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Did Market Closure Stabilize or Delay the Market? Price Discovery and Volatility Resilience in the Tehran Stock Exchange under Geopolitical Shock

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Abstract

This paper documents nominal price adjustment and daily return variability in the Tehran Stock Exchange around the equity-market suspension from 28 February to 18 May 2026 and reopening on 19 May. It uses a TGJU broad-market proxy and the TSETMC equal-weight index, with 68 valid post-reopening observations. The analysis combines a reproducible suspension audit, historical reference windows, dependence-preserving resampling, pre-event forecast validation, interrupted time-series regressions and volatility-normalization sensitivity tests. The twenty-session cumulative nominal returns are 37.33% and 41.90%, respectively. Both exceed simple forecast benchmarks, but historical tail probabilities lose significance after adjustment across outcomes and horizons, and resampling conclusions depend on block length. Equal-weight absolute-return variability rises more relative to its own baseline; the broad-market conclusion changes when centered standard deviation replaces absolute returns. Normalization estimates of five and twenty-seven sessions arise only under a permissive retrospective rule. Under a rolling-distribution threshold with five consecutive compliant observations, the broad-market confirmation occurs at session 32 and the equal-weight series does not confirm within 68 sessions. A restricted forecasting exercise selects models using pre-event validation and finds no uniform post-event superiority. These findings describe adjustment under a combined geopolitical shock, closure and controlled reopening. They do not identify the causal effect of suspension or establish real recovery, informational efficiency or liquidity resilience.

Keywords: Geopolitical Risk, Market Closure, Price Discovery, Volatility Resilience, Tehran Stock Exchange.
JEL Classification: G01, G10, G14, G15, C22, C53.

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1. Introduction

Geopolitical shocks pose a special challenge for financial markets because they simultaneously affect information flows, investor sentiment, liquidity, inflation expectations, exchange-rate risk, and the credibility of market institutions. In normal conditions, equity markets process news through continuous trading. Under extreme stress, however, authorities may suspend trading in an attempt to protect investors and prevent disorderly price formation. The central question is whether such a closure stabilizes the market or simply transfers the adjustment to the reopening date.

This paper examines the 2026 suspension and controlled reopening of the Tehran Stock Exchange (TSE). The exchange announcement circulated by the Federation of Euro-Asian Stock Exchanges (FEAS, 2026) confirms that equities resumed trading on 19 May 2026 after an 80-day interruption. Reuters (2026) had reported the planned 19 May reopening on 16 May and described the suspension as a measure intended to protect investors, limit panic-driven trading, and improve information for pricing. Iran International (2026) described the actual reopening under tight controls, with several major firms still suspended. The episode provides a case study of post-interruption price adjustment in a sanctioned, inflationary market with limited international integration.

The study makes three contributions. It documents nominal price adjustment after a prolonged exchange-level suspension during a geopolitical shock; compares a TGJU broad-market proxy with the TSETMC equal-weight index; and evaluates the sensitivity of return and variability measures to historical benchmarks, forecast specifications and normalization criteria. Both indices were exposed to the event, so neither serves as an untreated comparison. The analysis describes the combined episode rather than identifying the separate effects of conflict, suspension and reopening restrictions.

The first twenty observed sessions produced nominal cumulative returns of 37.33 percent for the broad-market proxy and 41.90 percent for the equal-weight index. The sixty-session mean absolute-return ratios relative to the pre-event baseline were 1.13 and 1.64. Centered standard-deviation ratios were 0.85 and 1.28, showing that return magnitude and dispersion need not move together. Historical-window tail probabilities do not remain significant after adjustment across eighteen comparisons. Normalization dates vary with the definition, and five- and twenty-seven-session estimates arise only under a permissive retrospective rule.

The rebound is consistent with substantial repricing after reopening, including possible incorporation of accumulated information. However, inflation expectations, changing exchange rates and risk premia, subsequent news and managed trading provide alternative

explanations. A positive nominal index change does not establish real recovery or successful price discovery. The central policy question in the title therefore remains a motivation rather than an identified causal conclusion.

The rest of the paper is organized as follows. Section 2 describes the institutional background and event design. Section 3 reviews the literature. Section 4 describes the data and variables. Section 5 presents the empirical methodology. Section 6 reports the empirical results and discussion. Section 7 discusses policy and risk-management implications. Section 8 summarizes limitations and robustness considerations, and Section 9 concludes.

2. Institutional Background and Event Context

The episode combines a prolonged interruption, a controlled reopening and a challenging macroeconomic setting. Unlike brief trading halts or intraday circuit breakers, the suspension prevented domestic equity trading for an extended period. The reopening involved continued suspensions of some firms and restrictions intended to contain selling pressure (Iran International, 2026). Sanctions, inflation and exchange-rate pressure further limit direct comparisons with deep, internationally integrated markets.

The equity-market suspension ran from 28 February through 18 May 2026 inclusive (80 calendar days), and trading resumed on Tuesday, 19 May 2026. The analysis uses the verified 18 May endpoint throughout. The 80-calendar-day closure is distinct from the 68 valid observed post-reopening returns. The FEAS (2026) exchange statement separately reports 51 foregone trading sessions, a count not reconstructed from a generic weekday calendar.

The primary contemporaneous source is the Tehran Securities Exchange reopening announcement circulated by the Federation of Euro-Asian Stock Exchanges (FEAS, 2026) on 19 May. It confirms that equity-market trading resumed on that date following an 80-day closure; the opening-bell statement by the chairperson of Iran's Securities and Exchange Organization also reports 51 foregone trading days and refers to wartime uncertainty and the need for company disclosures. Mondo Visione (2026) republished the exchange announcement on 20 May, while explicitly dating the ceremony Tuesday, 19 May. Reuters (2026) independently reported on 16 May that equity trading was scheduled to resume on Tuesday, 19 May. The verified institutional dates determine the exclusion interval used throughout the analysis.

The last valid pre-event observation is 25 February 2026. The first reopened return on 19 May bridges this last pre-event price and the first valid reopened level. It is retained

because the adjustment across the interruption is relevant, but it is a long-gap holding-period return rather than a conventional one-day innovation. The 68 valid post-reopening observations end on 1 September 2026.

The episode permits three related descriptive exercises: measuring the direction and timing of observed repricing, comparing return variability across the two affected indices, and testing pre-event forecast relationships during the reopening period. The intervention is not isolated from the geopolitical shock or administrative controls. No untreated market or other identifying variation establishes the counterfactual path under continued trading.

3. Literature Review and Contribution

3-1. Geopolitical risk and financial-market responses

Caldara and Iacoviello (2022) provide a news-based framework for measuring geopolitical risk (GPR) and relate geopolitical threats and adverse events to macroeconomic and financial risks. Subsequent research examines equity returns, volatility and liquidity. Emerging markets are particularly relevant because exchange-rate exposure, commodity dependence and limited financial buffers can shape their responses to geopolitical shocks. A first strand studies the aggregate volatility effect of geopolitical risk. Bouras *et al*. (2019), Yang *et al*. (2021), Salisu *et al*. (2022), and Zhang *et al*. (2023) show that GPR is associated with higher stock-market volatility and has forecasting content in GARCH-MIDAS and related frameworks. Ndako *et al*. (2021) reach similar conclusions in Islamic equity markets. Jaffar *et al*. (2026) extend the evidence to South African industries and show that the volatility effect of geopolitical risk is heterogeneous across sectors. Chatziantoniou *et al*. (2025) document that U.S. sectoral volatility responds differently to distinct categories of geopolitical risk. These studies establish that geopolitical risk matters for volatility, but they generally study markets that remain continuously open.

A second strand focuses on returns and cross-sectional heterogeneity. Zaremba *et al*. (2022) show that geopolitical news can have non-uniform effects across emerging-market equities. Fiorillo *et al*. (2023) and Liu *et al*. (2024) emphasize liquidity and stock-liquidity channels. Yilmazkuday (2024) examines stock-price responses to geopolitical risk, while Ginn and Saadaoui (2025) use local projections to examine equity-return responses across economies. SenGupta and Debnath (2026) provide detailed evidence for India and highlight heterogeneous resilience across sectors and factor-style indices. Huynh and Khoa (2026) provide another emerging-market perspective by studying Vietnam. This literature motivates our distinction between the broad market and the equal-weight index.

A third strand examines military conflicts. Studies of the Russia–Ukraine war document heterogeneous return and volatility responses across countries and markets (Ahmed *et al*., 2023; Assaf *et al*., 2023; Kamal *et al*., 2023; Oyadeyi *et al*., 2024). Altemur *et al*. (2024) examine stock-market responses to the Hamas–Israel conflict. Lakkol and Rao (2026) study resilience to geopolitical shocks and emphasize variation across episodes. The present paper extends this discussion to a market in which trading was suspended during the shock itself.

3-2. Market closures, trading halts, and price discovery

The second relevant literature examines how no-trading intervals affect price formation. Hong and Wang (2000) provide a theoretical foundation by showing that periodic market closures alter return and trading patterns. Corwin and Lipson (2000) show that order flow during trading halts is informative for reopening prices. Christie *et al*. (2002) demonstrate that halt and reopening mechanisms affect prices, trading activity, and execution costs. Hautsch and Horvath (2019) show that trading pauses can assist price discovery but may also create adverse effects for liquidity and post-pause volatility.

These studies make the institutional design of a trading interruption central to its interpretation. Reopening prices and order flow can reveal information accumulated during a halt, while changes in trading mechanisms can affect liquidity and execution costs (Corwin and Lipson, 2000; Christie *et al*., 2002). Evidence from shorter trading pauses also cautions against treating price adjustment and market quality as interchangeable outcomes (Hautsch and Horvath, 2019). A prolonged exchange-level suspension requires an additional distinction between observed post-reopening changes and the unobserved path under uninterrupted trading.

3-3. Volatility resilience and model-based counterfactuals

Resilience has several distinct dimensions: the persistence of return disturbances, the time required for variability to return to a specified baseline, and the restoration of trading conditions. Evidence on heterogeneous market responses to geopolitical shocks motivates examining these dimensions separately (Zaremba *et al*., 2022; Lakkol & Rao, 2026). With daily aggregate index data, this study measures return variability and operational normalization times. It does not directly measure liquidity recovery or a return to fundamental value.

Forecasting models can supply conditional benchmarks for observed post-event paths. GARCH models impose a structured variance recursion, while flexible methods can

capture additional patterns. In a short crisis sample, however, searching many specifications on the evaluation period can create optimistic rankings. This study therefore separates pre-event validation from post-event evaluation and restricts its principal forecasting comparison to transparent candidates. It does not claim that the forecast-implied path is an identified no-closure path.

3-4. Positioning relative to recent studies

Table 1. Positioning of the study within the recent and foundational literature. The study documents a prolonged exchange-level interruption and distinguishes observed repricing, historical rarity and definition-dependent return variability from causal price-discovery identification.

