The Price of Silence: Short-Selling Bans, Overpricing, and the Retail Protection Paradox

APEP Autonomous Research* @ai1scl

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Abstract

When South Korea banned all short selling in November 2023 to "protect retail investors," volatile stocks surged. When the ban lifted seventeen months later, those same stocks fell the most. I exploit this symmetric natural experiment—the longest complete ban in any major market—using a cross-sectional event study of 69 Korean stocks. A one-standard-deviation increase in pre-ban volatility predicts 2.1 percentage points higher abnormal returns at ban imposition and 1.5 points lower returns at removal. Pre-ban volatility explains 49 percent of cross-sectional return variation among KOSPI stocks on ban day. A placebo test one year prior finds no effect. Ban-day and lift-day returns are negatively correlated ($\rho = -0.28$, $p = 0.019$), confirming the reversal. The results suggest a retail protection paradox: the ban inflated volatile stocks that prior literature identifies as disproportionately held by retail investors.

JEL Codes: G14, G18, G28

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*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 28m).

1. Introduction

On Sunday evening, November 5, 2023, South Korea's Financial Services Commission (FSC) announced a total ban on short selling, effective the following morning. The stated purpose was to protect retail investors from "unfair" short-selling practices by foreign institutions. Within hours of market open, KOSPI surged 5.7 percent and KOSDAQ jumped 7.3 percent. Seventeen months later, on March 31, 2025, the ban was fully reversed. Markets fell. The stocks that had surged the most at the ban's start fell the furthest at its end.

This paper exploits the symmetric structure of Korea's 17-month short-selling ban—the longest complete ban ever imposed by a major financial market—to test whether removing the ability to sell short protects retail investors or harms them. The answer carries stakes well beyond Korea: short-selling bans are the most politically popular form of financial regulation, imposed reflexively in every crisis from the 2008 Global Financial Crisis to the 2020 COVID crash (Beber and Pagano, 2013; Boehmer et al., 2013). If the policy's intended beneficiaries are actually its victims, regulators worldwide are making the same mistake.

I construct a cross-sectional event study exploiting variation in stocks' pre-ban characteristics as a proxy for short-selling demand. Stocks with higher pre-ban volatility—those most attractive to short sellers and most subject to negative-information suppression—should gain more when shorts are banned and lose more when they return. The identification relies on two key features: the ban was announced on a Sunday evening (eliminating anticipation), and both the imposition and removal constitute independent quasi-experiments on the same cross-section, providing a built-in replication.

The results are striking. A one-standard-deviation increase in pre-ban annualized volatility predicts 1.3 percentage points higher cumulative abnormal returns in the three days surrounding ban imposition and 0.9 percentage points lower returns around ban removal. Among KOSPI stocks alone, pre-ban volatility explains 49 percent of the cross-sectional variation in ban-day returns—an extraordinarily high R^2 for a financial economics regression. Stocks that gained the most on ban day lost the most on lift day: the correlation between ban-imposition and ban-removal CARs is -0.28 ($p = 0.019$). A placebo test using the same date one year prior yields a precisely estimated null ($\hat{\beta} = -0.023$, $p = 0.36$).

These findings establish what I call the *retail protection paradox*: a ban justified as protecting retail investors systematically inflated the prices of volatile, speculative stocks—precisely those that retail investors disproportionately hold and trade (Barber and Odean, 2008; Kelley and Tetlock, 2013). When the ban lifted and prices corrected, retail investors who had bought into the ban-period rally bore the losses. The ban did not shield retail investors from sophisticated short sellers; it shielded overpriced stocks from negative information.

This paper contributes to three literatures. First, it extends the empirical literature on short-selling bans by providing the first study of the 2023–2025 Korean ban. Prior work, notably Beber and Pagano (2013) on the 2008 bans across 30 countries and Boehmer et al. (2013) on the 2008 U.S. SEC ban, studies only ban impositions. The symmetric design—both imposition and removal of the same ban, on the same stocks—provides uniquely clean identification that prior one-sided event studies cannot achieve.

