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Locker Regulation and Fee-Income Behaviour in Indian Banking: Evidence from the 2021 Safe Deposit Locker Reform

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Abstract:

Safe deposit lockers in India are rationed through non-price mechanisms, with reported demand persistently exceeding capacity. In August 2021, the Reserve Bank of India (RBI) issued revised instructions, in force from January 2022, that limited the term deposit banks may require at allotment to three years' rent plus breaking-open charges and required banks to maintain branch-wise lists of vacant lockers and waitlists. Using an unbalanced panel of 112 scheduled commercial banks (after merger consolidation; FY2005–FY2025) from the RBI's Database on Indian Economy, we estimate a two-way fixed-effects difference-in-differences model, with standard errors clustered by bank, of how fee-based ("other") income changed for public sector banks relative to other banks after the reform. Because locker income is not separately disclosed, total other income serves as a proxy. Contrary to expectations, public sector banks' fee income grew significantly faster than other banks' after the reform (β = 0.344, clustered SE = 0.123, p = 0.005). Event-study coefficients for FY2018 and FY2019 are statistically indistinguishable from the FY2020 base year, and the result is robust to an alternative policy cutoff and to restricting the sample to public and private sector banks, but loses significance under an asset-normalized dependent variable. A placebo test, however, indicates a pre-existing divergence, so the evidence is consistent with, but does not conclusively isolate, a reform-specific effect. We discuss implications for RBI's data disclosure and for monitoring how locker access is rationed.

Keywords: bank fee income; non-price rationing; locker regulation; difference-in-differences; Indian banking; safe deposit lockers

1. Introduction

Safe deposit lockers in India remain a persistently rationed retail banking service. Press and industry reports routinely describe multi-year waiting lists for locker access at urban bank branches, with one major private bank publicly estimating existing capacity of roughly 6 million lockers against a projected demand exceeding 60 million by 2030 (Axis Bank, 2026). This claim originates from bank marketing communications rather than from an independently verified regulatory or academic source. This absence of independently verified data is itself informative: despite widespread public awareness of locker scarcity, the phenomenon has, to our knowledge, attracted no dedicated quantitative academic study in India.

This scarcity is not merely a matter of physical capacity. Locker rental is a regulated financial service: the Reserve Bank of India (RBI) has long regulated the terms on which banks offer lockers **(Reserve Bank of India, 2021, which supersedes instructions dating from 1984)** . In August 2021, RBI issued revised instructions, in force from 1 January 2022, tightening this framework by limiting the term deposit banks may take at allotment to three years' rent plus breaking-open charges, requiring branch-wise lists of vacant lockers and waitlists, and fixing banks' liability for certain losses at 100 times annual rent (Reserve Bank of India, 2021). This reform is a discrete, dated regulatory change affecting a specific ancillary banking service, which offers a quasi-experimental setting.

We exploit this policy change to ask a narrower, tractable version of the broader locker-scarcity question: how did banks' fee-based ("other") income change after the 2021 reform, and did the change differ between public sector (PSU) banks and other banks? This question is motivated by the well-established literature on non-price rationing when prices do not clear the market (Barzel, 1974; Cheung, 1974), which predicts that when the price mechanism does not allocate a scarce good, suppliers adjust along other margins (service quality, discretionary fees, or rationing intensity) toward a new equilibrium. Because locker scarcity itself is not observable in public

data, we study the margin that is observable: fee-based income. PSU banks, which plausibly operate larger and older branch and locker networks than other banks in India, might respond differently to a tightening of locker-specific regulation than banks with smaller, newer networks.

Ex ante, the direction of the effect is ambiguous. The reform's deposit-cap and disclosure provisions could constrain public sector banks' fee income more than other banks' if their legacy networks relied more on discretionary allocation; alternatively, their broader retail footprint could let them capture locker-driven relationships or offset lost locker revenue through other fee channels. We initially expected the former, but treat the net effect as an empirical question.

Locker-specific revenue is not separately disclosed in RBI's published bank-wise statistical tables; it is embedded in the broader "other income" category, most plausibly within its commission or miscellaneous components (Section 3.2). We therefore treat "other income" as a proxy for the aggregate ancillary-fee channel, of which locker revenue is one undisaggregated component, and we acknowledge this limitation throughout.

Using a panel of 112 scheduled commercial banks (after merger consolidation) over FY2005–FY2025, constructed from RBI's published statistical tables, we estimate a two-way fixed-effects difference-in-differences model. We find that public sector banks' fee income grew significantly faster than other banks' following the 2021 reform, contrary to our initial expectation. An event-study specification yields coefficients for FY2018 and FY2019 close to zero, consistent with parallel trends, although the coefficients rise from FY2015 onward. The result is robust to an alternative policy cutoff, to restricting the sample to public and private sector banks, to additional controls, and to excluding merger-affected entities, but it loses statistical significance under an asset-normalized dependent variable. A placebo test at a counterfactual date, however, also yields a significant coefficient, so we interpret the evidence as consistent with, but not conclusive of, a reform-specific effect (Sections 5.3 and 6.2).

