the year. Post is a binary variable equal to 1 for financial years after 2020, corresponding to the period following the RBI's external benchmarking mandate (effective October 1, 2019), and 0 for earlier years. Bank is a binary variable equal to 1 if the lender is a bank and 0 otherwise. We include Firm $\times$ Year and lender-type fixed effects in all the models and columns. We include the following lender-type level control variables in Columns (2) and (4) - asset size, the ratio of nonperforming assets to total assets, capital adequacy, return on assets (ROA), and outstanding loans to the respective lender type - averaged at the firm-lender-type level for their bank and non-bank lenders separately. The interactions of the respective control variables with $repo_{t-1}$ are also included in the model. The standard errors reported in parentheses are robust, double-clustered at the firm and year levels, and adjusted for heteroskedasticity. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Log(borrowing)	
	(1)	(2)
Post $\times$ Bank $\times$Repo$_{t-1}$	0.594**	0.527***
	(0.213)	(0.040)
Bank $\times$ $Repo_{t-1}$	-0.375***	-0.183***
	(0.048)	(0.027)
Post $\times$ Bank	-3.402**	-2.861***
	(1.149)	(0.126)
Observations	1,182	1,104
R-squared	0.698	0.762
Control variables	No	Yes
Firm $\times$ Year F.E.	Yes	Yes
Lender type F.E.	Yes	Yes

Table A10: Transmission of Monetary Policy to Firm Borrowing - Constant Sample

This table presents results for the differential transmission of the repo rate to borrowing amounts in the RBI-mandated post-repo benchmark period. The data are organized at the firm–lender type–year level and span the sample period from 2017 to 2023. The data are limited to small firms that borrow in both pre- and post-EBLR periods. The dependent variable is the natural logarithm of the total borrowing amount. $Repo_{t-1}$ is the weighted average yearly repo rate for the previous year. The weighted average is calculated by weighting each daily repo rate by the number of days it persisted during the year. Post is a binary variable equal to 1 for financial years after 2020, corresponding to the period following the RBI's external benchmarking mandate (effective October 1, 2019), and 0 for earlier years. Bank is a binary variable equal to 1 if the lender is a bank and 0 otherwise. We include Firm $\times$ Year and lender-type fixed effects in all the models and columns. We include the following lender-type level control variables in Columns (2) and (4) - asset size, the ratio of nonperforming assets to total assets, capital adequacy, return on assets (ROA), and outstanding loans to the respective lender type - averaged at the firm-lender-type level for their bank and non-bank lenders separately. The interactions of the respective control variables with $repo_{t-1}$ are also included in the model. The standard errors reported in parentheses are robust, double-clustered at the firm and year levels, and adjusted for heteroskedasticity. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Log(borrowing)	
	(1)	(2)
Post $\times$ Bank $\times$ $Repo_{t-1}$	0.331***	0.453***
	(0.077)	(0.009)
Bank $\times$ $Repo_{t-1}$	-0.200**	-0.192***
	(0.055)	(0.009)
Post $\times$ Bank	-1.819**	-2.608***
	(0.511)	(0.039)
Observations	7,512	6,774
R-squared	0.705	0.734
Control variables	No	Yes
Firm $\times$ Year F.E.	Yes	Yes
Lender type F.E.	Yes	Yes

Table A11: Comparison of Lenders - Pre- and Post-EBLR

This table compares the balance sheet structure of banks and non-banks in pre- and post-EBLR periods. The comparison is presented for the constituents of both the asset and liability sides. The sample is restricted to 2017 to 2023. The years after 2020 are identified as post-EBLR period.