Representative studies	Literature strand and method	Relevance for the present paper
Caldara and Iacoviello (2022)	News-based geopolitical risk measurement	Geopolitical risk raises downside macro-financial risk and provides the conceptual basis for treating war-related events as priced uncertainty.
Salisu *et al*. (2022)	GARCH-MIDAS; emerging markets	Geopolitical risk improves stock-volatility forecasting in emerging markets, motivating volatility-based event assessment.
Zaremba *et al*. (2022)	Cross-section of emerging-market returns	Equity responses to geopolitical news are heterogeneous rather than uniformly negative.
Zhang *et al*. (2023)	Global GPR and stock volatility	Geopolitical volatility effects vary across countries and market states.
Kamal *et al*. (2023); Ahmed *et al*. (2023); Assaf *et al*. (2023)	Russia-Ukraine war event studies	Recent military conflicts generated heterogeneous return and volatility responses across global and regional markets.
Fiorillo *et al*. (2023); Liu *et al*. (2024)	Geopolitical risk and liquidity	Geopolitical uncertainty affects the liquidity channel, not only returns and volatility.
Yilmazkuday (2024); Ginn and Saadaoui (2025)	Global and local-projection evidence	Geopolitical risk can depress stock prices, but dynamic responses differ across economies.
Chatziantoniou *et al*. (2025); Jaffar *et al*. (2026)	Sectoral GPR and industry volatility	Risk transmission is strongly sector-specific; defensive segments may display resilience.
SenGupta and Debnath (2026); Huynh and Khoa (2026)	Emerging-market stock responses	Single-country emerging markets show rich heterogeneity in sensitivity to global and country-specific geopolitical risk.
Lakkol and Rao (2026)	Multi-event resilience event study	Recent markets show selective resilience and longer assimilation of geopolitical events after the pandemic period.
Hong and Wang (2000); Christie *et al*. (2002); Corwin and Lipson (2000)	Market closures and trading halts	No-trading intervals shift the timing of price discovery and affect reopening volatility and order flow.
Hautsch and Horvath (2019)	Trading pauses and market quality	Trading interruptions can stabilize information processing but may also intensify post-reopening volatility.

3-5. Research gap and innovations

Despite the rapid growth of research on geopolitical risk and financial markets, three gaps remain. First, most studies analyze continuously traded markets. They therefore estimate how prices react to geopolitical risk when trading is available, not how prices react after a long no-trading interval. Second, most market-closure studies examine short trading halts, circuit breakers, trading-floor closures, or regular overnight closures. They rarely study a prolonged national stock-market suspension during war. Third, the resilience literature often focuses on aggregate market recovery, while less attention is paid to differences between headline and equal-weight market behavior.

The paper addresses these gaps by documenting the suspension chronology, applying a common cleaning rule to two differently weighted market measures, and comparing event outcomes with historical and model-based benchmarks. Sensitivity analysis distinguishes return magnitude from centered dispersion and tests alternative normalization definitions. Model selection uses pre-event validation data. The contribution is a reproducible case study; the design does not estimate a structural information share, an optimal suspension duration or the causal effect of closure.

4. Data and Variables

4-1. Data sources and sample construction

The analysis draws on daily records from TSETMC, TGJU and Yahoo Finance for the extraction period from 1 January 2010 to 2 September 2026. Raw observations, including missing values, are aligned to a complete calendar; filled observations are not used to construct target returns. Because direct retrieval of TEDPIX was unsuccessful, tgju_bourse is treated as a TGJU broad-market proxy rather than an independently verified official total-return index.

Target returns are calculated from observed index levels without forward filling, after excluding all nonmissing target records dated from 28 February through 18 May 2026 inclusive, regardless of whether the reported index level changes. No target records dated 19 May or later are removed merely because their value repeats an earlier value. The underlying series are sourced from TSETMC, TGJU and Yahoo Finance. The four external predictors are log changes in USD/IRR, international gold and the S&P 500, together with the log VIX level. Domestic 18k gold provides an alternative valuation numeraire. Other source variables appear only in the descriptive inventory.

The available data contain date labels rather than intraday publication timestamps, vendor revision histories, target trade counts or index constituent histories. Provider labels

and field names are documented in the variable definitions below. Units and transformations of variables actually used are documented below. Unused fields, including the unusually scaled us10y series and WTI observations that can be nonpositive, are excluded from the predictors; no economic interpretation is based on transformations of these unused fields.

Target returns use successive observed target dates, with no target interpolation, backfill or synthetic closure zeros. The two target calendars coincide after reopening, but neither is forced onto a U.S. trading calendar. External transformations are first calculated between successive actually recorded source observations. Their most recent known transformation is carried forward for at most seven calendar days and delayed by two calendar days before being matched to a target observation. This conservative delay excludes same-date U.S. closing information from Iranian-session forecasts and handles different weekends without assuming synchronized closes.

Calendar dates are treated as provider-local session labels. Iran uses Asia/Tehran and U.S. sessions use America/New-York, with different intraday closing times. The two-day rule avoids relying on an undocumented same-day time-zone conversion. Missing predictor values are imputed using medians estimated only in each training window; regression medians use the pre-event baseline only. Scaling and Ridge estimation are also fitted within each training window. There is no backward fill or full-sample normalization. The source-date alignment requires predictor observations to precede target dates by at least two calendar days.

The date-based delay reduces look-ahead from same-day observations but does not establish historical vintage availability. Intraday release timestamps and subsequent vendor revisions are unavailable. Target-only benchmarks therefore provide comparisons that do not depend on external-data timing, while results using external predictors are interpreted as conditional sensitivity checks.

Table 2. Variable definitions and transformations. Source fields are retained by name; the four predictors use a two-calendar-day availability delay and training-only preprocessing. Unused source variables are explicitly identified.

Variable	Definition	Role	Transformation
equal_weight_index	TSETMC equal-weight index in points	Outcome	Observed-session log return × 100
tgju_bourse	TGJU broad-market proxy in index points	Outcome	Observed-session log return × 100
usd_irr	TGJU quotation in IRR per USD	Predictor	Log change × 100; 2-calendar-day delay
coin_emami	TGJU raw level field; inventory only	Inventory only	Excluded from predictors

Variable	Definition	Role	Transformation
domestic_gold_18k	TGJU 18k gold quotation in IRR per provider unit	Alternative numeraire	Matched-date price ratio
domestic_gold_24k	TGJU raw level field; inventory only	Inventory only	Excluded from predictors
mesghal	TGJU raw level field; inventory only	Inventory only	Excluded from predictors
global_gold_tgju	TGJU raw level field; inventory only	Inventory only	Excluded from predictors
oil_opec_tgju	TGJU raw level field; inventory only	Inventory only	Excluded from predictors
gold_global	Yahoo international gold price field in provider units	Predictor	Log change × 100; 2-calendar-day delay
silver_global	Yahoo Finance raw level field; inventory only	Inventory only	Excluded from predictors
oil_wti	Yahoo Finance raw level field; inventory only	Inventory only	Excluded from predictors
oil_brent	Yahoo Finance raw level field; inventory only	Inventory only	Excluded from predictors
sp500	Yahoo S&P 500 index in points	Predictor	Log change × 100; 2-calendar-day delay
vix	Yahoo volatility index in points	Predictor	Log level; 2-calendar-day delay
dxy	Yahoo Finance raw level field; inventory only	Inventory only	Excluded from predictors
us10y	Yahoo Finance raw level field; inventory only	Inventory only	Excluded from predictors
Closure interval	28 February–18 May 2026 inclusive	Date exclusion	80 calendar days
Return magnitude	$\lvert r_t \rvert$ in percentage points	Outcome proxy	Mean and baseline percentile shares
Squared return	r^2_t in squared percentage points	Second-moment proxy	RMS and QLIKE evaluation

4-2. Descriptive statistics of level variables

Table 3 summarizes the raw level-variable inventory on a complete calendar from 1 January 2010 to 2 September 2026. Missing rates include weekends, holidays and periods before a series begins. They are not conditional trading-day data-quality rates. The U.S. yield field retains its provider scale in the inventory but is excluded from estimation; nonpositive WTI values also preclude an automatic log transformation.

Table 3. Descriptive statistics of raw level variables. N counts nonmissing raw entries, while missing rates use the complete calendar. Provider units are retained; the U.S. yield field is not interpreted or used as a predictor.

Variable	N	Mean	SD	Median	Min.	Max.	Missing
equal_weight_index	2996	344776.83	364885.77	322999.50	9099.30	1.83e+06	0.508
tgju_bourse	2752	1.05e+06	1.19e+06	376331.00	61163.70	6.58e+06	0.548
usd_irr	3940	314277.37	418083.35	131905.00	13350.00	2.14e+06	0.353
coin_emami	3473	2.61e+08	4.52e+08	9.70e+07	8.40e+06	2.18e+09	0.430
domestic_gold_18k	3492	2.53e+07	4.53e+07	9.34e+06	860000.00	2.22e+08	0.427
domestic_gold_24k	3351	3.52e+07	6.12e+07	1.39e+07	1.19e+06	2.96e+08	0.450
mesghal	3494	1.10e+08	1.96e+08	4.05e+07	3.73e+06	9.62e+08	0.426
global_gold_tgju	4362	1795.38	835.33	1562.14	1051.74	5400.25	0.284
oil_opec_tgju	2572	67.62	20.66	67.58	12.22	145.24	0.578
gold_global	4191	1780.55	811.51	1556.70	1050.80	5318.40	0.312
silver_global	4191	25.19	12.84	21.85	11.73	115.08	0.312
oil_wti	4192	71.98	20.68	71.94	-37.63	123.70	0.312
oil_brent	4161	77.87	23.27	75.68	19.33	127.98	0.317
sp500	4191	3114.21	1682.28	2672.63	1022.58	7798.99	0.312
vix	4193	18.42	6.77	16.73	9.14	82.69	0.311
dxy	4193	92.99	9.27	95.54	72.93	114.11	0.311
us10y	4189	0.27	0.10	0.25	0.05	0.50	0.312

5. Empirical Strategy

5-1. Event definition and target cleaning

All observed index levels dated within the documented suspension, 28 February–18 May 2026 inclusive, are excluded before target returns are calculated. The equal-weight source contains 50 such records: 42 dated 28 February–6 May and eight dated 9–13 May (five) and 16–18 May (three). Thus, 42 + 8 = 50 excluded equal-weight observations. All 50 repeat the last pre-event level of 952,681.5 within a tolerance of 10^{-8} index points. The additional eight records predate the actual reopening on 19 May; none is an excluded post-reopening observation. The broad-market proxy contains two closure-dated records, one equal to its last pre-event level and one different. Both are excluded by their dates, regardless of their values.

The target-cleaning procedure is institutional and date-based. Observed levels dated before 28 February 2026 or on or after 19 May 2026 are eligible; levels dated from 28 February through 18 May are excluded. The final observed pre-event level is dated 25 February for both indices. No separate filter removes flat-valued observations after reopening. Constant prices outside the documented suspension cannot, on their own, demonstrate an absence of trading. Matched intraday transactions and vendor revision histories are unavailable, so the executability of each quoted level cannot be independently verified.

The date audit yields 2,996 raw equal-weight levels minus 50 closure-dated records, leaving 2,946 retained levels (2,878 pre-event levels and 68 reopened levels). The broad-market proxy contains 2,752 raw levels minus two closure-dated records, leaving 2,750

retained levels (2,682 pre-event and 68 reopened). The two samples therefore yield 2,877 and 2,681 pre-event adjacent-session returns, respectively, and 68 reopened returns for each index, including the first return spanning the no-trading interval. The exclusion counts by index and period are summarized in Table 4. No synthetic closure zero returns, interpolated target prices or post-reopening stale-value deletions are introduced.