This paper contributes to three literatures. First, it provides empirical evidence relevant to the theory of non-price rationing under price ceilings (Barzel, 1974; Cheung, 1974; Naor, 1969), in a financial-services setting distinct from the goods-market and queuing applications in which that theory is typically examined. Second, it contributes to the literature on non-interest income in emerging-market banking by using a specific, dated regulatory change as a source of identifying variation in fee income, a design that, to our knowledge, is rarely used in this literature. Third, it contributes to the sparse literature on bank locker services, which, to our knowledge, comprises regulatory text and industry commentary but no quantitative analysis; this paper applies panel-econometric methods to this setting.

The remainder of the paper is organized as follows. Section 2 reviews the literature. Section 3 describes the data and empirical strategy. Section 4 presents the baseline results and robustness checks. Section 5 reports the event-study evidence on parallel trends and additional robustness checks, including a placebo test. Section 6 discusses interpretation, limitations and policy implications. Section 7 concludes.

2. Literature Review

2.1 Non-Price Rationing Under Price Ceilings

The theoretical starting point for this paper is the classical economics of rationing under binding price controls. **Barzel (1974) and Cheung (1974) s**howed that when a binding price ceiling prevents a market from clearing through price, adjustment shifts to non-price margins, such as waiting time **(Barzel, 1974)** and quality, side payments or discriminatory allocation **(Cheung, 1974). Naor (1969)** analysed the welfare implications of queue joining and the role of tolls in regulating queue length; the framework has since been applied to settings such as healthcare waitlists and public housing allocation .

Bank lockers in India appear consistent with this framework in one respect: demand exceeds supply and access is rationed. The 2021 instructions do not set locker rents, but industry commentary reports persistent excess demand and multi-year waitlists for lockers **(Finshots, 2026; Business Standard, 2021),** so access appears to be rationed through non-price devices such as waitlists and the bundling of lockers with term deposits. The 2021 instructions

limited the term deposit banks may require at allotment, treated larger deposits as a restrictive practice, and required banks to maintain branch-wise lists of vacant lockers and waitlists, limiting banks' scope to ration lockers by bundling them with other products, a practice reported in the financial press **(Whalesbook, 2026).**

2.2 Non-Interest Income in Banking

A large body of banking literature examines the determinants of non-interest income and its implications for bank performance and stability, reflecting banks' shift toward diversified fee revenue. Studies of Indian banks have examined the composition and determinants of fee-based income **(Uppal, 2010; Subrahmanyam, 2009; Thakur and Arora, 2023)**, generally finding that non-interest income has grown as a share of total income and that its determinants differ systematically by bank ownership type and size. This literature typically treats non-interest income as an outcome shaped by bank-level strategic choices (branch network, product mix, digital adoption) with less attention to regulatory changes affecting a single ancillary product.

This paper's contribution to this literature is methodological: instead of correlational cross-sectional or panel evidence, we use a dated regulatory change, the 2021 locker reform, as a source of identifying variation. This design allows a more credible, though not definitive, estimate of how a specific regulatory constraint on one ancillary product affects the broader fee-income category in which that product's revenue is embedded, at the cost of specificity: locker rental income is not separately disclosed (Section 3.2), so the locker-specific channel cannot be isolated.

2.3 Household Demand for Physical Asset Storage

A separate but related literature examines household demand for gold and other physical-asset storage in India, motivated by gold's cultural and financial significance as a savings vehicle **(Dasgupta, 2022; World Gold Council, n.d.).** This literature is relevant to the demand side of the locker market, since industry commentary suggests bank lockers are used largely to store gold jewellery and other valuables **Amutha (2022) after Dasgupta (2022)** . While this paper's empirical design does not directly estimate locker demand, this literature suggests why locker demand may be large and provides context for interpreting the demand pressures underlying the regulatory environment we study.

2.4 Bank Lockers as a Research Object

To our knowledge, no peer-reviewed study has quantitatively examined Indian bank lockers as an economic phenomenon. The available material comprises regulatory text (RBI circulars and directions), press and industry commentary on locker shortages and RBI's responses, and a bank-sourced demand projection that we could not independently verify (Axis Bank, 2026). This paper partly addresses this gap by applying panel-econometric methods to a regulatory change affecting a service that has received little quantitative attention.

3. Data and Methodology

3.1 Data Sources

We construct a bank-year panel from three tables from the Reserve Bank of India's Database on Indian Economy (DBIE), Statistical Tables Relating to Banks in India: (i) Earnings and Expenses of Scheduled Commercial Banks, providing bank-wise income statement items; (ii) T(ii) Liabilities and Assets of Scheduled Commercial Banks, providing bank-wise balance sheet items (published in two formats, to FY2019 and from FY2020, which we reconcile by matching line items by name) ; and (iii) Select Ratios of Scheduled Commercial Banks, providing pre-computed performance ratios including Return on Assets, Return on Equity, and the ratio of non-interest income to total assets. All three tables are published annually on an as-on-31-March basis and cover scheduled commercial banks in five groups—public sector, private sector, foreign, small finance, and payments banks.

The raw data span FY2005–FY2025 (21 fiscal years ending 31 March) and contain 139 distinct bank names. Several banks merged or changed names during this period, most notably the 2017 merger of State Bank of India's associate banks into SBI, the 2019 merger of Vijaya Bank and Dena Bank into Bank of Baroda, and the 2020 consolidation of ten public sector banks into four, for example Oriental Bank of Commerce and United Bank of

India into Punjab National Bank. To keep a consistent panel entity, we map every historical bank name to its eventual surviving entity and consolidate all years under that entity, so that earlier years are pro forma combinations of the constituent banks. Levels (INR crores) are summed across constituents within each year; ratios, which cannot be summed, are asset-weighted averages in years with more than one constituent. This reduces the 139 names to 112 bank entities.

