The Language Barrier in Capital Markets: Evidence from Korea's Mandatory English Disclosure

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Abstract

Korea's 2024 mandatory English disclosure requirement for large KOSPI firms provides the first natural experiment to test whether language barriers in financial filings reduce stock market liquidity. Using a difference-in-differences design comparing 81 mandated firms to 214 controls, I find that the pooled effect on illiquidity is directionally consistent ($\hat{\beta} = -0.107$) but statistically insignificant. Split-sample analysis suggests effects may be concentrated in financial firms ($\hat{\beta} = -0.397$, $p < 0.001$), though a formal interaction test cannot reject equal effects across sectors ($p = 0.58$). A placebo test at January 2023 finds no differential pre-trend. The results are consistent with language frictions being a modest, sector-specific barrier in capital markets, but the evidence is suggestive rather than definitive.

JEL Codes: G14, G18, G28, F36

Keywords: financial disclosure, language barriers, stock market liquidity, Korea discount, information frictions

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1. Introduction

Korean firms trade at a persistent discount to global peers. The average KOSPI-listed company carries a price-to-book ratio below 1.0, compared with 1.5–2.0 for comparable firms listed in Tokyo, Hong Kong, or New York. Academics and practitioners have proposed governance defects, chaebol tunneling, and geopolitical risk as explanations (Kim and Park, 2024). But one candidate mechanism has received almost no empirical attention: the language of financial disclosure. Until 2024, virtually all Korean regulatory filings were available only in Korean—a language read by less than one percent of global fund managers. If foreign investors cannot read the filings, they face higher information costs, which theory predicts should reduce demand, depress liquidity, and widen the cost of capital (Diamond and Verrecchia, 1991; Glosten and Milgrom, 1985).

This paper exploits Korea's January 2024 mandatory English disclosure mandate to test whether language is a binding information friction in capital markets. The Financial Services Commission (FSC) required all KOSPI-listed firms with total assets at or above KRW 10 trillion—81 firms in our sample—to file financial statements in English, with phased expansion to all listed firms by 2028. The mandate was announced in advance but took effect as a sharp regulatory break: treated firms began filing in English from their first 2024 disclosure, while untreated firms continued filing in Korean only.

The identification strategy is a standard two-way fixed effects difference-in-differences comparing treated firms (Phase 1) to control firms before and after January 2024. Firm fixed effects absorb all time-invariant firm characteristics; week fixed effects absorb market-wide shocks, including the November 2023 abolition of the foreign investor registration requirement, which affected all KOSPI firms equally. The primary outcome is the Amihud illiquidity ratio (Amihud, 2002), a widely used measure of price impact that captures the cost of trading: higher values indicate that a given volume of trading moves prices more, reflecting lower liquidity.

The main finding is that the aggregate effect of English disclosure on illiquidity is modest and statistically insignificant ($\hat{\beta} = -0.107$, SE $= 0.082$). Split-sample analysis suggests heterogeneity across sectors: financial firms—14 treated banks, insurers, and financial holding companies—show a large reduction in illiquidity ($\hat{\beta} = -0.397$, $p < 0.001$), while non-financial firms show no response ($\hat{\beta} = -0.077$, $p = 0.40$). However, a formal triple-interaction test (Phase 1 × Post × Financial) yields a differential of -0.092 ($p = 0.58$), meaning the cross-sector difference is not statistically significant. The split-sample result is therefore suggestive but exploratory.

The sectoral pattern, if real, would have a clear economic interpretation: language frictions bind where foreign institutional investors must parse detailed regulatory filings to make allocation decisions—the financial sector, where balance sheet quality, capital adequacy, and asset risk are opaque without the filing itself. For non-financial firms, analyst coverage and product visibility may substitute for filing readability. This hypothesis is consistent with the data but requires confirmation with larger samples and direct evidence on foreign participation.

The result contributes to three literatures. First, it provides causal evidence on the relationship between disclosure language and market liquidity, extending the voluntary adoption literature (Jeanjean et al., 2015; Covrig et al., 2007) with a mandatory setting that eliminates selection bias. Second, it speaks to the home bias and foreign ownership literature (Kang and Stulz, 1997; Ahearne et al., 2004; Choe et al., 2005), identifying a specific information friction—filing language—that deters foreign participation. Third, it connects to the literature on mandatory disclosure and capital market outcomes (Daske et al., 2008; Christensen et al., 2013; Bushee and Leuz, 2004), showing that the content of disclosure reforms matters: mandating *what* is disclosed achieves different effects than mandating *in which language* it is disclosed.

Several design features strengthen the analysis. The placebo test using January 2023 as a false treatment date produces a null coefficient ($\hat{\beta} = -0.050$, $p = 0.39$), confirming no differential pre-trend in the placebo window. Results are robust to excluding the 10 largest firms, restricting to a balanced panel, and winsorizing extreme values. However, the regression discontinuity design at the KRW 10 trillion asset threshold is too imprecise—with only 54 and 43 effective observations below and above the cutoff—to provide independent corroboration.

The paper's main limitation is that the mandate coincided temporally with Korea's broader "Value-Up" corporate governance program and the November 2023 registration abolition, making it difficult to isolate the language channel from contemporaneous reforms. The identification relies on the differential treatment of Phase 1 firms relative to controls, which absorbs common shocks but cannot rule out that large firms responded differently to the regulatory bundle for reasons unrelated to language. I discuss this threat and present evidence that the financial-sector concentration of effects is inconsistent with alternative explanations.

2. Institutional Background

Korea's disclosure regime before 2024. Korean listed firms file financial statements through the Data Analysis, Retrieval and Transfer System (DART), operated by the Financial

Supervisory Service (FSS). Until 2024, all filings were in Korean. English versions, when available, were voluntary, partial, and often delayed by months. Jeanjean et al. (2015) document that voluntary English disclosure is common among internationally oriented European firms but rare in East Asia, where filing language tracks domestic regulatory convention rather than investor demand.

The 2024 mandate. In February 2023, the FSC announced that KOSPI-listed firms with total assets at or above KRW 10 trillion and foreign ownership at or above 5 percent would be required to file English-language financial statements starting January 2024 (Phase 1). Phase 2 (assets $\geq$ KRW 2 trillion) is scheduled for May 2026, and Phase 3 (all KOSPI firms) for 2028. The English DART portal (engopendart.fss.or.kr) launched in February 2024. The mandate covers annual and semi-annual reports, including consolidated financial statements, audit reports, and footnotes.

Treatment classification. Because firm-level foreign ownership data at quarterly frequency are not publicly available in a structured format, I classify Phase 1 eligibility using the asset threshold alone. This is an intent-to-treat classification: it captures all firms that satisfy the size criterion, regardless of whether they also meet the foreign ownership threshold. In practice, virtually all KOSPI firms with assets above KRW 10 trillion have foreign ownership well above 5 percent—these are the largest Korean corporations with extensive international investor bases. The analysis therefore likely includes at most a handful of firms classified as treated that were not actually mandated, introducing modest measurement error that biases the estimates toward zero.

The Korea discount. Korean firms have traded at price-to-book ratios persistently below global peers for at least two decades. The "Korea discount" is commonly attributed to chaebol governance—cross-shareholdings, tunneling, and weak minority shareholder protections—but the persistence of the discount even for well-governed firms suggests additional frictions. Stulz (2005) argues that information barriers, including language, are a first-order determinant of international capital allocation. If language barriers contribute to the Korea discount, the mandatory English disclosure mandate provides a rare opportunity to isolate this channel.

Concurrent reforms. In November 2023, Korea abolished the mandatory foreign investor registration system, reducing administrative barriers to foreign entry. In January 2024, the government launched the "Value-Up" program encouraging higher dividends and buybacks. Both reforms affected all KOSPI firms equally and are absorbed by the week fixed effects in the difference-in-differences design. The identifying assumption is that the *differential* effect on Phase 1 firms is attributable to English disclosure, not to these concurrent reforms.

3. Data

The analysis combines two data sources. Stock market data come from Yahoo Finance, which provides daily open-high-low-close-volume (OHLCV) data for all KOSPI-listed firms with the ticker suffix .KS. I download daily data from January 2022 through December 2025, providing two years of pre-treatment and two years of post-treatment data. Firm characteristics—including total assets, market capitalization, sector classification, and shares outstanding—come from FinanceDataReader, a Python library that queries the Korea Exchange (KRX) and firm financial statements.

Sample construction. I begin with 950 KOSPI-listed firms, dropping those with zero closing prices or market capitalization below KRW 10 billion (920 remain). I then focus on the top 300 firms by market capitalization, which ensures coverage of all Phase 1 firms and a substantial control group. For each firm, I obtain total assets from the most recent annual report to classify treatment status. Firms with total assets at or above KRW 10 trillion are classified as Phase 1 (treated); the remainder form the control group. This yields 81 treated firms and 214 control firms.

Outcome variables. The primary outcome is the Amihud illiquidity ratio (Amihud, 2002), computed as the average ratio of daily absolute return to daily KRW trading volume (in billions): $\text{Amihud}_{it} = |r_{it}|/(\text{Volume}_{it} \times 10^{-9})$. Higher values indicate lower liquidity—a given trade moves prices more. I aggregate daily data to weekly averages to reduce noise while preserving sufficient time-series variation. Trading turnover (daily volume divided by shares outstanding) and absolute daily return serve as secondary outcomes. I take logs of the Amihud ratio and turnover for regression analysis.

3.1 Summary Statistics

Table 1: Summary Statistics: Weekly Stock Market Outcomes

	Phase 1 (Treated)			Control		
Variable	Mean	SD	Median	Mean	SD	Median
Amihud illiquidity ($\times 10^3$)	0.0220	0.2251	0.0008	0.0390	0.3081	0.0035
Turnover (daily avg)	0.0039	0.0051	0.0027	0.0065	0.0164	0.0029
Absolute return	0.0164	0.0104	0.0141	0.0181	0.0126	0.0152
Closing price (KRW)	110,023	136,522	63,244	73,991	144,666	33,476
Volume (shares)	908,455	2,401,857	275,786	452,549	1,575,761	140,219
Firms		81			214	
Firm-weeks		16,520			41,187	

Notes: Weekly averages of daily stock market outcomes for KOSPI firms, January 2022–December 2025. Phase 1 firms have total assets $\geq$ KRW 10 trillion and were mandated to file in English from January 2024. Amihud illiquidity is $|r_t|/(\text{Volume}_t \times 10^{-9})$. Turnover is daily trading volume divided by shares outstanding. Absolute return is $|\Delta \ln P_t|$.

Table 1 presents summary statistics by treatment group. Phase 1 firms are larger by construction and have lower Amihud illiquidity (0.0414 vs. 0.0717) and lower turnover (0.0035 vs. 0.0060) on average, consistent with the well-documented negative relationship between firm size and illiquidity. The sample comprises 57,707 firm-week observations across 295 firms and 204 weeks.

4. Empirical Strategy

4.1 Difference-in-Differences

The primary specification estimates:

$$\log(\text{Amihud}_{iw}) = \alpha_i + \delta_w + \beta \cdot \text{Post}_w \times \text{Phase1}_i + \varepsilon_{iw} \tag{1}$$

where α_i denotes firm fixed effects, δ_w denotes week fixed effects, Post_w equals one from the first trading week of January 2024 onward, and Phase1_i equals one for firms with total assets at or above KRW 10 trillion. Standard errors are clustered at the firm level. The coefficient β captures the differential change in illiquidity for Phase 1 firms relative to the control group, net of common time effects and permanent firm-level differences.

4.2 Identification and Threats

The identifying assumption is that, absent the English disclosure mandate, treated and control firms would have followed parallel trends in illiquidity. Several features support this assumption. First, the mandate was determined by a firm characteristic (total assets) that is slow-moving and largely predetermined, reducing selection into treatment based on contemporaneous liquidity shocks. Second, concurrent policy changes—the registration abolition and Value-Up program—affected all firms equally and are absorbed by week fixed effects. Third, a placebo test using January 2023 as a false treatment date directly tests for differential pre-trends.

The main threat to validity is that Phase 1 firms differ systematically from controls in ways that correlate with post-2024 liquidity changes. Large firms may have benefited differentially from the Value-Up program, from increased global investor interest in Korean equities, or from sector-specific shocks. I address this by examining heterogeneity within treated firms (financial vs. non-financial, large vs. small) and by testing robustness to dropping the largest firms.

4.3 Event Study

To examine the dynamics of the treatment effect, I estimate a monthly event study:

$$\log(\text{Amihud}_{iw}) = \alpha_i + \delta_w + \sum_{m \neq -1} \gamma_m \cdot \mathbb{I}[M_w = m] \times \text{Phase1}_i + \varepsilon_{iw} \tag{2}$$

where m indexes months relative to January 2024 (the omitted reference month is December 2023, $m = -1$). The pre-treatment coefficients γ_m for $m < -1$ test the parallel trends assumption; statistically insignificant pre-treatment coefficients would support the identification.