5-2. Return, volatility, and event-window measures

For each target index, daily log returns are calculated as:

$$r_t = 100[ln\, P_t - ln\, P_{t-1}]$$

Here P_t is the raw index level on the current observed target session and P_{t-1} is the preceding valid target level. Absolute return is a daily return-magnitude proxy:

$$RV_t = |r_t|$$

The squared return supplies a second-moment proxy:

$$RV_t^{(2)} = r_t^2$$

For a post-reopening horizon H, cumulative nominal return is:

$$\mathrm{CR(H)} = 100\{\exp[\frac{1}{100}\sum_{h=1}^{H} r_h] - 1\}$$

The cumulative abnormal log return is defined as the sum of deviations from the selected forecast benchmark:

$$CAR_{log}(\mathrm{H}) = \sum_{h=1}^{H}(r_h - \hat{r}_h)$$

Return-magnitude resilience compares the average absolute return over H reopened sessions with its mean over the final 252 pre-event sessions. In the following expression, h = 1 denotes the first reopened return, and s = 0 denotes the last pre-event return:

$$VR_H = \frac{\frac{1}{H}\sum_{h=1}^{H}|r_h|}{\frac{1}{252}\sum_{s=-251}^{0}|r_s|}$$

Absolute-return ratios compare the event mean of |r| with its mean over the final 252 pre-event sessions. Root-mean-square (RMS) ratios use √mean(r^2) for both periods, whereas centered standard deviations remove each window's sample mean. High- and extreme-magnitude shares use the 75th and 90th percentiles of baseline daily absolute returns. Absolute returns and RMS can increase during a sustained positive drift even if centered dispersion falls. These measures are observable proxies rather than direct observations of latent volatility.

The primary normalization criterion compares the post-event 10-session rolling mean of |r| with the 75th percentile of the corresponding pre-event rolling-mean distribution. Only complete windows are used. Normalization is confirmed after five consecutive rolling observations are at or below the threshold. The onset is the first observation in that run, and confirmation occurs four sessions later. If no qualifying run occurs, normalization is right-censored at session 68. This operational definition describes return-magnitude normalization rather than recovery of fundamental value.

Sensitivity grids vary the pre-event baseline over 126, 252 and 504 sessions, rolling length over 5, 10 and 20 sessions, percentile over 75 and 90, and the metric over absolute-return means, mean squared returns and centered rolling variance. The last two have the same crossing dates as RMS and rolling standard deviation under their corresponding transformed thresholds. A permissive sensitivity criterion is also evaluated: a 10-session absolute-return average with as few as five observations is compared with the percentile of individual daily absolute returns, and at least 70% of the next up-to-ten rolling values must comply. That rule uses future observations and an unmatched reference distribution.

5-3. Placebo windows

Historical pseudo-starts begin after 252 pre-event return observations. Each has a complete subsequent 60-session window ending before the actual event, and horizons 5, 20 and 60 share the same start dates. A dense design advances starts by five sessions, producing 474 broad-market and 514 equal-weight reference windows. Event windows overlap in that design. A second design advances by sixty sessions, producing 40 and 43 nonoverlapping event windows; their baseline windows can still overlap. Starts are deterministic, not selected for favorable outcomes.

For each horizon, the outcomes are cumulative return, the absolute-return ratio and the high-magnitude share. The upper-tail reference probability is (1 + number of reference statistics at least as large as the event statistic)/(1 + number of reference windows). This finite-sample convention prevents zero probabilities. The Holm (1979) adjustment covers 18 comparisons: two indices × three horizons × three statistics, separately for each reference design. The resulting probabilities describe historical rarity; they are not exact randomization-test p-values because the event date was not randomly assigned and market conditions may change over time.

A separate circular moving-block exercise draws 1,999 histories of length 312 from each complete pre-event return sequence using contiguous blocks of 5, 10 or 20 sessions. The first 252 simulated observations form a local baseline and the last 60 an event path.

Each horizon and statistic is recalculated within the simulated history, and the same finite-sample and Holm conventions are applied. Blocks retain short-run temporal dependence; they do not reproduce an 80-day closure or guarantee stationarity. Contrasts across block lengths are reported rather than selecting a preferred significance result. Seed 20261017 fixes all simulations.

5-4. Counterfactual forecasting

The forecast exercise uses six fixed candidates: the final-252-session historical mean, the expanding historical mean, a recursive AR(1), and standardized Ridge regressions with penalty 1, 10 or 100 using the four delayed external predictors. Three consecutive 63-session validation blocks end before the shock. Each model is fitted only on observations preceding its validation block. The selected candidate minimizes pooled validation squared error across the 189 validation observations. It is then refitted through 25 February 2026. All candidates, including those not selected, remain visible in the sensitivity results.

Target-only forecasts are fixed-origin sequences indexed by observed post-reopening sessions. No daily target returns are imputed for the closure, and these forecasts do not construct a traded price path inside it. The reopening bridge return is compared with a session-return benchmark, so its horizon mismatch is explicitly acknowledged; exclusion of that first observation is checked. Ridge paths condition on delayed external realizations arriving after the shock. Their coefficients are estimated pre-event, but their predictor paths are not wholly known at the pre-event origin. They are conditional benchmark paths, not unconditional no-war or no-closure counterfactuals.

The cumulative benchmark deviation is $\Sigma(r - \hat{r})$, in log percentage points. The associated relative-price gap is $100\{\exp[\Sigma(r - \hat{r})/100] - 1\}$. This ratio measure is distinguished from the arithmetic difference between two cumulative percentages. Forecast prediction bands add cumulative, mean-centered, out-of-sample validation residuals resampled in circular blocks of 5, 10 and 20 sessions to the forecast sum. The 2.5th and 97.5th percentiles give 95% conditional predictive bands. They capture validation-error dependence, but not full parameter-selection uncertainty, changing regimes or a causal policy counterfactual; 189 validation errors limit their precision.

5-5. Interrupted time-series regressions

The interrupted time-series model uses 252 pre-event sessions and 60 observed sessions after reopening. Three mutually exclusive indicators identify reopening sessions 1–5, 6–20

and 21–60. The pre-event period serves as the comparison baseline, with all three reopening indicators equal to zero. The specification is:

$$y_t = \alpha + \rho r_{t-1} + \gamma|r_{t-1}| + \tau t + \beta_1 D_{1-5,t} + \beta_2 D_{6-20,t} + \beta_3 D_{21-60,t} + \delta' X_t + u_t$$

The dependent variable y_t is the daily log return or its absolute value. X_t contains the four delayed external predictors, and t is an observed-session trend scaled by 252. Lags refer to successive observed target returns, so the first reopened return remains in the regression sample. Missing controls are imputed using pre-event medians. Ordinary least squares estimates use heteroskedasticity- and autocorrelation-consistent (HAC) standard errors following Newey and West (1987), with a Bartlett kernel, maximum lag 5 and finite-sample correction n/(n − k).

Sensitivity specifications use HAC maximum lags 1, 10 and 20; an unadjusted mean-shift model; a trend without external controls; a calendar-day trend; an additional post-event slope; exclusion of the first reopening return; and squared return as the outcome. The post-slope variant can weaken significance. All estimates describe conditional associations under the combined event, not a separately identified effect of closure.

5-6. Volatility model comparison

To avoid reselecting among numerous models on 68 event observations, the comparison restricts candidates to a constant pre-event variance, EWMA with decay 0.94 or 0.97, and Gaussian quasi-maximum-likelihood GARCH(1,1). Candidate selection uses pooled QLIKE over the same three pre-event validation blocks. GARCH parameters and the return mean are refitted using all pre-event returns and then frozen. One-step filters update only with preceding observed post-event returns. The first reopening forecast uses the pre-event state; no zero returns are inserted during suspension. This is a session-clock approximation, whose first forecast does not model 80 calendar days of latent risk accumulation.

$$\text{QLIKE} = \frac{1}{T}\sum_{t=1}^{T}[\ln(h_t) + \frac{r_t^2}{h_t}]$$

The evaluation loss is QLIKE = mean[$\ln(h_t) + r^2_t/h_t$], where h_t is the one-step conditional variance forecast. The return-squared proxy is noisy and has a mean component; it is not high-frequency realized variance (Patton, 2011). GARCH dynamics are:

$$h_t = \omega + a\varepsilon_{t-1}^2 + bh_{t-1}$$

The GARCH parameters obey $\omega > 0$, $a \geq 0$, $b \geq 0$ and $a + b \leq 0.999$. Innovations are demeaned using the pre-event mean. EWMA candidates have fixed decay 0.94 or 0.97. All parameters are frozen after pre-event estimation; states update using only preceding

observed returns. Paired moving-block intervals for loss differences versus constant variance use 1,999 draws with block lengths 5, 10 and 20. They remain exploratory in a 68-observation event sample.

The forecasting exercise is retrospective in its research design: the restricted candidate set was defined after the episode. Keeping estimation and validation entirely within pre-event data reduces evaluation-sample leakage but does not make the study prospectively registered. Model rankings are interpreted as exploratory forecast comparisons.

6. Empirical Analysis and Discussion

The results follow the empirical sequence in Section 5: sample construction, return diagnostics, event windows, historical comparisons, normalization, forecast benchmarks, interrupted time-series estimates, volatility-model comparisons and predictor availability. The analysis distinguishes the 80-calendar-day suspension from counts of observed trading sessions.

The analysis asks whether observed nominal returns are large relative to pre-event conditions, whether historical reference windows and dependence-preserving resampling agree, how variability differs across the two affected indices, and how normalization changes with its definition. None of these questions identifies the separate effect of suspension. Forecast comparisons supplement the event evidence rather than establish a no-closure counterfactual.

The daily return of each target index is computed as:

$$r_t = 100[ln\, P_t - ln\, P_{t-1}]$$

P_t denotes a valid observed target level. The absolute return is a return-magnitude proxy:

$$RV_t = |r_t|$$

For an H-session reopening window, cumulative nominal return is:

$$\mathrm{CR(H)} = 100\{\exp[\frac{1}{100}\sum_{h=1}^{H} r_h] - 1\}$$

The abnormal log-return sum is measured relative to a forecast-implied benchmark:

$$CAR_{log}(\mathrm{H}) = \sum_{h=1}^{H}(r_h - \hat{r}_h)$$

The event absolute-return ratio compares matching event and baseline statistics, using the event-time indexing defined in Section 5.2:

$$VR_H = \frac{\frac{1}{H}\sum_{h=1}^{H}|r_h|}{\frac{1}{252}\sum_{s=-251}^{0}|r_s|}$$

6-1. Event Definition, Data Cleaning, and Data Sufficiency

Table 4 reports the institutional chronology and reconciles the complete raw and retained index-level counts. For the equal-weight index, 42 closure-dated records occur through 6 May and another eight occur on 9–13 and 16–18 May. All 50 are within the documented 28 February–18 May suspension, before the 19 May reopening, and are excluded once. The broad-market proxy has two suspension-dated records, both excluded under the same date rule.

Table 4. Institutional suspension dates and transparent reconciliation of raw and retained index levels. The 42 + 8 decomposition concerns closure-dated equal-weight records, not post-reopening deletions.

Item	Broad market index	Equal-weight index
Documented suspension	28 February–18 May 2026	28 February–18 May 2026
Last pre-event level	25 February 2026	25 February 2026
First reopened session	19 May 2026	19 May 2026
Calendar closure duration	80 days	80 days
Valid target levels	2750	2946
Pre-event returns	2681	2877
Post-event returns	68	68
Raw target-level entries	2,752	2,996
Excluded 28 February–6 May	2	42
Excluded 9–13 and 16–18 May	0	8
Excluded on or after reopening (19 May)	0	0
Excluded suspension entries	2	50
External predictors	4	4

The arithmetic is consistent across the two indices. The equal-weight sample contains $2{,}996 - 42 - 8 = 2{,}946$ retained levels; the broad-market sample contains $2{,}752 - 2 = 2{,}750$ retained levels. Their valid pre-event return counts are 2,877 and 2,681, respectively, with 68 post-reopening returns for each index. The first reopening return bridges the trading suspension. The unchanged equal-weight readings during closure corroborate their status as carried-forward values, but exclusion is determined by the institutional dates rather than the absence of a level change.

