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Did the Removal of Stock-Price Floors Improve Short-Run Price Discovery? An Event Study of the Dhaka Stock Exchange

Md. Mustak Ahmed[1], Aishi Dhar[1], Mazharul Haque Jubaed[2]*, Ali Akbar[1], Zobayer Ahmmad[3]

[1]Shahajalal University of Science and Technology, Sylhet, Bangladesh, [2]Department of Accounting, Kishoreganj University, Kishoreganj, Bangladesh, [3]Kishoreganj Government Mohila College, Kishoreganj, Bangladesh. *Email: jubaed.sust57@gmail.com

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ABSTRACT

This study examines whether the January 2024 removal of stock-price floors improved short-run price discovery and reduced price rigidity in the Dhaka Stock Exchange (DSE). An event-study technique was applied to 338 securities: 306 firms in the main removal cohort, 20 firms in the second-stage cohort, and 12 firms that retained their floors. Market-adjusted abnormal returns were used as the primary specification, while a market model and complete-window samples were used for robustness. Cross-sectional one-sample t-tests assessed abnormal returns, paired-sample t-tests compared pre- and post-removal zero-return rates and intraday high-low ranges, and Welch tests compared treatment cohorts with the retained-floor benchmark. The main cohort recorded a cumulative average abnormal return (CAAR) of −6.77% over [−5, +5], while the second-stage cohort recorded −18.87% over [−3, +3]. Zero-return rates fell from 49.84% to 5.36% in the main cohort and from 97.85% to 3.50% in the second-stage cohort, while intraday ranges increased significantly. The findings are robust for the shorter event windows but sensitive to sample completeness over the longest window. Abnormal returns, zero-return frequency, and the high-low range are proxy indicators rather than comprehensive measures of market efficiency.

Keywords: Stock-Price Floor, Price Discovery, Price Rigidity, Event-Study Technique, Dhaka Stock Exchange
JEL Classifications: G14, G18, G12, C58

1. INTRODUCTION

An efficient stock market contributes to economic development through three mechanisms: Facilitating capital formation, efficiently allocating financial resources, and pricing securities in line with available information. The efficient market hypothesis implies that stock prices respond expeditiously to new information, so investors cannot systematically earn abnormal returns from publicly disseminated information (Fama, 1970). But this price discovery requires investors to trade freely at prices set by supply and demand. The Market prices of securities may cease to reflect their underlying economic value when regulatory restrictions prevent prices from adjusting freely and rapidly to public information. This becomes especially relevant in frontier and emerging markets where market efficiency may be impaired by low liquidity, information asymmetry, concentrated ownership, limited institutional participation, and high transaction costs (Borman et al., 2024; Sochi and Swidler, 2018).

During times of financial uncertainty, securities regulators regularly set price limits to regulate excessive volatility in the prices of a security and to prevent investors from panicking to sell a security. Supporters argue that such restrictions reduce speculative excessive market response and provide investors with time to process new information. However, their effectiveness is subject to scholarly debate. Price limits are often criticized for postponing rather than reducing market volatility, preventing prices from adjusting efficiently to their equilibrium levels, and restricting investors' ability to execute transactions. The volatility-spillover hypothesis, the delayed price-discovery hypothesis, and the trading-interference hypothesis (Kim and Rhee, 1997) are these arguments. Tokyo Stock Exchange and Istanbul Stock

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Exchange indicate that the price limits may obstruct the market price adjustment process and halt trading rather than permanently increase the stability of the market (Kim and Rhee, 1997; Bildik and Gülay, 2006). Recent evidence from China also shows that easing price limits can enhance price efficiency, but the effect can vary with other market features, including the short-sale restrictions (Chen et al., 2023).

The floor-price mechanism adopted in Bangladesh is different from the traditional daily price limits that have been studied in most other countries. A daily price band is a range within which prices are allowed to rise or fall and is revised for each trading session. A stock-price floor, however, sets a minimum price that a security can't be transacted below intrinsic value. This is thus a one-sided restriction, one which is directly aimed at the downward price adjustment (Li and Li, 2022; Wang et al., 2023). If the investors think that the stock is trading at a price lower than its regulatory floor, they might want to sell, but buyers might not be willing to buy it at the minimum price. As a result, sell orders might not be matched, trading volume might be reduced, and the stock's price might not be adjusted even if the economic value of the stock has dropped. A floor can thus be used to provide apparent price stability while masking illiquidity, building up selling pressures, and delaying price discovery.

The Dhaka stock exchange (DSE) offers a unique platform to explore such impacts. In response to continuing market declines and the prevailing uncertainty in the global economy, the Bangladesh Securities and Exchange Commission (BSEC) implemented the floor-price mechanism on 28 July 2022. The floor price for each security was set at the average of the closing price on 28 July 2022 and the closing prices of the previous four trading days (Bangladesh Securities and Exchange Commission [BSEC], 2022). This policy was intended to prevent a further downward trend of the market indices, safeguard the interests of investors, and stabilise the country's capital market. The regulator lifted the floor from 169 securities in December 2022 to enhance market activity. In March 2023, however, the restriction was reimposed on these securities and the wider market was once again put under the floor-price mechanism (Bangladesh Sangbad Sangstha, 2024). The policy did not lead to any sharp reduction in share prices, but there were concerns about the liquidity of the market and the integrity of the share prices. Affected stocks frequently hit their lower bounds, often closed flat, were sold below fundamental value, experienced sharp drops, and left investors unable to close positions amid heightened illiquidity.

On 18 January 2024, the BSEC announced that the floor price would be removed from most listed securities with effect from 21 January 2024. Because 19 and 20 January were non-trading days, 21 January was the first trading session in which the affected securities could trade without the floor. The regulator initially retained the restriction on 35 securities (Bangladesh Sangbad Sangstha, 2024). It removed the floor from another 23 securities on 22 January, effective from 23 January, leaving 12 securities under the restriction (The Business Standard, 2024). This staggered withdrawal provides a policy setting for examining short-run price adjustment, although the close timing of the two stages creates the possibility of spillover between them.

