

Regulation or rate cut? Pricing India's gold-loan directions

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Abstract

The Reserve Bank of India issued draft directions regarding lending against gold collateral on 9 April 2025. It issued softened, finalised directions in the opposite direction on 6 June 2025, a decision made on two separate monetary policy announcement days, both followed by rate cuts. Such an event study does not address how much of gold lenders' price movement is purely regulatory rather than monetary policy driven. This paper separates regulatory from monetary effects using a control group: listed non-bank lenders that received the same monetary policy shock but had neither substantial gold collateral loan books, and compares abnormal return differences between groups. The prior announcements on the draft regulations, which appeared highly restrictive to market investors, resulted in a 4.7 percentage point decline in three-day stock performance compared to the control group-even after consideration of gold price in a two-factor model; three different estimation windows as well as exclusion of the biggest firm size, are confirmed, not sensitive to parameter choice and to a 2000 pseudo-event dates randomisation test. However, the later announcements show strikingly different results: although gold lenders' stock performed well, with gross abnormal returns increasing by 5.8%, the control group also rose by 4.2% on those days,, so that the 1.6% difference is not statistically distinguishable. The repo cut made earlier that morning explains most of this phenomenon, while the residual rallies seem anticipated given the pre-event tests; this paper further delineates the effects by decomposing event days and suggests that monetary policy has its impact on the day, while regulatory policy has its impact on the day after. No particular effect can be detected for the event in September 2025. Overall, cumulative losses of 3.1% for the control group of loan-book owners from April to June suggest that any prior warning signal is not easily reversed.

Keywords: Gold loans; macroprudential regulation; loan-to-value; event study; non-banking financial companies; monetary policy announcements; India

JEL classification: G14; G21; G28; E52

1. Introduction

Gold-collateralised lending has no direct counterpart to the type of economy in which most regulation event studies have been run. Rather than taking a mortgage on a house, individuals deposit jewellery in a lending institution, a process that involves relatively small and short loans that may take mere minutes to

disburse and that primarily serves unbanked individuals. Thus, regulation of this market involves both prudential and distribution concerns. In 2025,, the Reserve Bank of India announced moves on this issue twice, but first had to contend with measurement difficulties that have since arisen from that double dose.

The first announcement was published on 9 April 2025 as draft instructions to harmonise lending regulations against gold and silver collateral. The RBI's proposed stricter valuations, monitoring rules, and limits on loan end-use implied a hit on existing specialist gold lenders, who may have struggled with their operating model under those constraints. However, on 6 June 2025, the RBI considerably relaxed the final instructions on several counts, raising loan-to-value ratios and providing significant concessions for smaller loans and consumption loans under Rs 2.5 lakh, allowing loan-to-value caps of up to 85 per cent for small loans and 75 per cent for higher sums. A later announcement of some interpretative clarification for this date's rules came on 29 September 2025, setting an operative date of 1 April 2026 for this regulation bundle.

This set of two announcements poses a measurement problem, though. At least since the February 2025 event described by the RBI Statement on Developmental and Regulatory Policies in relation to that announcement, the two have been published simultaneously with a monetary policy decision; hence, on 9 April 2025 and 6 June 2025, they were, respectively, reported with cut announcements in 9 April's and 6 June's respective central bank monetary policy reports (RBI, 2025d; RBI, 2025e). Those cuts of 25 and 50 basis points in the policy repo rate, respectively, were material shocks to highly leveraged lenders. A simple statistical measure applied to any broadly defined market index of firm returns would impute those losses solely to the regulation, and this is incorrect.

Hence, the second measurement point on the bundle has informed our approach here: we use listed, non-bank lenders without substantial gold-lending operations as a control group. Like actual gold lenders, these were impacted by the simultaneous central bank news of a rate cut and financing condition changes and thus sectoral sentiment. The control group's lack of response to gold-loan regulation allows a calculation of the actual impact,, separating the regulatory announcement effect from other announcements. That is essentially what Bruno et al. (2018) did: their study of the impact of changes in liquidity regulation on equity prices accounted for a simultaneously announced change in prudential lending requirements and builds on their framework to account for a non-incidental "announcement bundle" made in a central bank communication.

Three-day cumulative average abnormal returns for the regulation bundle went negative in this test: -4.7 percentage points; the result was significant through both parametric and rank tests, and also via randomised trials over 2000 simulated event dates. For the June directions: no significant market reaction was detectable by our measurement method. Raw abnormal return on that day went up, but control measures also registered a jump, and so the difference between a gold lenders' and control group's share prices failed to reach significance on a 3-day rolling average calculation.

The daily calculation shows that once the market processed the text of June direction releases, gold lenders jumped by an impressive 3.1 percentage points (and that for reasons other than the rate cut, led the control group's share price up at the announcement on the same morning of the first announcement) in the following session.

September release of clarification changes had no impact on either group, not even on the measure with regard to how other central bank-related events affect gold-backed lending shares compared to a control group.

No empirical study has measured a market reaction to this specific policy package to date, and there is even less research about the extent to which capital market theory has incorporated or rejected measures applied to gold-backed collateral. That broader point highlights the cost and misstatement resulting from announcement bundle ignorance, as the 5.8 per cent "relief rally" claimed to have resulted from June's announcement was clearly non-existent. Also, it bears stating that the overall change has still not been reversed and remains a tightened regulatory stance; reflecting loan-to-value loosening is perhaps not as impactful a tool as tightening loan-to-value limits (Richter et al., 2019), or it reflects that risk remains higher.

2. Regulatory setting and timeline

Indian gold loans are given by banks, by dedicated Non-Banking Financial Companies (NBFCs) and by still quite a large unorganised sector of pawn brokers. Regulatory treatment was a cumulation over more than thirty RBI circulars written between 1964 onward, with, for a start, different rules for the three separate categories of regulated entities – banks, NBFCs and co-operative societies. The circular of 2025 consolidated these into a single set applicable to all.

This sequence matters for identifying the events, and the timeline of these is presented below in Table 1. The draft directions of 9 April 2025 were directive (RBI, 2025a); the 6 June 2025 directions were more permissive on the loan-to-value ceilings (RBI, 2025b). The Loan-to-value ratios allowed were as much as 85% up to Rs 2.5 lakhs for consumption loans and were set to 80% and 75% for loans from Rs 2.5-5 lakhs and above, and full implementation was postponed to 1 April, 2026. One September amendment was explicitly classificatory (RBI, 2025c). There was an additional July 2025 RBI guidance document on pledging of gold and silver against agricultural and MSME loans, which is outside of all event intervals considered here and which we therefore exclude (RBI, 2025f).

Two out of the three dates corresponded to rate policy decisions by the RBI; one did not. The fact that there is an asymmetry across the timeline is helpful. The rate-policy neutral amendment of September offers the benefit of a check to confirm whether or not the control group reacts similarly in the absence of a common shock that all agents must accommodate.

Table 1. Regulatory and monetary policy events

E1	9 April 2025	Draft directions on lending against gold and silver collateral released for comment; tighter valuation, monitoring and end-use conditions proposed	Repo rate cut 25 bps to 6.00%
E2	6 June 2025	Final directions issued (RBI/2025-26/47); LTV raised to 85% for consumption loans up to Rs 2.5 lakh; compliance by 1 April 2026	Repo rate cut 50 bps to 5.50%; CRR cut 100 bps; stance changed to neutral

E3	29 September 2025	First amendment (RBI/2025-26/84), clarificatory in character	None
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Sources: RBI (2025a, 2025b, 2025c) for the regulatory content and RBI (2025d, 2025e) for the monetary decisions. Event dates are the dates of public release; all three fell on trading days.

3. Related literature and hypotheses

A long-standing idea involves employing share-price reactions to value regulation. Schwert (1981) outlines the main idea, but Binder (1985) demonstrates how unstable these estimations become once announcements can be foreseen or come close one after another, while Peltzman (1976) and Kane (1977) provide the political economy framework under which banks and their regulators get used to each other over successive policy rounds. Recent banking literature generally confirms the use of markets to price regulatory news, and is increasingly precise on exactly what is being tested.

Bruno et al. (2018) establish negative abnormal returns on liquidity regulation; the impact dissipates substantially after controlling for co-announcements with capital regulations, which directly mirrors the methodology applied in this paper.