6-2. Return and Tail-Risk Diagnostics

Tables 5a and 5b summarize the distributions and time-series properties of the retained observed-session returns. Both targets exhibit non-normality and serial dependence. These features motivate inference that allows for temporal clustering and sensitivity to resampling block length. The diagnostics characterize the series; they do not establish causal identification or validate a fundamental-recovery interpretation.

Table 5a. Distributional diagnostics. Returns and absolute returns are measured in percent. Jarque-Bera p-values reject normality for both targets and volatility proxies.

Target	Series	N	Mean	SD	Skewness	Excess kurtosis	JB p
Broad market index	Return	2749	0.161	1.250	0.279	5.395	<0.001
Broad market index	Absolute return	2749	0.818	0.959	2.256	9.369	<0.001
Equal-weight index	Return	2945	0.177	1.062	0.126	1.706	<0.001
Equal-weight index	Absolute return	2945	0.760	0.761	1.573	2.467	<0.001

Table 5b. Time-series diagnostics for the retained return series. ADF, Ljung–Box and ARCH-LM results refer to the same valid observed-session sample. Ljung–Box and ARCH-LM use lag 10; very small probabilities are shown as p < 0.001. These diagnostics do not identify a closure effect.

Target	Series	ADF p	Ljung–Box p	ARCH LM	ARCH p	Result
Broad market index	Return	<0.001	<0.001	200.25	<0.001	Serial dependence and ARCH
Broad market index	Absolute return	<0.001	<0.001	200.25	<0.001	Serial dependence and ARCH
Equal-weight index	Return	<0.001	<0.001	664.30	<0.001	Serial dependence and ARCH
Equal-weight index	Absolute return	<0.001	<0.001	664.30	<0.001	Serial dependence and ARCH

Figures 1 and 2 display the observed return and absolute-return paths around the suspension. The closure is a blank no-trading interval, not a synthetic string of zero returns. Positive return magnitude can reflect the reopening drift as well as centered dispersion.

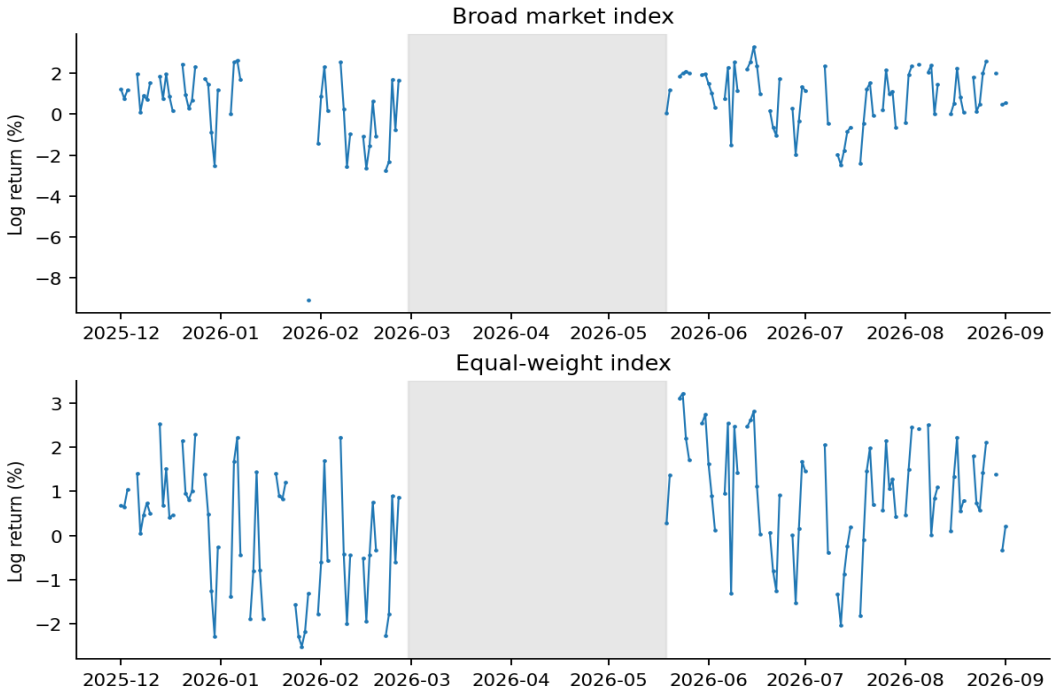

Fig. 1: Observed-session log returns around the documented suspension. Shading covers 28 February–18 May 2026; the return at 19 May bridges the preceding valid level.

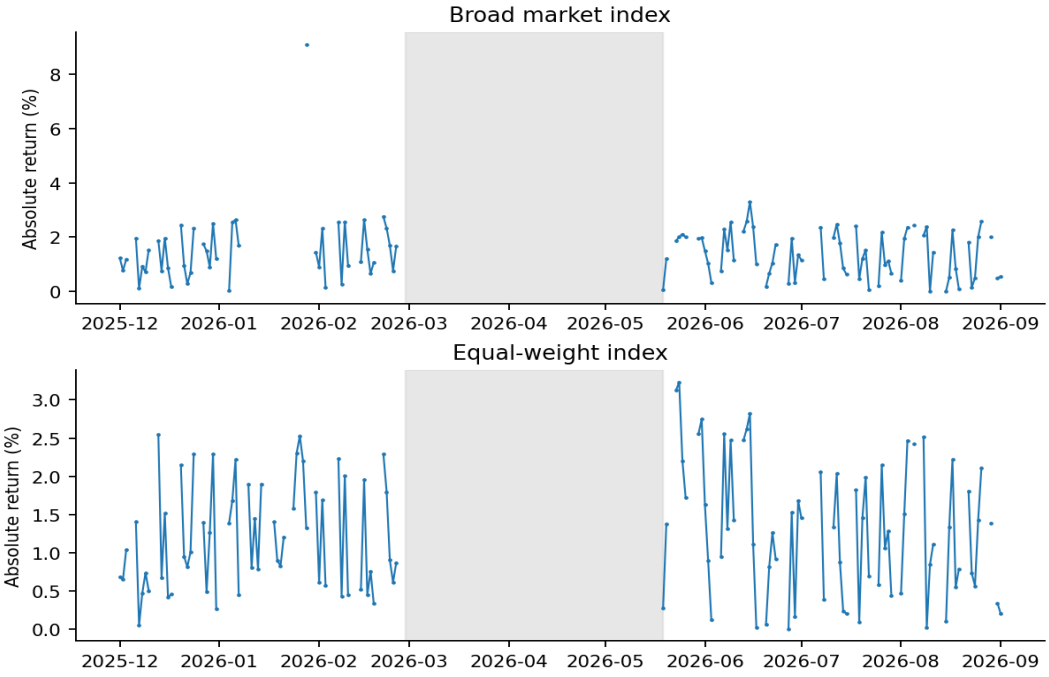

Fig. 2: Absolute log returns around the documented suspension. This daily magnitude proxy is distinct from centered volatility and high-frequency realized variance.

Figure 3 compares pre-event and reopened absolute-return distributions. Both are right-skewed, but the comparison is descriptive and affected by the short event sample. A different mean return can shift absolute magnitudes without an equivalent rise in centered dispersion.

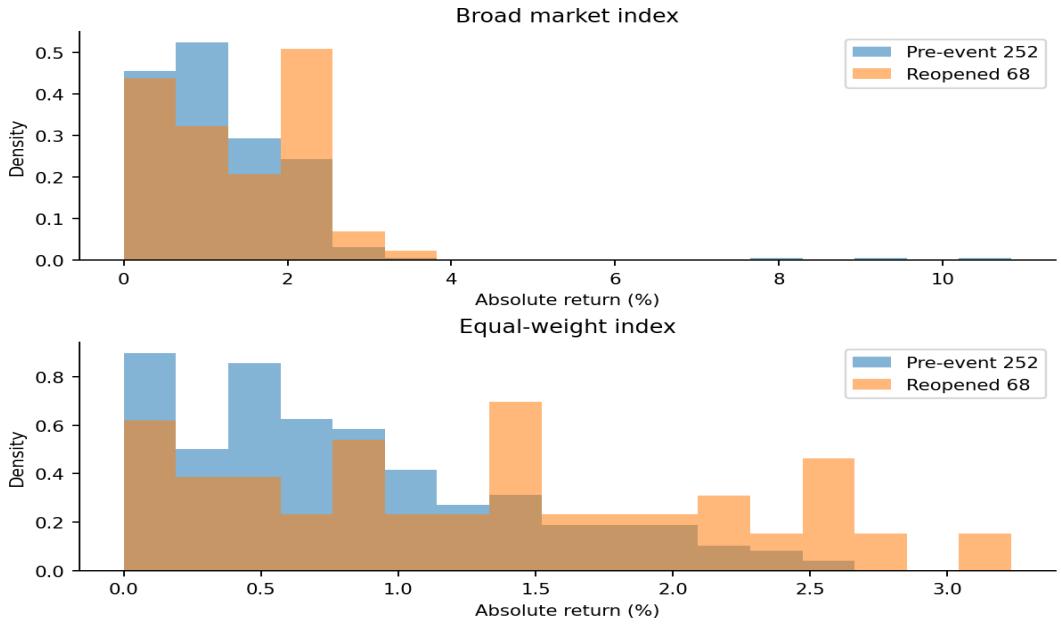

Fig. 3: Distribution of absolute returns in the final 252 pre-event sessions and the 68 observed reopening sessions, using identical bins for each index.

6-3. Event-Window Evidence: Post-Reopening Price Adjustment

Table 6. Event-window cumulative nominal returns, abnormal log-return sums and variability. CAR is Σ(r − r̂), in log percentage points, using the pre-event-validation-selected benchmark. High and extreme shares use daily baseline |r| percentiles 75 and 90.

Target	Window	CR %	CAR log pp	Mean |r|	Abs ratio	High %	Extreme %	Drawdown %
Broad market index	Pre 252	77.63	—	1.218	1.00	25.0	10.3	-26.34
Broad market index	Post 5	7.52	5.73	1.450	1.19	60.0	0.0	0.00
Broad market index	Post 20	37.33	28.03	1.738	1.43	60.0	30.0	-1.50
Broad market index	Post 60	60.28	37.71	1.377	1.13	43.3	21.7	-10.47
Equal-weight index	Pre 252	8.70	—	0.817	1.00	25.0	10.3	-22.88

Target	Window	CR %	CAR log pp	Mean |r|	Abs ratio	High %	Extreme %	Drawdown %
Equal-weight index	Post 5	10.76	10.05	2.043	2.50	80.0	60.0	0.00
Equal-weight index	Post 20	41.90	34.34	1.881	2.30	75.0	50.0	-1.30
Equal-weight index	Post 60	77.07	55.15	1.342	1.64	55.0	31.7	-6.38

Table 6 reports horizons of 5, 20 and 60 observed sessions and the 252-session local baseline. Cumulative nominal returns use exact exponential compounding of log returns. CAR denotes the sum of abnormal log returns in log percentage points, rather than an arithmetic simple-return product or an identified closure effect.