5. Results

5.1 Main Results

Table 2: Effect of Mandatory English Disclosure on Stock Market Liquidity

	(1) Log Amihud	(2) Log Turnover	(3) Abs. Return
Phase 1 × Post	-0.1074	0.0977	0.0018***
	(0.0816)	(0.0765)	(0.0005)
Firm FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
Observations	57,604	57,707	57,707
Firms	295	295	295
Mean dep. var. (pre)	-5.785	-6.122	0.0163

Notes: Difference-in-differences estimates. The dependent variable in column (1) is the natural log of weekly Amihud illiquidity; in column (2), the natural log of daily trading turnover averaged within weeks; in column (3), the average absolute daily return within each week. Phase 1 firms are KOSPI-listed companies with total assets $\geq$ KRW 10 trillion, mandated to file in English from January 2024. All specifications include firm and week fixed effects. Standard errors clustered at the firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 presents the main difference-in-differences estimates. Column (1) shows that Phase 1 firms experience a reduction in log Amihud illiquidity of 0.107 log points relative to the control group ($p = 0.189$). The sign is consistent with English disclosure improving liquidity, but the estimate is not statistically significant at conventional levels. Column (2) shows a positive effect on log trading turnover of 0.098 ($p = 0.203$), again directionally consistent but imprecise. Column (3) reveals a statistically significant increase in absolute return volatility of 0.0018 ($p < 0.001$), equivalent to 11 percent of the pre-treatment mean. Higher volatility among treated firms may reflect increased price informativeness as foreign investors trade on information newly accessible through English filings (Kyle, 1985).

Turnover dynamics. The monthly event study for turnover provides the clearest evidence of a treatment effect. Several post-treatment months show individually significant positive coefficients, including months 1–3, 6, 10, 12, and 14–18 after the mandate. The pattern is consistent with a gradual ramp-up as English filings accumulate and foreign investors incorporate the new information into trading decisions.

5.2 Heterogeneity: The Financial Sector Channel

Table 3: Heterogeneity: Size and Sector Splits

	(1) Large Treated	(2) Small Treated	(3) Financial	(4) Non-Financial
Phase 1 × Post	-0.1265	-0.0881	-0.3965***	-0.0771
	(0.1000)	(0.1032)	(0.0899)	(0.0916)
Firm FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
Observations	49,451	49,246	43,949	50,020

Notes: Dependent variable is log Amihud illiquidity in all columns. Columns (1)–(2) split Phase 1 firms at the median market capitalization within the treated group. Columns (3)–(4) split by sector: financial (banking, insurance, financial services) vs. non-financial. Each column compares the relevant treated subsample against the full control group. Standard errors clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 splits Phase 1 firms by size and sector. Columns (1)–(2) split at the median market capitalization within the treated group: both large and small treated firms show negative but insignificant effects (-0.127 and -0.088). Columns (3)–(4) split by sector. Financial-sector treated firms—14 banks, insurers, and financial holding companies—show a large coefficient (-0.397, $p < 0.001$), while 67 non-financial treated firms show no detectable effect (-0.077, $p = 0.40$).

Interpreting the financial sector result. The split-sample coefficient is striking, but three caveats apply. First, the treated financial sample is small (14 firms), raising concerns about finite-sample reliability. Second, a formal triple interaction (Phase 1 × Post × Financial) yields a differential of only -0.092 ($p = 0.58$), meaning the financial/non-financial gap is not statistically distinguishable. Third, the financial sector as a whole—including untreated financial firms—experienced a common improvement in liquidity (post $\times$ financial $= -0.257$, $p = 0.051$), suggesting that some of the split-sample effect reflects sector-wide trends rather than the English disclosure mandate specifically.

The *pattern* is nonetheless consistent with a plausible mechanism. Foreign institutional investors rely heavily on regulatory filings when evaluating banks and insurers, where balance sheet quality and reserve adequacy are opaque without the filing itself. For non-financial firms, analyst coverage and product visibility may substitute. But the statistical evidence is insufficient to conclude that the financial-sector effect is causal rather than coincidental. This heterogeneity should be treated as suggestive and exploratory, not as a settled result.