Pancotto et al. (2020) track shareholders' losses over the various dates of European Banking Union announcements; Bobiceanu et al. (2026) confirm that while prudential easing was priced negatively during the COVID pandemic, it was conditional on central bank independence.

A comparable tradition of study shows that monetary announcements move bank equity significantly and in a heterogeneous fashion. Ricci (2015) demonstrates that European banks reacted more strongly to unconventional policy measures than to rate changes, and weaker balance sheets amplify this response. The parallel documented in O'Donnell et al. (2024) in relation to 188 announcements during the pandemic similarly indicates disparate impact. Whenever monetary and regulatory policy news arrive at the same time, and the second policy is the primary driver of the lender value, attribution of news value entirely to the former becomes untenable. El Ghouli et al. (2022) address precisely this sort of attribution challenge.

As regards the instrument, while literature on macroprudential policy has grown well, it focuses nearly entirely on mortgages. Gatt (2023) provides a general survey. Research by Richter et al. (2019) estimates that every 10-ppt reduction in LTV will cut economic output by 1.1 per cent over four years (comparable to a 25bp policy rate hike) and that contractionary actions are more impactful than expansionary ones.

Mokas and Giuliadori (2023) document movement of household credit and house prices at the announcement level prior to final implementation in the case of LTV restriction and hence confirmation of an anticipatory motive driving an appropriate choice of announcement time.

Both Van Bekkum et al. (2024) and Tzur-Ilan (2023) trace household adjustment behaviours, and Braggion et al. (2023) find evidence of borrowers exiting into more loosely-regulated entities.

The foregoing literature contains three notable gaps. First, in all of the cases listed, the underlying collateral is residential property. Gold jewellery has high liquidity, variability of price, cultural

importance, and a client base of predominantly liquidity-constrained individuals; hence its credit value and that of other jewellery to banks might differ from that of housing and be priced very differently. The second gap narrows this scope to methodology; while market reaction to the bundling of discrete news (as in Bruno et al. (2018)) can be treated, the institutional design here requires careful consideration of a formal announcement that packs regulatory and monetary communication together. Lastly, the documented asymmetry between tightening and easing in terms of real variables has not, to the best of my knowledge, been tested with announcement prices, where the test is inexpensive but potentially informative about the resilience of regulatory impacts. Indian literature is still limited. Anantha Krishnan and Sengupta (2024) examine investor response to fraudulent and regulatory action, but there has been no announcement-based valuation of gold-collateral banking sector activity. This suggests three testable hypotheses.

H1: The initial announcement of directions, perceived as restrictive, will elicit negative abnormal returns for gold-collateral lenders when compared to a matched control group.

H2: The second (perceived as easier) announcement of directions, paired with monetary stimulus, will elicit positive abnormal returns for gold-collateral lenders, beyond those solely explainable by the concurrent ease in policy rates.

H3: The magnitude of abnormal returns resulting from both announcements will be more pronounced in the case of heavily gold-dependent lenders than in the case of highly diversified ones.

4. Data and method

4.1 The sample

The empirical analysis covers 15 lenders listed on the National Stock Exchange of India, comprising both non-banking financial companies and banks. Stock-price data for 390 trading days spanning 3rd May, 2024-28th November, 2025 have been used. Lenders are divided into a treated group and a control group of similar riskiness.

The treated group comprises 7 lenders with substantial exposure to the gold-backed segment – 3 speciality gold lenders and 4 lenders for whom it was just one part of their diversified book; while the control group comprises 8 financial intermediaries with similar business scope but offering only consumer and SME credit products, and which do not lend against gold jewellery. The benchmark market index is the NIFTY 50 (traded on the NSE); a gold-indexed ETF provides the gold price appreciation path in Indian rupees.

Lenders have been assigned to the treated and control groups purely based on the nature of their business, not by outcome. Muthoot Capital Services, a widely known lender with "Muthoot" in its brand name, had to be reclassified based on its loan book (primarily financing for vehicles and not jewellery), thus eliminating it from the treated group solely on the grounds of family branding. A single trading day that did not show any price movement across all sample stocks was also removed to eliminate issues of inert trading.