The central research problem concerns the trade-off between visible price stability and the ability of prices to incorporate information. Accordingly, the main research question is: To what extent did the removal of stock-price floors improve short-run price discovery and reduce price rigidity in the Dhaka Stock Exchange? A negative price correction after removal would not necessarily indicate that the reform damaged the market; it could represent the incorporation of adverse information that the binding floor had previously prevented from entering prices. Conversely, persistent zero returns after removal would indicate that lifting the formal restriction alone was insufficient to restore active price adjustment. The policy is therefore evaluated through the speed and magnitude of repricing, the frequency of unchanged prices, and intraday price variation rather than through the direction of returns alone.

While the DSE floor-price removal is policy relevant, there are some key gaps in the literature. First of all, most of the studies in the international literature have been on either symmetric daily price bands, circuit breakers, trading halts, or an individual occurrence of the price limit. There has been relatively little research on the removal of a fixed, extended, and unilateral price floor (Tang, 2024; Wan and Zhang, 2022). The DSE intervention is institutionally distinct as it clearly prevented the downward price adjustment and applied to some securities for about 18 months. Evidence on conventional price limits might thus not fully capture its impact.

Second, previous studies of DSE efficiency have mainly employed market-index data and long-term time-series methods. Islam and Khaled (2005) examined the weak-form efficiency under various data frequencies and showed that their results were dependent upon the structural changes and heteroskedasticity. However, Pervez et al. (2018) used random-walk tests on main indices of the DSE and concluded that the evidence was not in favor of weak-form efficiency. These studies offer insights into the statistical long-term behaviour of DSE returns, but they do not consider the impact of a clear regulatory move on the price discovery of individual stocks.

Third, previous research has rarely examined abnormal returns, price rigidity, and realised price variation together when evaluating the relaxation of a regulated price (Bing et al., 2022; Jin et al., 2023; Li and Li, 2022). Abnormal returns indicate whether prices changed unusually around the policy event, but they do not independently measure market efficiency. Zero-return frequency is a proxy for illiquidity, implicit transaction costs, stale prices, or an inability to execute trades at economically acceptable prices (Lesmond et al., 1999). Daily high-low ranges provide information about intraday price flexibility and volatility. These measures are therefore interpreted jointly as indicators of short-run price adjustment and trading functionality, not as a comprehensive test of market efficiency.

Therefore, this study examines whether removing stock-price floors improved short-run price discovery and reduced price rigidity in the DSE. It evaluates abnormal returns around the

removal dates, changes in zero-return frequency, and changes in the daily high-low price range. It also compares the main and second-stage removal cohorts with securities that remained subject to the floor, while treating the retained-floor firms as a benchmark rather than a randomized control group.

This study contributes to the literature in three ways. First, it adds to the literature on price restrictions in that it looks at the lifting of a price floor that was put in place for an extended period of time and was one-sided (Chen et al., 2024; Yang et al., 2025). Second, it contributes to the literature on DSE efficiency by going beyond the random walk test at the index level and considering a particular regulatory event at the individual company level. Third, it separates out true market stability from price rigidity that is imposed by administration by taking into account various dimensions of price discovery and trading activity. The study also has policy implications for regulators who contemplate imposing price floors when markets are stressed. Such measures might inhibit the price drop for a short time but may also dampen information, hinder liquidity, and make the price drop more instantaneous.

2. LITERATURE REVIEW

2.1. Market Efficiency and Regulatory Price Restrictions

Market efficiency concerns the extent to which security prices incorporate available information. Under the efficient market hypothesis, prices should adjust rapidly when new information arrives, thereby limiting investors' ability to earn abnormal returns systematically on the basis of pre-existing information to the market (Fama, 1970). This adjustment occurs through price discovery: Investors interpret information, submit orders, and move prices toward market-clearing values (El Ghoul et al., 2022). Market efficiency therefore depends not only on the availability of information but also on investors' ability to trade on that information.

Regulatory restrictions can impair price discovery when they prevent transactions at prices acceptable to buyers and sellers. This is especially pertinent in emerging and frontier markets, where low trading volume, high transaction costs, and information asymmetry may already slow adjustment (Borman et al., 2024; Sochi and Swidler, 2018). A stable quoted price may consequently reflect an absence of trading rather than efficiency.

Price limits are typically introduced to reduce panic selling, speculative overreaction, and excessive volatility. Supporters argue that they provide investors with time to evaluate information (Chen et al., 2024; Tang, 2024). In a related argument, Greenwald and Stein (1991) suggest that circuit breakers may reduce transactional risk when traders are uncertain about execution prices. Nevertheless, controlling the observed price does not eliminate the underlying information or order imbalance. If the equilibrium price lies outside the permitted range, adjustment is likely to be postponed rather than prevented.

The DSE stock-price floor was more restrictive than an ordinary daily price band. A daily band permits upward and downward movements within a recalculated range, whereas a fixed floor establishes a minimum price below which a security cannot trade. When investors value a stock below that minimum, sell orders may remain unmatched because buyers are unwilling to transact at the permitted price. The recorded price may therefore remain unchanged while selling pressure accumulates (Li and Li, 2022; Wang et al., 2023). Removing the floor should allow this suppressed information to enter prices, although the immediate consequence may be a negative return rather than a price increase.

2.2. Theoretical Effects of Price Floors

Kim and Rhee (1997) organise the principal criticisms of price restrictions into three related hypotheses: delayed price discovery, volatility spillover, and trading interference. These hypotheses provide the most relevant theoretical foundation for evaluating the DSE reform.

The delayed price-discovery hypothesis proposes that a binding restriction prevents a security from reaching its equilibrium price. If investors' market-clearing valuation is below the floor, the price adjustment cannot be completed while the restriction remains effective. When the floor is removed, prices should move toward their unrestricted values. Accordingly, significant negative abnormal returns around removal may represent the incorporation of previously suppressed information. Such returns should not automatically be interpreted as evidence that market efficiency deteriorated. Their concentration within a short event window may instead indicate that the market adjusted rapidly once the regulatory barrier disappeared (Tang, 2024; Wang et al., 2023).

The volatility-spillover hypothesis argues that price restrictions postpone volatility. Information that cannot be incorporated during the restricted period remains relevant and may generate larger price movements after the restriction is moderated. Consequently, a temporary increase in the daily price range following removal may reflect the completion of delayed price discovery. However, greater volatility may also reflect speculative trading or market instability. Volatility must therefore be interpreted jointly with abnormal returns and indicators of trading activity (Jin et al., 2023; Wan and Zhang, 2022).