Table 1 reports the sample composition in FY2025, the final year; the full panel contains 112 bank entities, as several banks present in earlier years exited or were absorbed.

Table 1: Sample Composition by Ownership Type (FY2025)

Ownership Type	Banks	% of Total
Foreign	41	44.6%
Private Sector	22	23.9%
Public Sector	14	15.2%
Small Finance Bank	11	12.0%
Payments Bank	4	4.3%
Total	92	100%

Note: This table reflects banks active as of FY2025. The full unbalanced panel (FY2005–FY2025) includes 112 distinct bank entities in total, as several banks present in earlier years exited or were absorbed via merger prior to 2025 (see merger consolidation methodology above).

Source: Reserve Bank of India, Database on Indian Economy (DBIE); ownership classification by the authors.

3.2 Variable Construction

Dependent variable.

Our dependent variable is Total Other Income, RBI's aggregate of non-interest income, which comprises commission, exchange, and brokerage income; net profit or loss on the sale and revaluation of investments, on the sale of land and other assets, and on foreign exchange transactions; and miscellaneous income. RBI does not report locker rental income separately; it is embedded in this aggregate. Total Other Income is therefore a proxy for the ancillary-fee channel affected by the 2021 reform, not a direct measure of locker revenue, and the results should be read with this limitation in mind. Our primary dependent variable is ln(Total Other Income + 1), with income in INR crores; the 24 bank-years with income of −1 crore or lower (all foreign banks), for which the log is undefined, drop out of this specification. We also estimate RBI's published ratio of non-interest income to total assets as an alternative, asset-normalized dependent variable (Section 4.2).

Treatment variables.

PSU_Dummy equals one for banks classified as Public Sector and zero otherwise. Post2021 equals one for fiscal years 2022 onward (i.e., the fiscal year ending 31 March 2022, in which RBI issued its August 2021 circular and the instructions came into force on 1 January 2022, and all later years) and zero for earlier years. Our key regressor of interest, PSU × Post2021, is the product of these two indicators, standard in a two-way fixed-effects difference-in-differences design. Because the instructions came into force on 1 January 2022, and banks had to renew agreements with existing locker customers by 1 January 2023 (Reserve Bank of India, 2021), only part of FY2022 was exposed to the reform; we therefore separately construct PSU × Post2023 (using a FY2023 cutoff, the first full fiscal year in which the instructions applied) as a robustness check.

Controls.

We control for bank size with the natural logarithm of total assets (balance-sheet table) and for profitability with return on assets (ROA, Select ratios table), since larger and more profitable banks tend to earn more fee income regardless of any locker-specific channel. Additional controls used in robustness checks (Capital Adequacy Ratio and Net NPA Ratio, both from the Select Ratios table) are reported in Section 5.3.

Table 2: Descriptive statistics for the core variables.

Variable	N	Mean	SD	Min	Max
Total Other Income (INR crores)	1,624	1,990.14	5,237.66	−330.99	61,683.06
Total Assets (INR crores)	1,624	168,931.90	486,309.56	30.08	6,676,053.27
ROA (%)	1,624	0.928	2.319	−26.22	17.86
ln(Other Income)	1,600	5.273	2.585	−2.356	11.03
ln(Total Assets)	1,624	9.612	2.592	3.436	15.714

Note: ln(Other Income) has 1,600 observations rather than 1,624 because 24 bank-year observations — exclusively foreign banks with negative Total Other Income in a given year — have negative Total Other Income and are consequently undefined under the log transformation; these are naturally excluded from the log-level specification but retained in the ROA and asset-based descriptive statistics above, and in the asset-normalized robustness specification (Section 4.2), which does not require a log transformation.

Source: Reserve Bank of India, Database on Indian Economy (DBIE); ownership classification by the authors.

3.3 Empirical Strategy

We estimate the following two-way fixed-effects difference-in-differences model by ordinary least squares with bank and year dummies:

$$\ln(\text{Total Other Income}_{it} + 1) = \beta_1 (\text{PSU}_i \times \text{Post2021}_t) + \beta_2 \ln(\text{Total Assets}_{it}) + \beta_3 \text{ROA}_{it} + \alpha_i + \lambda_t + \varepsilon_{it},$$

where i indexes banks and t indexes fiscal years ending 31 March, and α_i and λ_t are bank and year fixed effects. Because PSU_i is time-invariant and Post2021_t is common to all banks, their main effects are absorbed by α_i and λ_t and omitted. The reform applied to all banks, so the year effects absorb its common effect; β_1 measures the differential change in log other income of public sector banks relative to all other banks after the reform, with a proportional interpretation $\exp(\beta_1) - 1$. The panel is unbalanced, and observations are unweighted. In the baseline specification, PSU_Dummy equals one for public sector banks and zero for all other banks in the sample (private sector, foreign, small finance, and payments banks), so β_1 compares public sector banks with all other banks; Section 4.2 reports the comparison restricted to public and private sector banks only. Standard errors are clustered by bank, allowing for arbitrary serial correlation and heteroskedasticity within banks; p-values are based on the normal approximation, so given the modest number of clusters in some specifications, they should be read with some caution.

