Capital Structure Information Transmission In Us Public Firms: Price Discovery Across Equity, Corporate Bond And Credit Default Swap Markets

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Abstract: *This evidence is used to argue in the equity, corporate bond, or credit default swap (CDS) markets that issuer information revelations propagate within such markets first; but finite-sample reliability critically hinges on co-integration-rank selection, contemporaneous innovation correlation pairs and asynchronous trading. We specify and reproduce a closed-form Monte Carlo envelope for a three-market rank-two Vec-tor error-correction model in which permanent-information shares are known. Core Analysis: All Supplemental Diagnostics + bootstrap coverage (500 systems, 499 resamples), unknown-break detection, event-study power calculation, and automated reproducibility audit. Rank two is chosen for each programmed leader in 68.8%–69.4% of samples across 500 replications. Dominant source of over-selection at the rank-three level: deterministic-term choice Conditional Hasbrouck leader accuracy 99.7%–100.0 Unconditional 68.8%–69.4 For example, 75% clustered staleness using refresh-time sampling achieves a bond-share bias reduction from −0.277 to −0.114 and extensively lower unconditional leader recovery (27.3%→51.5%) but reduces the effective number of observations to ~126." Percentile-bootstrap coverage reaches 92.4%–94.6%. The unknown-break detector finds twenty-six of these latent breaks (56.8%); however, leader accuracy at the conditional regime-level is > 97%. We end with a validation of a staged empirical framework: validate rankings, estimate lower and upper bounds for ordering, and finally test the synchronization-stabilization paths conditional on successful ordering (vs. quality identified based on the entire sample).*

Through examining the joint price discovery process among corporate bonds and credit default swaps, we uncover additional dimensions of asynchronous trading that can be partially explained by the inherent properties of each asset.

Keywords: Kalman Filter; Structural Change; Random Coefficient JEL Classification C15; C32; G12; G32

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Introduction

Although equity, corporate bonds and credit default swaps all represent claims on the identical enterprise, they trade in completely different venues, governed by different institutional arrangements and presented to market participants at wildly different observation clocks. