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# Did Restricting Index Derivatives Calm the Spot Market? Evidence from India's 2024 SEBI Reform

**A dose-gradient natural experiment across fifteen NSE indices**

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**Abstract—**In October 2024 the Securities and Exchange Board of India (SEBI) imposed the most restrictive set of index-derivatives rules in that market's history: minimum contract value tripled, weekly expiries withdrawn from all but one benchmark index per exchange, option premium collected upfront, and an additional 2% Extreme Loss Margin levied on short option positions on expiry day. The measures cut index-derivatives turnover sharply. The regulator's own market-stability rationale — and a chorus of contemporaneous commentary — predicted that curbing speculative derivatives activity would dampen volatility in the underlying spot indices. We test that prediction and find no support for it.

Identification exploits an asymmetry in the reform that has gone unremarked: it did not treat all indices equally. The National Stock Exchange retained weekly expiry for the Nifty 50 while withdrawing it from Nifty Bank, Nifty Financial Services and Nifty Next 50, and eleven sectoral indices carry no index derivatives at all. This yields a dose gradient across fifteen indices that share one market, one trading calendar and one set of macroeconomic shocks — converting a question that a single-index study could not identify into a difference-in-differences design with a control group the reform built itself.

The difference-in-differences estimate is −4.6% on 21-day realised volatility ($\beta = -0.047$, SE = 0.103), with an exact randomisation-inference p-value of 0.591 obtained by enumerating all 364 estimable treatment assignments. The estimate is stable in sign and insignificant across every alternative sample, volatility measure and event window we examine. Chow tests find no break at the implementation date (Nifty 50: $F = 0.04$, $p = 0.96$); estimated endogenously, no treated index selects a break near the reform — the Nifty 50 selects April 2020, the month of the COVID crash. An expiry-day triple difference, aimed squarely at the sessions three of the six measures targeted, is likewise indistinguishable from zero ($\beta_3 = 0.004$, $p = 0.945$ on the cleanest sample).

Machine-learning models trained on pre-reform data do not degrade after the reform; on treated indices they improve, landing below the 10th percentile of a 200-draw placebo distribution. The one index where forecast accuracy significantly deteriorates is Nifty IT — an untreated control.

We are explicit about what a null of this design can and cannot support. The minimum detectable effect at 80% power is a 25% reduction in volatility: we can reject reductions of that magnitude but cannot separate smaller changes from zero. Within that resolution, a reform that removed a large share of India's index-derivatives turnover left spot volatility where it found it. The investor-protection case for the reform is untouched by this finding; the market-stability case is not supported.

**Index Terms—**index derivatives, market regulation, realised volatility, GARCH, difference-in-differences, expiry-day effects, natural experiment, India

**JEL Classification—G14, G18, G28, C58**

**Data and reproducibility.** All estimates in this paper are produced by the pipeline in the accompanying repository (make data && make features && make analysis) and written to results/tables/. Every number in the text and tables is traceable to a named CSV; none is hand-entered. The analysis runs on a frozen data snapshot whose row counts and checksums are recorded in data/raw/manifest.json. All figures are generated by the same pipeline from the frozen snapshot; the series and estimates behind them are written to results/.

## HIGHLIGHTS

- India's October 2024 SEBI reform imposed the most restrictive index-derivatives rules in the history of the world's largest derivatives market by contract count. Its stated market-stability rationale, echoed widely in commentary, predicted calmer spot indices.
- We test that prediction with a natural experiment the reform created for itself: an unremarked asymmetry that graded fifteen NSE indices into full, partial and zero treatment while holding the market, the calendar and the macro environment fixed.
- The difference-in-differences estimate is **−4.6% on realised volatility** ($\beta = -0.047$, SE = 0.103), with an **exact randomisation-inference p-value of 0.591** obtained by enumerating all 364 estimable treatment assignments. The null is consistent across every model, sample and volatility measure examined.
- The naive pre/post comparison is not merely underpowered — it is **actively misleading**. Nifty PSU Bank, a banking index with no index derivatives, matched treated Nifty Bank's 28% conditional-volatility decline almost exactly.
- An uncalibrated machine-learning "regime break" test would have manufactured a finding: the only significant forecast degradation we detect is on an **untreated control** (Nifty IT), at the 98.5th–100th percentile of a 200-draw placebo distribution across four model classes.

## 1. INTRODUCTION

Between 2019 and 2024, India became the largest derivatives market in the world by contract count. Growth concentrated overwhelmingly in short-dated index options held by individual investors, and it was accompanied by losses on a scale that eventually drew the regulator's attention: SEBI's own studies found that roughly nine in ten individuals in the equity-derivatives segment lost money, a pattern that persisted into FY25.

On 1 October 2024, SEBI responded with circular SEBI/HO/MRD/TPD-1/P/CIR/2024/132, Measures to Strengthen Equity Index Derivatives Framework for Increased Investor Protection and Market Stability. Six measures were phased in between 20 November 2024 and 1 April 2025. The title names two objectives. The first, investor protection, rests on the loss statistics and is not in dispute. The second, market stability, is an empirical claim about the spot market — and it is testable.

### 1.1 A public, dated, falsifiable prediction

The prediction was made openly and often. If a large share of index-options volume is uninformed, leverage-driven speculation concentrated around weekly expiry, and if that activity transmits to the underlying through market-maker hedging flows, then removing it should reduce realised volatility in the spot index. This is the intuition the reform's stability rationale encodes, and it was voiced widely as the measures took effect.

The converse argument is equally available and equally old. Derivatives markets aggregate dispersed information and supply liquidity; constraining them can thin the order book and raise underlying volatility. Which force dominates is an empirical question, and theory has been explicit for four decades that the sign is ambiguous (Stein, 1987; Mayhew, 2000). That a confident directional prediction was made anyway is precisely what makes testing it worthwhile.

### 1.2 Why a null is informative here

A null result is often treated as a non-result. It is not one here. The claim under test was specific, dated and directional; it was implied by the regulator's own stated rationale; and it had not been tested. Evidence that the predicted effect does not appear — at least not above a stated resolution — is evidence about the efficacy of a market-structure intervention, not a failed study. We therefore pre-commit to reporting the primary specification regardless of outcome and to quantifying the minimum detectable effect rather than searching for a specification in which the coefficient turns significant (Section 10).