The broad-market cumulative nominal returns are 7.52%, 37.33% and 60.28% at 5, 20 and 60 sessions. Equal-weight returns are 10.76%, 41.90% and 77.07%. The twenty-session abnormal log-return sums against the validation-selected benchmarks are 28.03 and 34.34 log percentage points. These correspond to relative-price gaps of 32.35% and 40.97%, respectively. A relative-price gap, an abnormal log-return sum and an arithmetic difference of cumulative percentages are different quantities.

Inflation expectations, nominal asset demand, evolving risk premia, firm composition, disclosure news and managed order flow remain plausible explanations. A matched CPI series is not included in the analysis, so CPI-deflated investor recovery is not estimated. Higher index levels do not establish restored purchasing power, the ability to sell at quoted prices, or welfare improvement. Continued firm suspensions can affect both index representativeness and the set of investors able to trade.

Excluding the first reopened return while retaining the same twenty-session end date gives cumulative nominal returns of 37.23% and 41.51%. The rebound therefore extends across subsequent sessions rather than arising entirely from the return spanning the suspension. These are changes in domestic nominal index levels, without an independently verified dividend-reinvestment adjustment.

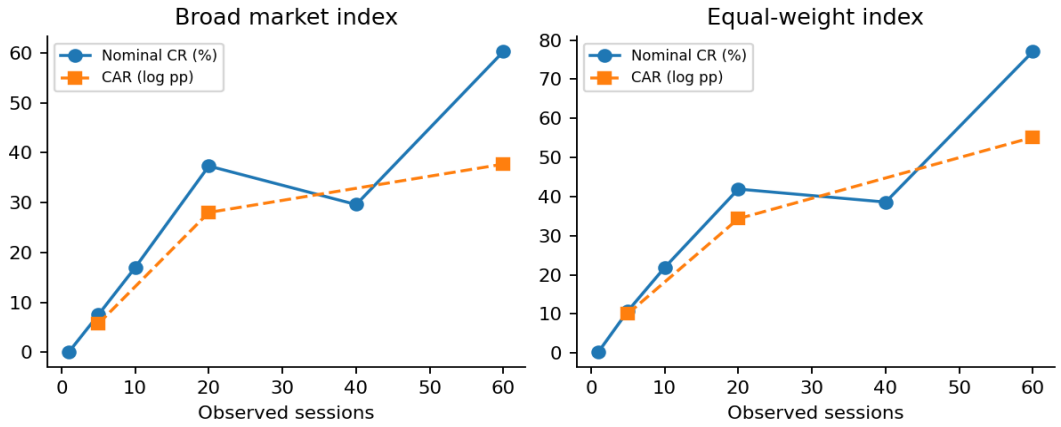

Fig. 4: Cumulative nominal returns and cumulative abnormal log-return sums over 1, 5, 10, 20, 40 and 60 observed sessions. The benchmark parameters are estimated before the shock.

6-4. Placebo Evidence

The historical comparison uses complete pre-event windows selected by a deterministic rule. Five-session spacing yields 474 broad-market and 514 equal-weight starting dates; sixty-session spacing yields 40 and 43 nonoverlapping event windows, although their baselines can still overlap. Table 7 reports cumulative returns, and Table 8 reports variability measures. The tail probabilities describe the reference distributions rather than randomized treatment effects.

Table 7. Historical reference comparison for cumulative nominal returns. Tail p = (exceedances + 1)/(windows + 1). Holm adjustment covers eighteen comparisons per design: two indices × three horizons × three outcomes.

Target	H	Actual %	Median %	p95 %	N windows	Tail p	Holm 18
Broad market index	5	7.52	0.37	7.38	474	0.0505	0.5558
Broad market index	20	37.33	1.88	21.54	474	0.0253	0.3789
Broad market index	60	60.28	4.37	60.29	474	0.0526	0.5558
Equal-weight index	5	10.76	0.51	7.72	514	0.0214	0.3631
Equal-weight index	20	41.90	1.49	26.15	514	0.0117	0.2097
Equal-weight index	60	77.07	5.25	69.18	514	0.0427	0.5301

The broad-market twenty-session gain exceeds 463 of 474 historical reference windows and the equal-weight gain exceeds 509 of 514. Their upper-tail probabilities are 0.0253 and 0.0117, but Holm-adjusted values are 0.3789 and 0.2097. Thus, the reference-window design does not establish family-wise significance at 5%. The nonoverlapping-event design

is even less precise, with only 40 and 43 windows. Its twenty-session probabilities are 0.0244 and 0.0455 before adjustment and 0.4390 and 0.7727 after it.

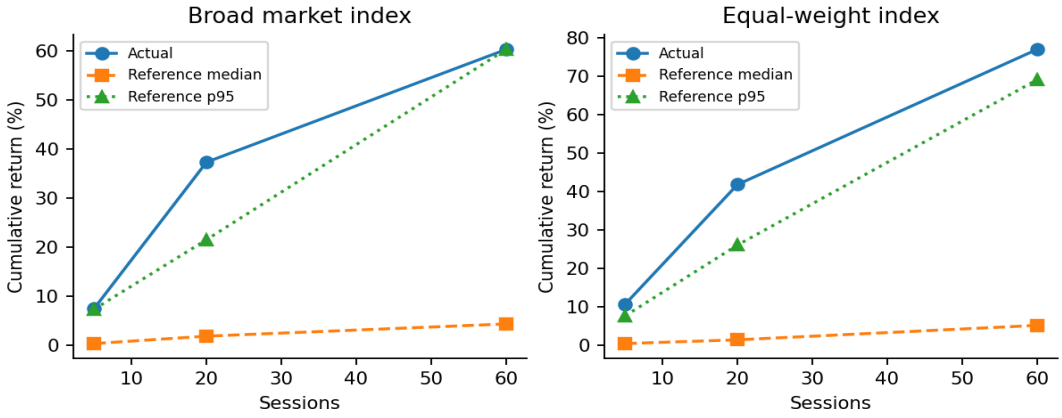

Fig. 5: Actual cumulative nominal returns and historical-reference medians and 95th percentiles. Visual extremity is distinguished from family-wise significance.

Table 8 reports the same reference calculations for absolute-return ratios and high-magnitude shares. The equal-weight magnitude response is larger relative to its baseline, but unadjusted tail probabilities must be read together with their multiplicity correction and dependence sensitivity.

Table 8: Historical reference probabilities for variability. Each cell reports unadjusted upper-tail p followed by Holm-adjusted p in parentheses; ratios use mean absolute returns and high shares use the daily baseline p75.

Target	H	Abs ratio	Ref median	p (Holm)	High %	Ref median %	p (Holm)
Broad market index	5	1.19	0.91	0.3179 (1.0000)	60.0	20.0	0.2000 (1.0000)
Broad market index	20	1.43	0.90	0.2168 (1.0000)	60.0	20.0	0.0905 (0.7242)
Broad market index	60	1.13	0.95	0.3747 (1.0000)	43.3	21.7	0.2232 (1.0000)
Equal-weight index	5	2.50	0.88	0.0408 (0.5301)	80.0	20.0	0.0932 (0.7242)
Equal-weight index	20	2.30	0.93	0.0311 (0.4350)	75.0	25.0	0.0233 (0.3728)
Equal-weight index	60	1.64	0.95	0.1107 (0.7242)	55.0	23.3	0.0602 (0.5558)

The block-resampling exercise gives smaller probabilities than the actual historical-window comparison. With blocks of ten sessions, adjusted twenty-session probabilities are 0.0425 and 0.0360. With blocks of twenty, they rise to 0.0540 and 0.0595. The historical-window design preserves realized long market episodes, whereas shorter resampled blocks can break sustained runs. The inference is therefore sensitive to the reference distribution.

The defensible conclusion is a large nominal rebound and substantial benchmark deviation, with uncertainty about the strength of historical-rarity inference after multiplicity and dependence adjustments.

6-5. Volatility Resilience

Table 9 summarizes return magnitude over sixty sessions and normalization over all 68 available sessions. Mean absolute returns are 1.377% for the broad-market proxy and 1.342% for the equal-weight index, compared with baseline medians of 1.007% and 0.704%. The ratios to baseline means are 1.13 and 1.64; the ratios to baseline medians are 1.37 and 1.91. Under the primary rolling-distribution criterion, broad-market normalization is confirmed at session 32, while equal-weight normalization is not confirmed within 68 sessions.

Table 9. Return magnitude and normalization. Magnitude summaries use sixty sessions; normalization uses all 68 sessions and complete rolling windows. Confirmation requires five consecutive 10-session rolling means at or below the 75th percentile of the pre-event rolling-mean distribution.

Target	H	Pre median	Post mean	Mean ratio	Median ratio	High %	p90 %	Confirm
Broad market index	60	1.007	1.377	1.13	1.37	43.3	21.7	32
Equal-weight index	60	0.704	1.342	1.64	1.91	55.0	31.7	Censored 68

The permissive sensitivity criterion yields five and twenty-seven sessions. Under the primary matched rolling criterion, broad-market compliance starts at session 28 and is confirmed at session 32. Equal-weight absolute-return normalization is not confirmed within 68 sessions. Under the rolling squared-return criterion at p75, confirmation occurs at sessions 33 and 32. Centered rolling variance gives 14 and 52. The ordering is therefore not invariant to the volatility definition.

Across the full sensitivity grid, some normalization definitions are right-censored and the remaining confirmation dates vary substantially. For example, centered-variance confirmation spans sessions 9–24 for the broad market and 9–62 for equal weight. A single recovery duration is not a structural market property. The evidence supports persistent equal-weight return-magnitude elevation under several definitions, while precise normalization times and cross-index rankings require qualification.

At sixty sessions, mean absolute-return ratios are 1.13 for the broad market and 1.64 for equal weight. Centered standard-deviation ratios are 0.85 and 1.28, respectively. The broad-market return magnitude therefore rises slightly while its centered dispersion falls relative to the local baseline. For equal weight, both magnitude and centered dispersion rise. This distinction weakens any general claim that volatility increased uniformly across market measures.

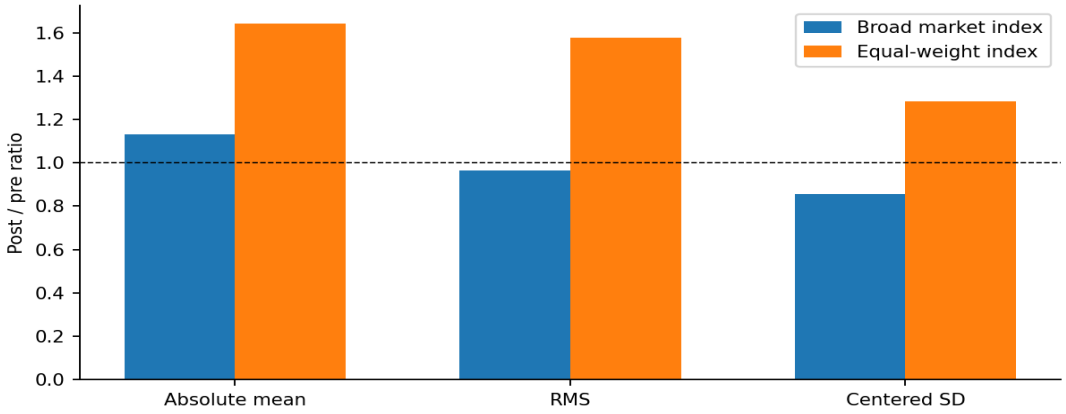

Fig. 6: Absolute-return, RMS and centered-standard-deviation ratios after sixty observed sessions. The three metrics need not imply the same volatility response.

