

7544210-do-equity-market-signals-add-to-accounting-based-default-risk-assessment-evidenc

SUMMARY: This study examines whether additional equity-market signals improve the assessment of default risk using accounting-based methods. The research uses 39,970 firm-year observations for Chinese listed firms from 2015 to 2025. The study estimates models with distinct information sets for a 2016-2019 training window and evaluates them without refitting from 2020-2025. An equity-market microstructure signal is selected using only pre-2020 data and is frozen for validation. The study finds that a model based on reported financial statement items attains an out-of-sample AUC of 0.8023 in the pooled 2020-2025 evaluation. Adding firm characteristics and price-scaled valuation multiples to the model improves performance, with valuation being the only market-based block that clearly improves performance. Adding structural market-risk measures lowers the AUC, while adding closing-price stasis worsens performance. The study concludes that financial statements provide a strong baseline, and price-scaled valuation information adds meaningful incremental content. Structural market-risk and microstructure variables do not deliver consistent incremental gains across discrimination and proper scoring measures. The evidence suggests that default-risk information contained in equity-market signals is incremental to reported financial statement fundamentals and does not persist over time or transport across markets.

METHOD: The study uses a dataset of 39,970 firm-year observations for Chinese listed firms from 2015 to 2025. Models are estimated with distinct information sets for a 2016-2019 training window and evaluated without refitting from 2020-2025. An equity-market microstructure signal is selected using only pre-2020 data and is frozen for validation. The study employs a model based on reported financial statement items and evaluates its performance using out-of-sample AUC. The study also evaluates the performance of the model with additional firm characteristics and price-scaled valuation multiples.

KEY FINDINGS:

- A model based on reported financial statement items attains an out-of-sample AUC of 0.8023 in the pooled 2020-2025 evaluation.
- Adding firm characteristics and price-scaled valuation multiples to the model improves performance, with valuation being the only market-based block that clearly improves performance.
- Adding structural market-risk measures lowers the AUC, while adding closing-price stasis worsens performance.
- Financial statements provide a strong baseline, and price-scaled valuation information adds meaningful incremental content.
- Structural market-risk and microstructure variables do not deliver consistent incremental gains across discrimination and proper scoring measures.

IMPLICATION FOR TRADING: The study suggests that financial statements provide a strong baseline for assessing corporate credit risk. Price-scaled valuation information adds meaningful incremental content to the assessment. The study's findings suggest that equity-market signals, while potentially useful, do not contain default-risk information that is incremental to reported financial statement fundamentals and does not persist over time or transport across markets. Practitioners should use caution when incorporating equity-market signals into their credit risk assessment models, as

they may not provide consistent incremental value.

Do Equity-Market Signals Add to Accounting-Based Default-Risk Assessment? Evidence from China

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Keywords: default risk; accounting information; market-based signals; stock liquidity; price discreteness; China

JEL classification: G14; G32; G33; M41

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Funding

This work was supported by the National Natural Science Foundation of China [grant number 72101199] and the Fundamental Research Funds for the Central Universities [grant number SK2021019]. The funders had no role in the study design; data collection, analysis, or interpretation; manuscript preparation; or the decision to submit the article for publication.

Do Equity-Market Signals Add to Accounting-Based Default-Risk Assessment? Evidence from China

Abstract

This paper asks whether different types of equity-market information add to accounting-based default-risk assessment. Using 39,970 firm-year observations for Chinese listed firms from 2015 to 2025, we estimate models with distinct information sets on a 2016–2019 training window and evaluate them without refitting in 2020–2025. We also select an equity-market microstructure signal using only pre-2020 data and freeze both its definition and the model-implied default-probability (EDF) tail cutoff for validation. A model based on reported financial statement items attains an out-of-sample AUC of 0.8023 in the pooled 2020–2025 evaluation. The specification that additionally includes firm characteristics and price-scaled valuation multiples attains an AUC of 0.8230 and improves the Brier score and log loss, making valuation the only market-based block that clearly improves performance. Adding structural market-risk measures lowers pooled AUC to 0.8211 despite raising AUC within each validation year. Adding closing-price stasis lowers pooled AUC further to 0.8183 and worsens both proper scoring measures. Firm-level block-bootstrap inference confirms that structural market-risk and microstructure variables do not deliver consistent incremental gains across discrimination and proper scoring measures. We use closing-price stasis—the annual share of valid trading days with positive trading value and a zero adjusted return—as a diagnostic case rather than as the strongest market-friction predictor. Stasis is positively associated with EDF-tail re-entry after controls for prior risk history, but relative-tick and fine price-level controls substantially attenuate the association, and the signal adds no predictive value to the accounting-informed model. The evidence shows that financial statements provide a strong baseline, price-scaled valuation information adds meaningful incremental content, and statistical association or temporal persistence need not imply incremental decision usefulness.

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1 Introduction

Corporate credit risk is assessed with two broad classes of information: financial statement fundamentals and market prices. Accounting-based models predict failure and distress from reported ratios and firm characteristics (Beaver, 1966; Altman, 1968; Ohlson, 1980; Zmijewski, 1984), and their performance depends on the properties of reported accounting information (Beaver et al., 2005). Market-based measures instead infer credit conditions from equity prices,

equity volatility, and trading activity (Merton, 1974; Bharath and Shumway, 2008). Because creditors can use both information sets, the question that matters for accounting-based credit assessment is whether market signals add to what financial statements already convey. A market-based signal may be statistically related to subsequent distress and yet contribute nothing to an assessment that already uses accounting information, or it may contribute information that reflects the market environment rather than firm fundamentals. Distinguishing these possibilities requires an accounting benchmark against which market information is evaluated.

Stock-market liquidity and corporate default risk are empirically related, but interpreting this relation is difficult. Poor liquidity may raise financing costs, weaken price discovery and market feedback, and reduce firms' ability to absorb shocks. At the same time, common liquidity proxies and market-based default measures may load on the same underlying equity prices, volatility, and investor beliefs. This raises a broader question for accounting-based credit assessment: does an equity-market signal contain default-risk information that is incremental to reported financial statement fundamentals, and does any such information persist over time or carry across markets? Temporal persistence alone is not sufficient evidence of price-independent default-risk information: a persistent association may instead reflect financing conditions, shared information, or common market-design features. Cross-market transportability is the incremental information an equity-market signal provides for credit-market outcomes that are not mechanically constructed from the same equity-price information set; it is an external-information criterion rather than a claim of causal identification.

Temporal persistence, price independence, and cross-market transportability need not coincide. China provides a useful setting for separating them because its equity market combines a fixed RMB 0.01 tick with price limits, special-treatment rules, and formal suspensions. The absolute tick makes the price grid coarser for low-priced shares, while trading rules affect whether and how prices may adjust. Uneven corporate-bond trading also provides an external setting with different market conventions and coverage. We do not assume that these arrangements generalize mechanically; rather, they expose how market design can limit the interpretation of equity-derived distress signals used alongside accounting information.

Our sample contains 39,970 firm-year observations from 2015–2025 and 9.81 million official stock-day records. The sample consists predominantly of nonfinancial firms. We construct a one-year Merton-style model-implied default probability (EDF) using annual equity-return volatility, equity market value on the last available trading day of the year, annual balance-sheet debt, and the annual mean benchmark interest rate. The observation labeled year t uses year- t inputs; all prospective specifications use the equity signal and risk controls from $t - 1$ to explain the year- t outcome. We set the P90 EDF cutoff using 2015–2019 data and keep it fixed throughout the 2020–2025 validation period. We distinguish first-observed entry, for firms not previously observed in the EDF tail, from re-entry, defined as a return to the tail after a prior tail episode and subsequent exit. Re-entry specifications control for prior risk history; threshold buffers and minimum-history requirements address repeated cutoff crossings and left-censoring.

We separate signal selection from subsequent temporal validation. Using only pre-2020 information, we compare five candidate signals spanning price impact, raw and abnormal trading

intensity, zero adjusted returns conditional on positive trading, and inferred gaps in observed trading days. Each signal is oriented so that a larger value denotes greater trading friction and is standardized on the same 2016–2019 first-observed-entry discovery sample. We retain a signal only if its discovery coefficient is positive, consistent with the directional prediction that greater trading friction is associated with higher default risk; no statistical significance threshold is imposed. Closing-price stasis is the only candidate that passes this screen. Because the discovery sample contains only 40 events, however, the positive coefficient may partly reflect sampling variation. We therefore place no inferential weight on the discovery estimate itself and evaluate the selected signal using later observations while holding both the signal definition and the EDF cutoff fixed.

Closing-price stasis is therefore not interpreted as the strongest market-friction predictor. Instead, it is used as a diagnostic case in which temporal persistence and statistical association can be separated from incremental predictive usefulness.

For the 2020–2025 validation period, we further distinguish first-observed entry from re-entry as a transparent decomposition of the fixed validation sample rather than as a formally preregistered design choice. We use temporal persistence in an operational, rather than parameter-stability, sense: a signal selected using pre-2020 information must retain the predicted direction in the fixed post-2020 evaluation sample and in leave-one-validation-year-out exercises. We do not require coefficient magnitudes to be equal across periods. Official trading-state controls leave the re-entry coefficient close to its baseline value, whereas relative-tick and fine price-level controls substantially attenuate the relation. This pattern indicates substantial price dependence, but does not identify a causal price-grid mechanism.

For an external test, we match corporate-bond yields to same-day government yields by remaining maturity. The estimates are imprecise and sensitive to issuer coverage. Active and other issuer-years differ substantially in state ownership, reported spreads, and yield-record composition, and a positive estimate under a narrow activity rule is not reproduced when activity is defined using prior-year information. The bond evidence therefore informs the boundary of cross-market transportability rather than supplying a second main result.

Rather than treating market information as a single block, we separate price-scaled valuation information, structural market-risk measures, and equity-market microstructure signals and evaluate their incremental content relative to a price-free financial-statement benchmark.

Our contribution is threefold. First, we build an accounting-only benchmark that excludes equity prices and use it to distinguish the incremental value of different types of market information. In the pooled 2020–2025 evaluation, with the estimation window held fixed, a model estimated on reported financial statement items alone attains an out-of-sample AUC of 0.8023 for subsequent EDF-tail states. The specification that additionally includes firm characteristics and price-scaled valuation multiples attains an AUC of 0.8230 and improves both proper scoring rules. Because the multiples embed the equity price, they are not financial-statement data. Price-scaled valuation multiples therefore provide meaningful incremental information, whereas structural market-risk and microstructure variables do not deliver consistent additional gains; this valuation block is the only market-based block that clearly improves predictive per-

formance. Adding structural market-based risk measures lowers pooled AUC to 0.8211 despite raising within-year AUC in all six validation years; adding the selected equity-market microstructure signal lowers pooled AUC further to 0.8183 while raising both the Brier score and log loss. A model that uses only market-based risk measures attains 0.7625. The liquidity benchmark raises AUC modestly when it is added to specification (4), but it also raises the Brier score and log loss, so no market block other than valuation multiples delivers consistent incremental gains across discrimination and the proper scoring rules. Firm-level block-bootstrap inference confirms that structural market-risk and microstructure variables do not deliver consistent incremental gains across discrimination and proper scoring measures, and an external rating-deterioration outcome, which is not mechanically built from equity prices, gives mixed evidence: market risk lowers AUC but modestly improves PR-AUC and the proper scoring rules, and none of these increments is statistically distinguishable from zero in the 952 issuer-years with rating coverage. Because the default point in the EDF construction is a function of reported liabilities, we also quantify the overlap between the two information sets: financial-statement variables alone explain 29% of the variation in lagged log EDF, firm characteristics add little, and valuation multiples bring the total to 31%. Second, we show that statistical association, out-of-sample predictive content, and accounting decision usefulness are distinct properties of a signal. Closing-price stasis serves as a diagnostic case: it is positively associated with EDF-tail re-entry in the fixed 2020–2025 evaluation sample and in every leave-one-validation-year-out exercise, and yet it carries no incremental predictive content once financial statement fundamentals are included; a post-2020 validation of all five candidate signals shows that stasis is not even the strongest of them. Third, we trace why. The association is substantially attenuated by relative-tick and fine price-level controls on a common sample, so part of what appears to be credit-relevant information is better understood as price-grid dependence, and the external credit-market evidence for the signal is imprecise and sensitive to bond-market coverage. For accounting-based credit assessment, the implication is that the information content of market signals should be established relative to the accounting information set rather than inferred from statistical association or temporal persistence.

Adding valuation multiples to financial-statement variables and firm characteristics increases AUC by 0.0204 in the pooled evaluation, a nontrivial improvement over a benchmark already above 0.80. We do not translate it into counts of lending errors because that mapping depends on the decision threshold and the relative costs of false-positive and false-negative classifications.

The remainder of the paper is organized as follows. Section 2 reviews the related literature and develops the empirical implications. Section 3 describes the data and empirical design. Section 4 builds the accounting benchmark and evaluates the incremental out-of-sample information content of market-based risk measures and microstructure signals, together with annual calibration, forecast-timing robustness, and external credit outcomes. Section 5 then examines the economic content of the equity-market signals themselves: the liquidity–risk relation, pre-2020 signal selection, temporal persistence in EDF-tail entry and re-entry, and the price dependence of closing-price stasis. Section 6 reports the cross-market corporate-bond evidence as an external-validity check. Section 7 discusses the interpretation and limitations of the findings and concludes.

2 Related Literature and Empirical Implications

2.1 Accounting information, market signals, and default-risk assessment

The accounting literature provides the benchmark against which market-based default-risk information should be evaluated. Financial statement ratios have been used to predict failure and distress since [Beaver \(1966\)](#) and [Altman \(1968\)](#), and the probabilistic formulation of [Ohlson \(1980\)](#) together with the estimation framework of [Zmijewski \(1984\)](#) established the accounting-ratio default model as the standard comparator in credit-risk research. This literature also does not imply a uniform loss of usefulness over time: [Beaver et al. \(2005\)](#) document substantial stability in the bankruptcy-predictive ability of financial ratios over four decades, with only a slight deterioration between subperiods. The relative performance of accounting and market information nevertheless remains an empirical question rather than a maintained assumption. Because several inputs of the conventional Altman Z-score and Ohlson O-score are not available for the full Chinese A-share panel used here, we implement the accounting benchmark as a reduced-form logit on reported ratios rather than as a re-estimated version of those scores, while following the same logic of combining reported statement items.

Two strands of accounting research make that comparison central for credit assessment. The first concerns the informational environment in which reported numbers are produced. Disclosure, earnings quality, and the properties of accruals affect the extent to which statements summarize credit-relevant fundamentals ([Kothari, 2001](#); [Healy and Palepu, 2001](#); [Dechow et al., 2010](#)), and those properties vary with firms' incentives and with institutional features of the reporting regime ([Ball et al., 2000](#)). The second concerns the relative information content of accounting and market measures. Capital-markets research in accounting has long examined whether accounting numbers and prices convey distinct information, and how that division of labor depends on the reporting environment ([Ball and Brown, 1968](#); [Barth et al., 2001](#)). Because ratings and credit spreads are used in contracting and regulation as well as in pricing, the relevant question is not only whether a market signal predicts distress, but whether it adds to what the accounting information set already indicates ([Kaplan and Urwitz, 1979](#)).