The trading-interference hypothesis states that price restrictions obstruct transactions. When a stock is held at a floor above its market-clearing value, potential buyers may withdraw while sellers remain unable to execute orders. Trading can fall substantially, producing repeated days with unchanged closing prices. If the floor is a major source of this inactivity, its removal should reduce zero-return frequency by permitting transactions at lower prices (Li and Li, 2022).

These mechanisms are mutually reinforcing. Trading interference prevents orders from revealing valuations, delayed discovery creates a gap between recorded and equilibrium prices, and closing that gap produces volatility spillover. A unilateral price floor may exacerbate these effects by constraining the incorporation of adverse information while permitting favorable information to influence security prices (Chen et al., 2023; Qi, 2023; Tang,

2024). Evidence from lower-limit events is therefore especially relevant to the DSE.

2.3. Empirical Evidence on Price-Limit Effectiveness

International evidence generally confirms that price limits affect price discovery, but there is no consensus that they improve or damage every dimension of market quality (Bing et al., 2022; Yang et al., 2025; Zhang et al., 2022). Kim and Rhee (1997) examined limit-hit stocks on the Tokyo Stock Exchange and compared them with stocks experiencing large price movements without reaching their limits. They found evidence of delayed price discovery, volatility spillover, and trading interference, suggesting that the restrictions postponed adjustment rather than stabilizing prices permanently.

Bildik and Gülay (2006) reached a similar conclusion for the Istanbul Stock Exchange. Price-limit events were followed by continuing price adjustments and changes in volatility and trading activity. The effects were stronger when securities became remained at their limits, demonstrating that a restriction is particularly disruptive when it prevents investors from completing transactions. This finding is relevant to the DSE, where many securities remained at their floor prices for extended periods.

Evidence from Korea shows that the relationship is not straightforward. Berkman and Lee (2002) reported that wider limits were followed by higher long-term volatility and lower trading volume. Thus, greater price flexibility does not necessarily improve every dimension of market quality.

Research on China has generated similarly mixed results. Kim et al. (2013) concluded that price limits improved price discovery, reduced volatility, and restrained abnormal trading. In contrast, Li et al. (2014) found volatility spillover and trading interference, despite some reduction in price persistence. Differences in samples, comparison groups, and market regimes may explain these conflicting conclusions.

More recent studies exploit regulatory reforms that widen existing limits. Lien et al. (2019) found that wider limits in Taiwan improved order execution but increased intra-day volatility and time-weighted spreads while reducing market depth. Bing et al. (2022) reported that widening price limits in China improved liquidity and reduced information asymmetry. Similarly, Zhang et al. (2022) found that expanding the permitted price range improved liquidity but increased volatility and informed trading. Collectively, these studies suggest that higher post-reform volatility can operate concurrently with improved information incorporation and transaction execution.

Chen et al. (2023) provide additional evidence from China that relaxing price limits improves price efficiency. The improvement was greater for stocks that could be sold short, highlighting the importance of mechanisms through which investors can express negative information. This result is directly relevant to a lower price floor because downward price discovery depends on investors' ability to sell when their valuations decline (Tang, 2024).

Qi (2023) examined the expansion of the price limit in China's ChiNext market and confirmed delayed price discovery, volatility spillover, and trading interference. Importantly, the negative effects were more severe when lower limits were reached than when upper limits were reached. This asymmetry suggests that restrictions on downward adjustment may create stronger distortions than restrictions on upward movements. Therefore, the DSE's fixed lower boundary could have produced substantial price rigidity and accumulated selling pressure (Jin et al., 2023; Wang et al., 2023).

Recent evidence further shows that regulatory design matters. Lower-price restrictions can have stronger effects on liquidity and price discovery than upper limits, while the consequences also depend on market conditions and the strictness of the rule (Li and Li, 2022; Tang, 2024; Yang et al., 2025). Circuit breakers may also alter trading incentives and investor welfare rather than simply suppress volatility (Chen et al., 2024).

The international literature consequently offers two central lessons. First, the visible stability created by a price restriction should not be equated with informational efficiency. Second, the effects of removal cannot be judged using returns or volatility alone (Chen et al., 2024; Tang, 2024). A decline in prices and an increase in observed variation may be consistent with improved price discovery if trading inactivity simultaneously decreases. These lessons justify examining abnormal returns, zero-return frequency, and high-low price ranges together.

2.4. Abnormal Returns, Zero Returns, and Price Range

An event study assesses whether the returns around a specific event are different from the returns that would have occurred in the absence of the event (MacKinlay, 1997). Abnormal returns can be used to determine if the reform has released constrained price pressure for the removal of floors (El Ghoul et al., 2022). If the floor has been holding a stock at a price higher than its market-clearing price, then a negative abnormal return is expected. Cumulative abnormal returns reflect immediate response as well as adjustment in the near future.

But abnormal returns do not necessarily indicate an improvement in market functioning. A negative response can be a sign of effective repricing, investor negativity, or market-wide factors (El Ghoul et al., 2022). To interpret the evidence from the event studies, indicators of liquidity and price-rigidity are therefore required.

Lesmond et al. (1999) show that zero-return frequency can be used as a proxy for illiquidity and implicit transaction costs. Investors may not trade if the expected value of their information is not enough to cover the trading costs or other impediments. Moreover, Bekaert et al. (2007) demonstrate that the share of zero daily returns carries significant liquidity information in emerging markets. In the case of DSE, frequent zero returns during the period when stocks were at their floors may mean that trading was not possible at the prices administered (Li and Li, 2022). A significant post-removal reduction would therefore be consistent with lower price rigidity and improved tradability.

The daily high–low range offers an alternative indication of price change realised within the day. Alizadeh et al. (2002) demonstrate that the range provides valuable information on volatility. A very narrow range during the floor period could be an indicator of low economic uncertainty, but it can also be an indicator of suppressed price discovery. Following removal, a wider range may indicate that transaction prices have regained the ability to respond to information and differences in investor valuations (Jin et al., 2023). However, a larger range may also be a sign of instability and should be viewed with the zero-return rate. A reduction in zero returns and an expansion of the range would be more indicative of a return to trading and price flexibility than an expansion of the range alone.