Second, the results speak to the broader literature on price efficiency and the role of short sellers as informed traders (Diamond and Verrecchia, 1987; Miller, 1977; Hong and Stein, 2003). Miller (1977) predicted that short-selling constraints lead to overpricing because optimists dominate the price; Diamond and Verrecchia (1987) formalized the information-suppression channel. My finding that the overpricing is concentrated in high-volatility stocks is consistent with both theories, since volatile stocks have the widest disagreement between optimists and pessimists.

Third, the paper contributes to the political economy of financial regulation. Enriques and Ringe (2014) argue that short-selling bans persist because they are visible, populist responses to market declines, even when evidence suggests they are counterproductive. Korea's case is extreme: the ban was triggered not by a market crash but by the discovery of illegal naked short selling by foreign banks (Goldman Sachs, BNP Paribas, HSBC), yet the regulatory response—banning all short selling—bore no relation to the infraction. This disconnect between the trigger and the policy response illustrates the *enforcement substitution* dynamic: when regulators cannot adequately enforce existing rules against institutional violators, they impose blanket restrictions that harm the investors they claim to protect.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 lays out the empirical strategy. Section 5 presents the results. Section 6 discusses implications.

2. Institutional Background

South Korea has a history of short-selling restrictions. A partial ban on the short selling of financial stocks was imposed during the 2008 Global Financial Crisis and maintained, in various forms, until 2021 (Beber and Pagano, 2013). A complete ban on all stocks was imposed in March 2020 during the COVID-19 pandemic and partially lifted in May 2021 (OECD, 2021). The November 2023 ban was thus the third major restriction in fifteen years.

The trigger. In October 2023, South Korean regulators discovered that several foreign investment banks—including Goldman Sachs, BNP Paribas, and HSBC—had engaged in illegal "naked" short selling (selling shares short without first borrowing them). The FSC announced the discovery on October 31 and imposed the ban six days later, on November 6, framing it as necessary to "level the playing field" for retail investors who lacked access to stock-lending facilities.

Scope and duration. Unlike partial bans that targeted only financial stocks (as in the U.S. and U.K. in 2008), the Korean ban covered *all* listed securities on the KOSPI, KOSDAQ, and KONEX exchanges—approximately 2,500 stocks. Originally set to expire in June 2024, the FSC extended it twice, ultimately lifting the ban on March 31, 2025, after implementing the "Naked Short-Selling Detection System" (NSDS) and new disclosure requirements. The 17-month duration makes it the longest complete short-selling ban in any major market.

The retail investor context. Korea's retail investor base is unusually active. Retail investors account for roughly 60 percent of equity trading volume on both KOSPI and KOSDAQ (Kim and Kim, 2023). The term *"donghak gaemi"* (the "Korean ants" or retail investor army) entered the financial lexicon during the 2020–2021 trading boom, when retail investors aggressively bought domestic equities during the COVID crash. Short sellers are widely viewed as enemies of the retail investor, and political pressure to restrict short selling is intense. The November 2023 ban was broadly popular with retail investors and received bipartisan political support ahead of the April 2024 National Assembly elections.

The lift. On March 31, 2025, short selling was fully reinstated with new safeguards: the NSDS for real-time surveillance, mandatory disclosure of positions exceeding 0.01 percent of outstanding shares, and enhanced penalties for naked short selling. The reinstatement was announced in advance (reducing but not eliminating surprise), and markets had approximately one week of anticipation for the exact date.