3.4 Identification Assumptions

Identification rests on four assumptions. (i) Parallel trends: absent the reform, the gap in log other income between public sector banks and the comparison group would have evolved in parallel, conditional on controls and fixed

effects. This cannot be tested for the post-reform period; we probe it with an event study (Sections 5.1–5.2) and a placebo test (Section 5.3). (ii) No anticipation: banks did not change fee-income behavior before RBI's August 2021 directions. (iii) No concurrent differential shocks: no other development changed public sector banks' fee income relative to other banks over the period, for example, differential effects of COVID-19, public sector bank mergers (partly addressed in Section 5.3), recapitalization, or changes in treasury income. (iv) Stable group definitions and no spillovers between groups. Violations of (iii) are the main threat to identification; Sections 5.3 and 6.2 discuss the evidence on this threat and its limits.

4. Results

4.1 Baseline Estimates

Table 3 reports estimates from the baseline two-way fixed-effects specification:

$$\ln(\text{Total Other Income}_{it} + 1) = \beta_1 (\text{PSU}_i \times \text{Post2021}_t) + \beta_2 \ln(\text{Total Assets}_{it}) + \beta_3 \text{ROA}_{it} + \alpha_i + \lambda_t + \varepsilon_{it}$$

where α_i denotes bank fixed effects and λ_t denotes year fixed effects, with standard errors clustered at the bank level (1,600 bank-year observations; 112 banks).

The coefficient on the interaction term PSU × Post2021 is positive and statistically significant ($\beta = 0.344$, clustered SE = 0.123, $p = 0.005$), indicating that public sector banks experienced a proportionally larger increase of about 41 percent in fee-based (other) income relative to all other banks following the August 2021 locker reform, controlling for bank size and profitability. This result runs counter to the study's initial expectation, which anticipated … more severely than other banks', given the former's plausibly larger retail branch and locker networks. Section 6.1 discusses possible explanations for this sign.

The model explains a substantial share of variation in fee income ($R^2 = 0.950$), with bank size (ln Total Assets, $\beta = 0.983$, $p < 0.001$) and profitability (ROA, $\beta = 0.096$, $p < 0.001$) both strongly and positively associated with fee income, as expected.

This baseline coefficient was independently cross-validated using two separate statistical computing environments—a hand-coded fixed-effects estimator with analytically derived cluster-robust standard errors and jamovi's Linear Regression module using an equivalent least-squares-dummy-variable specification—which returned an identical point estimate ($\beta = 0.344$) in both cases, differing only in standard error due to jamovi's implementation not clustering by bank. This replication checks the estimation itself and is distinct from the robustness checks reported in Sections 4.2 and 5.3.

Table 3: Baseline Regression Results

Variable	Coefficient	Clustered SE	t	p
PSU × Post2021	0.344	0.123	2.787	0.005
ln(Total Assets)	0.983	0.139	7.08	<0.001
ROA	0.096	0.018	5.23	<0.001
Bank FE	Yes			
Year FE	Yes			
N	1,600			
Banks (clusters)	112			
R^2	0.950			

Note: The dependent variable is ln(Total Other Income + 1). The model includes bank and year fixed effects, which absorb the main effects of PSU_Dummy and Post2021; the interaction PSU × Post2021 is the difference-in-differences estimate, and the comparison group is all banks other than public sector banks. Standard errors are clustered by bank (112 clusters); p-values use the normal approximation. N = 1,600 rather than 1,624 because 24 bank-years with an income of −1 crore or lower (all foreign banks) are undefined under the log. The point estimates were replicated in jamovi (linear regression with bank and year dummies) and are identical; jamovi's standard errors are not clustered and are therefore smaller

Source: Reserve Bank of India, Database on Indian Economy (DBIE); authors' calculations.

4.2 Robustness Checks

Table 4 summarizes three robustness checks, each also independently cross-validated in jamovi as described in Section 4.1.

Alternative dependent variable. Re-estimating the model using RBI's own published ratio of non-interest income to total assets (rather than the log level of other income) yields a positive coefficient (0.344 percentage points of total assets), but the estimate loses statistical significance (SE = 0.285, p = 0.227). The estimate is imprecise (t = 1.21), so the result does not hold once fee income is scaled by bank size; this suggests the baseline difference may partly reflect scale rather than fee intensity, a limitation we return to in Section 6.3.

Alternative policy cutoff. The revised instructions came into force on 1 January 2022, so FY2023 is the first full fiscal year in which they applied. Shifting the post-treatment indicator to begin in FY2023 rather than FY2022 yields a coefficient of similar sign and magnitude (β = 0.290, clustered SE = 0.116, p = 0.012), indicating the result is not an artifact of the specific cutoff year chosen.

Domestic banks only. Since locker provision is likely concentrated in domestic retail banking, we re-estimate the baseline model excluding foreign, small finance, and payments banks (N = 754, 43 banks). The interaction coefficient remains positive and significant (β = 0.274, clustered SE = 0.114, p = 0.016), indicating the result is not driven by foreign, small finance or payments banks. This is the comparison closest to the public-versus-private contrast that motivates the paper.