2.5. Evidence from the Dhaka Stock Exchange and Research Gap

Previous studies generally question the weak-form efficiency of the DSE (Sochi and Swidler, 2018). Islam and Khaled (2005) reported that the results on efficiency were sensitive to return frequency, structural change, and heteroskedasticity. Their more stringent tests detected short-term predictability at certain periods of the sample. Pervez et al. (2018) explored the DSE General Index, DSEX and DSE30 by applying unit-root, autocorrelation, runs, normality and variance-ratio tests. Their findings contradicted the random-walk hypothesis and suggested that historical information still had predictive power.

These studies demonstrate that the DSE has suffered from informational frictions, and their index designs over long horizons make it difficult to determine the impact of a specific regulatory move (Borman et al., 2024). A more recent study by Sadique et al. (2026) explores return-volume relationships and market inefficiency under various DSE conditions, such as the wider floor-price period. It supports evidence of a link between trading activity and returns, but does not offer a firm-level event-study assessment of the staggered floor-price removal in January 2024 based on the three measures of trading activity presented here.

A policy gap therefore remains in the literature. Most international studies examine symmetric price bands or isolated limit-hit events, whereas the DSE employed a sustained fixed lower bound (Tang, 2024; Wan and Zhang, 2022). Studies on Bangladesh have primarily examined return predictability using aggregate indices rather than whether the January 2024 removal produced abnormal repricing, reduced zero-return frequency, and changed intraday price variation across different removal cohorts.

This study addresses the gap by evaluating three related but distinct indicators of short-run market functioning. Abnormal returns capture unusual repricing around the policy event; zero-return frequency indicates changes in price rigidity or inactivity; and the high-low range reflects intraday price flexibility as well as volatility (Bing et al., 2022; Jin et al., 2023; Li and Li, 2022). Their combined interpretation provides broader evidence about price discovery than any single measure. Nevertheless, these indicators do not constitute a comprehensive or direct measure of overall market efficiency.

2.6. Hypothesis Development

The delayed price-discovery hypothesis implies that a binding floor will allow less than full incorporation of negative information in stock prices. Removal should relieve deferred downwards pressures and create a negative adjustment surge if the restricted prices are higher than the market-clearing prices (Tang, 2024; Wang et al., 2023). Therefore:

H_1: The removal of stock-price floors is associated with significant negative abnormal returns among the affected DSE-listed stocks.

According to the trading-interference hypothesis, a binding floor will result in no trading when investors are not willing to buy at the minimum floor price. Securities should trade more often when they are allowed to trade below the floor, which will decrease the number of unchanged closing prices (Li and Li, 2022). Therefore:

H_2: The frequency of zero-return observations is significantly lower after the removal of stock-price floors.

Finally, the volatility-spillover and delayed-adjustment explanations suggest that the release of previously constrained information may produce greater price variation after removal. A wider range may reflect renewed price flexibility, but it can also represent higher short-term volatility; it must therefore be interpreted together with the abnormal-return and zero-return evidence (Jin et al., 2023; Wan and Zhang, 2022). Therefore:

H_3: The daily high-low price range is significantly higher after the removal of stock-price floors.

3. METHODOLOGY OF THE STUDY

3.1. Research Design, Event Dates and Sample

This study adopts a quantitative event-study design to examine whether stock-price floor removal was followed by improved short-run price discovery and reduced price rigidity in the DSE. The event-study technique identifies whether stock returns around a clearly defined regulatory event differ from expected returns under normal market conditions (MacKinlay, 1997). It is combined with within-firm pre-post comparisons of zero-return frequency and intraday price ranges. These variables are treated as proxy indicators of price adjustment, rigidity, and flexibility rather than direct measures of complete market efficiency.

The BSEC announced the main removal on 18 January 2024, but the policy became effective on 21 January 2024. As 21 January was the next DSE trading session and the 1st day on which the affected securities could trade without the floor, it is defined as event day 0 for the main cohort. The announcement date, 18 January, is trading day −1 in the short event window. Event day 0 for the second-stage cohort is 23 January 2024. Securities that remained subject to the floor after the second-stage removal form the retained-floor benchmark.

After matching regulatory classifications with the available daily price data, the final sample contains 338 DSE-listed securities: 306 in the main removal cohort, 20 in the second-stage cohort with sufficient observations, and 12 in the retained-floor

benchmark. Mutual funds, debt securities, duplicate records, and securities without sufficient observations around the event dates were excluded. The 12 retained-floor firms were selected by the regulator rather than randomly assigned and may differ from the treatment firms in size, liquidity, index importance, or financial condition. They are therefore used as a contemporaneous benchmark, not as a causal control group.

3.2. Data Sources, Preparation and Analysis Software

Secondary daily market data is utilized within the study, which includes the company code, trading date, opening, high, low, and closing prices. Daily values of the DSEX broad-market index are used to estimate general market returns (El Ghoul et al., 2022). The relevant event dates and the categorization of securities in the main removal, second-stage removal, and retained-floor groups are based on BSEC regulatory orders and the respective market notices issued on time.

Before conducting the analysis, company codes and trading dates were standardised, and duplicate company-date observations were removed. Price variables were coded numerically and examined for response omissions and internally inconsistent, zero, and negative responses. For instance, if a high price was observed and reported to be less than a low, the observation was deemed inconsistent. Missing observations were not replaced with distorted values. The companies excluded from the analysis were those that did not have the required data for a specific calculation. DSE trading days, excluding weekends, public holidays, and exchange-closure days, were used for all windows.

All the data was organised and cleaned, event windows identified, event-study calculations made, statistical tests performed, and final tables and figures prepared in Microsoft Excel. Daily stock returns, DSEX returns, abnormal returns, average abnormal returns, cumulative abnormal returns, cumulative average abnormal returns, zero-return frequency, and daily high-low price ranges were calculated using the Excel formulas. Descriptive statistics and significance tests were performed using the functions in Excel and the Data Analysis ToolPak.

3.3. Measurement of the Variables

The daily return of the company *i* on trading day t is calculated as

$$R_{it} = \left(\frac{P_{it} - P_{i,t-1}}{P_{i,t-1}} \right) \times 100 \quad (1)$$

In this function, R_{it} represent the percentage return of company *i* on day *t*, P_{it} is its closing price, and $P_{i,t-1}$ is its closing price on previous day.