6-6. Counterfactual Return and Volatility Paths

Pre-event validation selects AR(1) for the broad-market return and the 252-session historical mean for equal weight. Their twenty-session forecast returns are 3.76% and 0.66%; corresponding relative-price gaps are 32.35% and 40.97%. All six benchmarks predict much smaller nominal gains than observed. The twenty-session observations lie above their block-10 predictive bands. This is evidence of predictive instability around the combined event, not an estimated benefit or cost of market closure.

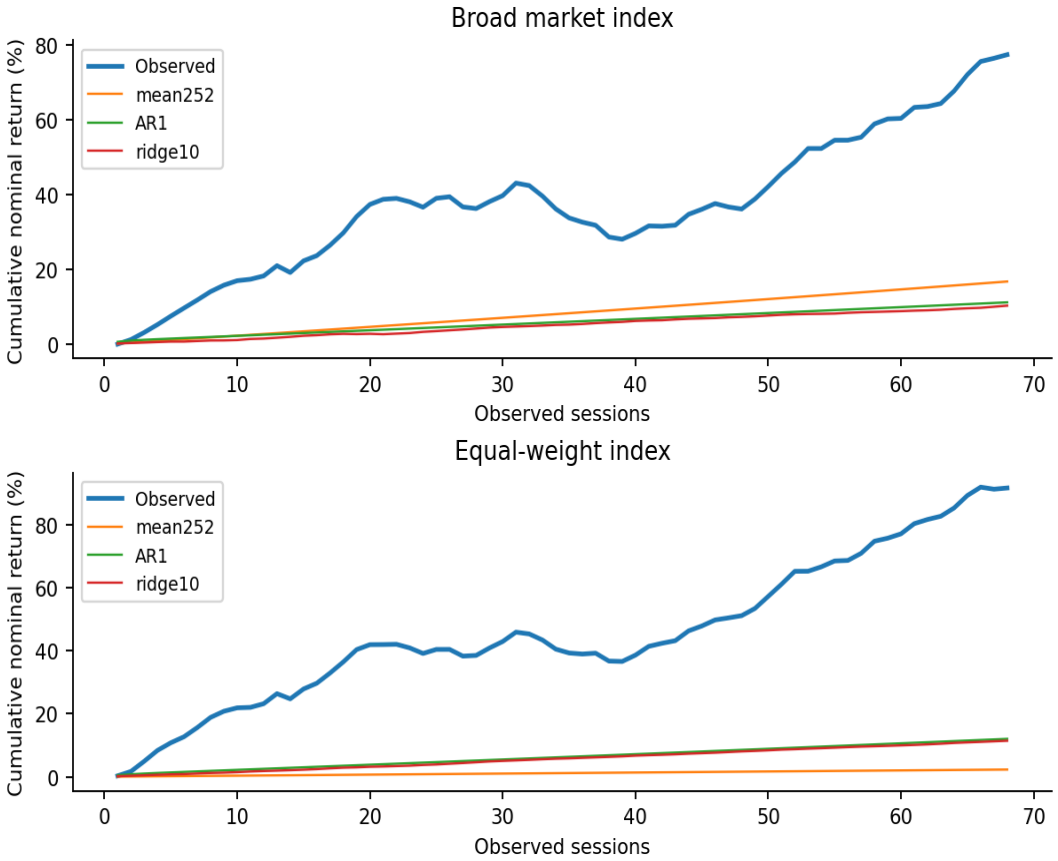

Fig. 7: Actual cumulative nominal return and alternative forecast benchmarks. Target-only paths are fixed-origin forecasts; Ridge uses post-event external realizations delayed by two calendar days.

Figure 8 compares absolute observed returns with square-root variance forecasts from the restricted pre-event-selected models. These are one-step session-clock forecasts, not a reconstruction of latent risk during the suspension. No closure zeros are inserted. A forecast mismatch indicates a challenge for predictive stability under the combined event; it does not identify why the closure occurred or how uninterrupted trading would have behaved.

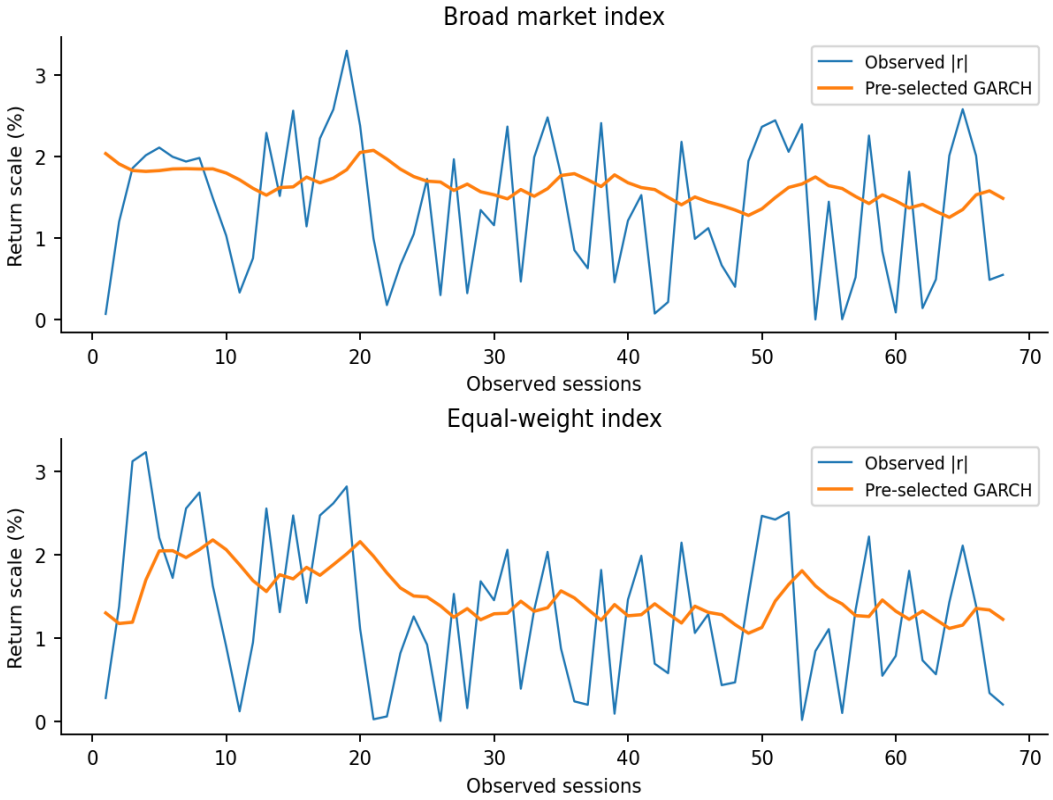

Fig. 8: Observed absolute returns and one-step pre-event-selected volatility forecasts. The first forecast uses the last pre-event state; subsequent forecasts use only previous observed returns.

6-7. Interrupted Time-Series Evidence

The interrupted time-series estimates use the specification in Section 5.5. Three indicators distinguish the reopening intervals, with lagged returns, a time trend and four delayed external predictors included as controls:

$$y_t = \alpha + \rho r_{t-1} + \gamma|r_{t-1}| + \tau t + \beta_1 D_{1-5,t} + \beta_2 D_{6-20,t} + \beta_3 D_{21-60,t} + \delta' X_t + u_t$$

The dependent variable y_t is r_t or $|r_t|$. The pre-event period serves as the comparison baseline. Each primary regression uses the same 312 observations, with lags defined over observed sessions and a HAC maximum lag of 5. The reopening coefficients measure conditional differences relative to the pre-event baseline; they do not isolate the causal effect of suspension. Sensitivity specifications vary controls, trends, HAC lags and inclusion of the first reopened return.

Table 10a. Interrupted time-series estimates for daily log returns. All regressions use 312 observations, lagged returns, a trend and four delayed controls. The pre-event period is the comparison baseline. Standard errors use a Bartlett HAC estimator with maximum lag 5. Coefficients are percentage points per observed session.

Target	Sessions	Coefficient	HAC SE	p	N	R^2
Broad market index	1–5	0.942	0.373	0.011	312	0.149
Broad market index	6–20	1.078	0.405	0.008	312	0.149
Broad market index	21–60	0.212	0.348	0.542	312	0.149
Equal-weight index	1–5	1.245	0.328	<0.001	312	0.316
Equal-weight index	6–20	0.851	0.266	0.001	312	0.316
Equal-weight index	21–60	0.179	0.256	0.485	312	0.316

Table 10b. Interrupted time-series estimates for absolute log returns, using the specification in Table 10a. Reported p-values are unadjusted and do not test a separately identified causal effect of closure.

Target	Sessions	Coefficient	HAC SE	p	N	R^2
Broad market index	1–5	0.214	0.349	0.540	312	0.037
Broad market index	6–20	0.524	0.345	0.129	312	0.037
Broad market index	21–60	-0.073	0.249	0.770	312	0.037
Equal-weight index	1–5	0.925	0.332	0.005	312	0.214
Equal-weight index	6–20	0.643	0.206	0.002	312	0.214
Equal-weight index	21–60	-0.031	0.139	0.821	312	0.214

In the primary controlled specification, the return coefficients for sessions 6–20 are 1.078 for the broad market and 0.851 for equal weight. The sessions 21–60 coefficients are small and not statistically distinguishable from zero. The primary absolute-return coefficients are not significant for the broad market; they are positive and significant for equal weight in the first two bins. The results indicate that the variability response differs between the two indices.

For sessions 6–20, changing HAC lag between 1 and 20 keeps the primary return p-values below 0.008 for the broad market and below 0.005 for equal weight. Adding a post-event slope reduces the coefficients to 0.715 and 0.550, and their maximum p-values across HAC lags rise to 0.083 and 0.101. Thus, the timing of the average rebound is visible, but its division into level and slope changes depends on specification. The sensitivity checks also consider unadjusted means, calendar-day trends, exclusion of the first reopening return and squared-return outcomes; these are not treated as independent confirmation of a preferred specification.

6-8. Post-Reopening Volatility Model Comparison

The restricted volatility comparison uses a constant pre-event variance, EWMA decays 0.94 and 0.97, and Gaussian GARCH(1,1). Three pre-event validation blocks select a

candidate; post-event performance is then reported once for all 68 observed sessions. The common loss is:

$$\text{QLIKE} = \frac{1}{T}\sum_{t=1}^{T}[\ln(h_t) + \frac{r_t^2}{h_t}]$$

Here h_t is the one-step forecast variance and r^2_t is the daily squared-return proxy. GARCH follows:

$$h_t = \omega + a\varepsilon_{t-1}^2 + bh_{t-1}$$

Parameters are estimated on pre-event demeaned returns and then held fixed. Filter states update only using preceding observed returns. Paired circular-block loss intervals versus constant variance allow dependence within blocks of 5, 10 and 20 sessions. The short evaluation sample and the retrospectively chosen candidate set limit any superiority claim.

GARCH is selected using pre-event validation data for both indices, but EWMA achieves slightly lower post-event QLIKE in both. For the broad market, GARCH minus constant-variance mean QLIKE is −0.122, with a paired block-10 interval from −0.299 to 0.028. For equal weight, the difference is −0.406, with interval −0.853 to −0.032. These exploratory differences do not establish dominance among all dynamic candidates, and 68 observations offer limited precision. The GARCH persistence constraints approach their upper boundary for both targets, reinforcing caution about regime stability. These comparisons do not support a general model-superiority claim or an operational trading recommendation.