Our setting is well suited to this question. China's equity market combines price limits, special-treatment designations, formal suspensions, and a fixed absolute tick with an accounting regime and a bond market that cover a particular and potentially selective population of firms. Accounting-based distress research in China must therefore contend both with the measurement properties of reported information ([Haw et al., 2001](#); [Liu and Lu, 2007](#)) and with the possibility that market-based signals reflect market design rather than firm fundamentals. We accordingly treat the accounting information set as the benchmark, and ask whether equity-market signals, including closing-price stasis, provide incremental information beyond it.

2.2 Liquidity, equity-market information, and default risk

A broad literature links stock-market liquidity to asset prices, corporate financing conditions, and default risk. Investors require compensation for illiquidity, while more informative market prices can improve corporate decisions ([Amihud, 2002](#); [Acharya and Pedersen, 2005](#); [Chen et](#)

al., 2007; Chordia et al., 2008; Fang et al., 2009). Financing frictions can also increase firms' dependence on internal funds and make firms more vulnerable to financing and refinancing shocks (Bolton et al., 2011; Hadlock and Pierce, 2010). More directly, Brogaard et al. (2017) show that greater stock liquidity is associated with lower default risk, providing a natural benchmark for the liquidity–default relation. Subsequent studies document related evidence in international samples and in China (Nadarajah et al., 2021; Zhao et al., 2026).

This literature motivates a broader question about the information content of equity-market signals rather than another estimate of the average liquidity–default association. Equity-based default measures and many commonly used liquidity or trading variables rely, at least partly, on overlapping equity-market information. EDF is constructed from equity value and volatility, whereas measures such as Amihud use stock returns and trading value. Their observed association may therefore reflect financing frictions, shared information, or common dependence on equity-market inputs rather than price-independent default-risk information. We accordingly ask whether an equity-market signal remains informative after accounting for prior risk history and price dependence, whether the relation persists in a fixed post-2020 temporal validation period, and whether it carries over to credit-market outcomes formed outside the same equity-price information set.

2.3 Distress prediction and cross-market transportability

A large literature shows that market variables contain forward-looking information about corporate distress and default. Merton (1974) provides the structural foundation by linking equity value and volatility to the value of risky corporate debt. Shumway (2001) recasts bankruptcy prediction in a dynamic hazard framework, while subsequent studies combine market-based indicators with accounting information to model default and financial distress (Chava and Jarrow, 2004; Duffie et al., 2007; Bharath and Shumway, 2008; Campbell et al., 2008). This literature motivates the use of equity-market signals in default-risk analysis, while also emphasizing the importance of genuine out-of-sample evaluation and the limited statistical power available when realized default events are rare. Recent evidence for China further shows that model-implied default probabilities can complement domestic credit ratings in default prediction and credit price discovery (Li et al., 2025).

We distinguish three increasingly demanding forms of evidence. The first is a state-transition relation: whether an equity-market signal is associated with subsequent entry or re-entry into the EDF tail. The second is an incremental out-of-sample forecasting criterion: whether the signal improves ranking, calibration, or proper scoring measures for subsequent EDF states beyond prior risk and other predictors. The third is external credit evidence: whether the signal contains incremental information for outcomes that do not rely mechanically on the same equity-price inputs, including rating deterioration, corporate-bond spreads, and realized first default. Among these outcomes, corporate-bond spreads provide the most direct cross-market test because they are credit-market outcomes formed outside the equity-price information set used to construct EDF. Realized first-default events are rare in our sample, so evidence from realized default is treated as complementary rather than primary.

2.4 Zero returns and price discreteness

A separate literature uses zero stock returns to study trading frictions and limited price adjustment. Infrequent trading can generate observed zero returns (Dimson, 1979), while transaction costs can create regions in which information shocks are too small to induce observed price changes (Lesmond et al., 1999). Related market-microstructure research emphasizes discrete pricing, bid–ask effects, and price clustering (Roll, 1984; Harris, 1991). Goyenko et al. (2009) evaluate daily liquidity proxies against transaction-level liquidity benchmarks and explicitly consider zero-return measures, including positive-volume zero-return days. Evidence from emerging markets likewise shows that the incidence of zero returns contains useful information about market liquidity (Bekaert et al., 2007). Related evidence from China cautions against mapping short-horizon price patterns mechanically into illiquidity (Kang et al., 2022). Research in the Pacific-Basin literature further emphasizes that trading protocols and price-limit regimes can materially shape liquidity, price formation, and market quality in China (Zhang et al., 2022; Peng et al., 2024); related evidence shows that price limits can affect price discovery and market efficiency (Chen et al., 2023). Taken together, this literature suggests that zero returns need not represent a single economic friction. They may reflect infrequent trading, transaction costs, limited information incorporation, or price discreteness.

Our measure is the annual share of valid stock-day observations for which the CSMAR cash-dividend-reinvested-adjusted return is zero despite positive trading value. We refer to this measure as closing-price stasis. It is closely related to the positive-volume zero-return measures studied in the market-microstructure literature, but we use the term stasis to emphasize the empirical event of limited net price adjustment conditional on trading. Intuitively, a stasis day is one on which trading occurs but the adjusted close-to-close return is zero. A stasis event therefore excludes formal suspensions and observed no-trading days by construction. It should not, however, be interpreted as implying an absence of intraday price movement or, on company-action dates, literal equality of the unadjusted nominal closing prices.

Economically, closing-price stasis may reflect several overlapping features of the trading environment. A traded zero-return day may be consistent with limited marginal price adjustment, trading frictions, or a coarse nominal price grid relative to the nominal share price. The last channel is particularly relevant in China, where a fixed RMB 0.01 minimum price increment implies that the relative tick size rises mechanically as the nominal share price falls. Accordingly, we do not interpret stasis ex ante as a pure measure of illiquidity. Instead, we examine formal trading states, relative tick size, and fine price-level controls separately to assess how much of the observed relation is associated with these distinct features. These exercises are diagnostic rather than structural: they assess whether the signal is price dependent but do not identify a unique causal mechanism. In this sense, closing-price stasis is best viewed as an equity-market microstructure signal whose economic content must be assessed empirically.

2.5 Corporate yield spreads and external credit measurement

Corporate bond yield spreads contain information about credit conditions, but they do not measure expected default losses alone. Elton et al. (2001) show that expected default losses explain

only part of U.S. corporate bond spreads, with tax treatment and compensation for systematic risk accounting for additional components. Using credit-default-swap information, Longstaff et al. (2005) further show that the nondefault component of corporate bond spreads is related to bond-specific illiquidity. These findings motivate treating yield spreads as broad credit-market outcomes rather than pure measures of expected default losses, while also requiring careful maturity matching and attention to bond-level characteristics.

This distinction is central to our cross-market analysis. We use same-day corporate–government YTM differences matched on remaining maturity to ask whether an equity-market signal provides incremental information for credit-market outcomes formed outside the equity-price information set. We do not interpret the resulting spread as a structural decomposition of credit risk or as a pure measure of default-risk compensation. In particular, it is neither an option-adjusted spread nor a cash-flow-matched zero-coupon credit spread. The exercise therefore provides an external-information test of cross-market transportability rather than an attempt to identify expected default losses themselves.

2.6 Empirical Implications and Validation Strategy

The preceding literature motivates a sequence of empirical tests designed to distinguish temporal persistence, price dependence, incremental prediction, and cross-market transportability. These implications are not structural hypotheses or causal restrictions. Rather, they organize the empirical analysis and clarify what different forms of evidence can and cannot establish.

First, if closing-price stasis contains information about subsequent deterioration among firms with previously elevated default risk, such information may be particularly visible when a firm re-enters the EDF tail after a prior tail episode and subsequent exit. Threshold buffers and minimum-history requirements for first-observed entry assess sensitivity to repeated threshold crossings and short observed EDF histories. A signal selected using pre-2020 information should retain its predicted direction in the 2020–2025 validation period, which we interpret as temporal persistence rather than parameter stability or incremental forecasting ability.

Second, the interpretation of the stasis–re-entry association depends on how strongly it is tied to the equity-market measurement environment. Controls for price limits, special-treatment status, and suspensions assess formal trading-state dependence, while relative-tick and fine price-level controls assess price-grid dependence on the same estimation sample.

Third, temporal persistence within an equity-based EDF framework does not by itself establish either incremental prediction or cross-market transportability. Incremental predictive content is assessed by whether stasis improves out-of-sample ranking, calibration, or proper scoring beyond prior risk and other predictors. Transportability is assessed with rating deterioration, maturity-matched corporate-bond spreads, and realized first default after controlling for prior EDF and firm characteristics; corporate-bond spreads are the most direct cross-market test because they are formed outside the equity-price information set.

Finally, incremental information should be assessed against the information set that credit assessment already uses: an accounting-only specification estimated on the same training window. A market signal is informative for accounting-based default-risk assessment only if it

improves out-of-sample performance relative to that benchmark; a stable panel transition association is not sufficient. This comparison is the subject of Section 4.3.

3 Data and Empirical Design

3.1 Equity-market, accounting, and default-risk data

The main sample covers Chinese A-share listed firms from 2015 to 2025 and consists predominantly of nonfinancial firms. Daily returns, trading value, shares traded, stock prices, market capitalization, financial statements, listing information, ownership, and industry classifications are obtained from CSMAR. Following the structural framework summarized by Bharath and Shumway (2008), we construct an annual one-year Merton-style distance to default and the corresponding model-implied default probability. For brevity, we refer to the latter as EDF throughout the paper. Our use of the term EDF denotes the default probability implied by this Merton-style structural model and should not be confused with Moody's KMV proprietary, empirically calibrated Expected Default Frequency measure.

The CSMAR extract used for the analysis was finalized in September 2026. The 2025 daily equity-market file covers the full exchange calendar through December 31, 2025, and the annual accounting panel includes fiscal years ending in 2025 as available in that extract. No 2026 interim observations are used. Thus, 2025 is treated as a completed sample year rather than as a within-year data cutoff.

Let E and σ_E denote equity value and equity volatility, V and σ_V denote the unobserved asset value and asset volatility, DP denote the default point, r denote the benchmark interest rate used in the structural model, and $T = 1$ year. The implemented structural system is

$$E = V\Phi(d_1) - DPe^{-rT}\Phi(d_2), \quad \sigma_E = \frac{V}{E}\Phi(d_1)\sigma_V,$$

where

$$d_1 = \frac{\ln(V/DP) + (r + \frac{1}{2}\sigma_V^2)T}{\sigma_V\sqrt{T}}, \quad d_2 = d_1 - \sigma_V\sqrt{T}.$$

Equity value is measured as total market capitalization on the last available trading day of the calendar year. Equity volatility is the standard deviation of daily CSMAR returns over the calendar year, annualized by multiplying by $\sqrt{252}$. Following a common empirical Merton-style implementation, the default point is defined as current liabilities plus one-half of noncurrent liabilities from the annual balance sheet.

The benchmark-rate input is the one-year RMB time-deposit rate obtained from CSMAR. We convert the reported rate from percentage points to decimal form and define r as the arithmetic mean of all available daily observations within each calendar year. The resulting series varies across years and provides a consistently defined one-year RMB benchmark throughout the sample period.

This deposit-rate benchmark is applied uniformly across firms within each year, but it is not equivalent to a market-traded Treasury yield. Accordingly, we interpret EDF as a consis-

tently constructed Merton-style model-implied risk state rather than as an empirically calibrated physical default probability. As with standard structural implementations, its level depends on modeling choices including the benchmark interest rate, the equity-volatility window, and the default-point convention.

We initialize $V^{(0)} = E + DP$ and $\sigma_V^{(0)} = \sigma_E E / (E + DP)$ and solve the structural system by fixed-point iteration using 100 updates, which are sufficient to achieve numerical convergence. Using the resulting values, we compute

$$DD = \frac{\ln(V/DP) + (r - \frac{1}{2}\sigma_V^2)T}{\sigma_V \sqrt{T}}, \quad EDF = \Phi(-DD).$$

Thus, an observation labeled year t is an annual risk-state measurement constructed entirely from year- t inputs.

To align preprocessing with the temporal design, we estimate the 1st and 99th percentile cutoffs for EDF, DD, Amihud, equity volatility, and continuous firm-level controls using only 2015–2019 observations and apply these fixed cutoffs to all years. Bounded share variables, including closing-price stasis, are not winsorized. This pre-2020 preprocessing is used throughout the equity-state analysis, including the discovery screen, transition and price-dependence regressions, fixed-split forecasting exercises, and the corresponding Appendix robustness checks. The earlier pooled full-panel preprocessing is reported only as a comparison in Table B.1. Results are similar under the legacy pooled cutoffs, indicating that the main conclusions are not driven by the preprocessing window. ST, *ST, and PT firms are retained because these designations are directly relevant to the distress states studied in this paper. The available panel contains no firm-years classified in banking, securities, or insurance (industry codes J66–J68), but retains five firm-years classified as other financial activities (J69). Firm-years with missing industry classifications enter only specifications that do not require industry controls. In an unreported robustness check, excluding the five J69 observations leaves the history-adjusted 2020–2025 re-entry association positive (coefficient 0.8639, year-wild $p = 0.0243$, $N = 6,186$).

The structural-model construction produces 41,733 firm-year observations before the final liquidity-sample restriction. Requiring at least 120 nonmissing daily returns per firm-year yields a broad analysis panel of 39,970 firm-year observations. After requiring the variables used in the core specification, the complete sample contains 31,856 observations over 2017–2025.

Firm-years excluded from the complete core sample are, on average, smaller and more likely to be in the EDF tail. Table 1 summarizes the resulting sample attrition and differences in risk composition. To avoid attributing coefficient differences to changes in sample composition, we use common estimation samples within each comparison and report the exact samples used in the signal-selection and validation exercises.

Table 2 reports descriptive statistics for the principal outcome, market, accounting, valuation, and ownership variables in the broad analysis panel. The EDF-tail indicator applies the P90 cutoff estimated from the 2015–2019 EDF distribution and held fixed thereafter. Pairwise correlations among the principal continuous variables are reported in Table A.1.

Table 1: Sample attrition and risk composition

Sample	Observations	P90 incidence	Mean EDF	Size	Leverage
Full panel, 2015–2025	39,970	0.1000	0.002549	22.323	0.415
Lag-eligible years, 2017–2025	35,258	0.0836	0.002111	22.331	0.414
Complete core sample	31,856	0.0705	0.001489	22.419	0.424
Excluded within 2017–2025	3,402	0.2066	0.007931	21.508	0.320

Notes: This table uses the same pre-2020-winsorized broad analysis panel as Table 2. For descriptive purposes, P90 incidence here is based on the pooled full-panel EDF distribution. All prospective transition analyses instead use the P90 cutoff estimated from 2015–2019 data and held fixed thereafter. Observations excluded from the complete core sample are those missing one or more variables required by the core specification.