Moreover, the daily market return is estimated from DSEX index:

$$R_{mt} = \left(\frac{M_t - M_{t-1}}{M_{t-1}} \right) \times 100 \quad (2)$$

Where, R_{mt} is the DSEX return on day *t*, M_t is the current DSEX value, and M_{t-1} is its previous trading day value.

Furthermore, the study implements market adjusted return model (El Ghoul et al., 2022). The abnormal return is the difference between an individual stock's return and DSEX return:

$$AR_{it} = R_{it} - R_{mt} \quad (3)$$

A negative abnormal return indicates that the stock performed worse than the overall market. However, a positive abnormal return indicates that it performed better.

The average abnormal return across all *N* companies on event day *t* is:

$$AAR_t = \frac{1}{N} \sum_{i=1}^{N} AR_{it} \quad (4)$$

The cumulative abnormal return of the company *i* over an event window beginning on a day *a* and ending on a day *b* is:

$$CAR_{(a,b)} = \sum_{t=a}^{b} AR_{i,t} \quad (5)$$

The cumulative average abnormal return is calculated by averaging the CAR of all companies:

$$CAAR_{(a,b)} = \frac{1}{N} \sum_{i=1}^{N} CAR_{(a,b)} \quad (6)$$

The study assess four symmetric event windows:

[−1, +1], [−3, +3], [−5, +5], [−10, +10]

The [−1, +1] window captures the immediate three-trading-day response around the effective removal date (El Ghoul et al., 2022). For the main cohort, 18 January is trading day −1 and 21 January is event day 0; because the policy became effective on the first trading session after the announcement, 21 January is the first unrestricted adjustment day. Broader windows examine whether price adjustment began before or persisted after removal. Calculations are conducted separately for the main and second-stage cohorts, although the wider second-stage windows may overlap with the first-stage market reaction.

Additionally, a zero return occurs when a company's closing price remains unchanged from the previous trading day. It is defined as:

$$Z_{it} = \begin{cases} 1, \text{ if } R_{it} = 0 \\ 0, \text{ if } R_{it} \neq 0 \end{cases} \quad (7)$$

The zero return rate of the company *i* is calculated as:

$$ZR_i = \left(\frac{\sum_{t=1}^{T_i} Z_{it}}{T_i} \right) \quad (8)$$

Where T_i is the number of valid observations for company *i* for the trading days. The zero return rate is computed in two 20-day

intervals before/after the corresponding event but not on the event day 0. The change is determined by:

$$\Delta ZR_i = ZR_{i,post} - ZR_{i,pre} \quad (9)$$

A negative value of ΔZR_i indicates that zero-return frequency fell after floor removal. This is consistent with reduced price rigidity and improved tradability, although zero-return frequency is also influenced by liquidity and transaction costs and is not a direct measure of market efficiency (Bekaert et al., 2007; Lesmond et al., 1999).

The normalized daily price range is calculated as:

$$Range_{it} = \left(\frac{H_{it} - L_{it}}{C_{it}} \right) \quad (10)$$

Here, H_{it}, L_{it}, and C_{it} represent the highest, lowest, and closing prices, respectively, of firm i on trading day t. The average normalized range is calculated over the 20 trading days before and after the event, and the change in the average range is defined as:

$$\Delta Range_i = \overline{Range_{i,post}} - \overline{Range_{i,pre}} \quad (11)$$

A positive value indicates that intraday price variation increased after removal. This may reflect restored price flexibility, higher short-term volatility, or both; therefore, the range is interpreted jointly with abnormal returns and zero-return frequency and not as an independent measure of market efficiency (Alizadeh et al., 2002).

Cross-sectional one-sample t-tests were used to assess whether AARs and CAARs differed from zero. Paired-sample t-tests compared firm-level zero-return rates and average high-low ranges before and after removal. Welch tests compared mean CAARs between each treatment cohort and the retained-floor benchmark because the groups differed substantially in size and could have unequal variances. All tests were two-tailed, with significance assessed at the 1%, 5%, and 10% levels. Economic magnitude was evaluated alongside statistical significance. Importantly, a significant Welch test indicates a difference in group means; it does not establish that floor removal caused the difference.

Because many firms share the same policy-event date, their abnormal returns may be cross-sectionally correlated, and event-day volatility may differ from normal-period volatility. Under these conditions, conventional cross-sectional t-tests can understate standard errors and overstate significance (Kolari and Pynnönen, 2010; Pelagatti and Maranzano, 2021). Market adjustment, the alternative expected-return model, and complete-window analysis provide useful sensitivity checks, but they do not directly eliminate common-event dependence. The conventional p-values are therefore treated as baseline inference and interpreted cautiously.

The second-stage findings also require caution because the 23 January event occurred only two trading days after the main removal. The first-stage reaction could have altered market sentiment and investors' expectations about the remaining constrained securities, allowing selling pressure to accumulate before their own floors were removed. Consequently, the second-stage estimates may combine a direct removal effect with spillover from the first-stage event and cannot isolate the two effects using daily price data.

4. DATA ANALYSIS RESULT AND DISCUSSION

4.1. Sample Composition and Event Design

Table 1 presents the sample composition and policy-event structure. The final sample contains 338 DSE-listed securities. The main cohort consists of 306 firms whose floors were removed on 21 January 2024, while 20 firms form the second-stage cohort released on 23 January 2024. The remaining 12 firms retained their floors and are used as a benchmark rather than a randomized control group.

The retained-floor firms experienced the same general market environment but not the same immediate regulatory treatment, making them a relevant contemporaneous benchmark. However, the group contains only 12 firms. Its small size reduces statistical power, makes the group mean more sensitive to individual observations, and limits representativeness. The benchmark evidence is therefore supplementary to the event-study and within-firm pre-post results rather than conclusive on its own.

The two-stage policy structure also affects interpretation. The second-stage firms remained constrained during the first removal and were released two trading days later. Their reaction may therefore reflect both their own removal and information or market sentiment generated by the first-stage response. The second-stage results are treated as supplementary evidence rather than a cleanly isolated policy effect.

4.2. Abnormal Returns Following Floor Removal

Table 2 reports cumulative average abnormal returns for four event windows. The market-adjusted model is used as the primary specification, under which a firm's daily percentage return is compared with the corresponding DSEX return.