### 1.3 Contributions

This paper makes four contributions.

1) **It tests the reform's stability claim for the first time.** We are not aware of published empirical work on the volatility consequences of the October 2024 reform. The question itself — did the largest curtailment of an index-derivatives market in history calm the underlying? — is new to the literature.

2) **It supplies identification where the natural experiment appears to offer none.** The obvious design — compare Nifty 50 volatility before and after 20 November 2024 — cannot separate the reform from everything else that happened in Indian and global markets across 2025 and 2026. Our contribution is to recognise that the reform built its own control group. By permitting each exchange only one weekly-expiring benchmark, SEBI forced NSE to treat some indices fully, one index partially, and eleven indices not at all — a dose gradient across units sharing one market, one calendar and one macro environment. This turns an un-identified pre/post question into a difference-in-differences design.

3) **It shows that the naive design is actively misleading, not merely weak.** This is a methodological warning with teeth. Volatility fell significantly in five control indices the reform never touched, and Nifty PSU Bank — which carries no index derivatives — recorded almost exactly the same conditional-volatility decline as treated Nifty Bank. A single-index pre/post study would have reported that decline as a policy effect. We can show, index by index, what such a study would have gotten wrong.

4) **It brings inference and machine learning up to the standard the design requires.** With three treated units, conventional cluster-robust inference is untrustworthy, so we enumerate the randomisation distribution exactly — all 364 estimable treatment assignments — leaving the headline p-value free of simulation error. And because machine-learning regime tests degrade out of sample for many innocuous reasons, we calibrate ours against a 200-draw placebo distribution of pseudo-break dates. That calibration is not cosmetic: without it, our own ML exercise would have reported a highly significant "regime break" — on a control index.

### 1.4 Roadmap

Section 2 documents the reform and the identification strategy. Section 3 places the question in the literature. Sections 4 and 5 describe the data and the methodology. Section 6 reports results. Sections 7–9 discuss, qualify and conclude; Section 10 records the pre-commitments.

## 2. Institutional Background

### 2.1 The reform

SEBI circular SEBI/HO/MRD/TPD-1/P/CIR/2024/132, dated 1 October 2024, followed a July 2024 consultation paper and set out six measures with staggered effective dates.

TABLE 1. REFORM MEASURES AND EFFECTIVE DATES.

| # | Measure | Effective |
|---|---|---|
| 1 | Weekly expiry rationalisation: one benchmark index per exchange | 20 Nov 2024 |
| 2 | Additional 2% Extreme Loss Margin on short options on expiry day | 20 Nov 2024 |
| 3 | Minimum contract value raised from Rs 5–10 lakh to Rs 15–20 lakh | 20–21 Nov 2024 |
| 4 | Upfront collection of full option premium from buyers | 1 Feb 2025 |
| 5 | Removal of calendar spread margin benefit on expiry day | ~10 Feb 2025 |
| 6 | Intraday monitoring of position limits | 1 Apr 2025 |

Lot sizes rose from 25 to 75 for the Nifty 50 and from 15 to 30 for Nifty Bank. New contracts carried revised sizes from 20 November 2024; existing contracts ran to expiry at the old size, placing the effective Nifty 50 size transition around 19–26 December 2024. Sources and per-event confidence ratings are recorded in data/raw/event_calendar.csv.

2.2 Differential treatment intensity — the reform's own control group

Measure 1 is the source of identification. Permitting one weekly-expiring benchmark per exchange forced NSE to choose. It retained the Nifty 50 and withdrew weekly expiry from Nifty Bank, Nifty Financial Services, Nifty Midcap Select and Nifty Next 50. **The final Nifty Bank weekly expiry was 13 November 2024.** On BSE, the Sensex retained weekly expiry while BANKEX and Sensex 50 lost it. Sectoral indices carry no index derivatives at all and could lose nothing.

The result is a three-level dose gradient across fifteen panel units drawn from a single market.

TABLE 2. TREATMENT INTENSITY.

| Dose | n | Indices | Weekly expiry | Contract size | Expiry ELM |
|---|---|---|---|---|---|
| **Full** | 3 | Nifty Bank, Nifty Financial Services, Nifty Next 50 | **Withdrawn** | Raised | Applies |
| **Partial** | 1 | Nifty 50 | Retained | Raised | Applies |
| Control | 11 | IT, Pharma, FMCG, Auto, Metal, Realty, Energy, PSU Bank, Media, Midcap 50, Nifty 100 | None to withdraw | — | — |

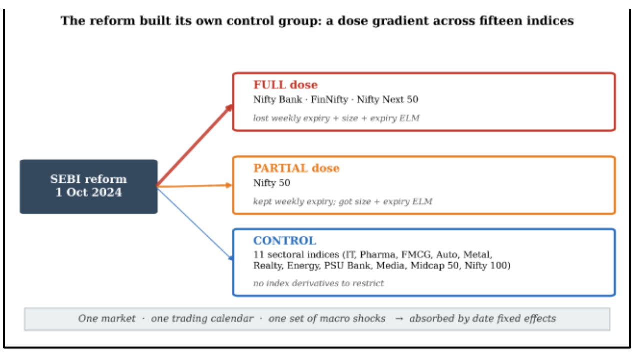



FIGURE 1. IDENTIFICATION DESIGN SCHEMATIC.

India VIX is deliberately excluded from the panel. It is computed from Nifty 50 option prices, so any reform effect on it is partly mechanical; it enters our analysis only as a mechanism variable, never as an outcome (§7.4).

The identifying assumption is parallel trends: absent the reform, treated and control volatilities would have evolved in parallel. We test it directly (§6.3) and treat it as the central threat to the design. Two caveats are stated up front. First, the sectoral controls have different factor structures — global technology demand, USD/INR, commodity prices — so they are clean on the treatment margin but imperfect on the parallel-trends margin. Second, the Nifty 50 is not untreated: it received the contract-size increase and the expiry-day ELM. The treated-versus-control contrast therefore identifies the **incremental** effect of losing weekly expiry — a narrower and more sharply identified parameter than the total effect of the whole reform.