Table 2: Descriptive statistics

Variable	N	Mean	SD	P25	Median	P75
EDF	39,970	0.002549	0.008838	0.000000	0.000006	0.000453
EDF-tail indicator	39,970	0.091469	0.288278	0.000000	0.000000	0.000000
Closing-price stasis	39,970	0.026276	0.027210	0.008230	0.016575	0.033960
Amihud	39,970	0.087452	0.417205	0.013167	0.026419	0.049301
Equity volatility	39,970	0.485163	0.170537	0.364934	0.460416	0.573670
Log assets	39,970	22.323072	1.299869	21.398715	22.120721	23.055328
Leverage	39,970	0.415096	0.204057	0.251575	0.404352	0.560167
ROA	39,970	0.028506	0.073444	0.009437	0.033357	0.063790
Tangibility	39,967	0.206172	0.153549	0.085966	0.174782	0.294303
Current ratio	39,970	2.519822	2.291975	1.203558	1.754925	2.901060
Loss indicator	39,970	0.165474	0.371613	0.000000	0.000000	0.000000
Market-to-book	39,530	3.546922	3.296244	1.640642	2.579643	4.148843
Tobin's Q	39,683	2.056384	1.325853	1.241068	1.629882	2.351109
Listing age	36,886	10.784769	8.243371	4.000000	9.000000	18.000000
SOE indicator	34,735	0.304678	0.460278	0.000000	0.000000	1.000000
Ownership balance	34,735	1.020424	0.813274	0.412651	0.803451	1.399375

Notes: The table uses the broad 2015–2025 analysis panel. The EDF-tail indicator is defined using the P90 cutoff estimated from the 2015–2019 EDF distribution and held fixed thereafter. Closing-price stasis is the annual fraction of valid stock days with positive trading value and a zero cash-dividend-reinvestment-adjusted return. Reported sample sizes vary where listing-age, valuation, or ownership data are unavailable. EDF and Amihud are reported in their raw levels for descriptive purposes; regression specifications use the logarithmic transformations defined in Section 3.2.

3.2 Equity-market signals and risk histories

We use $\ln(EDF + 10^{-6})$ as the continuous measure of the model-implied default-risk state. The additive constant regularizes the logarithmic transformation in the extremely low-EDF region, where model-implied probabilities can become very close to zero.

The conventional Amihud illiquidity measure is constructed as

$$Amihud_{i,t} = \frac{1}{N_{i,t}} \sum_{d \in \mathcal{D}_{i,t}} 10^8 \frac{|R_{i,d}|}{Value_{i,d}},$$

where $R_{i,d}$ is the cash-dividend-reinvestment-adjusted daily stock return and $Value_{i,d}$ is daily trading value. $\mathcal{D}_{i,t}$ contains days with a nonmissing return and positive trading value, and $N_{i,t} = |\mathcal{D}_{i,t}|$. The annual liquidity panel requires at least 120 nonmissing daily-return observations per firm-year.

In regressions, we use

$$\widetilde{Amihud}_{i,t} = \ln(Amihud_{i,t} + 10^{-6}).$$

Firm-level controls include equity volatility, firm size, leverage, return on assets, tangibility, the current ratio, listing age, market-to-book, Tobin's Q, state ownership, and ownership balance. Unless otherwise stated, when the outcome is measured in year t , equity-market signals and firm-level controls are measured in year $t - 1$.

Let C_{90} denote the P90 EDF cutoff estimated from the 2015–2019 distribution and held fixed thereafter. Define

$$Tail_{i,t} = \mathbf{1}(EDF_{i,t} \geq C_{90}).$$

A first-observed entry occurs when $Tail_{i,t-1} = 0, Tail_{i,t} = 1$, and no earlier entry into the EDF tail is observed for firm i . A re-entry occurs when $Tail_{i,t-1} = 0, Tail_{i,t} = 1$, and the firm has experienced at least one earlier observed tail spell, subsequently exited the tail, and later returned to it. Re-entry specifications additionally control for the log of one plus years since the most recent tail exit, the log of one plus the length of the preceding tail spell, the historical maximum of the log-transformed EDF risk measure, and the log of one plus the number of prior tail entries.

The equity-market signal retained by the pre-2020 screening exercise is closing-price stasis. Let $\mathcal{S}_{i,t}$ denote the set of stock-day observations for which both the adjusted daily return and trading value are observed. We define

$$S_{i,d} = \mathbf{1}(R_{i,d} = 0 \text{ and } Value_{i,d} > 0), \quad Stasis_{i,t} = \frac{1}{|\mathcal{S}_{i,t}|} \sum_{d \in \mathcal{S}_{i,t}} S_{i,d}.$$

Thus, the numerator identifies valid stock days on which trading occurs but the cash-dividend-reinvestment-adjusted daily return is zero, whereas the denominator contains all days for which both return and trading value are observed. Positive trading is therefore part of the definition of a stasis event, but not a restriction on the denominator. The denominator does not correspond to the full exchange calendar.

The baseline construction imposes no additional minimum-day requirement beyond the

broad-panel inclusion rule of at least 120 nonmissing daily returns per firm-year. As a robustness check, we also impose a minimum of 120 valid observations under the specific stasis definition.

Because the baseline return is adjusted for cash-dividend reinvestment, a stasis event need not imply literal equality of unadjusted nominal closing prices on a company-action date. As a robustness check, we construct a nominal-close counterpart, defined as the annual share of valid stock days on which trading value is positive and the unadjusted closing price is unchanged from its immediately preceding observed value, and we separately exclude observable company-action dates from that construction.

At the daily level, we define relative tick size as

$$RelativeTick_{i,d} = 100 \times \frac{0.01}{Price_{i,d}},$$

where $Price_{i,d}$ is the unadjusted closing price in RMB per share. Because the minimum price increment is fixed at RMB 0.01, relative tick size increases mechanically as the nominal share price falls. The factor of 100 expresses the measure in percentage points.

Annual relative tick size and nominal share price are measured as firm-year averages. Annual price deciles are formed separately within each calendar year using the annual mean unadjusted closing price.

We also construct a stricter intraday-stasis measure that requires both the daily high and low prices to equal the immediately preceding observed closing price while trading occurs. This measure is nonzero in only one firm-year in the matched validation sample and therefore provides insufficient variation for meaningful analysis.

The five candidate signals are log-transformed Amihud, negative mean log trading value (raw low trading), negative residual trading activity after removing market and industry-year components (abnormal low trading), closing-price stasis, and the inferred share of missing internal market days. All signals are oriented so that larger values indicate greater trading friction, standardized within the common 2016–2019 first-observed-entry risk set, and entered one at a time into the same first-observed-entry specification.

The directional screen retains a candidate if its discovery coefficient is positive; closing-price stasis is the only signal that satisfies this rule. Both the candidate set and the directional rule are defined using only pre-2020 information and held fixed for the 2020–2025 validation exercise. The design was not formally preregistered, and no discovery-period p -value threshold is imposed.

3.3 Corporate bond spreads and market coverage

CSMAR provides government yield-to-maturity and spot yield curves. We use the government yield-to-maturity curve so that the benchmark is measured on the same YTM basis as the corporate-bond outcome. Because the set of reported government-curve maturities varies across dates, the benchmark for each corporate bond is constructed using only government yields observed on the same date.

For each corporate bond-day observation, we identify the nearest observed government maturities below and above the bond's remaining maturity and linearly interpolate between the corresponding yields. When an exact maturity match is available, the observed government yield is used directly. We neither carry government yields across dates nor extrapolate beyond the maturity range observed on that date. The resulting spread is

$$Spread_{b,d} = YTM_{b,d}^{Corp} - Y_d^{Gov}(\tau_{b,d}),$$

where $\tau_{b,d}$ is the bond's remaining maturity and $Y_d^{Gov}(\tau_{b,d})$ is the same-day government yield matched to that maturity.

Using this nearest-node rule, all 25.95 million prefiltered valid bond-day yield observations can be assigned a same-day maturity-matched government benchmark.

As a robustness check, we also implement a more restrictive fixed-grid rule that requires the theoretically adjacent government-curve maturity nodes to be observed on the same date. This rule retains 12.41 million bond-day observations, or 47.82% of the valid sample, and is used only as a secondary benchmark to the broader nearest-node construction.

Daily spreads are aggregated to the bond-year level using the median, requiring at least 20 valid reported-yield observations per bond-year. Issuer-year spreads are then constructed as the median across bonds mapped to the same listed issuer. Because the available documentation does not establish whether zero-turnover yield observations represent stale closing values, quotations, valuations, or another reporting convention, we refer to the outcome as a reported market yield rather than an executed-trade yield. We separately examine observations with positive turnover to assess sensitivity to bond-market trading activity.

We use the government YTM curve to match the reported corporate-bond YTM on the same yield basis. The resulting corporate–government YTM spread is treated as a reduced-form credit-market outcome rather than as a cash-flow-matched zero-coupon spread. The government spot curve is used in a separate robustness exercise.

After matching the bond data to the equity panel and applying the lagged-variable and fixed-effects sample requirements, the main issuer-level specification contains 1,739 issuer-year observations from 322 issuers. The positive-turnover specification additionally requires at least 20 positive-turnover days within a bond-year.

We additionally define bond-market activity using coverage observed in the prior calendar year. Missing prior-year coverage is retained as missing rather than mechanically classified as inactivity. This lagged coverage definition allows us to relate $Stasis_{t-1}$ to $Spread_t$ without conditioning the estimation sample on bond-market activity observed in year t .

3.4 Guarantee coverage, ratings, and realized default

Guarantee information is used as an additional credit-market control. Identified guarantee coverage is linked to listed issuers and treated as active only from the first observed announcement date onward.

Because the available data do not provide a complete indicator for the absence of guarantees,

unmatched bonds are treated as having unidentified rather than verified non-guaranteed status. The data also do not identify guarantee strength or the amount currently covered. We therefore use identified guarantee coverage only as a control variable rather than as the basis for a separate pricing hypothesis.

Rating deterioration is an issuer-year indicator equal to one if any observed rating record reports a downgrade, a negative outlook, or a downward credit watch during that calendar year. Downgrades compare the current grade with the immediately preceding observed grade from the same rating agency for the same issuer. Multiple records are aggregated to the issuer-year level by taking the maximum of each event indicator. Thus, negative outlooks and downward watches denote observed adverse statuses, not necessarily newly initiated outlook or watch changes.

We use rating deterioration and realized first default as additional external credit outcomes. Firm-years without observed rating coverage are left missing rather than coded as non-events. First-default analyses use the available risk set with complete lagged predictors. Because realized first-default events are rare in our sample, these estimates are interpreted as complementary external credit evidence rather than as the primary test of cross-market transportability.

3.5 Estimation and Inference

3.5.1 Panel transition models

We estimate the transition specifications using linear probability models with firm and year fixed effects:

$$Y_{i,t} = \alpha + \beta \text{Signal}_{i,t-1} + \rho' R_{i,t-1} + \gamma' X_{i,t-1} + \mu_i + \delta_t + \varepsilon_{i,t}, \quad (1)$$

where $Y_{i,t}$ denotes an EDF-tail transition, $R_{i,t-1}$ contains lagged default-risk controls, $X_{i,t-1}$ contains lagged firm characteristics, and μ_i and δ_t denote firm and year fixed effects.

For first-observed entry, we control flexibly for the lagged EDF risk state using a cubic polynomial in its distance from the fixed P90 cutoff. Letting

$$q_{i,t-1} = \text{EDF}_{i,t-1} - C_{90},$$

the specification includes $q_{i,t-1}$, $q_{i,t-1}^2$, and $q_{i,t-1}^3$. Re-entry specifications instead include the lagged log-transformed EDF risk measure together with the transition-history controls defined above.

Standard errors are two-way clustered by firm and year. Because the number of year clusters is small, we additionally report year-cluster wild-bootstrap p -values using the implementation of [Roodman et al. \(2019\)](#). Tables reporting both conventional clustered standard errors and wild-bootstrap p -values omit significance stars, and inference follows the reported wild-bootstrap p -values.

3.5.2 Credit-spread models

Let

$$\text{LogEDF}_{i,t-1} = \ln(\text{EDF}_{i,t-1} + 10^{-6}),$$

and let $X_{i,t-1}$ contain lagged equity volatility, log assets, leverage, return on assets, and listing age. The issuer-year specification is

$$Spread_{i,t} = \alpha_i + \delta_t + \beta Stasis_{i,t-1} + \rho LogEDF_{i,t-1} + \eta \widetilde{Amihud}_{i,t-1} + \gamma' X_{i,t-1} + \theta' W_{i,t} + \varepsilon_{i,t}, \quad (2)$$

where α_i and δ_t denote issuer and year fixed effects. The bond-market control vector $W_{i,t}$ contains identified guarantee coverage, remaining maturity, and reported-yield coverage. Nested specifications are estimated on the same complete sample so that changes in the coefficient on stasis are not driven by changes in sample composition.

The corresponding bond-year specification is

$$Spread_{b,t} = \alpha_b + \delta_t + \beta Stasis_{i(b),t-1} + \rho LogEDF_{i(b),t-1} + \eta \widetilde{Amihud}_{i(b),t-1} + \gamma' X_{i(b),t-1} + \Gamma' Z_{b,t} + \varepsilon_{b,t}, \quad (3)$$

where $i(b)$ denotes the listed issuer associated with bond b , and α_b and δ_t denote bond and year fixed effects. $Z_{b,t}$ contains remaining maturity, reported-yield coverage, and identified guarantee coverage when it varies over time. Bond fixed effects absorb time-invariant bond characteristics.

To examine changes in credit-market conditions, we define

$$\Delta Spread_{i,t} = Spread_{i,t} - Spread_{i,t-1}$$

for consecutive observed issuer-years and re-estimate the issuer-level specification using the spread change as the dependent variable. We also estimate the corresponding bond-level change specification and, for comparison, a level specification on the same issuer-year sample used for the change analysis. This approach avoids introducing lagged spread into a dynamic fixed-effects model, but it does not eliminate endogeneity or changes in bond composition across years. Because some specifications include contemporaneous bond-market controls, the estimates are interpreted as conditional associations rather than as real-time forecasts based only on information available at $t - 1$.

Issuer-level spread regressions report standard errors two-way clustered by issuer and year, while bond-level and spread-change regressions report issuer-clustered standard errors. Because the number of year clusters is limited, we additionally report null-imposed year-level wild-cluster bootstrap p -values for the stasis coefficient. These bootstrap p -values provide a finite-year-cluster inference check and do not replace the conventional clustered standard errors reported in parentheses.

For economic magnitudes, we invert the same wild-bootstrap test to construct confidence intervals for the stasis coefficient and scale the estimates by the estimation-sample standard deviation of stasis, expressing the implied spread effects in basis points. These intervals characterize uncertainty about economically plausible magnitudes; they are not equivalence tests and should not be interpreted as establishing economically negligible effects. Sensitivity and leave-one-year-out exercises are treated as exploratory.

3.5.3 Prediction and external outcomes

Prediction models are trained using only the 2016–2019 pre-2020 sample and evaluated without refitting on the fixed 2020–2025 out-of-sample period. We report AUC, PR-AUC, Brier score, log loss, and calibration measures (Fawcett, 2006; Davis and Goadrich, 2006; Brier, 1950; Gneiting and Raftery, 2007). For every firm-year observation dated year t , accounting variables are taken from the fiscal year ending in $t - 1$ and equity-market variables are measured over the trading year $t - 1$. Section 4.1 defines the information sets explicitly and reports an additional accounting-lag specification in which the accounting variables and valuation multiples are lagged two years.