The main treatment cohort recorded a CAAR of −3.34% over [−1, +1]. The statistically significant estimate indicates an immediate downward adjustment around the effective removal date. The CAAR reached −5.29% over [−3, +3] and −6.77% over [−5, +5], showing that adjustment continued during subsequent trading sessions. Over [−10, +10], the CAAR declined in absolute magnitude to −2.43% and remained significant at the 5% level, suggesting partial reversal after the initial correction. These estimates describe a policy-associated market reaction rather than a definitive causal effect.

The second-stage cohort recorded larger negative CAARs: −13.36% over [−1, +1] and −18.87% over [−3, +3]. The pattern is consistent with accumulated downward price pressure while the floors remained in place. However, because this event occurred

Table 1: Sample composition and event design

Cohort	Event date	Number of firms	Share of sample (%)	Analytical role
Main floor removal	21 January 2024	306	90.5	Primary treatment cohort
Second-stage removal	23 January 2024	20	5.9	Secondary treatment cohort
Retained-floor benchmark	After 23 January 2024	12	3.6	Retained-floor benchmark group
Total eligible sample	-	338	100.0	Event-study universe

Source: Authors' calculations based on DSE daily market data

Table 2: Primary cumulative average abnormal return result

Cohort	Event window	N	CAAR (%)	t-statistic	P-value	Inference (%)
Main floor removal	[−1, +1]	306	−3.34	−6.508	<0.001	Significant at 1
Main floor removal	[−3, +3]	306	−5.29	−7.176	<0.001	Significant at 1
Main floor removal	[−5, +5]	306	−6.77	−7.103	<0.001	Significant at 1
Main floor removal	[−10, +10]	306	−2.43	−2.367	0.0185	Significant at 5
Second-stage removal	[−1, +1]	20	−13.36	−8.544	<0.001	Significant at 1
Second-stage removal	[−3, +3]	20	−18.87	−7.672	<0.001	Significant at 1
Second-stage removal	[−5, +5]	20	−17.64	−6.094	<0.001	Significant at 1
Second-stage removal	[−10, +10]	20	−12.15	−4.211	<0.001	Significant at 1

CAAR denotes cumulative average abnormal return. Abnormal returns are calculated using the market-adjusted return model. The reported t-statistics are conventional cross-sectional tests and should be interpreted cautiously because common-event dependence may affect their standard errors

only two trading days after the main removal, the estimates may also contain spillover from the first-stage market reaction and should not be attributed exclusively to the second-stage removal.

Negative CAARs do not by themselves demonstrate either market damage or improved efficiency. They show that treated securities underperformed the market around the removal dates. When considered together with the sharp reduction in zero returns, the pattern is consistent with the release of previously constrained downward price pressure, although other firm-specific influences cannot be completely excluded.

This interpretation is consistent with the delayed price-discovery hypothesis (Tang, 2024; Wang et al., 2023). Kim and Rhee (1997) suggest that securities may not attain their equilibrium values as a result of price restrictions and that the necessary adjustments are passed along to subsequent trading periods. The present outcomes provide related evidence for a wide minimum-price limit, as opposed to a standard day-to-day price cap.

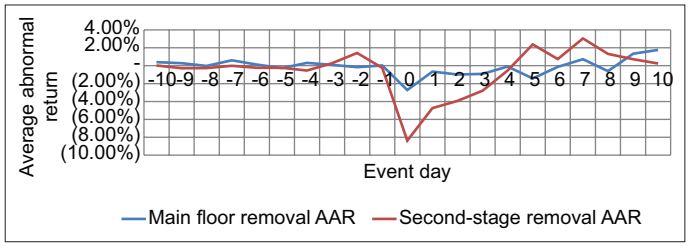
The graph in Figure 1 demonstrates that abnormal returns were around zero prior to the removal. A clear break point was identified on day 0 of the event, with the main cohort having an AAR of about −2.72% and the second stage cohort having an AAR of −8.38%.

Figure 1 shows that abnormal returns were generally close to zero before removal. On event day 0, the main cohort recorded an AAR of approximately −2.72%, while the second-stage cohort recorded −8.38%.

The next few trading days saw negative returns. For the main cohort, AARs were −0.65%, −0.99% and −0.91% on days +1, +2 and +3. The amount of returns for the second-stage cohort was −4.73%, −3.94% and −2.79%, respectively. This adjustment was not sudden, but a gradual one.

A positive abnormal return was then observed, mainly for the second-stage cohort at day +5 and +7. This recovery is the reason

Figure 1: Daily average abnormal returns around floor removal



Source: Authors' calculations based on DSE daily prices and DSEX returns

that the wider window CAARs are smaller in absolute value than some of the medium window estimates. Thus, the graph shows an initial downward correction and some price reversal.

An informationally efficient market should incorporate available information rapidly (Fama, 1970). The continuation of abnormal returns after the event indicates that adjustment was not instantaneous. The observed pattern is consistent with improved short-run price discovery after removal, but abnormal returns alone do not establish that overall market efficiency was restored.

4.3. Changes in Price-Rigidity and Intraday-Range Indicators

Table 3 reports the zero-return rate and average normalized intraday high-low range over the 20 trading days before and after each event. Event day 0 is excluded. These variables are interpreted as indicators of price rigidity and price flexibility, not as direct or comprehensive measures of market efficiency.

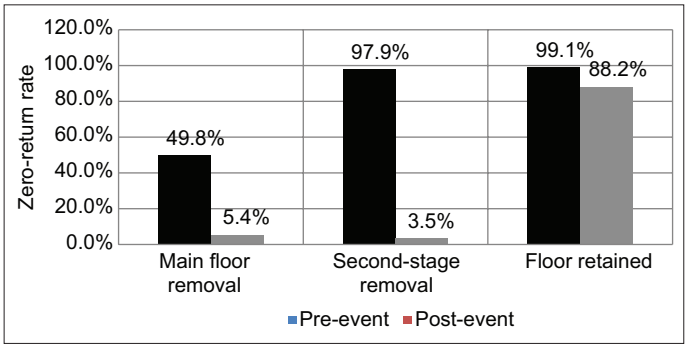
For the main cohort, the zero-return rate declined from 49.84% to 5.36%, a statistically significant reduction of 44.48% points. Before removal, roughly half of the available firm-day observations had unchanged closing prices; after removal, zero returns became uncommon. This pattern is consistent with substantially reduced price rigidity, although zero-return frequency may also reflect changes in liquidity and trading activity.