	Pre	Post	Difference	T-stat
Banks				
Demand Deposit	0.110	0.116	-0.006	-0.442
Savings Deposit	0.251	0.296	-0.045	-3.800
Term Deposit	0.646	0.592	0.054	3.556
Long Term Loans	0.404	0.382	0.022	1.027
Short Term Loans	0.595	0.618	-0.023	-1.027
NBFCs				
Long Term Loans	0.388	0.377	0.011	0.602
Short Term Loans	0.612	0.623	-0.011	-0.602
Bank Borrowings	0.312	0.260	0.052	1.900
Commercial Papers	0.259	0.171	0.088	2.984
Bonds	0.442	0.398	0.044	1.975

Table A12: Comparison of Borrowers - Pre- and Post-EBLR

This table compares the borrowers who borrow from both banks and non-banks in both pre- and post-EBLR periods. The comparison is presented for the average firm quality across the pre- and post-EBLR periods. The firm quality is measured using year-on-year sales growth, profit margin, leverage ratio (debt/asset) and cash to asset ratio. We present the differences and the t-stat corresponding to the differences.

	Pre-EBLR	Post-EBLR	Difference	T-stat
Profit Margin	0.226	0.268	-0.042	-1.776
Sales Growth	0.330	0.354	-0.024	-1.426
Leverage	0.465	0.460	0.005	0.366
Cash to Asset Ratio	0.007	0.007	0.000	2.496

Table A13: Transmission to Interest Rates: Robustness to COVID

This table presents results for the differential transmission of the repo rate to the interest rate for firms in the RBI-mandated post-repo benchmark period. The data is organized at the loan level for the sample period 2017-2023. The sample is limited to MSME borrowers. The dependent variable is the loan level interest rate of a firm as captured from the MCA database. The explanatory variable, $Repo_{m-1}$, is the average repo rate in the previous month. We include the interaction term - COVID *times* Repo *times* Bank - as a control variable in both columns. We drop the year 2021 from the sample. Post is an indicator variable that takes a value of one after the implementation of EBLR and zero otherwise. Bank is an indicator variable; it is one if the lender is a bank and zero otherwise. The data are organized at a loan level for the sample period 2017-2023. We include firm × year and lender fixed effects in all the columns. We also include bank-year level control variables and their respective interactions with $Repo_{m-1}$ in column (2). The standard errors reported in parentheses are robust, double-clustered at the firm & year levels, and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Interest rate	
	(1)	(2)
Post × Bank × $Repo_{m-1}$	0.782*	0.493*
	(0.356)	(0.296)
Bank × $Repo_{m-1}$	-0.129	-0.053
	(0.251)	(0.145)
Post × $Repo_{m-1}$	-0.478	-0.178
	(0.404)	(0.393)
Post × Bank	-4.692*	-3.462
	(2.196)	(1.828)
$Repo_{m-1}$	0.581	0.475
	(0.353)	(0.321)
Post	3.092	1.619
	(2.347)	(2.241)
Observations	3,004	2,713
R-squared	0.803	0.794
Control variables	No	Yes
Firm × Year F.E.	Yes	Yes
Lender F.E.	Yes	Yes

Table A14: Transmission to Lending: Robustness to COVID

This table presents results for the differential transmission of the repo rate to the borrowing amount of firms in the RBI-mandated post-repo benchmark period. The data are organized at a firm-year-lender type level for the sample period 2017-2023. The sample is limited to MSME borrowers. The dependent variable is the natural logarithm of the total borrowing amount of a firm in the given financial year. The explanatory variable, $Repo_{t-1}$, is the weighted average yearly repo rate for the previous year. The weighted average repo rate is calculated by weighing the daily repo rate with the number of days for which that repo rate persisted. We include the interaction term - COVID *times* Repo *times* Bank - as a control variable in both columns. We drop the year 2021 from the sample. Post is an indicator variable that takes a value of one after the implementation of EBLR and zero otherwise. Bank is an indicator variable; it is 1 if the lender is a bank and 0 for non-banks. We include firm × year fixed effects in all the models and columns. We include bank-year level control variables listed in Table 4 and their respective interactions with $Repo_{m-1}$ in column (2). The standard errors reported in parentheses are robust, double-clustered at the firm & year levels, and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Log(borrowing)	
	(1)	(2)
Post × Bank × $Repo_{m-1}$	5.642***	7.152***
	(0.025)	(0.005)
Bank × $Repo_{m-1}$	-0.175**	-0.209***
	(0.066)	(0.003)
Post × Bank	-22.981***	-29.206***
	(0.122)	(0.026)
Observations	8,790	7,802
R-squared	0.739	0.770
Control variables	No	Yes
Firm × Year F.E.	Yes	Yes
Lender type F.E.	Yes	Yes