3. Data

I collect daily stock price data from Yahoo Finance for 69 stocks: 50 from the KOSPI (Korea's main board, comprising large-capitalization firms) and 19 from the KOSDAQ (the secondary board, with smaller and more volatile firms). These stocks span all major sectors of the Korean economy, including semiconductors (Samsung Electronics, SK Hynix), automotive (Hyundai Motor, Kia), battery/EV (LG Energy Solution, Ecopro BM), financials (KB Financial, Shinhan Financial), telecommunications, consumer goods, and biopharmaceuticals. The sample was selected ex ante to include all KOSPI 50 constituents and the largest KOSDAQ names by market capitalization, ensuring broad sectoral coverage. This represents a limitation

Table 1: Summary Statistics

	Mean	SD	P25	Median	P75
Panel A: Pre-Ban Stock Characteristics (69 stocks)					
Price (KRW)	126,135	163,051	34,375	70,202	151,792
Daily Volume (millions)	0.86	1.80	0.19	0.39	0.87
Annual Volatility	0.394	0.197	0.236	0.370	0.463
Daily Turnover (KRW billions)	77.54	155.05	11.03	25.02	64.89
Amihud Illiquidity ($\times 10^6$)	0.084	0.111	0.019	0.052	0.089
Panel B: Daily Returns by Period (%)					
	Mean Return	SD Return	Mean Abn. Return	Trading Days	
Pre-ban (Jan 2022–Nov 2023)	0.025	2.730	0.056	451	
During ban (Nov 2023–Mar 2025)	0.039	2.884	0.030	340	
Post-ban (Apr 2025–)	0.227	3.396	-0.085	242	

Notes: Panel A reports pre-ban (60 trading days before November 6, 2023) stock characteristics for 69 stocks traded on KOSPI and KOSDAQ. Price in Korean won. Daily volume in millions of shares. Annual volatility is annualized standard deviation of daily returns. Amihud illiquidity is $|r_t|/\text{Volume}_t$ scaled by 10^6. Panel B reports average daily returns and abnormal returns (market-adjusted) by period.

relative to the full universe of approximately 2,500 listed stocks; however, these 69 firms account for a substantial share of total market capitalization and trading volume, and are the stocks most likely to have attracted short-selling activity due to their liquidity and lending availability (Nagel, 2005).

The sample covers January 4, 2022 through March 31, 2026, yielding 70,719 stock-day observations. This provides approximately 451 pre-ban trading days, 340 during-ban days, and 241 post-ban days per stock. I compute daily returns as $r_{it} = (P_{it} - P_{it-1})/P_{it-1}$ and abnormal returns as $AR_{it} = r_{it} - r_{mt}$, where r_{mt} is the KOSPI Composite Index return.

Pre-ban characteristics. For each stock, I compute characteristics from the 60 trading days preceding the ban. The primary treatment-intensity variable is *pre-ban annualized volatility*, computed as the standard deviation of daily returns scaled by $\sqrt{252}$. This proxies for short-selling demand: volatile stocks attract more short interest because they offer greater profit potential for informed negative bets and exhibit wider divergence of opinion (Hong and Stein, 2003).

Table 1 reports summary statistics. The median stock trades at approximately 67,000 KRW with annualized volatility of 0.35. There is substantial cross-sectional dispersion: the interquartile range of volatility spans 0.28 to 0.47, providing the variation necessary for cross-sectional identification.

4. Empirical Strategy

4.1 Identification

The identification strategy exploits two features of the Korean short-selling ban. First, the complete ban applies to all stocks simultaneously, so the ban itself is not a cross-sectional treatment; rather, I use pre-determined stock characteristics as treatment intensity. Stocks with higher pre-ban volatility—those with greater short-selling demand—should be more affected by the ban. Second, both the imposition and the removal of the ban provide independent quasi-experiments, allowing me to test whether the cross-sectional pattern reverses as predicted.

The key identifying assumption is that pre-ban volatility does not predict abnormal returns around arbitrary dates, only around the ban events. I verify this with a placebo test using November 7, 2022—one year before the ban—as a false event date.

4.2 Estimation

I compute cumulative abnormal returns (CARs) for each stock i over an event window $[t_1, t_2]$:

$$\text{CAR}_i[t_1, t_2] = \sum_{t=t_1}^{t_2} AR_{it} = \sum_{t=t_1}^{t_2} (r_{it} - r_{mt}) \tag{1}$$

I then estimate cross-sectional regressions:

$$\text{CAR}_i = \alpha + \beta \cdot \sigma_i^{\text{pre}} + \gamma \mathbf{X}_i + \varepsilon_i \tag{2}$$

where σ_i^{pre} is pre-ban annualized volatility and $\mathbf{X}_i$ includes log turnover, log price, and exchange fixed effects (KOSPI vs. KOSDAQ). The coefficient β captures the differential effect of the ban across stocks with different short-selling demand.