The decline was even larger for the second-stage cohort, whose zero-return rate fell from 97.85% to 3.50%. The high pre-event rate indicates that these prices had been almost completely immobilized. The sharp post-event reduction is strongly associated with the removal of the restriction, but it should not be interpreted as an isolated causal effect because of potential spillover from the first-stage event.

The retained-floor benchmark continued to report an exceptionally high zero-return rate. Its rate declined from 99.12% to 88.16%, but the change was not significant at the 5% level. The contrast is consistent with a removal-associated reduction in price rigidity among treated firms. However, the benchmark contains only 12 non-randomly selected firms, so it cannot by itself rule out differences in firm characteristics or establish causality.

Figure 2 illustrates that the post-event zero-return rates of the treatment cohorts were substantially lower than their pre-event rates, whereas the retained-floor benchmark remained highly inactive. The figure visually supports the paired-test results in Table 3 but does not provide a separate causal test.

Figure 2: Zero-return rates before and after floor removal



Source: Authors' calculations (Event day is excluded)

The average normalized high-low range increased from 2.09% to 5.17% for the main cohort and from 0.06% to 4.85% for the second-stage cohort. Both changes were significant at the 1% level. The retained-floor benchmark experienced a much smaller change that was not statistically significant at the 5% level.

A wider high-low range shows that prices varied more within the trading day, which is consistent with restored price flexibility after an artificially compressed period. However, the range is also a volatility measure. Its increase cannot independently demonstrate improved efficiency and instead indicates that greater price flexibility was accompanied by greater short-run price variation.

This is a central regulatory trade-off. A floor can suppress the volatility in prices, but not the information or selling pressure that causes the price decline. Rather, it postpones the adjustment (Chen et al., 2024; Wan and Zhang, 2022). As the restriction is lifted, the price swings become greater, reflecting the accumulated information.

4.4. Treatment-Benchmark Comparison

Table 4 compares mean CAARs for each treatment cohort with the retained-floor benchmark. The Welch test examines whether the two group means differ while allowing unequal variances and sample sizes. It does not control for non-random treatment assignment or other differences between the groups.

For the main cohort, treatment-benchmark differences were statistically significant over the first three windows. The treated firms recorded negative CAARs while the retained-floor firms recorded positive CAARs. Over [−10, +10], the difference became statistically insignificant, indicating that the comparative evidence is concentrated near the event and does not remain robust over the widest window.

The second-stage cohort differed significantly from the benchmark across all four windows. This indicates a larger and more persistent

Table 3: Pre- and post-event price-rigidity and intraday-range indicators

Cohort	Indicator	N	Pre-event mean (%)	Post-event mean (%)	Change	Paired t	P-value
Main floor removal	Zero-return rate	305	49.84	5.36	−44.48 pp	−17.781	<0.001
Main floor removal	Average high-low range	297	2.09	5.17	+3.09 pp	19.712	<0.001
Second-stage removal	Zero-return rate	20	97.85	3.50	−94.35 pp	−68.899	<0.001
Second-stage removal	Average high-low range	20	0.06	4.85	+4.79 pp	15.646	<0.001
Floor retained	Zero-return rate	12	99.12	88.16	−10.96 pp	−2.107	0.0589
Floor retained	Average high-low range	12	0.02	0.68	+0.67 pp	2.080	0.0616

pp denotes percentage points. The normalized high-low range is calculated as (High - Low)/close. The pre- and post-event periods contain 20 trading days each, excluding event day 0

Table 4: Treatment-versus-retained-floor CAAR comparisons

Treatment cohort	Window	Treatment CAAR (%)	Benchmark CAAR (%)	Difference	Welch P-value	Inference
Main removal	[−1, +1]	−3.34	1.43	−4.77 pp	<0.001	Statistically different
Main removal	[−3, +3]	−5.29	1.54	−6.83 pp	<0.001	Statistically different
Main removal	[−5, +5]	−6.77	3.07	−9.84 pp	<0.001	Statistically different
Main removal	[−10, +10]	−2.43	−0.85	−1.58 pp	0.1341	Not statistically different
Second-stage removal	[−1, +1]	−13.36	0.27	−13.63 pp	<0.001	Statistically different
Second-stage removal	[−3, +3]	−18.87	3.68	−22.54 pp	<0.001	Statistically different
Second-stage removal	[−5, +5]	−17.64	2.22	−19.86 pp	<0.001	Statistically different
Second-stage removal	[−10, +10]	−12.15	−1.45	−10.70 pp	0.0015	Statistically different

Difference equals treatment CAAR minus benchmark CAAR. Welch tests allow unequal cohort variances and sample sizes. Statistical significance indicates a difference in mean CAARs, not a causal effect of floor removal

mean market reaction among the second-stage firms. Nevertheless, the result cannot distinguish their own removal effect from spillover following the first-stage event and should be interpreted as a policy-associated difference rather than proof of causality.

The benchmark analysis is not a randomized causal design. The retained-floor group contains only 12 firms and may differ from the treatment cohorts in size, liquidity, index importance, or financial condition. Although the Welch test accommodates unequal variances and sample sizes, it does not address low statistical power, limited representativeness, selection bias, or unobserved firm differences. The benchmark results therefore provide supporting evidence only.

4.5. Robustness and Sample Sensitivity

Table 5 assesses whether the findings remain consistent when the expected-return model and sample convention are changed.

Under the market model, CAARs remain negative for both cohorts across all four windows. For the main cohort, the [−1, +1], [−3, +3], and [−10, +10] estimates are significant at the 5% level, whereas the [−5, +5] estimate is significant only at the 10% level. The second-stage estimates remain negative and significant across all windows. These results support the direction of the short-run market reaction, although they do not resolve common-event dependence.

The complete-window analysis provides a more cautious result. Negative and significant CAARs remain for both cohorts over the shorter windows, but sample size declines as the window expands. Over [−10, +10], the main-cohort CAAR reverses to +3.73%. The second-stage estimate is −5.27% and statistically insignificant, with only five firms retaining complete observations.