A RBI's EBLR circular

RBI/2019-20/53
DBR.DIR.BC.No.14/13.03.00/2019-20

September 04, 2019

All Scheduled Commercial Banks (excluding RRBs)
All Small Finance Banks
All Local Area Banks

Madam / Dear Sir,

External Benchmark Based Lending

As you are aware, Reserve Bank had constituted an Internal Study Group (ISG) to examine various aspects of the marginal cost of funds-based lending rate (MCLR) system. The final report of the ISG was published in October 2017 for public feedback. The ISG observed that internal benchmarks such as the Base rate/MCLR have not delivered effective transmission of monetary policy. The Study Group had, therefore, recommended a switchover to an external benchmark in a time-bound manner.

2. As a step in that direction, it was announced in the fifth bi-monthly Monetary Policy Statement for 2018-19 under 'Statement on Developmental and Regulatory Policies' dated December 05, 2018, that all new floating rate personal or retail loans and floating rate loans to Micro and Small Enterprises extended by banks from April 1, 2019 shall be linked to external benchmarks. Subsequently, it was announced in the first bi-monthly Monetary Policy Statement for 2019-20 under 'Statement on Developmental and Regulatory Policies' dated April 04, 2019 to hold further consultations with stakeholders and work out an effective mechanism for transmission of rates. Based on the consultations with stakeholders, it has now been decided to link all new floating rate personal or retail loans (housing, auto, etc.) and floating rate loans to Micro and Small Enterprises extended by banks with effect from October 01, 2019 to external benchmarks.

3. Accordingly, RBI instructions contained in Master Direction on Interest Rate on Advances issued vide DBR.Dir.No.85/13.03.00/2015-16 dated March 03, 2016 are amended as under:

3.1 The existing paragraph No. 7 of the aforesaid Master Direction stands replaced as under:

(a) All new floating rate personal or retail loans (housing, auto, etc.) and floating rate loans to Micro and Small Enterprises extended by banks from October 01, 2019 shall be benchmarked to one of the following:

- Reserve Bank of India policy repo rate
- Government of India 3-Months Treasury Bill yield published by the Financial Benchmarks India Private Ltd (FBIL)
- Government of India 6-Months Treasury Bill yield published by the FBIL
- Any other benchmark market interest rate published by the FBIL.

(b) Banks are free to offer such external benchmark linked loans to other types of borrowers as well.

(c) In order to ensure transparency, standardisation, and ease of understanding of loan products by borrowers, a bank must adopt a uniform external benchmark within a loan category; in other words, the adoption of multiple benchmarks by the same bank is not allowed within a loan category.

3.2 A new paragraph No.8(e) is added to the aforesaid Master Direction as given below:

Spread under External Benchmark

Banks are free to decide the spread over the external benchmark. However, credit risk premium may undergo change only when borrower's credit assessment undergoes a substantial change, as agreed upon in the loan contract. Further, other components of spread including operating cost could be altered once in three years.

3.3 A new paragraph No. 9(ii) is added to the aforesaid Master Direction as given below:

Reset of Interest Rates under External Benchmark

The interest rate under external benchmark shall be reset at least once in three months.

B Machine Learning Method

The machine learning approach serves two purposes. First, it provides a high-dimensional, nonparametric complement to our DID estimates, confirming whether aggregate patterns hold when the interest rate–setting function is learned directly from the data. Second, it allows us to compare observed and counterfactual outcomes across banks and nonbanks for both small and large firms.

To implement the ML approach, we begin by removing all variables with more than 30% missing observations. This threshold is commonly used in applied machine learning research to reduce the noise due to highly sparse features (Psyridou et al. (2024)). Among the remaining variables, we impute missing values in numerical columns using the median, a method that is robust to outliers and appropriate for skewed distributions (Little and Rubin (2002)). Imputing missing values allows us to retain observations that would otherwise be discarded due to missing entries in a small number of variables, thereby preserving sample size and reducing the risk of bias in the training model. Importantly, all imputations and transformations are performed using only the pre-EBLR data to avoid any form of look-ahead bias. Categorical variables are converted into numeric form using factor encoding. To ensure the model captures generalizable relationships, we omit variables like firm & creditor identifiers and exact charge dates that could tie predictions too closely to specific entities or events. To further mimic a realistic training environment and avoid observation-level look-ahead bias, we construct yearly lags of all financial variables. The resulting dataset includes a set of firm-level characteristics (e.g., size, asset proxies), loan-level features (e.g., loan amount), and lender attributes (including a binary indicator for whether the lender is a bank).