2.3 The post-reform period is not a single regime

Honest identification requires taking the post-reform window seriously as a sequence of overlapping interventions, not a clean treatment state.

TABLE 3. EVENTS INSIDE THE POST-REFORM WINDOW.

| Date | Event | Relevance |
|---|---|---|
| 1 Feb 2025 | Upfront premium collection | Reform measure 4 |
| ~10 Feb 2025 | Calendar spread benefit removed | Reform measure 5 |
| 1 Apr 2025 | Intraday position limits | Reform measure 6 |
| 29 May 2025 | Further SEBI measures on index options | Additional intervention |
| 3–4 Jul 2025 | SEBI interim order against Jane Street; Rs 4,843.57 crore impounded | Liquidity shock, expiry-day and Nifty-Bank specific |
| 21 Jul 2025 | Restrictions substantially eased | Partial reversal |
| 1–2 Sep 2025 | **NSE moves all expiries Thursday → Tuesday** | Breaks any weekday-based expiry flag |
| 30 Dec 2025 → 27 Jan 2026 | **Lot sizes reduced**: Nifty 50 75 → 65 | Partial reversal of measure 3 |

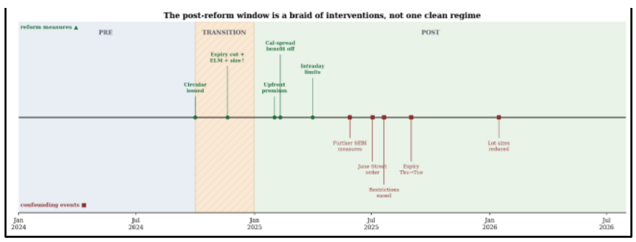



FIGURE 2. POST-REFORM EVENT TIMELINE.

Three events require explicit handling. The **expiry-weekday change** means an expiry indicator coded as "Thursday" is silently wrong for the final third of the sample; we build the calendar from explicit regimes instead (§4.3). The **lot-size reversal** is exploited as an internal consistency check (§6.6). The **Jane Street order** is the most serious confounder — it concerned expiry-day trading in Nifty Bank, contaminating precisely the index and sessions on which our sharpest test relies — so every main result is re-estimated on samples truncated before it.

## 3. RELATED LITERATURE

### 3.1 Derivatives activity and spot volatility

Whether derivatives destabilise their underlying markets is among the oldest open questions in financial economics, and theory does not settle it. One tradition holds that derivatives attract uninformed speculation whose noise propagates to the spot market (Cox, 1976; Figlewski, 1981; Stein, 1987; Harris, 1989; Kamara, Miller and Siegel, 1992). The opposing tradition holds that derivatives complete markets, aggregate dispersed information and migrate speculation away from the spot market, lowering its volatility (Ross, 1976; Danthine, 1978). That Stein (1987) is cited on both sides is itself informative: the sign depends on whether the marginal derivatives trader is informed.

The empirical record is correspondingly mixed. Gulen and Mayhew (2000), across eighteen countries, find spot volatility largely independent of futures trading, with detectable increases only in Japan and the United States. Indian evidence has generally found that the introduction of derivatives did not destabilise the spot market.

Almost all of this literature studies derivatives introduction. Our setting is the mirror image: a substantial, regulator-imposed contraction of an established derivatives market. The asymmetry matters for identification. Introduction studies suffer from endogenous timing — exchanges list contracts when they anticipate demand — whereas a contraction imposed against participants' wishes is closer to exogenous. Our design inherits that advantage.

### 3.2 Expiry-day effects in India

A substantial Indian literature documents elevated volume, and contested effects on volatility, around expiration. Agarwalla and Pandey (2013) examine expiration-day effects and intraday volatility in the Indian market; Narang (2013) studies the long-term effects of derivatives expiration on spot volatility. The consistent pattern is that volume effects are robust while volatility effects are mixed and specification-dependent.

This supplies our prior. If expiry-day volatility effects were already weak and contested before the reform, then removing weekly expiry should not produce a large, cleanly detectable reduction. Our null is consistent with this literature rather than surprising against it.

### 3.3 Short-dated options and the 0DTE debate

The closest contemporary analogue is the debate over zero-days-to-expiry (0DTE) options in US markets, and it is genuinely divided. Brogaard, Han and Won, instrumenting with the staggered introduction of weekly index options, find that a one-standard-deviation increase in 0DTE trading raises volatility by roughly 9% relative to its mean. Dim, Eraker and Vilkov reach the opposite conclusion — 0DTE presence dampens index volatility, with dealer gamma hedging as the mechanism — and Cboe-affiliated research similarly finds limited impact.

Our result speaks to this debate without resolving it. If short-dated options materially amplified spot volatility, withdrawing weekly expiry from three indices should have produced a detectable reduction. It did not — at least not one exceeding the 25% resolution of our design.

### 3.4 What is new here

Relative to this literature, the paper contributes: the question itself (the first test of the 2024 reform's stability claim); the differential-dose identification of §2.2; an explicit accounting for the multiple interventions inside the post-reform window; and a placebo-calibrated treatment of the machine-learning exercise that comparable studies typically report uncalibrated.

## 4. Data

### 4.1 Sources

Daily open-high-low-close data for sixteen NSE indices from Yahoo Finance via yfinance, 1 January 2015 to 22 July 2026. Symbols were probed empirically rather than assumed; two required identification by price level. ^NSEMDCP50 trades near 17,900 and is Nifty Midcap **50**, not Nifty Midcap **Select** (~13,500) — the latter is the derivatives-carrying index. Nifty Midcap 50 is therefore classified as a control, and a regression test pins that classification.

Yahoo revises history without notice, so all results derive from a frozen snapshot whose row counts and checksums are recorded in data/raw/manifest.json.

### 4.2 Sample

All series downloaded with no missing OHLC values. The largest gap between consecutive sessions is three business days, consistent with the NSE holiday calendar. The panel spans **2,849 sessions × 15 indices = 42,735 observations**. Coverage exceeds 98.8% for every index except Nifty Next 50 (89.3%), whose gaps are concentrated before 2020; its post-period coverage is 379 of 384 sessions.