Confidence intervals for the incremental performance changes are obtained by a firm-level block bootstrap. Firms are resampled with replacement from the fixed 2020–2025 evaluation sample. The models are not re-estimated, and each resampled firm retains its original out-of-sample predictions. The intervals therefore describe sampling variation in the evaluation sample rather than re-estimation uncertainty in the training window. Reported DeLong tests of paired AUCs are conventional reference statistics that assume independent evaluation observations; because firms contribute several annual observations, the block bootstrap intervals are the primary basis for inference.

As a flexible predictive benchmark, we estimate a five-fold LASSO logit within the 2016–2019 training sample. All observations from the same firm are assigned to the same fold, preventing within-firm information leakage across cross-validation folds. The penalty parameter is selected by minimum cross-validation deviance and is then held fixed when the model is evaluated on 2020–2025 data (Tibshirani, 1996).

Rating-deterioration and realized-first-default models use lagged equity signals on their respective coverage samples. Because the complete first-default design contains only 19 events, we supplement the baseline analysis with Firth logit (Firth, 1993; Heinze and Schemper, 2002) and a complementary-log-log discrete-time hazard model. These estimates are interpreted as complementary external credit evidence rather than as primary evidence of cross-market transportability.

We distinguish the primary information-set tests from diagnostic and sensitivity analyses. The primary exercises use the fixed 2016–2019 training window, the fixed 2020–2025 evaluation period, the frozen EDF cutoff and signal definition, nested information-set comparisons, and firm-level block-bootstrap inference. Leave-one-year-out estimates, threshold buffers, minimum-history restrictions, price bins, nominal-close variants, alternative bond-coverage definitions, the fixed-grid 224 issuer-year analysis, and alternative government-curve or yield constructions are diagnostic or exploratory. This hierarchy is used to limit interpretation across the many sensitivity checks; the exploratory results are not treated as additional confirmatory tests.

4 Accounting Fundamentals and the Incremental Information Content of Market Signals

4.1 Information sets, forecast origin, and accounting-variable availability

For every firm-year observation dated year t , baseline predictors are dated to year $t - 1$: reported accounting variables are taken from the fiscal year ending in $t - 1$, and equity-market variables are measured over trading year $t - 1$. The baseline design is best described as a fixed-split out-of-sample information-content evaluation rather than as a real-time forecast. Its purpose is to compare the incremental information in different predictor sets when the estimation window, the evaluation sample, and the prediction horizon are all held fixed; it is not intended to reproduce the information set that a creditor would have had on a particular calendar date. Annual reports are released several months after the fiscal year ends, so a forecast formed at the start of year t would not yet have the fiscal year $t - 1$ report available. To assess sensitivity to the dating of accounting information, we also estimate an additional accounting-lag specification, reported in Section 4.4, in which accounting variables and valuation multiples are lagged two years, while structural market-risk and microstructure variables are lagged one year. Both the reported-book-value and price-scaled components of market-to-book and Tobin's Q enter at $t - 2$. Because lagged EDF is itself constructed using fiscal-year $t - 1$ liabilities, this exercise should not be interpreted as a fully point-in-time forecast.

Three accounting-side information sets are used. The financial-statement set contains only items reported in the annual statements: firm size (log total assets), leverage, return on assets, asset tangibility, the current ratio, and a loss indicator. A second set contains firm characteristics that are recorded outside the statements themselves, namely listing age, state ownership, and ownership balance. Valuation multiples, market-to-book and Tobin's Q , are mechanically scaled by the equity price and are therefore not financial-statement data; we keep them in a separate set. The classical accounting-based default models of Beaver (1966), Altman (1968), Ohlson (1980), Zmijewski (1984), Shumway (2001), and Campbell et al. (2008) combine reported ratios in this spirit, so our financial-statement set is best read as a reduced-form accounting-ratio benchmark rather than as a re-estimated Altman Z -score or Ohlson O -score. Several inputs of the literal scores, such as retained earnings, earnings before interest and taxes, sales, and funds from operations, are not available for the full Chinese A-share panel used here, and we do not add new data to construct them.

The separation between statement items and valuation multiples also matters for how the model-implied default probability (EDF) should be interpreted. EDF is constructed from equity value and equity volatility, but the default point in the Merton-type construction is a function of reported liabilities. EDF is therefore not a pure equity-market measure, and part of any apparent default-risk content in it may be information that a creditor holding the financial statements already has. Lagged log EDF correlates 0.53 with lagged leverage and -0.34 with the current ratio. Decomposing the overlap by information type, financial-statement variables alone explain 0.287 of the variation in lagged log EDF, adding firm characteristics raises this to 0.293, and adding valuation multiples raises it to 0.313; for equity volatility the same three

layers give 0.117, 0.149, and 0.206. The overlap is therefore dominated by reported statement variables rather than by price-scaled multiples, although the multiples still account for the top layer. The two information sets share a substantial component, so their predictive performance can be compared meaningfully only when the accounting set excludes equity prices, as it does here.

4.2 Financial statements as the benchmark information set

Table 3 reports out-of-sample performance for the six information-set specifications (1) to (6), all estimated on the same 2016–2019 training sample (6,943 observations, 242 events) and evaluated without refitting on the same 19,117 firm-years from 2020–2025, in which the observed incidence of the EDF tail is 8.18%. Two stand-alone market-based information sets are added in Table 4. All specifications include industry indicators and cluster standard errors by firm, and all predictors are lagged one year.

Table 3: Out-of-sample EDF-tail prediction by information set, 2020–2025

	(1) Financial statements	(2) + firm characteristics	(3) + valuation multiples	(4) + market risk	(5) + stasis	(6) + Amihud
AUC	0.8023	0.8026	0.8230	0.8211	0.8183	0.8234
PR-AUC	0.2662	0.2654	0.2964	0.2895	0.2880	0.2897
Brier score	0.0706	0.0706	0.0692	0.0694	0.0695	0.0696
Log loss	0.2722	0.2721	0.2625	0.2634	0.2656	0.2642
Calibration intercept	0.0694	0.0296	0.1604	0.1310	0.1125	0.1823
Calibration slope	0.6495	0.6384	0.6733	0.6656	0.6563	0.6730
Mean predicted probability	0.0353	0.0360	0.0362	0.0365	0.0360	0.0353
Observed incidence	0.0818	0.0818	0.0818	0.0818	0.0818	0.0818
Observations				19,117		
Events				1,564		

Notes: All models are estimated on the 2016–2019 training sample (6,943 observations, 242 events) and evaluated without refitting on the same 19,117 observations from 2020–2025 (1,564 events). Columns (1)–(5) form a nested sequence: (1) financial statements only; (2) adds firm characteristics recorded outside the statements; (3) adds valuation multiples; (4) adds market-based risk; (5) adds closing-price stasis. Column (6) adds the Amihud liquidity benchmark to (4) as an alternative microstructure extension. The two stand-alone market-based information sets are reported in Table 4. Calibration intercept and slope are from a logit of the outcome on the predicted log odds. Observed incidence is identical across columns.

Three results stand out. First, the financial-statement set on its own already attains an AUC of 0.8023, a PR-AUC of 0.2662, and a Brier score of 0.0706. Adding the firm characteristics that are recorded outside the statements leaves ranking performance unchanged, at an AUC of 0.8026. Adding the valuation multiples raises AUC to 0.8230 and PR-AUC to 0.2964, lowers the Brier score to 0.0692, and lowers log loss to 0.2625. This valuation block is the only market-based block that improves ranking and both proper scoring rules at the same time, and the only one whose firm-level block bootstrap interval excludes zero in the direction of improvement (Table 6). Because the multiples embed the equity price, however, they are not financial-statement information, so what they add is the incremental value of price-scaled valuation information rather than of reported fundamentals. Adding firm characteristics recorded outside the financial statements contributes little incremental predictive content beyond the financial-

statement set.

Second, market-based risk measures are substantially weaker as a stand-alone information set. Lagged log EDF and equity return volatility alone attain an AUC of 0.7625 and a Brier score of 0.0727, and adding the microstructure signals raises this only to 0.7791 (Table 4). The DeLong test rejects equality of the AUCs of the market-based and accounting-based models ($p < 0.001$). That test is a conventional paired-AUC comparison that treats the evaluation observations as independent; because the evaluation sample contains repeated annual observations for the same firms, we treat it as a reference statistic and use these stand-alone comparisons descriptively. Table 6 reports firm-level block-bootstrap inference for the separate nested information-set increments. Because EDF embeds both equity-market inputs and reported liabilities, its stand-alone predictive performance should not be interpreted as isolating credit information specific to the equity market.

Table 4: Accounting-based versus market-based information sources

	(3) Accounting and valuation	(7) Market-based risk only	(8) Market-based risk and microstructure
AUC	0.8230	0.7625	0.7791
PR-AUC	0.2964	0.2086	0.2245
Brier score	0.0692	0.0727	0.0726
Log loss	0.2625	0.2787	0.2770
Calibration intercept	0.1604	0.1669	0.3549
Calibration slope	0.6733	0.7238	0.7609
Mean predicted probability	0.0362	0.0359	0.0334
Observed incidence	0.0818	0.0818	0.0818
DeLong p , (7) versus (3)		0.00000	
DeLong p , (8) versus (7)		0.00000	
DeLong p , (8) versus (3)		0.00000	

Notes: Models are estimated on the 2016–2019 training sample and evaluated without refitting on the same 19,117 observations from 2020–2025 (1,564 events). Column (3) is the accounting and valuation information set from Table 3; column (7) contains only lagged log EDF and equity return volatility; column (8) adds closing-price stasis and the Amihud liquidity benchmark to column (7). Reported p -values are DeLong tests of the null hypothesis that the two AUCs are equal. These conventional reference statistics do not account for within-firm dependence. Firm-level block-bootstrap inference for the separate nested information-set increments is reported in Table 6.

Third, in the pooled 2020–2025 evaluation, combining the information sets does not deliver a consistent improvement over the accounting-and-valuation benchmark. Adding lagged EDF and equity volatility to the accounting and valuation model lowers AUC from 0.8230 to 0.8211 and PR-AUC from 0.2964 to 0.2895, and worsens both proper scoring rules. Adding closing-price stasis lowers AUC further, to 0.8183, and raises the Brier score and log loss. Among the market-risk and microstructure additions after valuation, only the liquidity benchmark raises AUC relative to its base specification, and it does so at the cost of a higher Brier score and log loss. Every specification understates the observed 8.18% incidence, with mean predicted probabilities between 3.53% and 3.65% and calibration slopes between 0.64 and 0.67; Section 4.4 reports this drift year by year.

4.3 Incremental out-of-sample predictive performance across information sets

Table 5 reports each block’s marginal contribution relative to the stated base specification in the pooled 2020–2025 evaluation. The increments are small, and their sign depends on the type of information being added. Adding firm characteristics recorded outside the statements changes AUC by 0.0003. Adding valuation multiples is the only clearly positive contribution, raising AUC by 0.0204 and PR-AUC by 0.0310 while improving both the Brier score and log loss. Adding market-based risk measures lowers AUC by 0.0019 and worsens both proper scoring rules. Adding closing-price stasis lowers AUC by 0.0028 and raises the Brier score and log loss. Adding the liquidity benchmark raises AUC by 0.0023 and also raises the Brier score and log loss, so its gain in ranking does not improve probability accuracy under these proper scoring rules; its PR-AUC is essentially unchanged.

Table 5: Incremental out-of-sample predictive performance across information sets

Information added	Δ AUC	DeLong p	Δ PR-AUC	Δ Brier	Δ Log loss	Δ Cal. slope
Firm characteristics added to (1), (2)–(1)	0.00025	0.74809	-0.00079	-0.00003	-0.00015	-0.01108
Valuation multiples added to (2), (3)–(2)	0.02040	0.00000	0.03099	-0.00141	-0.00958	0.03486
Market-based risk added to (3), (4)–(3)	-0.00188	0.00288	-0.00688	0.00025	0.00093	-0.00765
Closing-price stasis added to (4), (5)–(4)	-0.00275	0.00000	-0.00145	0.00013	0.00222	-0.00932
Amihud added to (4), (6)–(4)	0.00229	0.00015	0.00019	0.00020	0.00083	0.00733

Notes: Each row reports the change in out-of-sample performance when the stated information is added to the indicated specification, on the 19,117 evaluation observations used in Table 3. Positive changes in AUC and PR-AUC indicate better ranking; positive changes in the Brier score and log loss indicate worse predictive accuracy. The DeLong p -value tests the null hypothesis that the two AUCs are equal and is reported as a conventional reference; inference follows the firm-level block bootstrap of Table 6.

Table 6 reports firm-level block bootstrap intervals for the same increments, resampling firms with replacement. The bootstrap accounts for the within-firm dependence of repeated annual observations without relying on asymptotic normal approximations. The intervals are consistent with Table 5: the valuation-multiples block improves AUC and PR-AUC and lowers both proper scoring rules, with each interval excluding zero. For market-based risk, the AUC and PR-AUC intervals lie below zero while both proper-scoring intervals lie above zero. For closing-price stasis, the AUC interval lies below zero and both proper-scoring intervals lie above zero, whereas the PR-AUC interval spans zero. The liquidity benchmark’s AUC gain is accompanied by a deterioration in both proper scoring rules, with a Brier-score interval of [0.00007, 0.00035] and a log-loss interval of [0.00009, 0.00163], so its gain in ranking comes with worse scores for the probability forecasts.

As a penalized predictive benchmark, we estimate a firm-grouped five-fold cross-validated LASSO logit using the 2016–2019 training sample. The model assigns a zero coefficient to closing-price stasis, and the specifications with and without stasis produce identical out-of-sample performance at the reported precision (Table 7).

The conventional market-information benchmark behaves as expected. Adding lagged Amihud to a prior-risk specification estimated on 2016–2019 raises AUC from 0.8185 to 0.8206 and PR-AUC from 0.2819 to 0.2823, while raising the Brier score from 0.0700 to 0.0702 and log loss from 0.2667 to 0.2676. Mean predicted probabilities of 3.39–3.51% again fall far below the observed 8.18% incidence of EDF-tail states.

Table 6: Firm-level block bootstrap of the incremental performance changes

Information added	Δ AUC [95% CI]	Δ PR-AUC [95% CI]	Δ Brier [95% CI]	Δ Log loss [95% CI]
Firm chars., (2)–(1)	[-0.00154, 0.00201]	[-0.00466, 0.00305]	[-0.00022, 0.00014]	[-0.00110, 0.00084]
Valuations, (3)–(2)	[0.01771, 0.02308]	[0.02461, 0.03754]	[-0.00171, -0.00111]	[-0.01114, -0.00807]
Market risk, (4)–(3)	[-0.00317, -0.00065]	[-0.00998, -0.00379]	[0.00011, 0.00040]	[0.00021, 0.00169]
Stasis, (5)–(4)	[-0.00371, -0.00185]	[-0.00372, 0.00048]	[0.00003, 0.00023]	[0.00166, 0.00283]
Amihud, (6)–(4)	[0.00101, 0.00353]	[-0.00309, 0.00347]	[0.00007, 0.00035]	[0.00009, 0.00163]

Notes: Intervals are 95% percentile intervals from 999 firm-level block bootstrap replications, in which firms are resampled with replacement from the common 2020–2025 evaluation sample of 19,117 observations. The resampling scheme preserves the firm-level clustering of the evaluation sample. The models are not re-estimated within a replication: the fixed out-of-sample predictions of Table 3 are resampled, so the intervals capture sampling variation in the evaluation sample rather than re-estimation uncertainty in the training window. An interval that excludes zero corresponds to a two-sided bootstrap p -value below 0.05. Positive changes in AUC and PR-AUC indicate better ranking; positive changes in the Brier score and log loss indicate worse predictive accuracy.

Table 7: Firm-grouped cross-validated LASSO-logit benchmark

Metric	LASSO base	LASSO + stasis	Difference
AUC	0.8250	0.8250	0.0000
Brier score	0.0669	0.0669	0.0000
Log loss	0.2471	0.2471	0.0000
Mean predicted probability	0.0321	0.0321	0.0000
Observed incidence	0.0775	0.0775	0.0000
Selected lambda	20.90021	20.90021	
Observations	21,187	21,187	

Notes: Models are estimated using the 2016–2019 training sample and evaluated without refitting in 2020–2025. Cross-validation folds are formed at the firm level, and the penalty parameter is selected within the training sample. Both LASSO specifications are evaluated on the same 21,187 complete observations. This support is broader than the 19,117 observations in Table 3 because the unpenalized industry-effect logits used there do not generate common predictions for 2,070 otherwise complete post-2020 observations in 30 industry categories with no EDF-tail event in the 2016–2019 training sample. Penalization allows the LASSO models to retain these observations.

From the perspective of accounting-based credit assessment, two results stand out. First, reported financial statement fundamentals combined with price-scaled valuation multiples capture most of the robust incremental predictive content observed across the main evaluation metrics. Second, the equity-market signal that is positively associated with EDF-tail transitions in the panel regressions adds no incremental information to an accounting-informed model, and its inclusion modestly worsens out-of-sample performance. The two exercises are not in conflict. A signal can carry conditional information about transitions within an equity-based risk state without improving an assessment that already uses financial statement information. For a signal whose economic content is partly attributable to price-grid dependence, the two criteria need not agree, which is consistent with the price-dependence results in Section 5.

4.4 Annual calibration and forecast-timing robustness

Table 8 reports AUC, PR-AUC, the Brier score, log loss, and calibration intercept and slope separately for each validation year and for the pooled 2020–2025 period. Performance varies substantially across years—the AUC of the financial-statement model ranges from 0.7889 in 2024 to 0.8527 in 2020—and the 2024 Brier score and log loss are markedly worse than in the other validation years. The accounting-and-valuation specification has higher AUC than the financial-statement-only specification in every validation year, and adding structural market-risk measures to the former raises AUC within every validation year. The market-risk addition nevertheless lowers AUC in the pooled 2020–2025 evaluation, and its effects on the Brier score and log loss vary across years. Adding closing-price stasis lowers AUC relative to the market-risk specification in each validation year. Calibration slopes lie between 0.58 and 0.93 across years, and positive calibration intercepts accompany the general underprediction. A model trained on 2016–2019 cannot be expected to reproduce the post-2020 level of tail incidence without recalibration; we treat this as a shift in the outcome distribution rather than as evidence about the relative information content of the specifications, which is what the comparison is designed to measure.

The fixed-training design consequently exhibits substantial base-rate drift: its mean predicted probability is about 3.5%, compared with an observed EDF-tail incidence of 8.18% in the pooled post-2020 sample. As a separate sensitivity analysis, Table D.1 applies forward intercept and intercept–slope recalibration to predictions from expanding-window models using only earlier evaluation years. This exercise differs from the main fixed-training design in both model updating and evaluation support (19,943 rather than 19,117 observations). Within this exercise, recalibration moves the average predicted probability closer to the observed outcome level and improves some absolute-calibration measures, although changes in the proper scoring rules are mixed. Adding Amihud within this exercise does not yield a consistent improvement over its base specification across the proper scoring measures. Our main inference concerns incremental information content rather than deployment-level probability calibration.

Table 9 repeats the comparison under the additional accounting-lag specification, using accounting variables and valuation multiples lagged two years and structural market-risk and microstructure variables lagged one year. Under the changed predictor timing and estima-

Table 8: Year-by-year out-of-sample performance, 2020–2025

	(1) Financial statements	(3) + valuation multiples	(4) + market risk	(5) + stasis
Panel: AUC				
2020	0.8527	0.8694	0.8772	0.8748
2021	0.8311	0.8317	0.8366	0.8340
2022	0.8517	0.8674	0.8709	0.8698
2023	0.8486	0.8667	0.8687	0.8663
2024	0.7889	0.8171	0.8197	0.8153
2025	0.8229	0.8587	0.8588	0.8518
Pooled 2020–2025	0.8023	0.8230	0.8211	0.8183
Panel: PR-AUC				
2020	0.3213	0.3507	0.3663	0.3641
2021	0.2453	0.2564	0.2576	0.2556
2022	0.3249	0.3531	0.3585	0.3573
2023	0.0952	0.1085	0.1150	0.1084
2024	0.4537	0.5110	0.5242	0.5214
2025	0.1464	0.2186	0.2068	0.1974
Pooled 2020–2025	0.2662	0.2964	0.2895	0.2880
Panel: Brier score				
2020	0.0667	0.0647	0.0640	0.0642
2021	0.0606	0.0601	0.0598	0.0598
2022	0.0675	0.0665	0.0663	0.0666
2023	0.0154	0.0155	0.0158	0.0158
2024	0.1485	0.1450	0.1466	0.1467
2025	0.0293	0.0278	0.0281	0.0283
Pooled 2020–2025	0.0706	0.0692	0.0694	0.0695
Panel: Log loss				
2020	0.2357	0.2236	0.2191	0.2212
2021	0.2227	0.2217	0.2174	0.2190
2022	0.2443	0.2391	0.2369	0.2388
2023	0.0703	0.0692	0.0704	0.0706
2024	0.5946	0.5684	0.5789	0.5840
2025	0.1186	0.1101	0.1108	0.1123
Pooled 2020–2025	0.2722	0.2625	0.2634	0.2656
Panel: Calibration slope				
2020	0.8992	0.9035	0.9283	0.9206
2021	0.7608	0.7049	0.7228	0.7181
2022	0.8370	0.8247	0.8530	0.8510
2023	0.8067	0.8489	0.8592	0.8501
2024	0.5771	0.6407	0.6765	0.6554
2025	0.7546	0.8838	0.8764	0.8455
Pooled 2020–2025	0.6495	0.6733	0.6656	0.6563
Panel: Calibration intercept				
2020	0.6525	0.5862	0.6434	0.6486
2021	0.1581	0.0008	0.0053	0.0120
2022	0.6048	0.6394	0.7172	0.7348
2023	-1.4768	-1.4036	-1.4361	-1.4495
2024	0.8809	1.1369	1.3252	1.2473
2025	-0.6863	-0.2770	-0.3834	-0.4509
Pooled 2020–2025	0.0694	0.1604	0.1310	0.1125

Notes: All models are estimated on the 2016–2019 training sample and evaluated without refitting. The pooled row uses all 19,117 evaluation observations (1,564 events). Calibration intercept and slope are from a logit of the outcome on the predicted log odds. The observed tail incidence is 8.18% on the pooled sample; annual observed incidence is plotted in Figure 1.

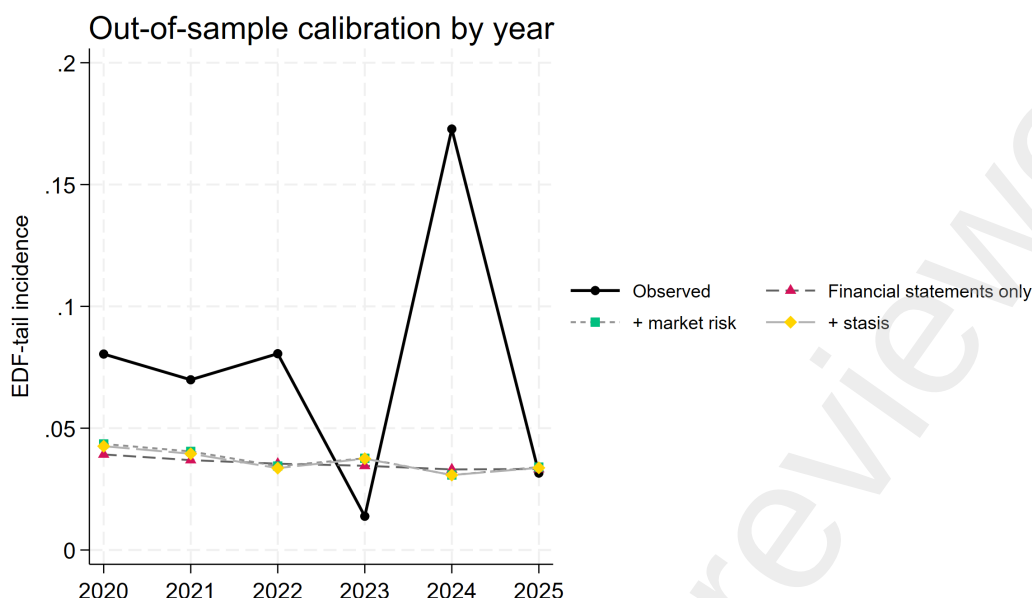


Figure 1: Annual observed and predicted EDF-tail incidence. Models are estimated using 2016–2019 data and evaluated without refitting in 2020–2025. The figure compares the observed incidence with the predictions of the financial-statement model, the accounting and valuation model augmented with market-based risk, and the same model augmented with closing-price stasis.

tion/evaluation samples, AUC is lower than in the baseline for every specification: from 0.8023 to 0.7686 for the financial-statement set and from 0.8230 to 0.7888 once valuation multiples are included. The evaluation sample is smaller, at 16,810 firm-years. In this variant, adding market-based risk slightly improves AUC (0.7888 to 0.7918) and the Brier score (0.0725 to 0.0724), but worsens log loss (0.2639 to 0.2667). Adding closing-price stasis to that specification worsens all three measures (AUC 0.7872, Brier score 0.0728, and log loss 0.2698). Because predictor timing and sample composition change together, this comparison does not isolate the effect of accounting-information age.

4.5 External credit outcomes: rating deterioration and realized default

The EDF-tail state is constructed from equity prices, so it is not an independent credit outcome. We therefore repeat the information-set comparison for issuer rating deterioration, an external credit event recorded by rating agencies, using the same fixed training window and the issuer-years with valid rating coverage in 2020–2025. The evaluation sample contains 952 issuer-years and 48 deterioration events.

The external rating outcome does not reproduce the EDF-tail ordering as cleanly. AUC rises from 0.8693 for the financial-statement set to 0.8861 once the characteristics recorded outside the statements are added and to 0.8910 once valuation multiples are included, but the additional market blocks do not improve ranking: adding market-based risk measures lowers AUC to 0.8881, adding closing-price stasis lowers it to 0.8829, and adding the liquidity benchmark lowers it to 0.8756. The proper scoring rules and PR-AUC move in the opposite direction for the market-risk block. Relative to the accounting and valuation specification, adding lagged

Table 9: Additional accounting-lag sensitivity: two-year accounting and valuation lags, one-year market lags

	(1) Financial statements	(3) + valuation multiples	(4) + market risk	(5) + stasis	(6) + Amihud
AUC	0.7686	0.7888	0.7918	0.7872	0.7869
PR-AUC	0.2164	0.2312	0.2366	0.2314	0.2315
Brier score	0.0752	0.0725	0.0724	0.0728	0.0726
Log loss	0.2869	0.2639	0.2667	0.2698	0.2684
Calibration intercept	0.1627	-0.2611	-0.2915	-0.3542	-0.4084
Calibration slope	0.7038	0.6586	0.6209	0.5973	0.5888
Mean predicted probability	0.0351	0.0546	0.0524	0.0526	0.0554
Observed incidence	0.0845	0.0845	0.0845	0.0845	0.0845
Observations	16,810				

Notes: Accounting variables are lagged two years and equity-market variables one year. Valuation multiples are treated as part of the accounting and valuation block: both the book-value component and the price-scaled component of market-to-book and Tobin's Q enter at $t - 2$, so no valuation input receives the one-year lag given to the structural market-risk and microstructure variables. Models are estimated on the pre-2020 training sample and evaluated without refitting on the same 16,810 observations from 2020–2025 (1,421 events). Because lagged accounting variables require two years of history, the training window effectively begins in 2017. Columns are otherwise defined as in Table 3.

Table 10: External credit outcome: rating deterioration by information set

	(1) Financial statements	(2) + firm characteristics	(3) + valuation multiples	(4) + market risk	(5) + stasis	(6) + Amihud
AUC	0.8693	0.8861	0.8910	0.8881	0.8829	0.8756
PR-AUC	0.5710	0.5883	0.6027	0.6083	0.6052	0.6009
Brier score	0.0371	0.0322	0.0316	0.0307	0.0308	0.0314
Log loss	0.1415	0.1274	0.1262	0.1246	0.1257	0.1290
Calibration intercept	-1.1982	-0.8957	-0.9272	-0.9001	-0.8836	-0.9750
Calibration slope	0.7164	0.7403	0.7320	0.7486	0.7426	0.7141
Mean predicted probability	0.0745	0.0616	0.0625	0.0628	0.0616	0.0630
Observed incidence	0.0504	0.0504	0.0504	0.0504	0.0504	0.0504
Observations	952					
Events	48					

Notes: The outcome is any rating downgrade, negative outlook, or downward credit watch observed during the year for issuer-years with valid rating coverage. Models are estimated on the pre-2020 sample and evaluated without refitting on 952 issuer-years from 2020–2025 containing 48 deterioration events. Issuer-years without valid rating coverage are left missing rather than coded as non-events. Columns are defined as in Table 3.

EDF and equity volatility raises PR-AUC from 0.6027 to 0.6083 and improves both the Brier score (from 0.0316 to 0.0307) and log loss (from 0.1262 to 0.1246). The external rating evidence is therefore mixed rather than uniformly favourable to the accounting specification.

Table 11 reports issuer-level block bootstrap intervals for these increments. Every interval for the market-risk block spans zero, so the small loss in AUC and the small gains in PR-AUC and the proper scoring rules are not statistically distinguishable from no change. The same is true for the stasis block. The only increments in this sample whose intervals exclude zero are the liquidity benchmark's AUC loss and its log-loss deterioration. With 952 issuer-years and 48 deterioration events, we therefore read the external rating evidence as providing no statistically precise evidence of a consistent incremental gain from market-risk or microstructure variables, and we do not treat it as independent confirmation of the EDF-tail ordering. All specifications overpredict deterioration slightly relative to the 5.04% observed incidence, with mean predicted probabilities of 6.2–7.5%.