Accordingly, the findings are robust for the immediate short-run correction but not for the direction or persistence of the effect over the longest window. The wide-window reversal may reflect changing sample composition, missing observations, partial price reversal, or contamination by unrelated developments. Shorter windows therefore receive greater interpretive weight because they are closer to the event and retain substantially more firms.

4.6. Hypothesis Assessment and Overall Discussion

Table 6 summarizes the hypothesis assessments. H_1 is supported by significant negative abnormal returns around the removal dates. H_2 is supported by the substantial decline in zero-return frequency among released firms, which is consistent with reduced price rigidity. H_3 is supported by the significant increase in the intraday high-low range, indicating greater price variation and flexibility. These hypotheses concern observable price-adjustment indicators and do not collectively constitute a complete test of market efficiency.

Overall, the results show that mechanically unchanged prices began to move, zero-return observations declined sharply, and intraday price variation increased after floor removal. Taken together, these patterns are consistent with improved short-run price discovery and reduced price rigidity. They do not, however, directly demonstrate that the DSE became fully informationally efficient.

The improvement was qualified. Price adjustment continued for several trading days, the wider high-low range also represented greater short-run volatility, and the complete-window long-horizon estimates were sensitive to sample composition. In addition, conventional t-tests may be affected by common-event dependence, the retained-floor benchmark is small and

Table 5: Robustness and sample sensitivity results

Cohort	Return model	Sample convention	Window	N	CAAR (%)	P-value
Main removal	Market model	Available observations	[−1, +1]	306	−1.44	0.0266
Main removal	Market model	Available observations	[−3, +3]	306	−3.48	<0.001
Main removal	Market model	Available observations	[−5, +5]	306	−2.19	0.0865
Main removal	Market model	Available observations	[−10, +10]	306	−3.74	<0.001
Second-stage removal	Market model	Available observations	[−1, +1]	20	−13.45	<0.001
Second-stage removal	Market model	Available observations	[−3, +3]	20	−20.45	<0.001
Second-stage removal	Market model	Available observations	[−5, +5]	20	−18.64	<0.001
Second-stage removal	Market model	Available observations	[−10, +10]	20	−11.64	<0.001
Main removal	Market-adjusted	Complete window	[−1, +1]	279	−2.18	<0.001
Main removal	Market-adjusted	Complete window	[−3, +3]	255	−1.91	0.0039
Main removal	Market-adjusted	Complete window	[−5, +5]	236	−1.78	0.0444
Main removal	Market-adjusted	Complete window	[−10, +10]	216	3.73	<0.001
Second-stage removal	Market-adjusted	Complete window	[−1, +1]	19	−13.06	<0.001
Second-stage removal	Market-adjusted	Complete window	[−3, +3]	10	−13.56	0.0034
Second-stage removal	Market-adjusted	Complete window	[−5, +5]	9	−12.90	0.0130
Second-stage removal	Market-adjusted	Complete window	[−10, +10]	5	−5.27	0.2950

The market model uses a pre-event estimation period. Complete-window results include only firms with every return observation required by the event window

Table 6: Hypothesis assessment and overall interpretation

Hypothesis	Empirical indicator	Assessment	Interpretation
H_1	Negative abnormal returns following removal	Supported	Negative policy-associated repricing occurred around removal.
H_2	Reduction in the zero-return rate	Supported	Prices changed more frequently, consistent with reduced price rigidity.
H_3	Increase in the intraday high-low range	Supported	Intraday price flexibility and short-run volatility increased.

Source: Summary of the event-study and market-functioning indicators reported in Tables 2-5

non-random, and the second-stage estimates may contain spillover from the first-stage event.

The most defensible conclusion is therefore that floor removal was associated with restored price movement and improved short-run price discovery, not that complete market efficiency was restored. The evidence is strongest close to the policy event and weaker regarding the persistence and causal magnitude of the effect.

From a regulatory point of view, the results suggest that a price floor can lead to an initial decrease in visible volatility but also to price imbalances that are building up over time, to a decrease in the informational value of prices, and to a decrease in trading activity (Chen et al., 2024; Yang et al., 2025). If such measures are introduced during exceptional market stress, they should be temporary, transparent and supported by a clearly communicated exit strategy.

5. CONCLUSION

This study examined whether removing stock-price floors improved short-run price discovery and reduced price rigidity in the Dhaka Stock Exchange. The event-study results show significant negative abnormal returns around the removal dates, indicating that the affected securities experienced substantial downward repricing. When considered with the reduction in zero-return frequency, this pattern is consistent with the release of price pressure that had accumulated while the floors were binding.

Zero-return rates declined sharply after removal, showing that unchanged closing prices became much less frequent. Intraday high-low ranges also increased, indicating greater price flexibility. However, a wider range also represents higher short-term volatility. Abnormal returns, zero-return frequency, and intraday ranges are therefore interpreted as complementary proxy indicators of short-run market functioning rather than direct measures of complete market efficiency.

The evidence is strongest over the shorter event windows. Alternative expected-return estimates generally preserve the negative short-run reaction, but the complete-window result reverses for the main cohort over [−10, +10], while the second-stage estimate becomes insignificant with only five firms. The findings are therefore robust regarding the immediate correction but not regarding its longer-window persistence.

The retained-floor firms provide a relevant but limited benchmark because the group contains only 12 non-randomly selected securities. Significant Welch-test differences show that mean CAARs differed across groups, but they do not establish causality. The second-stage results may also combine the direct effect of its own removal with spillover from the first-stage reaction. Accordingly, the evidence supports a policy-associated improvement in short-run price discovery, not a definitive causal claim that overall market efficiency was restored.

From a policy perspective, price floors may temporarily suppress visible declines but can delay price adjustment and contribute to accumulated selling pressure. Regulators should therefore use such restrictions only as temporary measures during exceptional market conditions and support them with a transparent exit strategy, clear investor communication, and effective market surveillance.

The study is limited by the small retained-floor benchmark, possible cross-sectional dependence among firms exposed to the same event, overlap between the two removal stages, and the absence of bid-ask spreads, trading value, and order-book data. Future research could use intraday data, cross-correlation-adjusted event-study tests, matched samples, or a difference-in-differences design to assess liquidity, price-discovery speed, and causal effects more directly.

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