Across all four specifications, the PSU × Post2021 coefficient is positive, and it is statistically significant in three of the four; Section 5.3 reports further checks, including a placebo test that qualifies these results.

Table 4: Robustness Checks (PSU × Post coefficient)

Specification	Coefficient	Clustered SE	p	N	Banks
Baseline	0.344	0.123	0.005	1,600	112
Alt. DV (non-interest income/assets)	0.344	0.285	0.227	1,624	112
FY2023 cutoff	0.290	0.116	0.012	1,600	112
Domestic banks only	0.274	0.114	0.016	754	43

Note: All models include bank and year fixed effects, ln(Total Assets) and ROA as controls, and standard errors clustered by bank; p-values use the normal approximation. The dependent variable in the baseline, FY2023-cutoff, and domestic-only specifications is ln(Total Other Income + 1) (INR crores), so coefficients are in log points. The alternative-DV specification uses RBI's published ratio of non-interest income to total assets (in percent), so its coefficient is in percentage points of total assets and is not comparable in size with the others. "Domestic banks only" includes public sector and private sector banks and excludes foreign, small finance and payments banks. In the FY2023-cutoff row, the key regressor is PSU × Post2023.

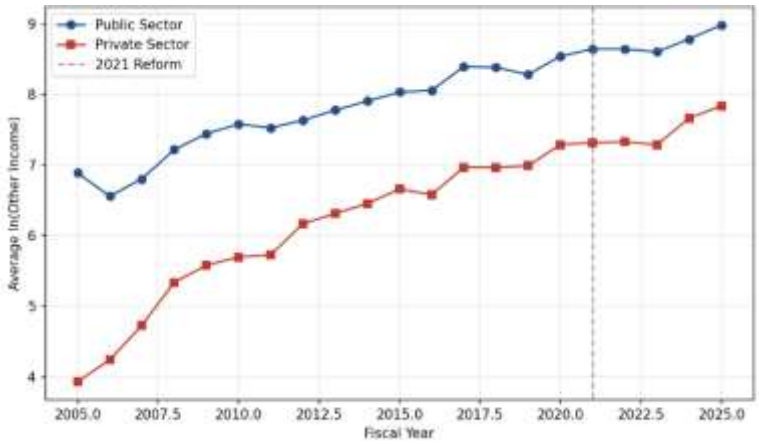
Source: Reserve Bank of India, Database on Indian Economy (DBIE); authors' calculations.

5. Event-Study Evidence on Parallel Trends

5.1 Motivation

The difference-in-differences design in Section 4 rests on the parallel-trends assumption: absent the 2021 reform, the fee income of public sector banks and of the comparison banks would have continued on similar relative trajectories, conditional on controls and fixed effects. Figure 1 (average log Other Income by year and ownership type) suggests that this assumption cannot be taken for granted in raw form: private banks' fee income appears to have been converging toward public sector banks' level throughout the 2005–2025 sample, with the gap between the two groups narrowing from roughly 3.0 log-points in 2005 to roughly 1.1–1.3 log-points by the 2020s. A pooled "before/after" comparison risks conflating this long-run convergence trend with any genuine effect of the 2021 reform.

Figure 1: Average ln (Other Income), PSU vs Private Banks



Note: Each line shows the simple (unweighted) average of ln(Total Other Income + 1) across banks in the group, by fiscal year ended 31 March. Domestic banks only (public sector and private sector); foreign, small finance, and payments banks are excluded. The dashed vertical line marks FY2021, the last fiscal year before RBI's revised locker directions (August 2021), which fall in FY2022, the first year of the post-reform period. Raw group averages are shown without controls or fixed effects. The panel is unbalanced: the public sector group contains 14 banks in every year, while the private sector group contains between 20 and 27 banks (27 in FY2005, falling to 20 by FY2012 as older private banks exited or were absorbed, and 22 from FY2016 onward as new entrants appear). Part of the apparent narrowing of the gap may therefore reflect changes in group composition rather than within-bank growth; the regression estimates in Section 4 control for this through bank fixed effects. PSU = public sector (government-owned) banks; ln = natural logarithm; Other Income is in INR crores (1 crore = 10 million).

Source: Reserve Bank of India, Database on Indian Economy (DBIE), Statistical Tables Relating to Banks in India; authors' calculations.

To address this, we re-estimate the model on public and private sector banks only (754 bank-years; 43 banks), replacing the pooled Post2021 indicator with year-specific PSU interaction terms, using FY2020 (the fiscal year ending 31 March 2020, two years before the reform year FY2022) as the omitted reference category:

$$\ln(\text{Total Other Income}_{it} + 1) = \Sigma_\tau \gamma_\tau (\text{PSU}_i \times 1[\text{Year}_t = \tau]) + \beta 2 \ln(\text{Total Assets}_{it}) + \beta 3 \text{ ROA}_{it} + \alpha_i + \lambda_t + \varepsilon_{it}$$

Each $\gamma\tau$ coefficient captures the gap in log other income between public and private sector banks in year τ relative to FY2020, controlling for bank and year fixed effects. If the pre-reform coefficients ($\tau \leq 2021$, since FY2021 ends before the reform) are flat and statistically indistinguishable from zero in the years closest to treatment, this is consistent with the parallel-trends assumption. Earlier divergence would still be a concern, because bank and year fixed effects do not absorb group-specific trends; Section 5.3 returns to this