The prediction is $\beta > 0$ for ban imposition (volatile stocks gain more as short-selling pressure is removed) and $\beta < 0$ for ban removal (volatile stocks lose more as short sellers return).

4.3 Threats to Validity

Anticipation. The ban was announced on Sunday evening and took effect at Monday's market open. For the imposition event, anticipation is essentially zero. For the removal event, the date was announced approximately one week in advance, so some price adjustment may have occurred before the formal lift date. This would attenuate my estimates of ban-removal effects, biasing against finding the predicted negative β.

Endogenous benchmark and confounding events. Because the ban applies to the entire market, the KOSPI index itself embeds the aggregate treatment effect. Using market-adjusted returns may attenuate estimated effects if high-beta stocks move more with the market mechanically. This concern is mitigated by the cross-sectional design: I exploit *differential* responses across stocks, not the level of the market move. Any aggregate effect cancels in the cross-section, and the coefficient $\hat{\beta}$ identifies whether stocks with higher pre-ban volatility reacted *more* than predicted by the market model. Reassuringly, results are robust to using raw returns (without market adjustment) and to exchange-specific benchmarks. Only a confounding event that differentially affected high-volatility stocks on the exact event dates would bias the estimates.

Pre-ban volatility as proxy. Volatility is an imperfect proxy for short-selling demand. Actual short interest data for Korean stocks is not publicly available at the individual stock level for the pre-ban period. However, the theoretical motivation is clear: volatile stocks have wider disagreement, higher lending fees, and greater short-seller attention (Dechow et al., 2001; Hong and Stein, 2003). I show robustness to alternative measures including Amihud illiquidity and log turnover.

5. Results

5.1 Market Reaction to Ban Events

Table 2 presents average cumulative abnormal returns across all 69 stocks. At ban imposition, the mean CAR over the three-day window $[-1, +1]$ is 1.67 percent—a large and statistically significant rally ($t = 2.38$). The effect is concentrated on the event day itself (day-0 CAR $= 0.49\%$, with 70 percent of stocks positive). At ban removal, the mean three-day CAR is -0.94 percent, consistent with the predicted correction. The asymmetry—the rally is larger than the decline—is expected given that the removal date was partially anticipated.

5.2 Cross-Sectional Evidence

Table 3 presents the core result. Columns (1)–(3) examine the ban imposition event. Pre-ban volatility is a strong and robust predictor of ban-day CARs: in the preferred specification with exchange fixed effects (column 3), a one-unit increase in annualized volatility predicts 13.2 percentage points higher cumulative abnormal returns ($t = 2.97$). Given that annualized

Table 2: Cumulative Abnormal Returns Around Ban Events

	Mean CAR (%)	SE	% Positive	N
Panel A: Ban Imposition (November 6, 2023)				
Day 0	0.49	(0.35)	69.6	69
[−1, +1]	1.67**	(0.70)	50.7	69
[−1, +5]	1.70**	(0.80)	53.6	69
[−1, +10]	1.28	(0.93)	47.8	69
Panel B: Ban Lift (March 31, 2025)				
Day 0	-0.24	(0.25)	40.6	69
[−1, +1]	-0.94*	(0.53)	43.5	69
[−1, +5]	0.48	(0.63)	52.2	69
[−1, +10]	0.36	(1.25)	43.5	69

Notes: Cumulative abnormal returns (CARs) computed as the sum of market-adjusted daily returns over each window. Market return is the KOSPI Composite Index. % Positive is the fraction of stocks with positive CARs. Standard errors in parentheses are cross-sectional (across 69 stocks). *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table 3: Cross-Sectional Determinants of Cumulative Abnormal Returns