Table 11: Issuer-level block bootstrap of the rating-deterioration increments

Information added	Δ AUC [95% CI]	Δ PR-AUC [95% CI]	Δ Brier [95% CI]	Δ Log loss [95% CI]
Market risk, (4)–(3)	-0.0029 [-0.0128, 0.0084]	0.0055 [-0.0064, 0.0193]	-0.0009 [-0.0023, 0.0001]	-0.0015 [-0.0054, 0.0019]
Stasis, (5)–(4)	-0.0052 [-0.0121, 0.0005]	-0.0031 [-0.0153, 0.0076]	0.0001 [-0.0005, 0.0007]	0.0011 [-0.0007, 0.0030]
Amihud, (6)–(4)	-0.0125 [-0.0237, -0.0023]	-0.0074 [-0.0249, 0.0080]	0.0007 [-0.0002, 0.0017]	0.0043 [0.0007, 0.0082]

Notes: Each row reports the change in out-of-sample performance for the rating-deterioration outcome of Table 10 when the stated information is added to the indicated specification. Point estimates are followed by 95% percentile intervals from 999 issuer-level block bootstrap replications, in which issuers are resampled with replacement from the 952 issuer-years used in that table; the models are not re-estimated within a replication. Positive changes in AUC and PR-AUC indicate better ranking; positive changes in the Brier score and log loss indicate worse predictive accuracy.

We also examine rating deterioration and realized default in panel specifications that isolate the coefficient on the equity-market signal on a coverage-matched sample. The coefficient on closing-price stasis is imprecise whether stasis is entered alone or jointly with lagged Amihud, and restricting the outcome to rating downgrades yields the same qualitative conclusion. Because these are coefficient-based tests rather than information-set comparisons, we report the realized-default evidence in Table D.2. Realized first default provides a more direct but substantially rarer external credit outcome. Only 19 first-default events have complete lagged predictors. In the linear probability model the coefficient on standardized stasis is 0.0002 ($p = 0.1713$), and the Firth-logit and complementary-log-log estimates are also imprecise. Given the small number of realized default events, these results are treated as complementary external credit evidence rather than as primary evidence of cross-market transportability.

5 The Economic Content of Equity-Market Signals

5.1 Baseline liquidity–risk relation

As a benchmark, conventional liquidity specifications reproduce the familiar positive association between stock illiquidity and model-implied default risk under both pooled and fixed P90 definitions. Table 12 reports the corresponding transition estimates; the pooled threshold is a descriptive benchmark, whereas the fixed P90 cutoff is determined from 2015–2019 EDF

observations and held constant thereafter.

Table 12: EDF-tail transitions under pooled and fixed P90 cutoffs

	(1) Entry Pooled	(2) Entry Fixed P90	(3) Persistence Pooled	(4) Persistence Fixed P90	(5) Re-entry Pooled	(6) Re-entry Fixed P90
Lagged ln Amihud	0.0216 (0.0067)	0.0202 (0.0063)	0.0552 (0.0254)	0.0410 (0.0248)	0.0712 (0.0166)	0.0708 (0.0166)
N	29102	29381	1550	1314	8969	8659
Outcome mean	0.0540	0.0483	0.3206	0.3189	0.1049	0.0957
Events	1572	1418	497	419	941	829
Year-wild p -value	0.0060	0.0068	0.1016	0.2303	0.0023	0.0017

Notes: Clustered standard errors are reported in parentheses; year-level wild-bootstrap p -values are reported in the table. The pooled cutoff is reported only as a descriptive benchmark, whereas the fixed P90 cutoff is determined using 2015–2019 EDF observations and held constant thereafter. All winsorized risk and control variables use the fixed 2015–2019 preprocessing cutoffs. “Persistence” denotes remaining in the EDF tail from year $t - 1$ to year t , i.e., $\text{Tail}_{i,t-1} = 1$ and $\text{Tail}_{i,t} = 1$. Models include lagged default risk, equity volatility, firm controls, and firm and year fixed effects. Reported p -values are based on the year-level wild cluster bootstrap.

5.2 Signal selection in the pre-2020 sample

We separate signal selection from subsequent temporal validation. Using only pre-2020 information, we compare the five candidate signals defined in Section 3. Each signal is oriented so that a larger value represents greater trading friction, standardized on the common 2016–2019 first-observed-entry risk set, and entered separately into the same first-observed-entry specification. The directional screen retains a candidate if its discovery coefficient is positive, consistent with the directional prediction of the liquidity–default relation; no statistical significance threshold is imposed.

Table 13 reports the standardized discovery-period estimates. Closing-price stasis is the only candidate that satisfies the directional screen. Its one-standard-deviation coefficient is 0.0020, with a two-way clustered standard error of 0.0023. Because the discovery sample contains only 40 entry events, the screen is used solely for signal selection, and we place no inferential weight on the discovery estimate itself.

Post-2020 outcomes play no role in the screening exercise. The candidate set, directional rule, selected signal, and EDF cutoff are held fixed when the analysis moves to the 2020–2025 validation period. The distinction between first-observed entry and re-entry is treated as a transparent decomposition of the validation sample rather than as a preregistered design choice.

The P90 cutoff is determined from the 2015–2019 EDF distribution, whereas signal selection uses the common 2016–2019 discovery sample. Neither the signal definition nor the cutoff is re-estimated using 2020–2025 observations. The validation exercise therefore asks whether the direction of the pre-2020 association persists in later data without reselecting the signal or threshold ex post.

Table 13: Standardized discovery-period screen of candidate equity-market signals

Candidate signal	1-SD coefficient	Clustered SE	Observations	Events
ln Amihud	-0.0005	0.0027	4,792	40
Raw low trading	-0.0002	0.0033	4,792	40
Abnormal low trading	-0.0017	0.0020	4,792	40
Closing-price stasis	0.0020	0.0023	4,792	40
Inferred no-trade gap	-0.0005	0.0021	4,792	40

Notes: The five candidate signals are compared on the common 2016–2019 first-observed-entry discovery sample. Continuous signals are standardized to one standard deviation within this sample, so each coefficient represents the change in entry probability associated with a one-standard-deviation increase in the corresponding signal. Selection is based only on the sign of the coefficient and imposes no statistical significance threshold. Because the discovery sample contains only 40 entry events, the estimates are not interpreted inferentially. Standard errors are two-way clustered by firm and year. Closing-price stasis is the only signal with a positive discovery coefficient and is held fixed for the 2020–2025 validation period.

5.3 Temporal persistence in EDF-tail entry and re-entry

Having fixed closing-price stasis as the selected equity-market signal, we examine its association with first-observed EDF-tail entry and with re-entry after a prior tail episode and subsequent exit. In the broader sample, the coefficient on stasis is 0.5924 for first-observed entry and 1.0901 for re-entry. Adding transition-history controls leaves the re-entry coefficient positive at 1.0231 ($p = 0.0027$).

The positive association persists in the 2020–2025 validation period. The coefficient on stasis is 0.5605 ($p = 0.0672$) for first-observed entry and 0.8678 ($p = 0.0228$) for history-adjusted re-entry. In the latter sample, a one-standard-deviation increase in stasis corresponds to a 3.10 percentage-point increase in re-entry probability, equivalent to 28.4% of the mean re-entry probability.

Panel B of Table 14 reports the history-adjusted re-entry specification separately for 2017–2019 and 2020–2025. The earlier coefficient is positive but small and imprecisely estimated, whereas the 2020–2025 coefficient is 0.8678. Because 2017–2019 overlaps with the discovery window, it is treated only as a descriptive pre-period benchmark rather than as independent validation evidence. We do not interpret the difference in coefficient magnitudes as evidence of parameter instability or as a formal cross-period difference.

Within 2020–2025, the result is not driven by any single validation year. The history-adjusted re-entry coefficient remains positive when each validation year is omitted in turn, ranging from 0.5693 to 1.1191 (Table C.2). We use temporal persistence in an operational rather than a parameter-stability sense: the signal selected using pre-2020 information must retain the predicted direction in the fixed 2020–2025 validation sample and in leave-one-validation-year-out exercises. We do not require coefficient magnitudes to be equal across periods. The evidence therefore supports directional temporal persistence in the history-adjusted re-entry relation.

The re-entry association does not by itself establish external credit content. It remains positive when a prior tail exit is required to fall below P89, P87.5, or P85 of the 2015–2019 EDF distribution before a subsequent return to the fixed P90 tail. This reduces concern that the re-entry result is driven solely by repeated crossings near the P90 threshold. First-observed-entry coefficients also remain positive when at least three or four years of prior EDF history are

Table 14: First-observed entry, re-entry, and the descriptive temporal split
Panel A: First-observed entry and re-entry after a prior tail episode and subsequent exit

	Full sample			Post-2020 validation, 2020–2025		
	(1) First	(2) Re-entry	(3) Re-entry +history	(4) First	(5) Re-entry	(6) Re-entry +history
Lagged stasis	0.5924 (0.1837)	1.0901 (0.3579)	1.0231 (0.2611)	0.5605 (0.2091)	1.4461 (0.4838)	0.8678 (0.2969)
<i>N</i>	20646	8668	8525	15493	6295	6191
Outcome mean	0.0261	0.0956	0.0956	0.0274	0.1091	0.1092
Events	538	829	815	424	687	676
Year-wild <i>p</i> -value	0.0141	0.0062	0.0027	0.0672	0.0197	0.0228
Years	2016–2025	2017–2025	2017–2025	2020–2025	2020–2025	2020–2025

Standard errors in parentheses

Panel B: History-adjusted re-entry by period

	2017–2019	2020–2025
Lagged closing-price stasis share	0.1335 (0.3266)	0.8678 (0.2969)
<i>N</i>	2,248	6,191
Outcome mean	0.0503	0.1092
Events	113	676
Years	2017–2019	2020–2025

Notes: “History” adds years since the most recent tail exit, prior tail-spell length, the historical maximum of the log-transformed EDF risk measure, and prior tail-entry count. Panel B estimates the same history-adjusted specification separately in 2017–2019 and 2020–2025. Because 2017–2019 overlaps with the discovery window, it is reported only as a descriptive pre-period benchmark rather than as independent validation evidence. Standard errors are two-way clustered by firm and year. Because the earlier period contains only three year clusters, Panel B reports neither a year-level wild-bootstrap *p*-value nor a formal test of coefficient equality.

required, reducing sensitivity to short observed risk histories.

All main equity-state analyses use preprocessing thresholds estimated from 2015–2019 data. Table B.1 reports the earlier pooled full-panel preprocessing as a robustness comparison and yields similar conclusions. The main findings are therefore not driven by the choice of preprocessing window.

5.4 Post-2020 behaviour of all five candidate signals

The discovery screen selected closing-price stasis because it was the only candidate with a positive coefficient on the pre-2020 sample. To show what that selection does and does not establish, Table 15 reports the standardized coefficient on each of the five candidate signals in the 2020–2025 first-entry sample and in the 2020–2025 re-entry sample.

Table 15: Post-2020 validation of all five candidate equity-market signals

Candidate signal	Post-2020 validation sample	
	First entry	Re-entry
ln Amihud	0.0167 (0.0060)	0.1096 (0.0336)
Raw low trading	0.0160 (0.0068)	0.0967 (0.0310)
Abnormal low trading	0.0014 (0.0031)	0.0034 (0.0130)
Closing-price stasis	0.0113 (0.0042)	0.0293 (0.0099)
Inferred no-trade gap	-0.0052 (0.0231)	0.0294 (0.0937)

Notes: Each signal is oriented so that a larger value represents greater trading friction and is standardized on the common 2016–2019 first-observed-entry discovery sample, so each coefficient corresponds to a one-standard-deviation change. Coefficients are from issuer-fixed-effects linear probability models; standard errors are two-way clustered by firm and year and are reported in parentheses. The first-entry models use 15,493 firm-year observations and 424 first-entry events, falling to 15,477 observations for abnormal low trading; the re-entry models use 6,295 observations and 687 re-entry events, falling to 6,286 for abnormal low trading. These columns are not used in signal selection; the pre-2020 screen coefficients are reported in Table 13.

Only stasis has a positive coefficient in the discovery window, which is why it was selected (Table 13). In the post-2020 data, four of the five candidate signals have positive first-entry coefficients, whereas all five have positive re-entry coefficients. The largest standardized coefficients are those on log Amihud and raw low trading rather than on stasis. Several candidates that fail the pre-2020 directional screen are therefore positively associated with post-2020 transitions. The screen identifies a signal whose sign is stable across the two periods, not a signal that is uniquely informative in the validation period, and we do not read the selection as evidence that stasis ranks above the other candidates. This is the intended asymmetry of the design: the screen fixes one signal in advance, and the validation period is reported without reselection, including results that are less favourable to the selected signal.

5.5 Price dependence of closing-price stasis

Raw low trading value is positively associated with EDF-tail entry in the broader sample, whereas abnormal low trading and inferred no-trade gaps do not show comparably precise positive associations. Raw low trading also fails the pre-2020 discovery screen, where its standardized coefficient is negative. These comparisons distinguish closing-price stasis from generic low-trading measures and motivate examining its microstructure content separately.

Table 16: Trading activity, closing-price stasis, and inferred no-trade gaps

	(1)	(2)	(3)	(4)
	Raw low value	Abnormal low	Stasis	No-trade gap
Lagged low log trading value	0.0434 (0.0109)			
Lagged abnormal low trading		0.0075 (0.0107)		
Lagged stasis			0.9331 (0.2358)	
Lagged inferred no-trade share				0.0334 (0.0413)
<i>N</i>	29102	29069	29102	29102
Year-wild <i>p</i> -value	0.0015	0.5687	0.0035	0.5328

Notes: Clustered standard errors are reported in parentheses; year-level wild-bootstrap *p*-values are reported in the table. Stasis is the annual share of valid stock days on which the cash-dividend-reinvestment-adjusted return is zero while trading value is positive. The denominator includes all days with nonmissing return and trading value. A stasis event does not imply an absence of intraday price movement or literal equality of unadjusted closing prices on company-action dates. All winsorized risk and control variables use the fixed 2015–2019 preprocessing cutoffs.

We next compare formal trading-state controls with price-related controls on a common 2020–2025 re-entry sample. The baseline coefficient on stasis is 0.8537 ($p = 0.0241$). Controls for price limits, special-treatment status, and formal suspensions leave the coefficient largely unchanged at 0.8753. By contrast, the coefficient falls to 0.1845 after adding the relative-tick block and to 0.2651 after adding fine price-level controls. Including all control blocks reduces it to -0.0646 . This attenuation indicates substantial price dependence of the stasis–re-entry association.

The nominal-close robustness yields a similar baseline association and attenuation pattern. The coefficient on nominal-close stasis is 0.8621 ($p = 0.0245$), compared with 0.8537 for adjusted-return stasis on the same estimation sample. Adding the relative-tick block reduces the coefficient to 0.1994, while fine price-level controls reduce it to 0.2726. Including both blocks yields a coefficient of -0.0514 . Thus, the price-dependence pattern is not specific to the use of a cash-dividend-reinvestment-adjusted return in the baseline stasis measure.