We then define the pre-policy sample as all loans with charge dates prior to October 1, 2019, the date of EBLR implementation. This sample is randomly split into 80% training and 20% validation sets, following standard ML practice (Bertsimas and Paskov (2020)). We estimate the mapping from characteristics to loan rates using an Extreme Gradient Boosting (XGBoost) regressor (Chen and Guestrin (2016)), an algorithm well suited to structured economic data (Mullainathan and Spiess (2017)). The model is trained with 500 boosting rounds, a learning rate of 0.05, and a maximum tree depth of 6, with other hyperparameters at default values. The target variable is the loan interest rate.

On the 20% pre-policy validation sample, the XGBoost model achieves an overall R^2 of 0.665, a mean absolute error (MAE) of 0.696, and a mean squared error (MSE) of 1.354. Crucially, mean prediction errors (MPEs) are very close to zero across all borrower–lender groups: MSME $\times$ Bank = 0.026 pp ($t = 4.35$), MSME $\times$ Non-Bank = –0.030 pp ($t = -2.82$), Non-MSME $\times$ Bank = 0.002 pp ($t = 0.75$), and Non-MSME $\times$ Non-Bank = –0.001 pp ($t = -0.33$). These results confirm that the model does not systematically under- or over-predict interest rates in the pre-EBLR period and captures meaningful variation in loan pricing.

Applying the trained model to post-EBLR loans yields counterfactual interest rates. The results are reported in Panel A of Table B1. Comparing observed and counterfactual values, we find systematic overprediction for loans to small firms. Average MPEs equal 1.45 percentage points for small firms' borrowing rate through banks ($t = 51.5$) and 0.82 percentage points for small firms' borrowing through non-banks ($t = 8.7$). For large firms, MPEs are smaller —0.49 pp for banks ($t = 9.69$) and 0.18 pp for nonbanks ($t = 17.3$). These results confirm that bank lending rates to small firms declined far more sharply than predicted by the pre-EBLR pricing model, consistent with the mandate's design.

B.1 Machine Learning Test for Borrowing

We extend this approach to lending quantities. Using the same training/validation split, we train an XGBoost model on pre-EBLR data to predict borrowing amounts from firm, loan, and lender characteristics. In the validation set, the model achieves an R^2 of 0.5183. Prediction errors are statistically indistinguishable from zero for both small firms and large firms, indicating no systematic bias in the pre-period.

We then generate counterfactual borrowing amounts for the post-EBLR period. The results are reported in Panel B of Table B1. The mean prediction error is 63.78 for banks (large overprediction) and only 0.36 for nonbanks, implying that actual bank lending to small firms expanded far less than expected under the pre-EBLR mapping. For large firms, the MPEs are much smaller (12.37 and 3.48 for banks and nonbanks, respectively).

Together, these ML results closely mirror our regression-based estimates. They reinforce our interpretation that the EBLR mandate—by enforcing stronger price pass-through—reduced the responsiveness of bank lending quantities to policy rate cuts.

Table B1: ML-Based Counterfactual Evaluation

This table presents results from the machine learning-based counterfactual analysis of repo rate transmission to firm outcomes in the pre- and post-EBLR periods. Panel A uses the interest rate on new loans, and Panel B uses the total loan amount as the dependent variable. We estimate an XGBoost regression model using the pre-EBLR data and generate counterfactual predictions for the post-EBLR period. We report mean prediction error (MPE), mean absolute error (MAE), and R-squared (R^2) for each lender × borrower group. MPE is defined as the average difference between predicted and actual values. MAE reflects the average magnitude of absolute prediction errors. t-statistics for the MPE values are reported in parentheses. The pre-period evaluation is based on a 20% validation split, while the post-period results are full-sample predictions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel A: Dependent Variable – Interest Rate