Table 11: Volatility-model comparison with model selection based only on pre-event validation. Validation QLIKE and post-event QLIKE are separate quantities. Lower post-event QLIKE or RMSE alone is not a new model-selection rule.

Target	Candidate	Validation QLIKE	Post QLIKE	Post RMSE	Selection
Broad market index	constant	1.934	2.104	0.857	Fixed comparator
Broad market index	EWMA0.94	1.836	1.969	0.924	Fixed comparator
Broad market index	EWMA0.97	1.864	1.962	0.931	Fixed comparator
Broad market index	GARCH	1.798	1.983	0.916	Selected pre-event
Equal-weight index	constant	1.073	2.359	0.918	Fixed comparator
Equal-weight index	EWMA0.94	1.030	1.919	0.908	Fixed comparator
Equal-weight index	EWMA0.97	1.038	1.937	0.907	Fixed comparator
Equal-weight index	GARCH	1.025	1.953	0.913	Selected pre-event

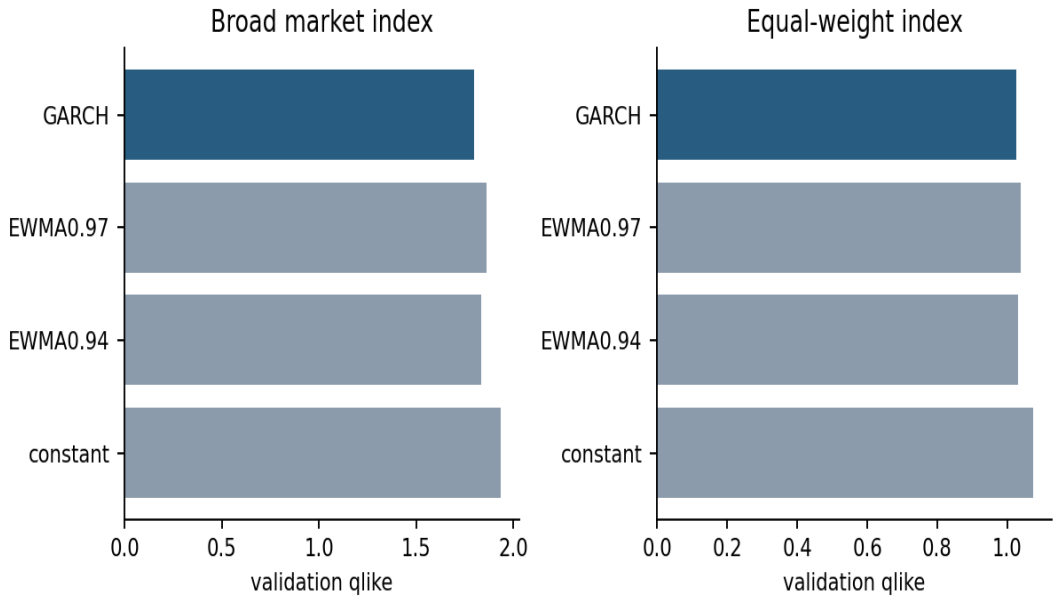

Fig. 9: Pre-event validation QLIKE for the fixed candidate set. Candidate selection uses validation loss; post-event QLIKE is reported separately in Figure 10.

Figures 9–11 distinguish validation QLIKE, post-event QLIKE and post-event RMSE. The criteria need not produce identical rankings. Selection is based exclusively on pre-event validation QLIKE, while the other criteria describe performance during the evaluation period.

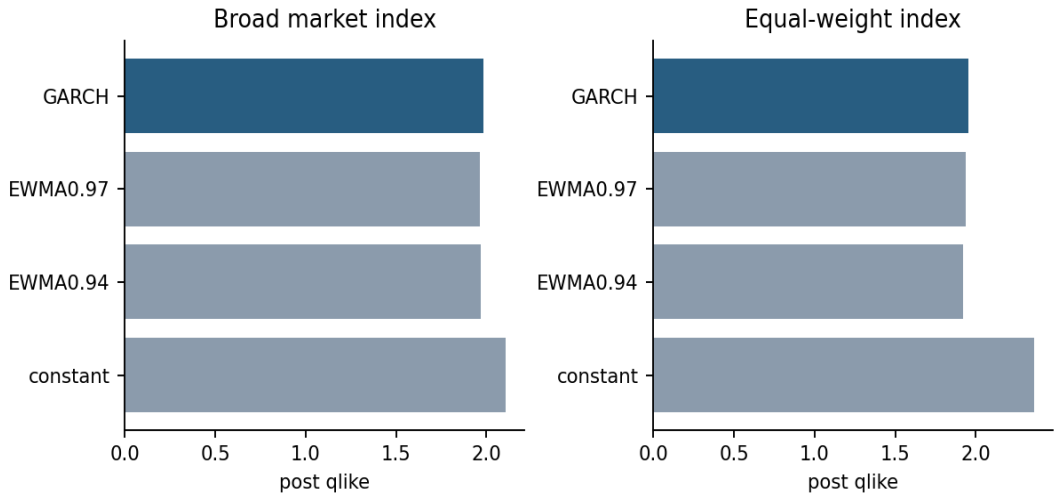

Fig. 10: Post-event QLIKE for the four fixed candidates. These evaluation-period rankings do not determine model selection, which uses pre-event validation data.

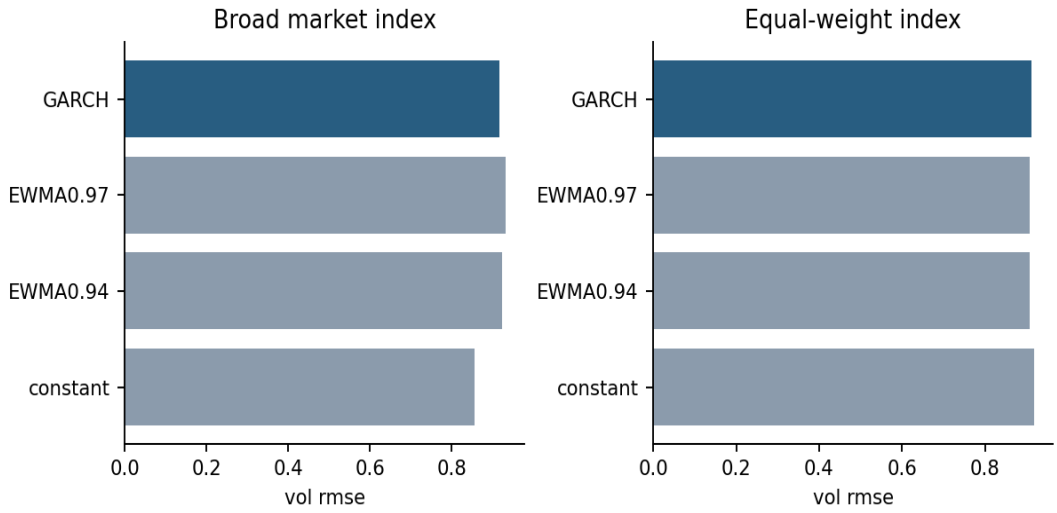

Fig. 11: Post-event RMSE between absolute returns and square-root variance forecasts for the same fixed candidates.

Table 12 summarizes selection by pooled pre-event validation QLIKE and reports paired post-event loss differences relative to constant variance. GARCH is selected for both indices. The loss intervals are exploratory comparisons rather than a basis for selecting another model on the event sample.

Table 12. Pre-event model selection and exploratory paired post-event loss comparison against constant variance. Confidence intervals use 1,999 paired circular-block draws of length 10.

Target	Criterion	Selected	Post QLIKE	Loss difference	95% paired interval
Broad market index	Validation QLIKE	GARCH	1.983	-0.122	-0.299 to 0.028
Equal-weight index	Validation QLIKE	GARCH	1.953	-0.406	-0.853 to -0.032

6-9. Feature Timing and Availability Evidence

The predictor audit assesses the timing of the four external series. A global market's closing observation is not assigned to an Iranian session that ends before that observation becomes available. Transformations are delayed by two calendar days, carried forward for a limited period and imputed using training data only.

Table 13 and Figure 12 report predictor availability on evaluation dates before imputation. Recorded source observations precede target dates by at least two calendar days. This confirms compliance with the date-based delay, while intraday release times and subsequent vendor revisions remain unverified. Predictor availability does not identify an economic mechanism or causal channel.

Table 13. Predictor timing and availability on the 68 reopened target dates, before imputation. Recorded source ages refer to daily observation dates, not independently audited release timestamps.

Predictor	Transformation	Available %	Minimum age	Maximum age
usd_irr	Log change × 100	100.0%	2	5
gold_global	Log change × 100	100.0%	2	5
sp500	Log change × 100	100.0%	2	5
vix	Log level	100.0%	2	5

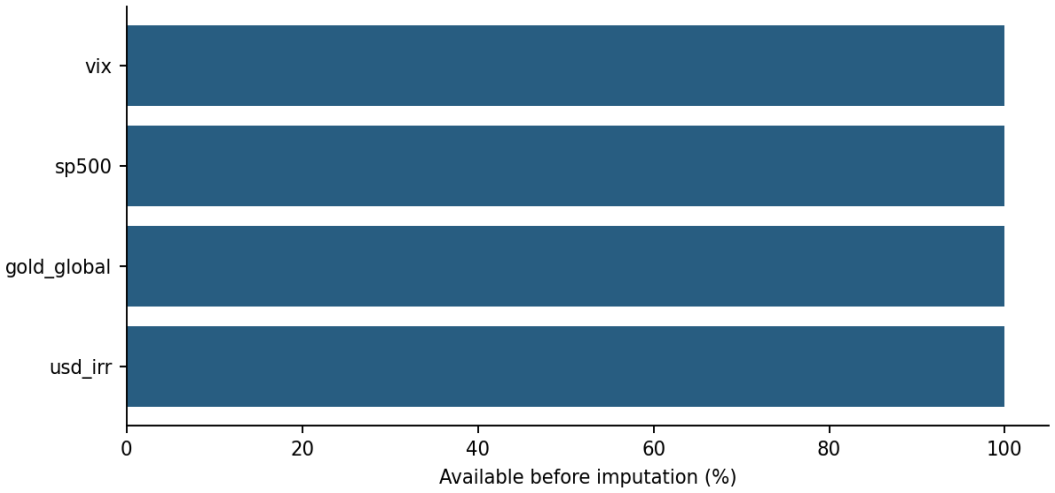

Fig. 12: Availability of the four delayed predictors before imputation.

6-10. Synthesis of Empirical Evidence

The main observed result is a substantial nominal rebound after reopening, concentrated in the first twenty sessions. The evidence is consistent with information accumulated during the interruption being incorporated after trading resumed, but does not isolate that mechanism from simultaneous geopolitical and administrative developments. Thus, the study does not conclude that closure caused delayed price discovery or failed to stabilize the market.

Historical windows and dependent-block resampling yield different assessments of rarity. In the historical-window design, small unadjusted probabilities do not generally remain significant after correction across eighteen comparisons. Resampling significance also depends on block length. The interrupted time-series estimates show positive early return differences, but allowing a post-event slope weakens some tests. These results support a qualified interpretation of the rebound.