Taken together, the common-sample results indicate substantial price dependence of the stasis–re-entry association. Relative-tick and fine price-level controls substantially attenuate the baseline stasis coefficient, whereas controls for formal trading states leave it largely unchanged. The interaction between stasis and relative tick size is negative in the relative-tick specification

Table 17: Common-sample comparison of official trading states and price dependence

	(1) Baseline	(2) +Official states	(3) +Relative-tick block	(4) +Price deciles	(5) +All blocks
Lagged closing-price stasis	0.8537 (0.2993)	0.8753 (0.2948)	0.1845 (0.4049)	0.2651 (0.3078)	-0.0646 (0.4342)
Lagged limit-up share		0.4975 (0.4815)			0.2923 (0.5188)
Lagged limit-down share		-0.2428 (0.8419)			-0.2624 (0.8886)
Lagged ST-status share		-0.0838 (0.0491)			-0.1242 (0.0620)
Lagged ln(suspension days+1)		0.0075 (0.0120)			0.0104 (0.0118)
Lagged relative tick (pp., centered)			0.9908 (0.3995)		0.5659 (0.4157)
Stasis x relative tick			-1.4820 (0.7079)		0.1328 (0.9608)
<i>N</i>	6182	6182	6182	6182	6182
Events	675	675	675	675	675
Year-wild p-value: stasis	0.0241	0.0379	0.7679	0.5859	0.9118

Standard errors in parentheses

Notes: All columns use the same 2020–2025 history-adjusted re-entry sample. Relative tick is defined as $100 \times 0.01 / \text{Price}$ and mean-centered before forming the interaction. Reported *p*-values are based on the year-level wild cluster bootstrap.

Table 18: Nominal-close stasis and attenuation by price-related controls

	(1) Baseline	(2) +Relative tick	(3) +Price deciles	(4) +Tick+deciles
Lagged nominal-close stasis	0.8621 (0.2955)	0.1994 (0.3945)	0.2726 (0.3001)	-0.0514 (0.4435)
Lagged relative tick (pp., centered)		0.9912 (0.3984)		0.4652 (0.3357)
Nominal stasis x relative tick		-1.5223 (0.6986)		0.2662 (0.9348)
<i>N</i>	6182	6182	6182	6182
Events	675	675	675	675
Year-wild p-value: nominal stasis	0.0245	0.7365	0.5567	0.9259

Notes: Clustered standard errors are reported in parentheses; year-level wild-bootstrap *p*-values are reported in the table. All columns use the same 2020–2025 history-adjusted re-entry sample. Nominal-close stasis is the lagged annual share of valid stock days on which trading value is positive and the unadjusted closing price is unchanged from its immediately preceding observed value. Relative tick is expressed in percentage points as $100 \times 0.01 / \text{Price}$ and mean-centered before forming the interaction. Reported *p*-values are based on the year-level wild cluster bootstrap. All winsorized risk and control variables use the fixed 2015–2019 preprocessing cutoffs.

but is not stable across specifications, so we do not interpret it as identifying a structural price-grid relation. The attenuation results instead show that temporal persistence and price-independent information are distinct properties of the signal.

A complementary price-bin exercise estimates the same history-adjusted re-entry model across low-, middle-, and high-price groups. The stasis coefficients are 0.9411, 0.3902, and 1.8025, respectively. The pattern is non-monotonic and therefore does not imply that the stasis association moves mechanically with nominal price or relative tick size. Instead, the price-bin evidence complements the common-sample attenuation results by showing that the association varies across the price distribution.

These findings point to a measurement boundary rather than a causal tick-size mechanism. Because the absolute tick is fixed, relative tick size varies mechanically with nominal share price, while price level may also capture differences in distress, firm characteristics, investor clientele, and the information environment. Our design does not separate these channels. The evidence therefore shows that the stasis association is not invariant to price-related controls, but it does not identify the economic source of that dependence. More generally, temporal persistence of a zero-return-based equity-market signal should not be interpreted as price-independent default-risk information.

6 Cross-Market Evidence: Corporate Bond Spreads

6.1 Corporate bond spread evidence

We treat credit-market evidence as external-validity support for the accounting-informed assessment rather than as a second line of analysis, because the bond sample covers a particular and potentially selective population of issuer-years. We therefore examine corporate bond spreads, which are credit-market outcomes formed outside the equity-price information set used to construct EDF. Because reported zero-turnover yields need not correspond to executed market-clearing trades, the bond-spread evidence should be interpreted as an external reported-yield test rather than as a clean secondary-market transaction-price test. The estimates do not establish the absence of cross-market information. Rather, they provide no precise and coverage-robust evidence that closing-price stasis carries incremental information for observed corporate-bond spreads.

Table F.1, Panel A reports issuer-year estimates based on the nearest-node same-day government-curve construction. The coefficient on lagged stasis remains positive but imprecisely estimated after controlling for lagged Amihud, firm characteristics, and bond-market controls. A one-standard-deviation increase in stasis corresponds to an estimated 5.77-basis-point increase in the corporate-government yield spread, with a 95% wild-bootstrap confidence interval of $[-23.62, 37.29]$ basis points. The interval spans effects of either sign and therefore indicates substantial uncertainty rather than equivalence or an economically negligible effect.

Bond-level fixed-effects estimates lead to a similar conclusion (Table F.1, Panel C): in the fully controlled specification, the coefficient on lagged stasis is 2.6932 ($p = 0.4139$) and remains imprecise.

Results remain imprecise when the outcome is a consecutive-year change in spreads (Table F.1, Panel B); a spread-level model on the same sample is also imprecise, so the lack of precision is not solely attributable to differencing or the change-sample restriction.

6.2 Bond-market coverage and sample selectivity

The cross-market evidence is sensitive to bond-market coverage. Because issuer-years with active secondary-market observations differ systematically from the rest of the spread sample, the credit-market estimates should be interpreted as conditional on the issuer-years represented under each coverage definition rather than as population-wide evidence of transportability. Of the 1,739 issuer-years in the main spread sample, 574 satisfy the current positive-turnover activity criterion. The two groups are similar in average size, EDF, leverage, and lagged ROA, but differ substantially in other characteristics. Most notably, active issuer-years have a much lower state-ownership share (47.43% versus 79.98%) and higher reported spreads (2.924% versus 1.687%). Table F.3, Panel A reports these differences.

The large ownership difference is consistent with economically meaningful segmentation in China's corporate bond market: state-owned and non-state-owned issuers may differ in perceived credit backing, financing access, investor clientele, and secondary-market activity, consistent with evidence on government support in Chinese credit markets (Geng and Pan, 2024) and on the role of state ownership in the liquidity–default relation (Zhao et al., 2026). Our design does not identify any specific ownership, government-backing, or liquidity channel. These comparisons document coverage selectivity; they do not correct for it.

The nearest-node positive-turnover outcome specification retains 538 issuer-years (Table E.2, Panel C), fewer than the 574 classified as currently active within the main spread sample. It does not yield precise evidence of a broadly transportable relation: the issuer-level coefficient on lagged stasis is 7.5365 ($p = 0.4140$), and bond-level and spread-change estimates are likewise imprecise. These specifications are coverage-sensitivity exercises rather than evidence that the relation generalizes. The earlier fixed-grid construction provides a useful diagnostic: holding fixed the 224 issuer-years selected by the fixed-grid positive-turnover rule, the stasis coefficient remains positive across all four curve–outcome combinations, suggesting that the positive estimate is more closely tied to the selected issuer-year coverage than to a particular curve or yield measure (Table E.2, Panel B).

To reduce conditioning on contemporaneous bond-market activity, we classify activity using prior-year information (Table F.3, Panel B). The full prior-coverage estimate is imprecise, and the coefficient on lagged stasis is -0.3517 ($p = 0.9875$) within the prior-active group, with a 95% wild-bootstrap interval of $[-130.21, 121.47]$ basis points for a one-standard-deviation increase. Prior-year classification therefore does not reproduce the positive fixed-grid estimate, and the wide intervals across broad and activity-conditioned specifications indicate substantial uncertainty about effect magnitude rather than equivalence or economically negligible effects.

7 Discussion and Conclusion

Reported financial statement fundamentals deliver most of the out-of-sample discrimination: the statement-only model attains an AUC of 0.8023 for subsequent EDF-tail states. The model with firm characteristics and price-scaled valuation multiples attains 0.8230 and improves both proper scoring rules in the pooled 2020–2025 evaluation. Valuation is the only market-based block with consistent gains. Structural market-based risk measures lower pooled AUC and worsen both proper scoring rules, even though they raise AUC within every validation year. Adding closing-price stasis lowers pooled AUC further and worsens both proper scoring rules; the liquidity benchmark raises pooled AUC only while worsening both proper scoring rules. Firm-level block bootstrap intervals confirm that no market block other than valuation multiples delivers consistent incremental gains across discrimination and the proper scoring measures.

Statistical association, out-of-sample predictive content, and accounting decision usefulness are distinct properties of a signal. Closing-price stasis is used as a diagnostic case rather than interpreted as the strongest market-friction predictor. It is positively associated with history-adjusted re-entry in the fixed 2020–2025 validation sample and across leave-one-validation-year-out exercises, yet it adds no incremental out-of-sample information to an accounting-informed model and is not the strongest of the five candidate signals in the post-2020 data. Relative-tick and fine price-level controls substantially attenuate the association, so temporal persistence and price-independent information are distinct properties of a signal.

Cross-market transportability is not established. Rating-deterioration evidence is mixed, and the corporate-bond estimates are sensitive to issuer coverage and provide no precise, coverage-robust evidence that stasis carries information for credit outcomes formed outside the equity-price information set. Active and other issuer-years differ systematically in ownership, reported spreads, and yield coverage, and the positive fixed-grid estimate is not reproduced under broader activity definitions or when activity is classified using prior-year information. These comparisons document coverage sensitivity rather than correct for sample selection. Matching to same-day government benchmarks reduces maturity mismatch but does not isolate expected default losses, and reported zero-turnover yields need not correspond to executed trades. Short panels and rare events further limit inference, and the transition estimates should be interpreted as conditional associations rather than causal effects.

The implication for accounting-based credit assessment is that market signals should be evaluated against the accounting information set that an assessment already uses and against local market design, rather than inferred from statistical association or temporal persistence. Zero-return-based measures should not be treated as price-independent distress signals without accounting for nominal price and relative tick size, and persistence within the equity market does not establish broadly transportable default-risk information where bond-market coverage is selective. Closing-price stasis thus displays directional temporal persistence while remaining substantially price dependent, and its apparent credit-market relevance varies with the issuer-years represented in the bond sample.

Appendix

A Descriptive Correlations

Table A.1: Pairwise correlations among principal continuous variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1) EDF	1.000												
(2) Stasis	-0.082	1.000											
(3) Amihud	0.058	-0.013	1.000										
(4) Equity volatility	0.485	-0.419	0.245	1.000									
(5) Log assets	0.058	0.253	-0.188	-0.361	1.000								
(6) Leverage	0.247	0.228	-0.073	-0.096	0.449	1.000							
(7) ROA	-0.124	-0.183	0.040	-0.051	0.060	-0.350	1.000						
(8) Tangibility	-0.004	0.160	-0.030	-0.097	0.128	0.104	-0.034	1.000					
(9) Current ratio	-0.101	-0.183	0.080	0.127	-0.347	-0.689	0.199	-0.263	1.000				
(10) Market-to-book	0.037	-0.250	0.124	0.383	-0.340	0.050	-0.021	-0.101	0.053	1.000			
(11) Tobin's Q	-0.106	-0.278	0.005	0.295	-0.365	-0.215	0.076	-0.114	0.195	0.775	1.000		
(12) Listing age	-0.027	0.351	-0.165	-0.313	0.458	0.334	-0.133	0.107	-0.288	-0.152	-0.108	1.000	
(13) Ownership balance	0.017	-0.087	0.043	0.115	-0.113	-0.082	-0.043	-0.091	0.086	0.083	0.067	-0.176	1.000

Notes: The table reports Pearson pairwise correlations in the broad 2015–2025 analysis panel. Variable definitions are the same as in Table 2 of the main manuscript. Binary indicators are omitted. Pairwise available observations are used, so the number of observations can vary where valuation, listing-age, or ownership data are missing. EDF and Amihud are reported in their raw levels for descriptive purposes; regression specifications use the logarithmic transformations defined in Section 3.2.

B Preprocessing and Prediction-Support Robustness

Table B.1: Pre-2020 preprocessing and legacy pooled-winsorization robustness

Specification or metric	Pre-2020 main	Legacy pooled	Main SE / p -value	Main N / events
History-adjusted re-entry	0.8678	0.8990	0.2969 / 0.0228	6,191 / 676
+ Relative-tick block	0.1845	0.2195	0.4049 / 0.7679	6,182 / 675
+ Price deciles	0.2651	0.2953	0.3078 / 0.5859	6,182 / 675
Prediction AUC: prior risk	0.8185	0.8182	–	19,117
Prediction AUC: +Amihud	0.8206	0.8200	–	19,117
Brier score: prior risk	0.0700	0.0699	–	19,117
Brier score: +Amihud	0.0702	0.0700	–	19,117
Log loss: prior risk	0.2667	0.2672	–	19,117
Log loss: +Amihud	0.2676	0.2680	–	19,117

Notes: The pre-2020 column uses the main preprocessing rule, with 1st and 99th percentile cutoffs estimated from 2015–2019 observations and held fixed thereafter. The legacy column uses pooled full-panel cutoffs. Fixed-split models are estimated using 2016–2019 data and evaluated without refitting in 2020–2025. Differences in evaluation sample size across prediction methods reflect model-specific prediction support. Regression rows report the stasis coefficient, clustered standard error, and year-level wild-bootstrap p -value.

C Transition-History and Selection Robustness

Table C.1: Transition-history robustness

Panel A: Re-entry with threshold buffers below P90

	(1)	(2)	(3)	(4)
	P90	P89	P87.5	P85
Lagged closing-price stasis	0.8678 (0.2969)	0.8851 (0.2917)	0.9283 (0.2861)	0.9659 (0.2902)
N	6191	6054	5808	5492
Prior EDF requirement	Below P90	Below P89	Below P87.5	Below P85
Events	676	650	599	541
Year-wild p-value	0.0228	0.0288	0.0094	0.0103

Panel B: First-observed entry with minimum EDF history

	(1)	(2)	(3)
	Baseline	3-year history	4-year history
Lagged closing-price stasis	0.5605 (0.2091)	0.3129 (0.1271)	0.3526 (0.1300)
N	15493	12156	10537
Minimum prior EDF history	No minimum	At least 3 years	At least 4 years
Events	424	308	277
Year-wild p-value	0.0672	0.0302	0.0138

Panel C: Stasis measurement and minimum-valid-day robustness

	(1)	(2)	(3)	(4)	(5)
	Adjusted	Nominal	Adjusted	Nominal	Nominal no actions
Lagged stasis share	0.8678 (0.2969)	0.8621 (0.2955)	0.8678 (0.2969)	0.8621 (0.2955)	0.8588 (0.2898)
N	6191	6182	6191	6182	6182
Coverage rule	All valid days	All valid days	At least 120 days	At least 120 days	At least 120 days
Outcome mean	0.1092	0.1092	0.1092	0.1092	0.1092
Events	676	675	676	675	675
Year-wild p-value	0.0228	0.0245	0.0228	0.0245	0.0154

Notes: Clustered standard errors are reported in parentheses; year-level wild-bootstrap p -values are reported in each table. Panel A requires $EDF_{i,t-1}$ to fall below the indicated percentile of the 2015–2019 EDF distribution before a subsequent return to the fixed P90 tail. Panel B requires at least three or four years of observed EDF history before the transition year. Panel C imposes the indicated minimum-valid-day requirements under the adjusted-return or nominal-close definition; nominal-close stasis is the annual share of valid stock days on which trading value is positive and the unadjusted closing price is unchanged from its immediately preceding observed value, and “No actions” excludes observable company-action dates. All specifications use the 2020–2025 validation period and the fixed 2015–2019 preprocessing cutoffs. Panels A and C examine re-entry, whereas Panel B examines first-observed entry.