	Pre-EBLR (Validation)		Post-EBLR (Prediction)	
	MPE	MAE	MPE	MAE
MSME × Bank	0.0257***	0.1002	1.4549***	1.7653
	(4.345)		(51.479)	
MSME × Non-Bank	-0.0299**	0.1176	0.8171***	2.0112
	(-2.822)		(8.723)	
Non-MSME × Bank	0.0017	0.0876	0.4887***	1.3302
	(0.747)		(9.694)	
Non-MSME × Non-Bank	-0.0011	0.0958	0.1810***	1.5820
	(-0.329)		(17.321)	

Panel B: Dependent Variable – Loan Amount

	Pre-EBLR (Validation)		Post-EBLR (Prediction)	
	MPE	MAE	MPE	MAE
MSME × Bank	-4.14	478.33	63.78***	530.06
	(-0.599)		(14.447)	
MSME × Non-Bank	1.12	336.93	0.36	366.67
	(0.216)		(0.112)	
Non-MSME × Bank	-1.31	56.13	12.37***	42.37
	(-1.128)		(9.148)	
Non-MSME × Non-Bank	-0.87	44.14	3.48***	34.67
	(-1.554)		(9.386)	

C Placebo Tests

C.1 False small firms Classification Placebo

We next conduct placebo tests to further rule out the possibility that our results are spurious. As discussed in Section 2, the EBLR mandate applied only to small firms. We exploit this fact to construct a placebo sample by randomly categorizing 40 percent of the large firms as small firms, matching the share of small firms in our actual sample.[1] We then re-estimate equations 2 and 3 on this placebo sample and repeat the exercise 1,000 times.

Panels A and B of Figure C1 plot the kernel densities of the estimated coefficients from equations 2 and 3, respectively. The red dashed lines indicate the 5th and 95th percentiles of the simulated distributions, while the blue dashed lines mark the actual coefficients reported in Tables 3 and 4. In both panels, the true coefficients lie outside the 95th percentile of the placebo distributions.

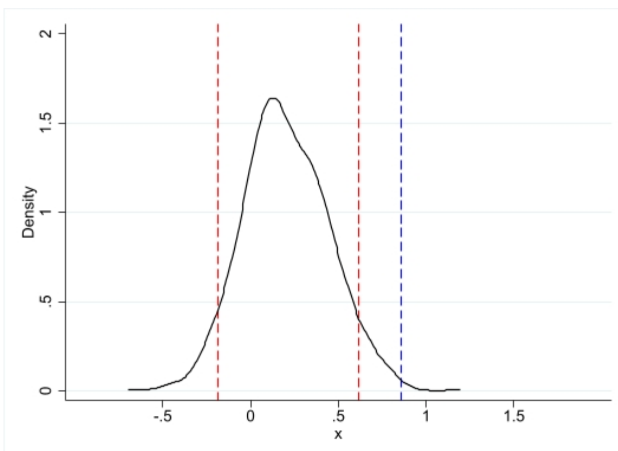
This placebo exercise confirms that our findings are not the result of spurious correlations or random sampling variation, but are specific to the mandated small firms segment.

C.2 False Year Placebo

We further test the robustness of our results by assigning false implementation years to the EBLR reform. Specifically, we designate 2016, 2017, and 2018 as placebo EBLR implementation years and re-estimate equation 3 using a three-year window around each of these dates. The window limits the analysis to the pre-EBLR period. Table C1 reports the results. Across all placebo years, the coefficient on the triple interaction term is small and statistically indistinguishable from zero. These findings reinforce that the observed decline in the transmission of policy rate cuts to loan quantities is unique to the actual implementation of the EBLR mandate, rather than an artifact of pre-existing or spurious trends.