Table 2. Sample composition and pre-event gold sensitivity

Muthoot Finance	Treated	0.86	0.476	1.53
Manappuram Finance	Treated	1.30	-0.180	2.65
IIFL Finance	Treated	1.60	0.117	2.28
Fedbank Financial Services	Treated	0.83	0.442	1.77
CSB Bank	Treated	0.49	0.347	1.61
South Indian Bank	Treated	1.15	0.280	1.84
Federal Bank	Treated	0.78	0.240	1.53
Bajaj Finance	Control	1.13	0.068	1.37
Shriram Finance	Control	1.62	-0.034	1.72
Cholamandalam Investment	Control	1.32	-0.024	1.91
M&M Financial Services	Control	1.49	0.219	1.62
L&T Finance	Control	1.54	0.151	1.70
Sundaram Finance	Control	1.64	0.067	2.60
Poonawalla Fincorp	Control	1.01	0.344	2.63
Aditya Birla Capital	Control	1.54	0.011	1.59

Coefficients are from a two-factor regression of daily log returns on the NIFTY 50 return and the return on an NSE-listed gold ETF, estimated over the 140 trading days ending 31 days before E1. Group mean gold beta is 0.246 for treated firms and 0.100 for controls (Welch $t = 1.51$, $p = 0.164$). Over the full sample, daily returns average 0.094 per cent for treated firms (s.d. 2.10) and 0.085 per cent for controls (s.d. 2.13); the NIFTY 50 averages 0.034 per cent (s.d. 0.85).

4.2 Estimates

The model estimated the market abnormal returns for the 140 trading days from $t-170$ to $t-31$ around each event, leaving an unobservable 30 days, since speculation has to remain outside the estimations. The abnormal returns are compounded over 5 windows. Tests applied for statistical inference over the group average mean are the standardised cross-sectional test of Boehmer et al. (1991), which is robust to variance due to the regulatory event, known for having a reliable impact on market price, and the group averages tested along with the sign test and Mann-Whitney rank test owing to small groups of assets.

Interest is paid on the difference in the mean returns between the treatment and control group- a difference-in-differences estimator applied to the abnormal returns rather than on raw ones. The assumption that each group had similar exposures to monetary and sectoral shocks but were only impacted differently due to gold loan regulation has important caveats. It is assumed that gold lenders are different in their price elasticity of demand for loans, and to avoid this, the Sep event, with no policy announcement, was used to check if the control group would behave normally if the common shocks were excluded (Welch's t-test and Mann-Whitney test were run). To address these potential concerns of robustness and contamination from real news, tests against an empirical distribution calculated on 2,000 randomised event days within the periods of real events are implemented.

5. Results

5.1 The draft directions were priced as materially restrictive

The following table shows the main estimates. While the treated group experienced negative returns to the tune of 4.59% over the 3-day event window, the control group had its price gain slightly, on average, 0.09%; the drop observed in the treated group's value exceeds 4.68 % in difference (t-test= - 2.54, p= 0.036, Mann-Whitney p = 0.009). The return recorded over the 7-day windows shows the larger gap between values of affected financial stocks. Between the event and subsequent events, market agents could have considered LTV reduction to be an even severer constraint, evidenced by its increased negative significance to -8.32%. Indeed, while control groups are positively trending on interest rate drops at 7-day windows from the event, loan lender companies continue to be under downward pressure.

A further interpretation comes from the magnitude in comparison to relevant industry indices. By using Richter et al. (2019)'s rule of thumb relating 10% points LTV with a 25 bps increase in monetary policy rate, a 4.68% loss in stock value represents an equivalent monetary tight policy of nearly 1.7% in rate equivalent.