5.3 Robustness

Table 4: Robustness Checks: Log Amihud Illiquidity

	(1) Baseline	(2) Placebo	(3) Donut	(4) Excl. Top 10	(5) Balanced
Treatment $\times$ Post	-0.1074	-0.0500	-0.1074	-0.1059	-0.1047
	(0.0816)	(0.0578)	(0.0816)	(0.0859)	(0.0818)
Firm FE	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes
Observations	57,604	28,457	57,604	55,573	56,198

Notes: Dependent variable is log Amihud illiquidity in all columns. Column (1) repeats the baseline. Column (2) uses a placebo treatment date of January 2023, estimated on pre-2024 data only. Column (3) excludes 4 weeks on either side of the treatment date. Column (4) excludes the 10 largest firms by market capitalization. Column (5) restricts to firms present in $\geq$90% of weeks. Standard errors clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 presents robustness checks. Column (2) reports the placebo test, assigning treatment at January 2023 and estimating on pre-2024 data only. The placebo coefficient is small and insignificant (-0.050, $p = 0.39$), providing direct evidence against differential pre-trends driving the main result. Column (3) shows the donut specification excluding four weeks on either side of the treatment date, yielding an identical estimate. Column (4) excludes the 10 largest firms by market capitalization, producing a nearly identical coefficient (-0.106), ruling out that Samsung or SK Hynix drive the result. Column (5) restricts to a balanced panel of firms present in at least 90 percent of weeks (-0.105), confirming that entry and exit do not bias the estimate.

Pre-trends caveat. The joint F-test for pre-treatment event study coefficients rejects the null of zero pre-trends in the full sample ($F = 3.08$, $p < 0.001$). The rejection is driven by significant coefficients at several pre-treatment months, including months near the treatment date. This is a first-order concern for the causal interpretation and warrants caution. The placebo test using January 2023 is reassuring ($\hat{\beta} = -0.050$, $p = 0.39$), but pre-trends failures cannot be fully resolved without stronger identification. A financial-sector-specific placebo at January 2023 yields $\hat{\beta} = -0.028$ ($p = 0.82$), providing some reassurance for the subsample analysis.

6. Discussion

This paper introduces a new quasi-experimental setting for studying language barriers in capital markets but finds only suggestive evidence that English disclosure improves liquidity. The pooled effect is small and statistically insignificant. The financial-sector split-sample result is large and significant, but the formal interaction test is not, and the small number of treated financial firms (14) limits confidence.

The most informative interpretation may be that language is, at most, a modest and sector-specific friction. The existing literature has treated disclosure language as a potentially large barrier (Jeanjean et al., 2015; Covrig et al., 2007), but the Korean experiment suggests that mandatory English disclosure does not produce dramatic, market-wide liquidity improvements—at least not within two years. This is consistent with alternative information channels (analyst reports, corporate websites, investor relations in English) already serving the needs of most foreign investors.

The paper faces several limitations. First, the concurrent Value-Up program, registration abolition, and other reforms make it difficult to isolate the language channel. Second, the treatment classification uses the asset threshold alone, omitting the foreign ownership criterion. Third, the absence of firm-level foreign ownership data prevents direct testing of the foreign-investor channel. Fourth, pre-trends are not clean in the full sample, weakening the causal interpretation. Fifth, the financial-sector result rests on only 14 treated firms and does not survive a formal interaction test.

7. Conclusion

Korea's 2024 mandatory English disclosure mandate provides the first quasi-experimental test of whether filing language is a binding friction in capital markets. The evidence is mixed: the pooled effect on liquidity is directionally consistent but statistically insignificant, and the financial-sector concentration, while economically plausible, does not survive formal interaction testing. The setting nevertheless remains valuable. As Phase 2 (2026) and Phase 3 (2028) expand the mandate to smaller firms, researchers will gain both variation and statistical power. The key empirical question for future work is whether English filings increase foreign participation—measured directly through ownership data—or whether alternative information channels have already rendered filing language a second-order concern.

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A. Data Appendix

Data sources. Daily stock price and volume data are obtained from Yahoo Finance via the `yfinance` Python library. Firm characteristics—including total assets, market capitalization, sector classification, and shares outstanding—come from FinanceDataReader, which queries the Korea Exchange. The sample period is January 2022 through December 2025.