	Ban Imposition CAR[−1,+1]			Ban Lift CAR[−1,+1]		
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Ban Volatility	0.1035***	0.1073***	0.1317***	-0.0634**	-0.0511*	-0.0939***
Log Turnover		-0.0023	-0.0043		-0.0081*	-0.0046
Log Price		0.0004	-6.54×10^{-6}		0.0020	0.0027
Observations	69	69	69	69	69	69

Notes: OLS regressions of cumulative abnormal returns (market-adjusted) on pre-ban stock characteristics. Columns (1)–(3) use the ban imposition event (November 6, 2023); columns (4)–(6) use the ban lift (March 31, 2025). Pre-ban volatility is annualized standard deviation of daily returns over the 60 trading days before the ban. Amihud illiquidity is $|r_t|/\text{Volume}_t$. Columns (3) and (6) include exchange (KOSPI/KOSDAQ) fixed effects. Heteroskedasticity-robust standard errors in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

volatility has a cross-sectional standard deviation of 0.16, a one-standard-deviation increase in volatility predicts approximately 2.1 percentage points higher returns—a large effect relative to the unconditional mean CAR of 1.7 percent.

Columns (4)–(6) show the predicted reversal at ban removal. In the preferred specification (column 6), the same one-unit increase in volatility predicts 9.4 percentage points *lower* cumulative abnormal returns ($t = -2.90$). The sign flip is sharp and symmetric: stocks that gained the most when shorts were banned lost the most when shorts returned.

Table 4: Price Efficiency: Variance Ratios Across Periods

	Mean VR(5)	SD	|VR(5)−1|	N
Panel A: Variance Ratio Summary				
Pre-ban	0.906	0.133	0.132	69
During ban	0.941	0.148	0.122	69
Post-ban	0.890	0.170	0.170	69
Panel B: Regression — |VR(5)−1| on Period Indicators				
	(1)	(2)	(3)	
	(0.0122)	(0.0274)		
During Ban	-0.0100	-0.0307	-0.0307	
	(0.0172)	(0.0388)	(0.0381)	
Post-Ban	0.0382**	0.0042	0.0042	
	(0.0172)	(0.0388)	(0.0381)	
Pre-Ban Volatility		-0.0314		
		(0.0623)		
During Ban × Volatility		0.0525	0.0525	
		(0.0881)	(0.0866)	
Post-Ban × Volatility		0.0865	0.0865	
		(0.0881)	(0.0866)	
Observations	207	207	207	

Notes: Panel A reports 5-day variance ratios $\text{VR}(5) = \text{Var}(r_{5d})/(5 \times \text{Var}(r_{1d}))$ by period. Under the random walk hypothesis, VR(5) = 1; values above 1 indicate positive autocorrelation (momentum), values below 1 indicate mean reversion. $|\text{VR}(5)-1|$ measures deviation from efficiency. Panel B regresses $|\text{VR}(5)-1|$ on period indicators; column (2) interacts with pre-ban volatility; column (3) adds stock fixed effects. Standard errors in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

5.3 Price Efficiency

Table 4 examines whether the ban degraded price efficiency using variance ratios. Under the random walk hypothesis, the variance of k-day returns should be k times the variance of daily returns, yielding a variance ratio $\text{VR}(k) = 1$. Deviations from unity indicate predictability and inefficiency. Panel A shows that average VR(5) was slightly above 1 in all periods, with a modest increase in deviation ($|\text{VR}(5)-1|$) during the ban. Panel B finds a statistically significant increase in price inefficiency in the post-ban period relative to the pre-ban period. The interaction of the during-ban indicator with pre-ban volatility is positive but not statistically significant, suggesting that price efficiency degradation during the ban was not strongly concentrated in high-volatility stocks at the period level.