Table 3. Cumulative average abnormal returns and the treated minus control difference

E1	(-1,+1)	-4.59%	0.064	+0.09%	0.960	-4.68 pp	-2.54	0.036
	(0,+1)	-3.86%	0.119	-1.01%	0.057	-2.85 pp	-1.48	0.180
	(0,+3)	-3.53%	0.190	+0.56%	0.911	-4.09 pp	-1.76	0.112
	(-3,+3)	-3.87%	0.160	+4.45%	0.007	-8.32 pp	-3.28	0.008
	(-5,+5)	+2.17%	0.543	+3.68%	0.125	-1.51 pp	-0.39	0.704
E2	(-1,+1)	+5.83%	0.014	+4.20%	0.002	+1.63 pp	0.84	0.424
	(0,+1)	+6.65%	0.003	+4.30%	0.001	+2.35 pp	1.46	0.180
	(0,+3)	+6.73%	0.011	+3.01%	0.010	+3.72 pp	1.84	0.101

	(-3,+3)	+6.57%	0.019	+2.25%	0.117	+4.32 pp	1.75	0.107
	(-5,+5)	+12.14%	0.012	+2.55%	0.130	+9.59 pp	2.61	0.029
E3	(-1,+1)	+1.93%	0.066	+1.09%	0.151	+0.84 pp	0.75	0.470
	(0,+1)	+2.53%	0.022	+1.88%	0.061	+0.65 pp	0.51	0.622
	(0,+3)	+3.14%	0.013	+2.19%	0.220	+0.95 pp	0.52	0.612
	(-3,+3)	+1.88%	0.309	+1.60%	0.367	+0.28 pp	0.12	0.903
	(-5,+5)	+5.33%	0.006	+2.60%	0.027	+2.72 pp	1.66	0.124

Abnormal returns from a market model on the NIFTY 50, estimated over t-170 to t-31. Group p-values are from the standardised cross-sectional test of Boehmer et al. (1991); the difference is tested with Welch's t-test. n = 7 treated and 8 control firms. pp denotes percentage points.

5.2 The June rally was almost entirely monetary

By the close over the final three days, the treated sample increased by 5.83 per cent, which would be described as an extreme positive regulatory event in a stand-alone market event study of three days. The control group gained 4.20 per cent over the same three days, leaving a treated-minus-control difference of only 1.63 percentage points ($p = 0.42$; empirical $p = 0.49$). Two and a half of what a simplistic regulatory event study would assign to relaxation of the gold-loan rules belongs to the 50bps repo cut, reserve ratio cut and morning-of surprise change of stance in reality, approximately.

In Table 4, the actual regulatory event shows up: on 6 June, controls gained 0.79% more than treated. That seems appropriate if a monetary shock, which might have been expected to support diversified leverage lenders just as much as it would gold lenders, was more important than the announcement. The gap on the next day, 9 June, is another issue entirely: treated firms averaged +4.75 per cent against +1.60 per cent for controls, a difference of 3.14 percentage points. Since the announcement on direction was made late one Friday night, it was priced in on the Monday when the terms were reviewed after reading the document, it is assumed.

Table 4. Daily average abnormal returns around the two bundled announcements (per cent)

-2	+0.06	+1.09	-1.03	-0.21	-0.70	+0.50
-1	-0.73	+1.10	-1.82	-0.82	-0.10	-0.72
0	-1.52	-0.10	-1.42	+1.91	+2.70	-0.79
+1	-2.34	-0.90	-1.43	+4.75	+1.60	+3.14

+2	-0.16	+0.72	-0.88	+0.05	-0.33	+0.38
+3	+0.49	+0.85	-0.36	+0.03	-0.97	+1.00

Day 0 is 9 April 2025 for E1 and 6 June 2025 for E2. Day +1 for E2 is Monday 9 June 2025. Averages are across firms within each group.

5.3 Business model, not collateral sensitivity

H3 is partially supported, but the direction of failure also provides insight. Separating out the treated group, the three specialised gold NBFCs cumulatively gain 9.94 per cent over the course of the directions and counter directions, versus 2.74 per cent for the four diversified lenders. The 7.20-point spread is significant at Welch $t = 4.17$, $p = 0.022$. Around the draft directions, the same ordering prevails at -5.61 points, but this does not achieve statistical significance (three firms in each cell).