Return magnitude and centered dispersion give different answers for the broad-market proxy. Equal-weight variability rises more consistently relative to its own baseline, but normalization times and sometimes their cross-index ordering change across matched

rolling definitions. Five- and twenty-seven-session estimates arise only under the permissive sensitivity criterion. The restricted forecast evaluation also supports no universal model superiority.

The contribution is therefore an auditable account of post-interruption repricing and definition-dependent resilience under a combined event. It distinguishes observed nominal changes from real investor recovery, conditional forecast gaps from causal counterfactuals, and historical rarity from policy effectiveness.

7. Policy and Risk-Management Implications

The results motivate transparent reopening calendars, company disclosure requirements and publication of trading restrictions. Because closure is not causally isolated, the analysis cannot prescribe an optimal suspension duration, quantify the benefit of protection from panic, or declare that uninterrupted trading would have been preferable. These are policy questions for richer comparison designs.

The distinction between broad-market and equal-weight measures supports monitoring more than a headline index. However, daily aggregate indices do not establish that smaller firms were less liquid or that their investors could execute transactions at quoted prices. Breadth, order-book conditions, spreads and firm-level trading status should be assessed alongside price changes before labeling a reopening successful.

Risk management should distinguish model validation from evaluation during a crisis and retain simple benchmarks. The short event sample does not support a general model-superiority claim or a trading strategy selected from event-period rankings. The forecasting exercise illustrates the limits of predictive stability during a major interruption.

Reopening protocols should state which firms may trade, the disclosure requirements for suspended firms, price-limit and sell-order rules, the treatment of leveraged positions and the criteria for restoring normal trading hours. Clear protocols would allow investors to interpret observed prices alongside the restrictions under which those prices were formed.

In inflationary settings, equity index levels can rise even when uncertainty remains elevated. Inflation expectations, currency changes, risk-premium revisions and administrative trading conditions may all contribute. The USD- and gold-relative comparisons below address valuation sensitivity but do not identify investor welfare or fundamental recovery. A positive post-reopening nominal return should therefore be evaluated together with trading access, liquidity and purchasing power.

8. Robustness Considerations and Limitations

This is a single-event, jointly treated market with no credible comparison market. Closure, conflict, policy intervention and reopening restrictions cannot be separated. Daily index data lack intraday prices, order books, volumes, spreads and firm-specific damage information, so information efficiency and liquidity resilience are not directly measured. The broad-market proxy is provider supplied, with no independently verified dividend convention or constituent reconstruction.

The first reopening return spans a long calendar gap, while the forecast models operate in observed-session time. Post-event estimates use only 68 observations, with 60 in the main event and regression window. Historical reference distributions span changing market regimes; moving blocks retain only dependence within their chosen block length. Holm adjustment addresses the explicitly defined 18 event comparisons, not every exploratory coefficient or every possible design choice. Regression and model-comparison intervals remain approximate.

External data are retrospectively downloaded daily records. The two-calendar-day delay addresses date-label look-ahead, but unrecorded intraday publication and later revisions cannot be audited. Conditional external forecasts may incorporate shock transmission channels. Predictive bands omit full model-selection and parameter uncertainty. Alternative-numeraire returns do not measure CPI-real recovery or investor welfare. These restrictions define the scope of the evidence and preclude a causal verdict on suspension policy.

The twenty-session rebound remains positive in the supplied USD and domestic-gold numeraires. Over sixty sessions, the broad-market USD-relative gain is 40.33% and the equal-weight gain is 55.03%, below their nominal gains because the USD/IRR quotation rises 14.21% over the matched interval. These comparisons address currency valuation mechanically, but do not isolate why stock prices rose. The gold and currency prices can themselves respond to conflict, market restrictions and changing expectations.

Continued firm suspensions may affect index representativeness as well as investors' ability to trade. In the absence of a matched CPI series, the analysis cannot determine whether the nominal rebound restored purchasing power. The currency- and gold-relative comparisons should therefore be interpreted as valuation checks rather than measures of investor welfare.

Tables A1–A3, included below, report the principal robustness checks. Table A1 summarizes alternative numeraires. Table A2 reports twenty-session return inference under

all three resampling block lengths. Table A3 shows the matched normalization definitions for a 252-session baseline and 10-session rolling windows.

Table A1: Alternative-numeraire robustness

Target	H	Numeraire	Change %	Relative return %
Broad market index	20	USD	-6.13	46.30
Broad market index	20	18k gold	-23.43	79.34
Broad market index	60	USD	14.21	40.33
Broad market index	60	18k gold	-3.33	65.81
Equal-weight index	20	USD	-6.13	51.18
Equal-weight index	20	18k gold	-23.43	85.32
Equal-weight index	60	USD	14.21	55.03
Equal-weight index	60	18k gold	-3.33	83.18

Common valuation base: 25 February 2026. Ending values use the corresponding observed equity-window end date. These are not CPI-real or executable-arbitrage returns.

Table A2: Dependence and multiplicity sensitivity

Target	Block length	Tail p	Holm 18
Broad market index	5	0.0020	0.0280
Broad market index	10	0.0025	0.0425
Broad market index	20	0.0030	0.0540
Equal-weight index	5	0.0005	0.0090
Equal-weight index	10	0.0020	0.0360
Equal-weight index	20	0.0035	0.0595

Circular moving blocks; 1,999 simulations; eighteen comparisons in each block-length family. The inference depends on the historical reference distribution.

Table A3: Matched normalization definitions

Target	Metric	Quantile	Onset	Confirmation
Broad market index	abs	0.75	28	32
Broad market index	abs	0.90	10	14
Broad market index	rms	0.75	29	33

Target	Metric	Quantile	Onset	Confirmation
Broad market index	rms	0.90	10	14
Broad market index	centered_sd	0.75	10	14
Broad market index	centered_sd	0.90	10	14
Broad market index	Permissive daily p75	0.75	5	Sensitivity only
Equal-weight index	abs	0.75	Not reached	Censored 68
Equal-weight index	abs	0.90	26	30
Equal-weight index	rms	0.75	28	32
Equal-weight index	rms	0.90	28	32
Equal-weight index	centered_sd	0.75	48	52
Equal-weight index	centered_sd	0.90	10	14
Equal-weight index	Permissive daily p75	0.75	27	Sensitivity only

The primary metric is abs at quantile 0.75. rms and centered_sd denote mean-squared-return and centered-variance thresholds; their square-root transforms have the same crossing dates under the corresponding transformed thresholds.

9. Conclusion

Following the 80-calendar-day equity suspension and reopening on 19 May 2026, both TSE measures recorded large nominal gains, concentrated in the early observed sessions. Correctly compounded twenty-session returns were 37.33% for the broad-market proxy and 41.90% for equal weight. Their forecast deviations were substantial, but historical-rarity inference was sensitive to multiplicity and temporal dependence. Return-magnitude elevation was greater relative to baseline for equal weight, whereas broad-market centered dispersion did not show the same increase.

The interrupted time-series estimates locate the rebound mainly in the early reopening intervals, although the division between level and slope changes is specification dependent. Differences between the two indices do not, by themselves, identify firm-size or liquidity mechanisms.

Normalization times depend on the metric and threshold. The five- and twenty-seven-session estimates arise only under a permissive retrospective criterion, while matched rolling thresholds give later confirmation or no confirmation within the sample. The restricted forecasting exercise likewise provides no general evidence of model superiority.

The study contributes a documented case of nominal repricing after a prolonged market interruption, with explicit cleaning rules and sensitivity to benchmark choice. Its policy implications concern transparent disclosure, reopening procedures and careful interpretation of index-based resilience measures. Establishing whether closure stabilized the market or delayed information incorporation requires a credible comparison design and richer trading data.

Acknowledgments

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Conflict of Interest

In adherence to ethical publication standards, the author affirms that there is no conflict of interest, either personal or financial, that could have influenced the content or conclusions presented in this research.

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آیا تعطیلی بازار به ثبات انجامید یا تعدیل را به تأخیر انداخت؟ کشف قیمت و تاب‌آوری نوسانات در بورس اوراق بهادار تهران تحت شوک ژئوپلیتیکی

هانیه صداقت‌کالمرزی[۱]

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چکیده

این پژوهش، تعدیل اسمی قیمت‌ها و تغییرپذیری بازده روزانه در بورس اوراق بهادار تهران را دربارهٔ توقف معاملات سهام از ۲۸ فوریه تا ۱۸ مه ۲۰۲۶م. و بازگشایی بازار در ۱۹ مه بررسی می‌کند. داده‌ها شامل شاخص کل بازار و شاخص هم‌وزن، با ۶۸ مشاهدهٔ معتبر پس از بازگشایی است. تحلیل، بررسی بازتولیدپذیر دورهٔ توقف معاملات، پنجره‌های مرجع تاریخی، بازنمونه‌گیری با حفظ ساختار وابستگی، اعتبارسنجی پیش‌بینی با داده‌های پیش از رویداد، رگرسیون‌های سری زمانی منقطع و آزمون‌های حساسیت بازگشت نوسان به سطح مبنا را دربر می‌گیرد. بازده اسمی تجمعی بیست جلسهٔ معاملاتی برای این دو شاخص به‌ترتیب ۳۷٫۳۳٪ و ۴۱٫۹۰٪ است. هر دو از معیارهای پیش‌بینی ساده فراتر می‌روند؛ با این‌حال، احتمال‌های دنباله‌ای مبتنی‌بر پنجره‌های تاریخی پس از تعدیل بابت مقایسه‌های متعدد میان متغیرهای پیامد و افق‌های زمانی، معناداری آماری خود را از دست می‌دهند و نتایج بازنمونه‌گیری نیز به طول بلوک وابسته‌اند. تغییرپذیری مبتنی‌بر قدرمطلق بازده در شاخص هم‌وزن، نسبت به سطح مبنای خود، افزایش بیشتری نشان می‌دهد؛ اما نتیجهٔ مربوط به شاخص کل بازار با جایگزینی قدرمطلق بازده با انحراف معیار حول میانگین تغییر می‌کند. برآوردهای پنج و بیست‌وهفت جلسه‌ای برای بازگشت نوسان به سطح مبنا تنها از یک قاعدهٔ پس‌نگر با شرایط سهل‌گیرانه حاصل می‌شوند. با استفاده از آستانه‌ای مبتنی‌بر توزیع پنجره‌های غلتان و الزام قرارگرفتن پنج مشاهدهٔ متوالی در محدودهٔ آستانه، بازگشت شاخص کل بازار به سطح مبنا در جلسهٔ سی‌ودوم تأیید می‌شود، ولی این وضعیت برای شاخص هم‌وزن طی ۶۸ جلسه تأیید نمی‌شود. مقایسهٔ محدود مدل‌های پیش‌بینی، با انتخاب مدل‌ها براساس اعتبارسنجی پیش از رویداد، برتری یکنواختی برای هیچ‌یک از مدل‌ها در دورهٔ پس از بازگشایی نشان نمی‌دهد. این یافته‌ها تعدیل قیمت را در شرایط هم‌زمان شوک ژئوپلیتیکی، تعطیلی بازار و بازگشایی کنترل‌شده توصیف می‌کنند، اما اثر علّی تعطیلی بازار را شناسایی نمی‌کنند و به‌معنای اثبات بازیابی واقعی بازار، کارایی اطلاعاتی یا تاب‌آوری نقدشوندگی نیستند.

کلیدواژگان: ریسک ژئوپلیتیکی، تعطیلی بازار، کشف قیمت، تاب‌آوری نوسانات، بورس اوراق بهادار تهران.
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