Table C.2: Leave-one-validation-year-out re-entry coefficients

Omitted year	Baseline coefficient	Price-adjusted coefficient	Observations	Events
None	0.8537 (0.2993)	-0.0660 (0.4545)	6,182	675
2020	0.6417 (0.4274)	-0.4056 (0.2895)	5,287	559
2021	0.6896 (0.3716)	0.1320 (0.4525)	5,335	566
2022	0.5693 (0.2578)	-0.4994 (0.3236)	5,242	519
2023	1.1191 (0.2962)	0.0486 (0.4943)	5,012	599
2024	0.9442 (0.3624)	0.4456 (0.4140)	4,626	362
2025	1.0060 (0.3420)	-0.0852 (0.6352)	4,990	653

Notes: The history-adjusted baseline and price-adjusted specifications use the common sample from the price-dependence analysis. Each row omits one year from the 2020–2025 validation period. Coefficients are reported descriptively without additional year-level wild-bootstrap inference after a validation year is removed.

Table C.3: Sample-selection robustness of entry, persistence, and re-entry

Panel A: Entry			
	Complete controls	Reduced controls	IPW complete
Lagged ln Amihud	0.0202 (0.0063)	0.0155 (0.0052)	0.0200 (0.0062)
<i>N</i>	29381	30956	29381
Outcome mean	0.0483	0.0469	0.0483
Events	1418	1453	1418
Year-wild p-value	0.0067	0.0048	0.0067
Panel B: Persistence			
	Complete controls	Reduced controls	IPW complete
Lagged ln Amihud	0.0410 (0.0248)	0.0352 (0.0127)	0.0393 (0.0246)
<i>N</i>	1314	2005	1314
Outcome mean	0.3189	0.2658	0.3189
Events	419	533	419
Year-wild p-value	0.2387	0.0763	0.2450
Panel C: Re-entry			
	Complete controls	Reduced controls	IPW complete
Lagged ln Amihud	0.0708 (0.0166)	0.0679 (0.0146)	0.0707 (0.0164)
<i>N</i>	8659	8750	8659
Outcome mean	0.0957	0.0961	0.0957
Events	829	841	829
Year-wild p-value	0.0016	0.0032	0.0014

Notes: The table reports robustness estimates for EDF-tail entry, persistence, and re-entry under alternative treatments of control-variable availability. Transition states use the fixed 2015–2019 P90 cutoff, and all winsorized risk and control variables use the fixed 2015–2019 preprocessing cutoffs. IPW denotes stabilized inverse-probability weighting for observed differences in complete-control sample inclusion. The weighting exercise is a sample-selection sensitivity analysis and does not address selection on unobservables.

D Prediction and External-Credit Robustness

Table D.1: Forward recalibration on a common evaluation sample

Metric	Raw	Raw	Intercept	Intercept	Int.+slope	Int.+slope
	Base	+Amihud	Base	+Amihud	Base	+Amihud
Brier score	0.0678	0.0681	0.0710	0.0710	0.0688	0.0688
Log loss	0.2486	0.2490	0.2537	0.2530	0.2434	0.2431
Mean predicted probability	0.0479	0.0463	0.0740	0.0730	0.0739	0.0732
Calibration intercept	-0.1153	-0.0620	-0.7237	-0.6985	-0.2938	-0.2809
Calibration slope	0.6729	0.6828	0.5628	0.5703	0.8062	0.8074
Observed incidence	0.0805	0.0805	0.0805	0.0805	0.0805	0.0805
Observations	19,943	19,943	19,943	19,943	19,943	19,943

Notes: All six columns use the same 19,943 observations in 2020–2025. The base model includes prior EDF and firm controls; the augmented model adds lagged log Amihud. Raw predictions are regenerated each year using an expanding training window starting in 2016, unlike the fixed-training design in the main text. For each evaluation year after 2020, recalibration parameters are estimated using only earlier out-of-sample evaluation years; no recalibration is applied to the initial 2020 evaluation year. The procedure therefore uses no information from the current or future evaluation year. All winsorized predictors use the fixed 2015–2019 preprocessing cutoffs.

Table D.2: First default: rare-event and discrete-time models

	(1)	(2)	(3)
	LPM	Firth logit	Discrete-time hazard
Lagged stasis share (standardized)	0.0002 (0.0002)	0.2043 (0.1683)	0.1434 (0.1749)
<i>N</i>	31446	34448	34448
Events	19	19	19
Year-wild <i>p</i> -value (LPM only)	0.1713	.	.

Standard errors in parentheses

Notes: Closing-price stasis is standardized within the broader reduced-control risk set. The LPM includes industry fixed effects and therefore uses a smaller sample than the Firth-logit and complementary-log-log specifications, which omit industry fixed effects. All three models contain the same 19 first-default events. Firth logit uses penalized likelihood, and the complementary-log-log specification is estimated as a discrete-time hazard model. The year-level wild-bootstrap *p*-value is reported only for the LPM; it is not reported for the Firth-logit or complementary-log-log specifications.

E Credit-Spread Construction, Coverage, and Sensitivities

The nearest-available same-day rule is the main spread construction. The fixed-grid rule retains 12.41 million observations, or 47.82% of prefiltered valid bond-day yield observations, and is used only as a robustness check. Comparisons between retained and excluded observations illustrate the coverage loss and sample selectivity introduced by the more restrictive construction.

Table E.1: Fixed-grid retained versus excluded bond-day characteristics

Characteristic	Matched	Unmatched	Matched N	Unmatched N
Bond YTM (percent)	5.686	4.995	12,407,991	13,538,525
Remaining maturity (years)	8.042	5.403	12,407,991	13,538,525
Daily turnover (RMB million)	2.038	0.813	12,407,991	13,538,525
Positive-turnover share (percent)	3.19	2.30	12,407,991	13,538,525
Issuer log assets	25.026	25.007	435,808	381,718
Issuer EDF (percent)	0.466	0.308	435,808	381,718

Notes: Means are calculated across bond-day observations. Turnover is the reported daily trading amount. A valid reported-yield observation does not necessarily correspond to a positive-turnover day.

The 224 issuer-years selected by the fixed-grid positive-turnover rule continue to produce positive stasis estimates across alternative government-curve constructions and corporate-yield definitions. Because the estimation sample is held fixed across these specifications, the same-sample comparison suggests that the positive fixed-grid estimate is more closely associated with the selected issuer-year coverage than with a particular curve construction or yield-measurement procedure. The result is not reproduced in the broader 538-issuer-year nearest-node active sample or under the prior-year activity definition.

Table E.2: Bond-market coverage and transportability robustness

Panel A: Alternative same-day bracketing and government-curve benchmarks

Specification	Stasis	SE	N	Wild p
Main nearest nodes	1.4264	3.3415	1,739	0.6821
Fixed-grid robustness	2.1318	3.0559	1,097	0.4382
Nearest: fixed-grid issuer-years	1.2626	3.1357	1,097	0.6728
YTM: identical-day sample	1.4582	3.3382	1,738	0.6743
Spot: identical-day sample	1.4674	3.3400	1,738	0.6726

Panel B: Fixed-grid active issuer-years under alternative curve and outcome definitions

Specification	Stasis	SE	N	Wild p
Fixed-grid positive turnover	26.8474	12.1565	224	0.0157
Nearest positive turnover: same years	25.4541	13.4349	224	0.0324
Fixed-grid reported: same years	27.4077	13.9905	224	0.0117
Nearest reported: same years	25.5626	13.9641	224	0.0365

Panel C: Positive-turnover outcomes under the main curve rule

Specification	Stasis	SE	N	Wild p
Nearest positive-turnover issuer levels	7.5365	10.5578	538	0.4140
Nearest reported yields: same issuer-years	4.1493	10.8713	538	0.6624
Nearest positive-turnover bond FE	12.9334	10.9735	748	0.2623
Nearest positive-turnover issuer changes	9.1781	16.8213	344	0.5740

Notes: Panel A uses the nearest-available same-day matching as the main construction and holds the issuer-year estimation sample fixed for the fixed-grid comparisons. Panel B uses the same 224 issuer-years selected by the fixed-grid positive-turnover rule, varying only the government-curve construction and corporate-yield definition; these comparisons are exploratory and reported p -values are not adjusted for multiple testing. Panel C reports annual positive-turnover outcomes requiring at least 20 positive-turnover yield days per bond-year, with the reported-yield comparison on the same 538 issuer-years. Standard errors are clustered by issuer; reported p -values are based on the year-level wild cluster bootstrap. These specifications are exploratory.

F Additional Cross-Market and Information-Set Evidence

This appendix collects the tables that are referenced in the main text but not reported there: the issuer-year spread estimates, the consecutive-year spread-change estimates, the bond fixed-effects estimates, the alternative activity and coverage classifications, and the economic-magnitude intervals.

Table F.1: Bond-spread robustness

Panel A: Issuer-year maturity-matched corporate yield spreads

	Base	+Amihud	+Stasis	Both signals +bond controls
Lagged log EDF	0.0630 (0.0823)	0.0629 (0.0830)	0.0634 (0.0813)	0.0609 (0.0825)
Lagged log Amihud	–	-0.0140 (0.1396)	–	-0.0202 (0.1484)
Lagged stasis	–	–	1.7862 (3.1885)	1.4264 (3.3415)
Identified guarantee coverage share	–	–	–	0.0797 (0.2844)
Remaining maturity	–	–	–	-0.1581 (0.0722)
Log yield-day count	–	–	–	-0.3273 (0.2599)
Observations	1,739	1,739	1,739	1,739
Issuers	322	322	322	322
Year clusters	10	10	10	10
Year-wild p -value: stasis	–	–	0.5858	0.6821

Panel B: Consecutive-year spread changes and same-sample level estimates

	Issuer level change sample	Issuer spread change	Bond spread change
Lagged log EDF	0.0775 (0.0812)	0.0321 (0.0696)	0.0879 (0.0891)
Lagged log Amihud	-0.1546 (0.1218)	-0.1295 (0.1175)	-0.3876 (0.2412)
Lagged stasis	1.0223 (3.3849)	1.4060 (2.7653)	1.1932 (3.3785)
Observations	1,453	1,453	2,974
Issuers	287	287	277
Year clusters	10	10	10
Year-wild p -value: stasis	0.7824	0.7020	0.6937

Panel C: Bond fixed-effects corporate yield-spread estimates

	Stasis + controls	+Amihud
Lagged log EDF	0.0415 (0.0838)	0.0431 (0.0823)
Lagged log Amihud	–	0.2324 (0.3169)
Lagged stasis	3.6207 (3.8048)	2.6932 (3.2667)
Remaining maturity	0.6656 (0.6193)	0.6669 (0.6220)
Log yield-day count	-0.3048 (0.2542)	-0.3096 (0.2583)
Observations	4,088	4,088
Issuers	319	319
Year clusters	10	10
Year-wild p -value: stasis	0.3083	0.4139

Notes: Panel A constructs the corporate–government spread using the nearest observed same-day government YTM nodes matched by remaining maturity. Panel B constructs spread changes only between consecutive observed years, and the level specification uses the same sample. Panel C includes bond and year fixed effects, lagged firm controls, remaining maturity, reported-yield coverage, and identified guarantee coverage when it varies over time. Standard errors are clustered by issuer or two-way clustered by issuer and year as indicated; reported p -values are based on the year-level wild cluster bootstrap.

Table F.2: One-standard-deviation stasis effects in basis points with wild-bootstrap confidence intervals

Specification	Stasis SD	Effect (bp)	95% wild CI (bp)
Main issuer levels	0.04046	5.77	[-23.62, 37.29]
Main bond FE	0.04513	12.15	[-18.24, 52.06]
Main issuer changes	0.04199	5.90	[-20.93, 36.38]
Current positive turnover	0.03694	27.84	[-42.22, 111.27]
Prior active coverage	0.04012	-1.41	[-130.21, 121.47]

Notes: Reported effects equal the stasis coefficient multiplied by the estimation-sample standard deviation of stasis and are expressed in basis points. Confidence intervals apply the same transformation to the wild-bootstrap coefficient limits. The intervals are confidence intervals for economic magnitudes rather than equivalence bounds.

Table F.3: Coverage selectivity in the corporate-bond sample

Panel A: Characteristics of active and other spread issuer-years

Characteristic	Active (574)	Other (1,165)	Std. diff.
EDF (percent)	0.231	0.251	-0.027
Log assets	24.543	24.509	0.028
Leverage	0.594	0.596	-0.010
Lagged ROA	0.029	0.028	0.011
State ownership (percent)	47.43	79.98	-0.719
Lagged stasis (percent)	4.33	4.98	-0.165
Reported spread (percent)	2.924	1.687	0.331
Bonds per issuer-year	2.12	2.61	-0.203
Remaining maturity (years)	2.43	2.66	-0.143
Yield records	319.1	511.9	-0.368
Positive-turnover days	90.4	7.2	0.708

Panel B: Prior-year bond activity and next-year reported spreads

	Prior coverage known	Prior active	Prior other	Prior active current controls
Lagged log EDF	0.0811 (0.0801)	0.3771 (0.1680)	0.0025 (0.0555)	0.3959 (0.1800)
Lagged log Amihud	-0.1711 (0.1190)	0.4434 (0.3807)	-0.5404 (0.2473)	0.4510 (0.3890)
Lagged stasis	1.2247 (3.4350)	-0.3517 (13.1886)	0.8038 (3.0144)	-1.7180 (12.9381)
Observations	1,453	433	969	433
Issuers	287	131	201	131
Year clusters	10	10	10	10
Year-wild p -value: stasis	0.7709	0.9875	0.7143	0.9323

Notes: Panel A compares characteristics within the 1,739 issuer-years in the main spread sample; “Active” denotes an issuer-year with at least one bond-year containing 20 or more positive-turnover yield days, standardized differences use the pooled group standard deviation, and the comparisons are descriptive. Panel B measures bond-market activity in year $t - 1$ and the spread outcome in year t ; issuer-years without valid prior-year bond-market coverage remain missing, subgroup fixed-effects regressions remove singleton issuers after the activity split, and standard errors are clustered by issuer with year-level wild-cluster-bootstrap p -values.

Data and Code Availability

The raw data used in this study are proprietary CSMAR data and cannot be redistributed under the terms of the data license. Researchers with licensed access to CSMAR can obtain the underlying source data directly from CSMAR. Replication code will be made publicly available in a repository upon publication.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work, the authors used ChatGPT (OpenAI) to assist with language editing, improving clarity and organization, and refining the presentation of the manuscript. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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