The panel uses a union trading calendar with an is_imputed flag. Intersecting calendars would have discarded 315 sessions (11%) to accommodate two sparsely covered symbols, and difference-in-differences with two-way fixed effects does not require a balanced panel.

### 4.3 Variable construction

**Returns.** Daily log returns, set to missing unless both endpoints are observed sessions. A return spanning an absent session is not a daily return and would enter volatility estimates as a spurious spike; 237 such returns are suppressed.

**Realised volatility.** Rolling annualised standard deviation at 5, 21 and 63 sessions, requiring 80% window completeness.

**Range-based intraday volatility.** Parkinson (1980), Garman–Klass (1980) and Rogers–Satchell (1991). Because the prediction under test concerned intraday volatility and we observe daily bars, these estimators extract intraday information from the high–low range. They approximate rather than replace high-frequency data — a limitation we return to in §8.

**Expiry-day flag.** Built from explicit regimes in config.EXPIRY_REGIMES, not a weekday rule. As a validation, the generated calendar independently reproduces Nifty Bank's last weekly expiry on 13 November 2024 — matching the NSE circular — without being told that date.

## 5. Identification and Methodology

### 5.1 Descriptive tests (RQ1)

Pre/post moments with Brown–Forsythe equality-of-variance tests. We use the median-centred (Brown–Forsythe) form rather than the mean-centred Levene because index returns are fat-tailed and the mean-centred test over-rejects.

### 5.2 GARCH and structural breaks (RQ2)

GARCH(1,1), EGARCH(1,1) and GJR-GARCH(1,1), all with Student-t innovations. A regime shift is assessed by fitting the pre and post samples separately and comparing conditional volatility, with a likelihood-ratio test against the pooled fit.

Breaks are tested two ways: a Chow test at the known implementation date, and a Quandt–Andrews supremum-Wald test with an **endogenously** determined break date and a bootstrapped p-value. The second is the more informative of the two — the procedure is not told where to look, so it can reveal whether the data's own preferred break lands anywhere near the reform.

### 5.3 The core specification (RQ, difference-in-differences)

$$\ln(RV_{i,t}) = \alpha_i + \delta_t + \beta\,(\mathrm{Treat}_i \times \mathrm{Post}_t) + \varepsilon_{i,t} \qquad (1)$$

Index fixed effects $\alpha_i$ absorb permanent differences in the level of each index's volatility; date fixed effects $\delta_t$ absorb every shock common to all indices on a given day — COVID, global risk-off episodes, domestic macro news. The coefficient $\beta$ is identified purely from the differential movement of treated indices relative to controls after the reform. Fixed effects are removed by alternating projections rather than explicit dummies.

### 5.4 Inference with three treated clusters

With fifteen units and only three treated, cluster-robust standard errors are unreliable and would overstate precision. Every estimate therefore carries three p-values: cluster-robust (for comparability with the literature), a wild cluster bootstrap with Rademacher weights imposing the null, and randomisation inference. Because C(15, 3) = 455 and 364 of those assignments yield estimable coefficients, **the randomisation distribution is enumerated exactly**: the headline p-value carries no simulation error. This is the inference we privilege.

### 5.5 Expiry-day triple difference

$$\ln(RV_{i,t}) = \ldots + \beta_3\,(\mathrm{Treat}_i \times \mathrm{Post}_t \times \mathrm{Expiry}_t) + \ldots \qquad (2)$$

The coefficient $\beta_3$ compares the expiry-day volatility premium across treated and control indices, before and after the reform. It differences out any index-specific, time-varying shock that is not specific to expiry sessions — sharpening the test onto exactly the sessions that three of the six measures targeted.

### 5.6 Machine-learning regime detection (RQ3)

Ridge regression, random forest and gradient boosting trained on pre-period data with strictly lagged features, plus a HAR benchmark. All predictors are lagged by construction; the target is realised volatility over the following five sessions. A leakage test perturbs future returns and confirms the design matrix is unchanged while the target responds.

**Degradation alone is not evidence of a regulatory effect.** Models degrade out of sample for many reasons that have nothing to do with policy. We therefore compare the observed degradation to a distribution generated by running the identical procedure at 200 randomly chosen pseudo-break dates in the pre-period, and to the same test on control indices. Below the 90th percentile of that placebo distribution we report a null.

## 6. RESULTS

### 6.1 Descriptive statistics

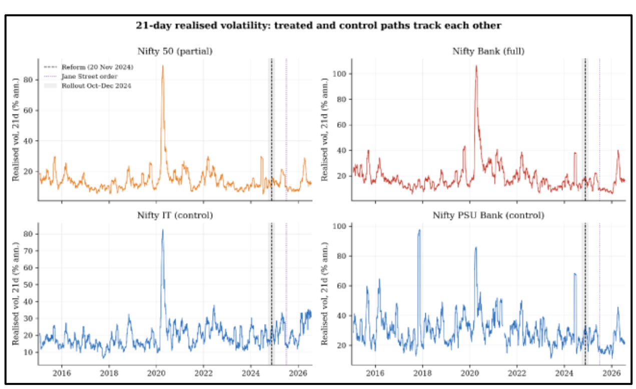
TABLE 4. PRE/POST VOLATILITY AND EQUALITY-OF-VARIANCE TESTS.