5.2 Results

Table 5 and Figure 2 report the event-study coefficients for public sector and private sector banks. Coefficients for the earlier pre-treatment years (2006–2015) are negative and, for 2011–2015, statistically significant at the 5 percent level, indicating that, conditional on controls and fixed effects, the public–private gap was smaller than in FY2020 and has since widened. This is the opposite of the narrowing in the raw averages in Figure 1; the difference may reflect the controls, bank fixed effects and changes in group composition. The coefficients for FY2018 and FY2019 are close to zero and statistically indistinguishable from the FY2020 baseline (2018: $\gamma = -0.029$, SE = 0.107, p = 0.789; 2019: $\gamma = -0.021$, SE = 0.108, p = 0.850), indicating that the gap was close to its FY2020 level in those years. This is consistent with the parallel-trends assumption, although the coefficients rise steadily from FY2015, and the coefficient for FY2021, the last pre-reform year, is positive ($\gamma = 0.094$, SE = 0.105, p = 0.369), so the assumption cannot be taken as established.

Coefficients for the reform year and after (FY2022–FY2025; FY2022 is only partly exposed to the reform) are all positive, consistent with the pooled baseline result, but none is individually statistically significant (all $p > 0.15$), with the largest point estimate in 2023 ($\gamma = 0.168$, p = 0.160). Statistical significance in the pooled baseline specification (Table 3) is therefore achieved only by combining variation across the full post-reform period. This pattern is consistent with a gradual rather than abrupt divergence, but because the coefficients were already rising before the reform, it cannot be attributed to the reform alone; Sections 5.3 and 6.2 return to this. We report this pattern transparently: the estimates are positive in every post-reform year but individually imprecise.

Table 5: Event-Study Coefficients (PSU × Year, relative to FY2020)

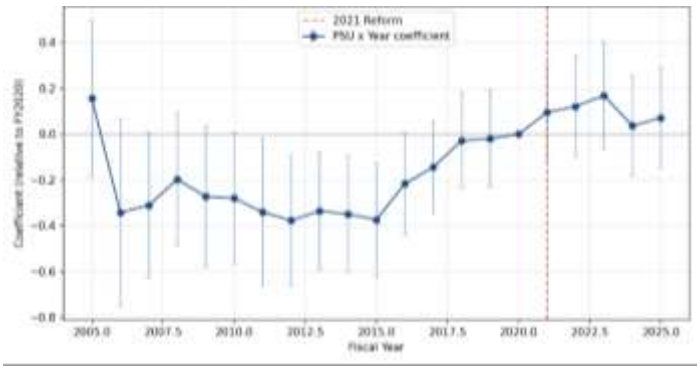
Year	Coefficient	SE	p
2005	0.155	0.174	0.372
2006	−0.342	0.208	0.100
2007	−0.311	0.162	0.055
2008	−0.198	0.148	0.179
2009	−0.273	0.156	0.081
2010	−0.281	0.146	0.054
2011	−0.340	0.164	0.038
2012	−0.378	0.145	0.009
2013	−0.336	0.131	0.010
2014	−0.349	0.130	0.007
2015	−0.374	0.127	0.003
2016	−0.216	0.114	0.057
2017	−0.145	0.101	0.151
2018	**−0.029**	**0.107**	**0.789**
2019	**−0.021**	**0.108**	**0.850**
2020 (base)	0	—	—
2021	0.094	0.105	0.369
2022	0.121	0.113	0.282
2023	0.168	0.119	0.160

2024	0.036	0.112	0.750
2025	0.071	0.111	0.523

Note: Sample restricted to domestic (public sector and private sector) banks, N = 754, 43 banks. Standard errors clustered by bank. Bold rows (FY2018 and FY2019) are statistically indistinguishable from the FY2020 baseline. FY2021 ends before the reform, which falls in FY2022. Controls: ln(Total Assets) and ROA; bank and year fixed effects. p-values use the normal approximation.

Source: Reserve Bank of India, Database on Indian Economy (DBIE); authors' calculations.

Figure 2: Event-Study Coefficients with 95% CI



Note: Each point is the estimated coefficient on PSU × (year indicator) from a two-way fixed-effects regression of ln(Total Other Income + 1) on year-specific PSU interactions, ln(Total Assets), ROA, bank fixed effects, and year fixed effects. Coefficients are relative to FY2020 (the omitted base year, shown at zero). Vertical bars are 95% confidence intervals (±1.96 standard errors, clustered by bank). Domestic banks only (N = 754; 43 banks). The dashed vertical line marks FY2021, the last fiscal year before RBI's revised directions (August 2021), which fall in FY2022. The confidence intervals are pointwise (not joint bands), and the number of clusters is modest (43), so intervals should be read as approximate. Pre-reform coefficients close to zero, with intervals that include zero, are consistent with the parallel-trends assumption; this holds for FY2018 and FY2019, while the FY2021 coefficient is positive but imprecisely estimated. PSU = public sector (government-owned) banks; ROA = return on assets; ln = natural logarithm.