The continuous measure does not perform as well. Rank all fifteen firms on pre-event gold beta and cross-correlate it with the announcement return. The Spearman correlation with event return is -0.17 for E1 ($p = 0.54$) and zero around event windows. Indeed, the treated firms do have higher average gold betas (0.246 versus 0.100), although the difference is not significant and one specialised firm exhibits a negative beta for the estimation window. Logically, the market responds not to whether a stock moved with the gold price in the past, but to whether a policy affects one's business model, which is what you expect for a regulation targeted at allowed lending volume or underwriting procedures, not at collateral pricing. Reporting the null anyway may serve the purpose of illustrating where this measure does not operate. Price-based proxies for exposure will not work whenever the regulation touches on business operations, not collateral prices.

5.4 Robustness

The three-day difference described above is subjected in Table 5 to four different specifications, plus a randomisation test. Adding an explicit gold factor to the return-generating equation widens the E1 effect slightly to -5.32 percentage points. Shortening the estimation window to 100 days gives the same -5.32 points; lengthening it to 170 days gives -4.89 points. Excluding the largest observed effect (Muthoot Finance, a single entity down by -14.24 percentage points), leaves a size effect of -3.07 percentage points with $p=0.009$. So there is no single large actor or choice of estimator or window driving the result. Every one of these five specifications yielded insignificance on E2 and E3.

Figure 2 displays results from a randomisation test. In each trial, 2,000 pseudo-event dates are drawn from the sample with the original announcement windows omitted. This yields a distribution with a mean of -0.42 percentage points and an SD of 1.99 points, and 5th and 95th percentiles of -3.61 and 2.53 points. The observed E1 difference (-4.68 points) lies outside this distribution, resulting in an empirical $p=.025$ in a two-sided test. By contrast, E2 (.49) and E3 (.66) fall inside.

Table 5. Robustness of the three-day treated minus control difference (percentage points)

Market (baseline)	model	-4.68	0.036	+1.63	0.424	+0.84	0.470
Two-factor: and gold	market	-5.32	0.030	+1.75	0.375	+0.76	0.516
Estimation window t- 120 to t-21	t-	-5.32	0.019	+1.79	0.368	+0.37	0.757
Estimation window t- 200 to t-31	t-	-4.89	0.032	+1.90	0.350	+0.81	0.508
Excluding Finance	Muthoot	-3.07	0.009	+1.05	0.635	+0.95	0.461
Randomisation empirical p	test,	-4.68	0.025	+1.63	0.488	+0.84	0.664

The window is (-1,+1) throughout. p-values from Welch's t-test except the final row, which reports the two-sided empirical p-value against 2,000 pseudo-event dates.

5.5 Pre-trends and anticipation

An assumption behind a DID estimator is parallel paths, and Figure 1 calls the assumption into question. The formal tests on the assumption confirm it for the draft directions, deny it for the final directions. For the draft policy direction, before 9 Apr gold lenders had outrun the control group from day -10 through day -2 (2.11 points; $p=0.373$); and then they ran along it (0.01 points) at least on day -20 through day -2. The outcome of the control group's run versus the treatment groups' in both draft directions was statistically inseparable, as seen in the two tests of 29 September pre-trends too, against all pre-windows included in tests. However, against the all-inclusive-window treatment-day set, including before the announcement, the direction of final policy reveals otherwise. Gold lenders had led the control group over days -10 through -2 (5.34 points; $p=0.011$; Mann-Whitney $p=0.009$), on the whole, and days -5 through -2 (4.55 points; $p=0.019$).

The June divergence has implications for two hypotheses. Firstly, in contrast to announcement effects as "surprise," the June divergence offers direct evidence of anticipations. This conclusion lines up with prior literature like Mokas & Giuliadori(2023) finding that announcements of LTV modifications affect prices prior to effective implementation, and it also aligns with a consultation process during which the regulator's policy path could be identified by its design choices. In the second way, the significant deviation before the announcement disqualifies one of my estimates of the long time window treatment E2 shown in Table 3. The 9.59-point difference over the time span of -5 days and +5 days mainly measures already existing deviation rather than announcement effect and should not be interpreted as such. Thus, the narrower time window E1, not showing any meaningful trend of policy direction, should be compared rather with the narrowed window E2, finding no policy component in 6th June itself.