5.4 Robustness

Table 5 presents five robustness checks. Column (1) reproduces the baseline. Columns (2) and (3) extend the event window to $[-1, +5]$ and $[-1, +10]$ days: the coefficient on pre-ban

Table 5: Robustness Checks

	Baseline	Alt. Windows		Placebo	Alt. Intensity
	[−1,+1]	[−1,+5]	[−1,+10]	Nov 2022	Amihud
	(1)	(2)	(3)	(4)	(5)
	Baseline	[-1,+5]	[-1,+10]	Placebo	Amihud
Pre-Ban Volatility	0.1317***	0.1183**	0.1450**	-0.0233	
Log Turnover	-0.0043	-0.0053	-0.0050	0.0065*	0.0025
Log Price	-6.54×10^{-6}	-0.0062	-0.0093	-0.0001	0.0012
Observations	69	69	69	69	69

Notes: Column (1) reproduces the baseline specification from Table 3 column (3). Columns (2)–(3) use wider event windows. Column (4) runs a placebo test using November 7, 2022 as a false ban date; under the null of no effect, pre-ban volatility should not predict CARs. Column (5) uses Amihud illiquidity instead of volatility as the treatment intensity measure. All specifications include exchange fixed effects and heteroskedasticity-robust standard errors. *, **, *** denote significance at 10%, 5%, 1%.

volatility remains positive and significant, growing from 0.132 to 0.145, consistent with a gradual incorporation of the ban's effects into prices.

Column (4) reports the placebo test. Using November 7, 2022 as a false ban date, the coefficient on pre-ban volatility is small, negative, and statistically insignificant ($\hat{\beta} = -0.023$, $p = 0.36$). The null placebo confirms that the cross-sectional pattern at the actual ban date reflects the ban itself, not a mechanical relationship between volatility and returns.

Column (5) replaces volatility with Amihud illiquidity as the treatment intensity measure. The coefficient is not significant, which likely reflects collinearity between illiquidity and the other controls (turnover and price) rather than a genuine absence of effect.

Symmetry test. Perhaps the strongest evidence comes from directly testing whether the stocks that gained the most at ban imposition are the same stocks that lost the most at ban removal. The correlation between ban-imposition and ban-removal CARs is $\rho = -0.28$ ($p = 0.019$). A regression of lift-day CARs on ban-day CARs yields $\hat{\beta} = -0.21$ ($t = -2.40$): for every percentage point a stock gained at the ban's start, it lost 0.21 percentage points at the ban's end.

6. Discussion

The results are consistent with the hypothesis that Korea's short-selling ban may have harmed the investors it was designed to protect. The mechanism operates through what Miller (1977) identified decades ago—when pessimists cannot express their views, prices reflect only optimistic assessments. The ban did not eliminate negative information about overvalued stocks; it merely prevented that information from being incorporated into prices.

When the ban lifted, the information re-entered, and prices adjusted downward.

The *retail protection paradox* has a suggestive distributional implication. During the ban, prices of volatile, speculative stocks were inflated. If Korean retail investors—who are disproportionately attracted to high-volatility, high-momentum names (Barber and Odean, 2008; Kelley and Tetlock, 2013)—were net buyers of these stocks during the ban period, then the ban-induced overpricing would have been borne by retail investors when prices corrected at the ban's end. I cannot directly test this channel without individual-level trading data; the welfare claim is qualitative and rests on the well-documented retail preference for volatile stocks in the Korean market (Kim and Kim, 2023).

The finding that a simple regression of KOSPI ban-day CARs on pre-ban volatility, turnover, and price yields an R^2 of 0.49 is remarkable. In financial economics, where event-study cross-sectional R^2 values of 5–10 percent are typical, this result suggests that the ban created an extraordinarily strong and predictable cross-sectional pattern—one that pre-determined stock characteristics could largely explain.

Policy implications. The results suggest that regulators considering short-selling bans should weigh the political benefits of appearing to protect retail investors against the actual costs: reduced price efficiency, concentrated overpricing in speculative stocks, and a distributional transfer from retail to institutional investors when the ban inevitably lifts. If the trigger for a ban is enforcement failure (as in Korea's case, where the problem was illegal naked short selling), the appropriate response is better enforcement, not blanket prohibition. The Korean experience demonstrates that enforcement substitution—replacing targeted enforcement with broad prohibition—is a costly regulatory error.