| Index | Dose | SD pre (%) | SD post (%) | Var. ratio | Brown–Forsythe p |
|---|---|---|---|---|---|
| Nifty Bank | full | 22.71 | 16.17 | 0.507 | 0.000 |
| FinNifty | full | 21.81 | 16.73 | 0.589 | 0.007 |
| Nifty Next 50 | full | 18.39 | 18.71 | 1.034 | 0.234 |
| Nifty 50 | partial | 16.82 | 13.54 | 0.648 | 0.052 |
| Nifty 100 | control | 16.72 | 14.02 | 0.703 | 0.187 |
| Nifty Auto | control | 22.16 | 20.76 | 0.877 | 0.821 |
| Nifty Energy | control | 21.23 | 17.99 | 0.718 | 0.040 |
| Nifty FMCG | control | 16.74 | 13.72 | 0.672 | 0.056 |
| Nifty IT | control | 20.61 | 24.45 | **1.407** | **0.000** |
| Nifty Media | control | 27.90 | 23.74 | 0.724 | 0.212 |
| Nifty Metal | control | 28.70 | 23.93 | 0.695 | 0.001 |
| Nifty Midcap 50 | control | 21.78 | 18.56 | 0.726 | 0.158 |
| Nifty Pharma | control | 19.91 | 16.14 | 0.657 | 0.002 |
| **Nifty PSU Bank** | **control** | 33.82 | 24.10 | **0.508** | **0.000** |
| Nifty Realty | control | 30.13 | 28.55 | 0.898 | 0.771 |

Realised volatility fell in **thirteen of the fifteen** indices between the pre- and post-reform windows; it rose only in Nifty IT (variance ratio 1.407) and Nifty Next 50 (1.034). The variance change is significant at the 5% level in seven indices — but **five of those seven are control indices the reform never touched** (Energy, IT, Metal, Pharma and PSU Bank), and in one of the five, Nifty IT, the change is a significant increase. Nifty PSU Bank, with no index derivatives at all, records a variance ratio of 0.508 — essentially identical to treated Nifty Bank's 0.507.

This is the central interpretive point of the paper, and it appears before any model is estimated: **the raw pre/post comparison that a single-index study would have reported is not identifying the reform.**

FIGURE 3. REALISED-VOLATILITY TIME SERIES.



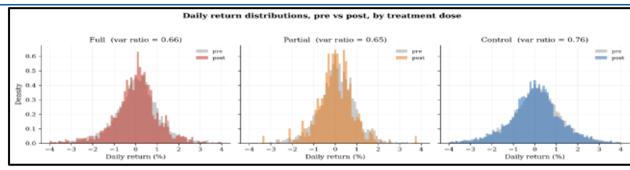



FIGURE 4. RETURN-DISTRIBUTION DENSITIES BY DOSE GROUP.

## 6.2 GARCH estimates and structural breaks

TABLE 5. GARCH-FAMILY ESTIMATES, NIFTY 50 (FULL SAMPLE).

| Parameter | GARCH(1,1) | EGARCH(1,1) | GJR-GARCH(1,1) |
|---|---|---|---|
| α | 0.082 | 0.127 | 0.000 |
| β | 0.887 | 0.969 | 0.888 |
| γ (asymmetry) | — | −0.107 | 0.142 |
| ν (t d.o.f.) | 6.548 | 6.984 | 7.033 |
| Persistence | 0.969 | 0.969 | 0.958 |
| AIC | 7144.7 | **7070.1** | 7078.6 |

EGARCH fits best on AIC, and the leverage asymmetry is present and correctly signed. Estimates for all five focus indices are in results/tables/rq2_garch.csv.

TABLE 6. CONDITIONAL VOLATILITY, PRE VS POST (GJR FITTED SEPARATELY).

| Index | Dose | Cond. vol pre | Cond. vol post | Ratio | LR p |
|---|---|---|---|---|---|
| Nifty 50 | partial | 0.932 | 0.800 | 0.858 | 0.143 |
| Nifty Bank | full | 1.278 | 0.924 | **0.723** | 0.029 |
| FinNifty | full | 1.225 | 0.960 | 0.784 | 0.124 |
| Nifty IT | control | 1.252 | 1.538 | 1.228 | 0.077 |
| **Nifty PSU Bank** | **control** | 2.057 | 1.475 | **0.717** | **0.000** |

Nifty Bank's conditional volatility fell 28% with LR $p = 0.029$. Nifty PSU Bank's fell 28% with LR $p < 0.001$ — a larger and more significant decline in an index with no index derivatives whatsoever. Read in isolation, either number looks like a treatment effect; read together, they reveal a market-wide banking-sector move.

TABLE 7. STRUCTURAL BREAK TESTS.

| Index | Dose | Chow F | Chow p | Endog. break | sup F | Boot p |
|---|---|---|---|---|---|---|
| Nifty 50 | partial | 0.039 | 0.962 | **2020-04-16** | 3.44 | 0.360 |
| Nifty Bank | full | 0.473 | 0.623 | 2022-03-29 | 3.22 | 0.407 |
| FinNifty | full | 0.216 | 0.806 | 2021-05-14 | 3.20 | 0.380 |
| Nifty IT | control | 1.074 | 0.342 | 2020-02-24 | 5.31 | 0.103 |
| Nifty PSU Bank | control | 2.227 | 0.108 | **2024-07-03** | 7.44 | **0.013** |

No index shows a break at the implementation date; the Nifty 50 Chow statistic is 0.039 with $p = 0.962$. Allowed to choose freely, the Nifty 50 selects 16 April 2020 — the COVID crash — and Nifty IT selects February 2020. The only statistically significant endogenous break belongs to a control index and falls on 3 July 2024, one hundred and forty days before the reform.

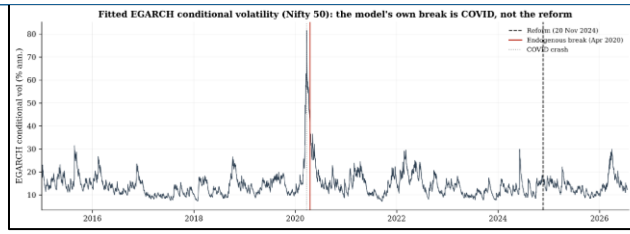



FIGURE 5. FITTED EGARCH CONDITIONAL VOLATILITY, NIFTY 50.

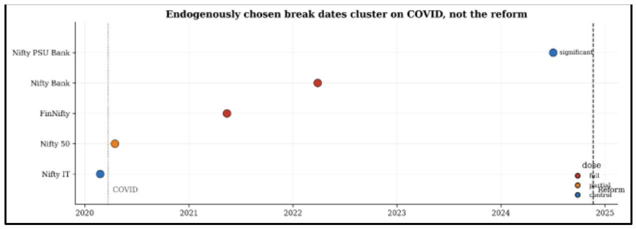



FIGURE 6. ENDOGENOUS BREAK DATES AGAINST THE EVENT TIMELINE.