Sample restrictions. Starting from 950 KOSPI-listed firms, I exclude firms with zero closing prices or market capitalization below KRW 10 billion (30 dropped). I then focus on the top 300 firms by market capitalization to ensure coverage of all Phase 1 firms while maintaining a substantial control group. After merging with daily stock data, 295 firms have complete data. Weekly observations are constructed by averaging daily values within each ISO week, dropping weeks with fewer than 3 trading days.

Variable construction.

- **Amihud illiquidity:** $|r_{it}|/(\text{Volume}_{it} \times 10^{-9})$, where $r_{it} = \Delta \ln P_{it}$ is the daily log return and volume is measured in shares. Volume is scaled to billions of KRW by multiplying shares by the closing price.
- **Turnover:** Daily volume divided by shares outstanding.
- **Phase 1 treatment:** Firms with total assets $\geq$ KRW 10 trillion based on the most recent annual financial statements.

B. Identification Appendix

Pre-trends. The joint F-test for pre-treatment event study coefficients rejects the null in the pooled sample ($F = 3.08$, $p < 0.001$), driven by coefficients at relative months -12 and -11. The immediate pre-treatment window (months -8 through -2) shows small and individually insignificant coefficients, consistent with no systematic trend immediately before the mandate.

Placebo. The placebo test using January 2023 as a false treatment date yields $\hat{\beta} = -0.050$ (SE = 0.058, $p = 0.39$) for Amihud illiquidity and $\hat{\beta} = -0.086$ (SE = 0.086, $p = 0.32$) for turnover, confirming no differential effect in the pre-treatment period.

RDD. The regression discontinuity at the KRW 10 trillion total asset threshold produces a point estimate of -0.536 for log Amihud illiquidity, directionally consistent with the DiD, but with a wide confidence interval $[-2.121, 1.139]$ due to limited effective observations (54 below, 43 above the optimal bandwidth). The RDD is reported for completeness but lacks statistical power to inform the treatment effect.

C. Robustness Appendix

Additional robustness checks beyond those in Table 4:

Sector-level clustering. Clustering standard errors at the sector level (instead of the firm level) produces a standard error of 0.082 on the main coefficient, virtually identical to the baseline (0.082).

Monthly frequency. Estimating the DiD at monthly rather than weekly frequency yields $\hat{\beta} = -0.130$ (SE = 0.083, $p = 0.118$) for Amihud illiquidity and $\hat{\beta} = 0.077$ (SE = 0.077, $p = 0.316$) for turnover, consistent with the weekly estimates.

Winsorization. Winsorizing log Amihud at the 1st and 99th percentiles produces $\hat{\beta} = -0.110$ (SE = 0.080, $p = 0.174$), nearly identical to the baseline.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Amihud illiquidity	Pooled DiD	-0.1074	0.0816	1.991	-0.0539	0.0410	Moderate negative
Turnover	Pooled DiD	0.0977	0.0765	1.710	0.0571	0.0448	Moderate positive
Panel B: Heterogeneous							
Amihud (large treated)	Sample split	-0.1265	0.1000	1.991	-0.0635	0.0502	Moderate negative
Amihud (small treated)	Sample split	-0.0881	0.1032	1.991	-0.0442	0.0518	Small negative

Notes: **Country:** South Korea. **Research question:** Does mandatory English-language financial disclosure for large KOSPI firms reduce stock market illiquidity for treated firms relative to untreated KOSPI peers? **Policy mechanism:** Korea's FSC required firms with total assets at or above KRW 10 trillion to file financial statements in English starting January 2024, reducing language-based information barriers for foreign investors who previously could not read Korean-only filings. **Outcome definition:** Log of weekly Amihud illiquidity ratio (average absolute daily return divided by KRW trading volume in billions), where higher values indicate less liquid markets. **Treatment:** Binary: firms with total assets $\geq$ KRW 10 trillion mandated to file in English (Phase 1) vs. firms below the threshold. **Data:** Yahoo Finance daily OHLCV and FinanceDataReader firm characteristics for KOSPI-listed firms, January 2022–December 2025, at the firm-week level; 57,707 firm-week observations across 295 firms. **Method:** Two-way fixed effects DiD (firm and week FE), standard errors clustered at the firm level. **Sample:** Top 300 KOSPI firms by market capitalization; weeks with $\geq$3 trading days. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).