Figure 1. Cumulative average abnormal returns around each announcement, treated and control groups

Cumulated from day -5. The vertical line marks the announcement date. Panel A shows the two groups separating after the draft directions. Panel B shows both groups rising on the monetary announcement, with the treated group continuing to rise on the following session. Panel C shows no separation.

Figure 2. Randomisation distribution of the treated minus control difference

Each draw recomputes the three-day treated minus control difference in cumulative average abnormal returns on a pseudo-event date sampled at random from the estimation period, with the three real event windows excluded. The observed E1 difference lies in the lower tail, with a two-sided empirical p-value of 0.025.

6. Discussion

The sequence of events tells something of how a regulation signal is priced and how long it lasts. The draft was received as a restriction against the highly specialised gold lending model and cut back that segment's value by some 4.7 percentage points compared to similar lenders in 3 days. The final directions, which opened the LTV up and moved the date out to April 2026, were perceived as welcome, though it was less of a welcome than it seemed, and most of it flowed to the specialist NBFCs rather than the more broadly diversified NBFCs with gold book. Over the whole April-June cycle, the treatment group came out 3.05 percentage points behind the control group, a gap which is insignificant at the rank test ($p = 0.094$), but which is of the right sign and magnitude to align with the inequality that Richter et al. (2019) find among real variables. It is never possible to take back the entire harm of having issued a restrictive rule by withdrawing it later.

There is a useful, although perhaps unsurprising, lesson in communication here for other central banks. It is efficient for the RBI to bundle monetary policy with regulatory measures, but it is inconvenient and confusing for anyone trying to isolate the market's reaction to any of these announcements. The June episode is a good illustration: same day price changes exaggerated the regulation impact by some three and a half fold, and only once the next day and a control group were considered could regulatory effects be isolated from monetary policy ones. Any regulator wishing to assess market reactions to its own proposed measures has a vested interest in not embedding it within rate calls. Failing this, any assessment requires the use of a control group.

The pattern fits best within a public choice theory of regulation where the regulator responds to feedback from the regulated industry, a perspective which is in keeping with a dialectical interpretation in the tradition of Kane (1977) better than a straightforward one. Firstly, a restrictive proposal is launched, and then, after seeing value erode, industry lobbies for modifications, leading to a softer, although targeted to the firms for which it matters most, outcome for which these firms recover more than proportionately, a pattern that can be found in. The data here do not, of course, indicate which effect-lobbying or RBI re-assessment-had more of an impact.

For practitioners, the key takeaway is narrower. Any exposure to the regulation will be business model based and not related to price correlations between business owners and collateral. Based solely on gold betas, lenders like those in the control group would not have made it into screens.

7. Conclusion, limitations and future research

The equity market reflected India's gold loan directions but not in ways standard announcement studies suggest. India's April 2025 draft regulation knocked nearly 5% off gold-backed lenders' valuations versus peers, an effect that held across various specifications, windows, samples, and random assignment. The June 2025 revisions, which were more favourable in a sense, mainly boosted their price in coincidence with monetary loosening. The regulatory component remains close to zero significance within windows, partly anticipated before, mainly benefiting specialised NBFCs with significant gold exposure, rather than all gold lenders broadly defined. The September 2025 announcement proved to be just what it was declared: a clarification of what was stated before.

However, four limits restricted conclusions: Only 7 NBFCs in the treatment group implies statistical inference problems. The focus on listed NBFCs means that unlisted institutions and more informal lenders that account for a substantial share of Indian gold loans are not included. The treatment selection classification is based on subjective judgement of NBFC business models, and while one firm was removed on a clear business-line ground, being a two-wheeler and used-vehicle financier despite its brand name, other researchers could classify different NBFCs based on their criteria. Finally, the study evaluates expectations, not outcomes; the accuracy of lenders' reactions to the initial draft proposal has yet to be fully substantiated after implementation has taken place.

The question of whether lenders' expectations are a good approximation of their actions can be assessed in April 2026, when the deadline to comply with the new regulations will come into force. In the study such data might show if lenders bunch at the new 85 per cent LTV ceiling and also whether larger ticket size loan tightness leads borrowers to move towards unregulated lenders as per the results of. The research question should now shift focus to consumer welfare, which is more important now with gold lending as it touches households at the bottom of the wealth distribution ladder.

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