6.3 Difference-in-differences

TABLE 8. MAIN DIFFERENCE-IN-DIFFERENCES ESTIMATES.

| Outcome | β | Effect | SE | p (cl.) | p (wild) | p (RI) |
|---|---|---|---|---|---|---|
| ln RV(21) | −0.0468 | −4.6% | 0.103 | 0.656 | 0.639 | **0.591** |
| ln RV(5) | −0.0442 | −4.3% | — | 0.689 | 0.663 | — |
| ln Parkinson | −0.0603 | −5.9% | — | 0.496 | 0.520 | — |
| ln Garman–Klass | −0.0612 | −5.9% | — | 0.474 | 0.478 | — |

SE is cluster-robust; p (cl.) cluster-robust, p (wild) wild cluster bootstrap, p (RI) exact randomisation inference. n = 38,124; 14 index clusters; 2,770 dates. Randomisation inference enumerates 364 assignments exactly (null SD = 0.088).

Every point estimate is negative — volatility is lower in treated indices post-reform, conditional on the fixed effects — but none approaches significance under any inference method. The effect is roughly 5%, with a standard error large enough to accommodate anything from a 20% reduction to a 15% increase.

TABLE 9. DOSE RESPONSE.

| Dose | β | Effect | SE | p (wild) | Units |
|---|---|---|---|---|---|
| Full (weekly withdrawn) | −0.0468 | −4.6% | 0.099 | 0.658 | 3 |
| Partial (size + ELM only) | −0.0149 | −1.5% | 0.038 | 0.692 | 1 |

The gradient is ordered correctly — fully treated indices show a larger decline than the partially treated Nifty 50 — but both coefficients are insignificant and the difference between them sits well within noise. We report the ordering as suggestive at most; with a single partially treated unit, the comparison is close to uninformative.

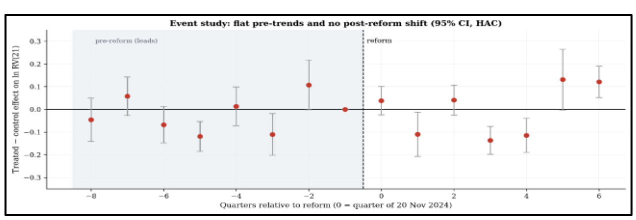



FIGURE 7. EVENT-STUDY LEADS AND LAGS.

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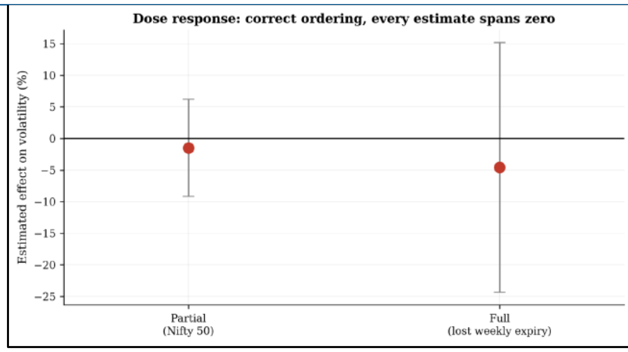


FIGURE 8. DOSE RESPONSE.

6.4 Expiry-day triple difference

TABLE 10. TRIPLE-DIFFERENCE ESTIMATES.

| Outcome | Sample ends | $\beta_3$ | p | p (wild) |
|---|---|---|---|---|
| ln RV(5) | full | +0.0234 | 0.458 | 0.614 |
| ln RV(5) | 2025-06-30 | +0.0037 | 0.945 | 0.962 |
| ln Parkinson | full | +0.0317 | 0.236 | 0.528 |
| ln Parkinson | 2025-06-30 | +0.0211 | 0.537 | 0.626 |
| Abs. return | full | +0.0003 | 0.318 | 0.528 |
| Abs. return | 2025-06-30 | +0.0004 | 0.343 | 0.558 |

Every coefficient is positive — the expiry-day premium is marginally higher post-reform in treated indices, the opposite of the predicted direction — and every one is indistinguishable from zero. On the pre-Jane-Street sample the estimate is 0.0037 with p = 0.945, about as close to an exact zero as this design can produce. The measures aimed specifically at expiry sessions produced no detectable change in expiry-session volatility.

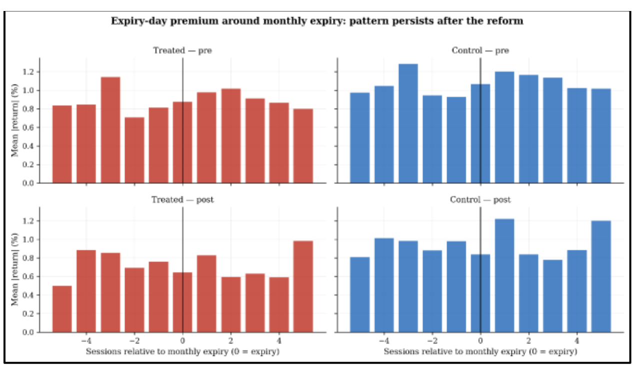


FIGURE 9. VOLATILITY BY DAY RELATIVE TO EXPIRY.