Source: Reserve Bank of India, Database on Indian Economy (DBIE), Statistical Tables Relating to Banks in India; authors' calculations.

5.3 Additional Robustness Checks

We conduct three further checks to probe the baseline result's sensitivity to specific design choices and, most importantly, to probe whether the effect could plausibly arise from causes unrelated to the 2021 reform. Table 6 summarizes the three checks.

Merger-consolidation robustness. Because our panel consolidates historically merged banks into their surviving entity (Section 3.1), it is possible the baseline result is sensitive to this consolidation choice. Re-estimating the model excluding the 17 bank entities whose history involves a merger or name change (334 bank-year observations removed; 1,266 observations, 95 banks remain) yields a coefficient of similar or slightly larger magnitude ($\beta = 0.382$, clustered SE = 0.105, $p < 0.001$; $\beta = 0.368$, clustered SE = 0.111, $p < 0.001$ when the additional controls below are also included), indicating the result does not depend on the merger-consolidation methodology.

Additional controls. Adding Capital Adequacy Ratio and Net NPA Ratio — both significant controls with negative coefficients (a higher NPA ratio is associated with lower fee income; we do not interpret the negative coefficient on capital adequacy) — leaves the PSU × Post2021 coefficient positive and significant, though

somewhat smaller (β = 0.279, clustered SE = 0.105, p = 0.008), indicating the baseline result is not driven by differences in capital strength or asset quality captured by these ratios between public sector banks and other banks.

Placebo test. The most important of these three checks directly probes whether the observed divergence of public sector banks from other banks is specific to the 2021 reform or reflects a broader pre-existing trend. Restricting the sample to genuinely pre-reform years only (FY2005–FY2021) and constructing a placebo treatment indicator "using a counterfactual "reform" date of FY2018 (a year in which we are not aware of any locker-related regulatory change) yields a coefficient of β = 0.612 (clustered SE = 0.163, p < 0.001; 1,238 observations, 106 banks): larger in magnitude and more statistically significant than the baseline result itself.

This is a genuinely important caveat that qualifies our central finding. A well-behaved placebo test should find no effect; finding a substantial "effect" at a counterfactual cutoff date indicates that at least part of the observed divergence of public sector banks from other banks reflects an ongoing structural trend in relative fee-income growth that predates, and is not specific to, the 2021 reform. Section 6.2 discusses the implications of this finding for how our central result should be interpreted.

Table 6: Additional Robustness Checks (PSU × Post coefficient)

Specification	Coefficient	Clustered SE	p	N	Banks
Merger-robustness (non-merged banks only)	0.382	0.105	<0.001	1,266	95
Merger-robustness + additional controls	0.368	0.111	<0.001	1,266	95
Additional controls (+CAR, +NPA)	0.279	0.105	0.008	1,600	112
Placebo test (fake FY2018 cutoff, pre-2021 data only)	0.612	0.163	<0.001	1,238	106

Note: All models include bank and year fixed effects, ln(Total Assets) and ROA, with standard errors clustered by bank; p-values use the normal approximation. The key regressor is PSU × Post2021 in all rows except the placebo, where it is PSU × 1[Year ≥ 2018]. The placebo specification uses only FY2005–FY2021 data and a counterfactual treatment date; a significant coefficient indicates a pre-existing trend not specific to the 2021 reform. "Non-merged banks only" excludes the 17 bank entities whose history involves a merger or name change (334 bank-years). "+CAR, +NPA" adds Capital Adequacy Ratio and Net NPA Ratio.

Source: Reserve Bank of India, Database on Indian Economy (DBIE); authors' calculations.

6. Discussion and Limitations

6.1 Interpreting the Main Finding

Our central result, that public sector banks' fee income grew significantly faster than other banks' following the 2021 locker reform, runs counter to our initial expectation. We had expected the reform's deposit-cap and disclosure provisions to constrain PSU banks' fee income more than other banks', given PSU banks' plausibly larger and older locker networks and reported practices of linking locker access to other product sales (Whalesbook, 2026). Several non-exclusive explanations may account for the observed sign.

First, PSU banks' larger retail branch footprint may have allowed them to capture a disproportionate share of locker-seeking customers once the reform curtailed discretionary bundling practices, redirecting rationed demand toward the larger incumbent network. Second, PSU banks may have offset locker-specific revenue constraints by expanding other fee-based products more aggressively than other banks, a substitution effect the aggregate "Other

Income" measure would capture without being able to isolate. Third, the requirement to give applicants a waitlist number may have made queuing more predictable and so encouraged applications at banks with the largest existing (if constrained) locker infrastructure, which are plausibly PSU banks. These explanations are conjectures that our data cannot test, and they presume that the divergence is related to the reform; Section 6.2 discusses why that cannot be assumed. We flag this as a priority for future research using more granular, locker-specific data (Section 6.4).

6.2 The Placebo Test and the Limits of Causal Interpretation

The placebo test reported in Section 5.3 is the most important limitation of this paper's causal claims. Finding a substantial, statistically significant "effect" at a counterfactual treatment date, with no locker-related policy change that we are aware of, indicates that at least part of the divergence of PSU banks' fee income from that of other banks reflects a longer-running trend rather than a response specific to the 2021 reform. The event-study coefficients in Table 5 point the same way: conditional on controls and fixed effects, PSU banks' fee income was already rising relative to private banks' from FY2015, well before the reform. This differs in sign from the unconditional averages in Figure 1, where the gap between the two groups narrows; the difference may reflect the controls, bank fixed effects and changes in group composition, and we have not reconciled it.