Figure C1: The figures plot the kernel density of the coefficient corresponding to false definitions of small firms. We take the set of non-MSME firms from our sample and randomly assign 40% of the firms as small firms. We then estimate equations 2 and 3 using the above sample of firms. The data are organized at the loan level in panel A, and the firm-lender type-year level in panel B. The dependent variable is the interest rate in Panel A, and the log borrowing in Panel B. The explanatory variable in the panel is the triple interaction between post, one month lagged repo, and an indicator variable identifying banks. The explanatory variable in the panel is the triple interaction between post, one year lagged repo, and an indicator variable identifying banks. We repeat the estimation 1000 times and plot the coefficients in panels A and B, respectively. The red dashed lines show the 95% confidence interval. The blue dashed lines show our estimated coefficients using the actual set of small firms.

(a) Interest Rate



(b) Borrowings

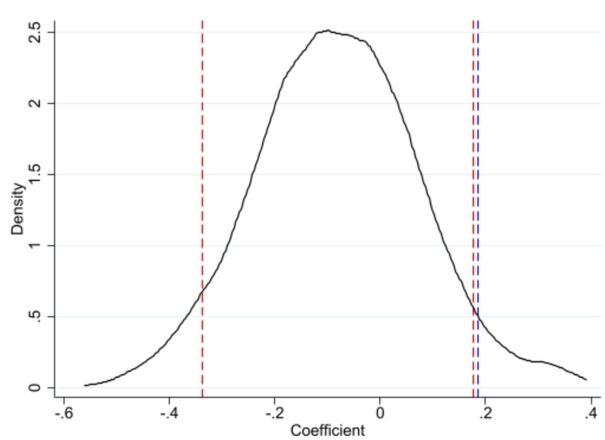


Table C1: Borrowing: False year placebo

This table presents results for the differential transmission of the repo rate to the unsecured borrowing amount of firms in the RBI-mandated post-repo benchmark period. The sample spans 2013 to 2018 in column 1, 2014 to 2019 in column 2, and 2015 to 2020 in column 3. The sample is limited to small firms, as defined in Table A1 & Section 2.2, in all columns. The dependent variable is the natural logarithm of the total borrowing amount of a firm in the given financial year. The explanatory variable, $Repo_{t-1}$, is the weighted average yearly repo rate for the previous year. The weighted average repo rate is calculated by weighing the daily repo rate by the number of days for which that repo rate persisted. Post is an indicator variable. It is 1 for the financial year 2016 and onwards in column 1, 2017 and onwards in column 2, and 2018 and onwards in column 3. Bank is an indicator variable; it is 1 if the lender is a bank and 0 for non-banks. We include Firm $\times$ Year and Lender type fixed effects in all the models and columns. The standard errors reported in parentheses are robust, double-clustered at the firm & year levels and adjusted for heteroskedasticity. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Log(borrowing)		
	2016	2017	2018
	(1)	(2)	(3)
Post X Bank X $Repo_{t-1}$	-0.230**	-0.249	-0.502
	(0.067)	(0.218)	(0.451)
Bank X $Repo_{t-1}$	0.196***	0.011	0.176**
	(0.012)	(0.215)	(0.062)
Post X Bank	1.617**	1.541	3.358
	(0.493)	(1.649)	(2.758)
Observations	10,266	10,244	10,044
R-squared	0.751	0.745	0.742
Firm X Year F.E.	Yes	Yes	Yes
Lender type F.E.	Yes	Yes	Yes

D Government Interventions During COVID-19 in India

- **Emergency Credit Line Guarantee Scheme (ECLGS), May 2020**: Collateral-free guaranteed loans to MSMEs (later expanded to other sectors), applicable through all lenders

- **PM SVANidhi Scheme, June 2020**: Collateral-free working capital loans to street vendors

- **Production Linked Incentive (PLI) Schemes, 2020–21**: Incentives to boost domestic manufacturing in key sectors

- **Moratorium on Loan Repayments, March 27, 2020**: Allowed a 6-month moratorium on term loan installments - across all lenders

- **Resolution Framework for COVID-19 Stress, August 2020**: Loan restructuring scheme for stressed borrowers - across all lenders

- **Regulatory Forbearance, 2020-21**: Relaxations in NPA recognition norms and capital adequacy compliance.

- **Special OMOs and G-SAP, 2021**: Secondary market operations to stabilize bond yields and ensure liquidity.