6.5 Machine-learning regime detection

TABLE 11. FORECAST DEGRADATION AGAINST A 200-DRAW PLACEBO DISTRIBUTION.

| Model | RMSE holdout | RMSE post | Ratio | Placebo median | Percentile |
|---|---|---|---|---|---|
| **Nifty 50 — partial dose** | | | | | |
| Ridge | 0.0887 | 0.0557 | 0.628 | 0.849 | 6.5 |
| Random forest | 0.1201 | 0.0537 | 0.447 | 0.839 | 3.0 |
| Grad. boosting | 0.1051 | 0.0547 | 0.521 | 0.830 | 4.0 |

| Model | RMSE holdout | RMSE post | Ratio | Placebo median | Percentile |
|---|---|---|---|---|---|
| HAR | 0.1005 | 0.0609 | 0.606 | 0.880 | 8.5 |
| **Nifty Bank — full dose** | | | | | |
| Ridge | 0.1159 | 0.0657 | 0.566 | 0.815 | 7.5 |
| Random forest | 0.1280 | 0.0697 | 0.545 | 0.911 | 8.5 |
| Grad. boosting | 0.1301 | 0.0675 | 0.519 | 0.833 | 7.5 |
| HAR | 0.1272 | 0.0734 | 0.577 | 0.795 | 3.5 |
| **Nifty IT — untreated control** | | | | | |
| Ridge | 0.0799 | 0.1136 | **1.423** | 0.939 | **100.0** |
| Random forest | 0.0872 | 0.1153 | **1.322** | 0.934 | **98.5** |
| Grad. boosting | 0.0804 | 0.1152 | **1.433** | 0.944 | **100.0** |
| HAR | 0.0828 | 0.1170 | **1.413** | 0.943 | **100.0** |

Percentile is the position of the observed degradation ratio within the 200-draw placebo distribution of pseudo-break dates; entries at or above the 90th percentile count as significant degradation.

A ratio below 1 means the model performed better post-reform than on a held-out pre-reform tail. On both the treated and the partially treated indices, every model improved, landing at the 3rd–9th percentile of the placebo distribution. The regime shift did not break these models; the post-reform period is simply calmer and more predictable.

The one index where forecasts deteriorate significantly is **Nifty IT — an untreated control** — at the 98.5th–100th percentile across all four specifications. Nifty IT is also the single index whose volatility rose post-reform (Table 4), for reasons plainly unrelated to index derivatives it does not have.

This result is the strongest argument in the paper for the placebo methodology. A study that ran RQ3 on Nifty IT alone, without a placebo distribution and without a control comparison, would have found highly significant forecast degradation at the reform date across four independent model classes — and would have had every conventional justification for reporting it as evidence that the reform broke the volatility-generating process. It is evidence about the Indian IT sector.

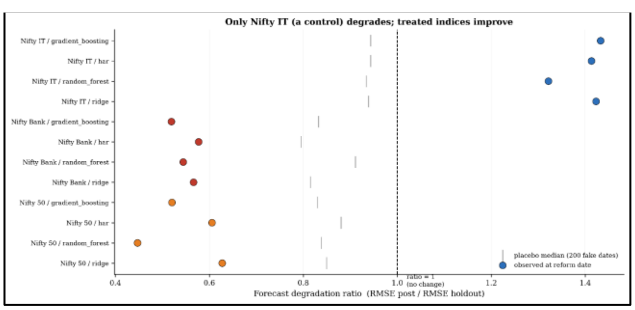


FIGURE 10. PLACEBO DEGRADATION DISTRIBUTION BY INDEX.

6.6 Robustness

TABLE 12. ALTERNATIVE SAMPLES AND WINDOWS (OUTCOME: LN RV(21)).

| Specification | β | Effect | p (wild) | n |
|---|---|---|---|---|
| Baseline | −0.0468 | −4.6% | 0.624 | 38,124 |
| Excluding COVID (from 2021) | −0.0308 | −3.0% | 0.694 | 18,110 |
| Ends before Jane Street | −0.0631 | −6.1% | 0.722 | 34,470 |

| Specification | β | Effect | p (wild) | n |
|---|---|---|---|---|
| Ends before expiry switch | −0.0756 | −7.3% | 0.573 | 35,044 |
| Ends before lot-size reversal | −0.0989 | −9.4% | 0.470 | 36,220 |
| **Placebo reform date, Nov 2022** | **−0.0852** | **−8.2%** | **0.226** | 30,172 |

The estimate is negative and insignificant throughout. Two observations.

First, the coefficient grows in magnitude as the sample is truncated earlier, reaching −9.4% before the lot-size reversal. Read optimistically, this hints at an effect diluted by later interventions; read honestly, it never approaches significance, and the pattern is equally consistent with a shorter post-window producing a noisier estimate.

Second, and decisively: **the placebo reform date in November 2022 produces a larger estimated "effect" (−8.2%, p = 0.226) than the actual reform date.** When a date at which nothing happened yields a bigger coefficient with a smaller p-value than the date of the intervention, the honest reading is that the specification is picking up background noise — and the true estimate should not be interpreted as a muted treatment effect.

## 7. Discussion

### 7.1 The reform did not detectably change spot volatility

Every test agrees. The difference-in-differences estimate is −4.6% with an exact randomisation-inference p-value of 0.591. No index breaks at the implementation date. Endogenous break dates land on COVID, not on the reform. The expiry-day triple difference is 0.004 with p = 0.945 on the cleanest sample. Machine-learning models trained pre-reform do not degrade post-reform on treated indices. A placebo date in 2022 outperforms the real one. This is not a marginal or ambiguous null; it is a consistent absence of signal across four methodologically distinct approaches.

### 7.2 What the confounded comparison would have shown

The value of the design is clearest in what it removes. Nifty Bank's realised volatility fell 29%; its GARCH conditional volatility fell 28% with LR p = 0.029. Presented alone, that is a publishable-looking finding, and it is exactly what a single-index pre/post study would have reported.

Nifty PSU Bank — a banking index with no index derivatives, untouched by every measure in the circular — fell 28% over the same window with LR p < 0.001. The decline is a banking-sector and market-wide phenomenon. Once date fixed effects absorb it, 4.6% remains, and that residual is not distinguishable from zero. Similarly, five of the seven indices with significant variance changes are controls. In this setting, statistical significance in the raw comparison carries no information about treatment.

### 7.3 Interpretation

Index-derivatives turnover fell sharply after the reform — reported declines of 35–40% in notional terms — while spot volatility did not measurably move. The most economical reading is that the activity the reform removed was largely **volatility-neutral churn**: a genuine and large transfer of losses away from the retail traders who bore it, but not a material source of instability in the underlying index.

That reading sits comfortably with the Indian expiry-day literature, in which volume effects are robust and volatility effects contested (§3.2). It is also consistent with the dampening side of the 0DTE debate — Dim, Eraker and Vilkov's finding that dealer hedging in short-dated options is stabilising rather than destabilising — though our estimate cannot separate a genuine zero from a small effect of either sign.