The event-study evidence in Section 5.2, in which the FY2018 and FY2019 coefficients are close to zero and the coefficients from FY2022 onward are positive but individually insignificant, is compatible with a reform-linked effect superimposed on a pre-existing trend. However, the placebo test means we cannot rule out that the entire post-2021 divergence, not merely a portion of it, reflects a continuation of that pre-existing trend. Our preferred interpretation is that the evidence is consistent with, but does not conclusively isolate, a reform-specific effect on public sector bank fee income. We present this as a limitation: even the more conservative reading, that the fee-income trajectories of PSU banks and other banks diverged further during the post-reform period for reasons that may include, but are not limited to, the reform itself, is an empirical pattern worth reporting. A related threat is that other developments affecting PSU banks differently from other banks over the period, for example differential effects of COVID-19, public sector bank mergers, recapitalization, or changes in treasury income (Section 3.4), could also explain the divergence; our data cannot rule these out.

6.3 Data Limitations

Four further limitations bear on how the results should be interpreted. First, and most fundamentally, locker rental income is not separately disclosed in RBI's published bank-wise statistical tables; our dependent variable is a proxy, aggregate Other Income, within which locker revenue is embedded alongside commission, exchange and brokerage income, gains and losses on investments, assets and foreign exchange, and miscellaneous income. We cannot rule out that the observed effect is driven substantially by non-locker components of this aggregate, including investment and foreign-exchange gains that move with market conditions. Second, the coefficient is no longer statistically significant when the dependent variable is the ratio of non-interest income to total assets (Section 4.2), indicating sensitivity to how the dependent variable is specified and suggesting that the baseline difference may partly reflect bank scale. Third, our merger-consolidation methodology involves judgment in mapping historical bank names to surviving entities, so earlier years are pro forma combinations and some mergers or name changes may be imperfectly captured, although the result is similar when merged entities are excluded (Section 5.3). Fourth, p-values rely on the normal approximation, and some specifications have a modest number of clusters (Section 3.3), so they may overstate precision.

6.4 Policy Implications and Future Research

Notwithstanding the causal-interpretation caveats above, this paper has two policy-relevant implications. First, although the 2021 instructions require banks to maintain branch-wise lists of vacant lockers and waitlists, we found no publicly available aggregate data on locker capacity, occupancy or waitlist length in the RBI statistical tables we used, which limits external evaluation of the reform's effects on locker availability. We suggest that RBI consider publishing aggregate locker capacity and waitlist statistics. Second, to the extent the faster growth of PSU banks' other income relative to other banks is partly related to the reform (Section 6.2), policymakers may

wish to monitor whether it reflects a reallocation of locker-related business toward better-positioned incumbents or continued informal rationing practices that operate around the 2021 instructions; our data cannot distinguish between these.

Future research would benefit from primary data collection (household or branch-level surveys of locker waitlist length, willingness to pay, and allocation practices), which would allow direct estimation of locker-specific demand and capacity effects rather than the aggregate fee-income proxy employed here. A formal analytical model of bank incentives under regulatory constraints on locker pricing and allocation, calibrated to the Indian locker market, would also complement this paper's reduced-form approach.

7. Conclusion

This paper examined how India's August 2021 bank locker reform, a dated regulatory change affecting a rationed banking service that has received little quantitative study, was associated with fee-based income at Indian commercial banks, and whether the change in fee-based income differed between public sector banks and other banks. Using a panel of 112 scheduled commercial banks (after merger consolidation) over FY2005–FY2025, we find that public sector banks' fee income grew significantly faster than other banks' following the reform, contrary to our initial expectation. The result is robust to an alternative policy cutoff, to restricting the sample to public and private sector banks, to additional bank-performance controls, and to excluding merger-affected entities, but it is not statistically significant when the dependent variable is scaled by total assets. Event-study coefficients for FY2018 and FY2019 are close to zero, although the coefficients rise from FY2015 onward. At the same time, a placebo test using a counterfactual treatment date yields an even larger "effect" in pre-reform data (FY2005–FY2021), suggesting that part of the observed divergence may reflect a longer-running trend rather than a response specific to the 2021 reform. The evidence is therefore consistent with, but does not conclusively isolate, a reform-specific causal effect.

This paper makes three contributions. It provides empirical evidence relevant to the theory of non-price rationing under price ceilings in a setting, Indian bank lockers, that has, to our knowledge, received no prior quantitative academic study. It contributes to the literature on non-interest income determinants in emerging-market banking by using a dated regulatory change as a source of identifying variation, a design that, to our knowledge, is rarely used in that literature. And it applies panel-econometric methods to bank lockers, a literature that, to our knowledge, comprises only regulatory text, industry commentary. We also found no publicly available aggregate data on locker capacity and demand in India, which limits both external evaluation of the reform and future research.

We hope this paper serves less as a definitive causal account and more as a foundation: both for future work bringing primary, locker-specific data to bear on the questions raised here, and for a broader research agenda applying panel-econometric methods to other rationed financial services in emerging banking markets.

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