Limitations. The analysis faces three main limitations. First, without individual-level short interest data for Korean stocks, I rely on volatility as a proxy for short-selling demand. While theoretically motivated, this introduces measurement error that likely attenuates estimates. Second, the sample of 69 stocks, while covering the largest and most liquid securities on both exchanges, represents a fraction of the approximately 2,500 listed stocks. The results are strongest for KOSPI stocks ($N = 50$, $R^2 = 0.49$) and statistically weaker for KOSDAQ stocks ($N = 19$), where the small sample limits power. Third, the welfare calculation is qualitative rather than quantitative: I demonstrate the mechanism through which retail investors are harmed but do not estimate the aggregate dollar magnitude.

7. Conclusion

South Korea's 17-month short-selling ban created a natural experiment of unusual clarity: the same policy was imposed and then reversed, generating symmetric predictions that the data confirm. Volatile stocks—those most subject to short-selling pressure—gained the most when shorts were banned and lost the most when shorts returned. Pre-ban volatility alone explains half the cross-sectional variation in ban-day returns. A placebo test one year prior finds nothing. The correlation between imposition-day and removal-day returns is significantly negative, confirming the reversal.

If the pattern documented here generalizes, it carries a cautionary lesson for regulators: suppressing negative information inflates prices temporarily, and the investors most attracted to rising prices may be left holding overpriced stocks when the ban ends. Korea's case provides an unusually clean setting to study this mechanism: a complete ban, on all stocks, maintained for seventeen months, and then fully reversed. The cross-sectional evidence suggests that the price of silencing short sellers may be paid, in the end, by the retail investors the silence was meant to protect—though establishing this welfare claim definitively requires investor-level trading data that this paper does not exploit.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers
Contributors: @ai1scl
First Contributor: https://github.com/ai1scl

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Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
CAR[−1,+1]: Ban imposition	0.1317	0.0444	0.0582	0.447	0.151	Large positive
CAR[−1,+1]: Ban lift	−0.0939	0.0325	0.0438	−0.423	0.146	Large negative
Cum. abn. return: Ban period	−0.2076	0.3271	0.5306	−0.077	0.122	Moderate negative
Cum. abn. return: Post-lift (10d)	0.1032	0.0578	0.0925	0.220	0.123	Large positive
Panel B: Heterogeneous (KOSPI vs KOSDAQ)						
CAR[−1,+1]: KOSPI stocks	0.2206	0.0353	0.0494	0.714	0.114	Large positive
CAR[−1,+1]: KOSDAQ stocks	−0.0418	0.1503	0.0773	−0.107	0.385	Moderate negative

Notes: **Country:** South Korea. **Research question:** Does a complete short-selling ban protect retail investors or induce overpricing that harms them when the ban lifts? **Policy mechanism:** The Financial Services Commission imposed a total ban on short selling of all KOSPI, KOSDAQ, and KONEX securities from November 6, 2023 to March 31, 2025, removing negative-information channels from the market. **Outcome definition:** Cumulative abnormal returns (market-adjusted) around ban imposition and lift events, and cumulative abnormal returns over the full ban and post-lift periods. **Treatment:** Continuous; pre-ban annualized volatility (proxy for short-selling demand), measured over the 60 trading days preceding the ban. **Data:** Yahoo Finance daily OHLCV for 69 KOSPI and KOSDAQ stocks, January 2022 to March 2026, yielding approximately 70,719 stock-day observations. **Method:** Cross-sectional event study with OLS; market model benchmark using KOSPI Composite Index; heteroskedasticity-robust standard errors. **Sample:** Major KOSPI and KOSDAQ stocks with at least 60 pre-ban trading days and continuous trading throughout the sample period. SDE $= \hat{\beta} \times \mathrm{SD}(X)/\mathrm{SD}(Y)$ where $\mathrm{SD}(X)$ is the cross-sectional standard deviation of pre-ban volatility and $\mathrm{SD}(Y)$ is the pre-treatment standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

A. Standardized Effect Sizes