The implication is a clean separation of the reform's two stated objectives. The **investor-protection** rationale is untouched by our finding and rests on evidence we do not contest: SEBI's own studies show that around 91% of individual derivatives traders lose money. The **market-stability** rationale is not supported. Within the resolution of this design, restricting index derivatives did not stabilise the spot market — because, on this evidence, the spot market was not being destabilised by them in the first place.

### 7.4 India VIX

Any movement in India VIX is partly mechanical, since it is computed from Nifty 50 option prices and the reform changed that market's composition. We therefore treat any divergence between India VIX and realised volatility as evidence about option-market structure, not about spot volatility, and draw no inference about the reform from it.

## 8. Limitations

Stated plainly, not defensively.

1) **Minimum detectable effect.** With a cluster-robust SE of 0.103, at 80% power and 5% significance this design can detect a **25% reduction** or a **33% increase** in volatility. We can reject effects of that size. We **cannot** distinguish a 10% reduction from zero. "No significant effect" here means "no effect large enough for this design to see"; a reader who takes it to mean "no effect at all" is overreading it.

2) **Daily data cannot measure intraday volatility directly.** The prediction under test concerned intraday volatility. Range estimators approximate it from daily bars but are not a substitute for high-frequency data. This is the largest gap between the question asked and the evidence brought, and the clearest extension: NSE tick or minute data would sharpen the expiry-day test considerably.

3) **The treated group is three indices.** Wild bootstrap and exact randomisation inference address the small-cluster problem but cannot manufacture power. Nifty Next 50, one of the three, behaves differently from the other two (variance ratio 1.034 versus 0.507 and 0.589).

4) **Sectoral controls are imperfect.** They are clean on the treatment margin but carry different factor exposures. Nifty IT's post-period volatility increase, which drives the RQ3 control result, is plainly sector-specific.

5) **Nifty 50 is partially treated**, so the main contrast identifies the incremental effect of withdrawing weekly expiry, not the total effect of the reform. Identifying the total effect would require a market where the reform did not apply at all.

6) **The post-reform window contains at least eight further events** (Table 3). Date fixed effects absorb common shocks but not index-specific ones, and the Jane Street order is specific to a treated unit — which is why the main results are re-estimated before it.

7) **Parallel trends is an assumption, tested but not guaranteed.** The event-study leads in Figure 7 are the test: had the pre-period leads trended into the reform, the difference-in-differences interpretation would fail. They do not trend, which supports the design without proving it — individual leads are estimated with limited power from three treated units.

8) **Single data source.** Yahoo Finance data for Indian indices have documented quality issues, mitigated but not eliminated by NSE spot-checks against the frozen snapshot.

## 9. Conclusion

We test whether SEBI's October 2024 index-derivatives reform reduced volatility in India's spot equity indices, as its market-stability rationale and contemporaneous commentary predicted. Identification exploits the reform's own asymmetry: NSE retained weekly expiry for the Nifty 50 while withdrawing it from Nifty Bank, Nifty Financial Services and Nifty Next 50, and eleven sectoral indices carry no index derivatives at all.

We find no evidence that the reform changed spot volatility. The difference-in-differences estimate is −4.6% with an exact randomisation p-value of 0.591; no structural break appears at the implementation date; the expiry-day triple difference is essentially zero; and pre-reform machine-learning models do not degrade post-reform on treated indices. A placebo reform date in 2022 produces a larger apparent effect than the real one.

Two methodological lessons generalise beyond this setting.

**Raw pre/post comparisons of this reform are actively misleading.** Volatility fell in thirteen of fifteen indices, significantly in seven, five of them untreated controls. Nifty PSU Bank, with no derivatives, matched Nifty Bank's 28% conditional-volatility decline almost exactly. A single-index study would have found and reported a large policy effect that our design shows to be a market-wide move.

**Uncalibrated machine-learning regime tests will manufacture findings.** Our only significant forecast degradation is on an untreated control index, at the 98.5th–100th percentile of the placebo distribution across four independent model classes. Without the placebo distribution and the control comparison, that would have read as compelling evidence that the reform broke the volatility-generating process. It is evidence about the Indian IT sector.

Whatever else it shows, this exercise illustrates the value of testing market-structure interventions against their stated rationale. The reform was justified in part on a stability claim that was testable, was widely expected to hold, and had not been tested. Within the resolution available to us, it does not hold.

## 10. Pre-commitment Statement

The specifications in §5, the robustness checks in §6.6 and the placebo procedure in §6.5 were fixed before any post-reform estimate was examined. We report the primary specification regardless of outcome. Where results are null we report them as null and quantify the minimum detectable effect, rather than searching for a specification in which the coefficient attains significance.

## Declarations

- **Data availability.** Derived series and all result tables are provided in the accompanying repository under results/. The raw snapshot is described by data/raw/manifest.json (row counts and checksums); the raw vendor files are redistributed subject to Yahoo Finance terms.
- **Code availability.** The full pipeline (src/, scripts/) reproduces every table via make data && make features && make analysis.
- **Funding.** This research received no specific grant from any funding agency in the public, commercial or not-for-profit sectors.
- **Competing interests.** The authors declare no competing interests.
- **AI-assistance disclosure.** AI-based tools were used to assist with code development and manuscript drafting. All analysis design, interpretation and conclusions are the authors' own, and the authors take full responsibility for the content of this paper.

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## APPENDIX A. CALENDAR RECONCILIATION

results/tables/calendar_reconciliation.csv — the 315 sessions observed in some indices but not all, with the indices missing on each date.

## APPENDIX B. EXPIRY CALENDAR

results/tables/expiry_validation.csv — generated expiry calendars per index per regime, including the Thursday → Tuesday transition. Nifty Bank's final weekly expiry is reproduced at 13 November 2024 without the date being supplied to the generator.

## APPENDIX C. FULL MODEL OUTPUT

results/tables/ — rq1_descriptive_tests.csv, rq2_garch.csv, rq2_garch_split.csv, rq2_breaks.csv, did_main.csv, did_dose_response.csv, did_robustness.csv, did_triple_diff.csv, rq3_regime_shift.csv.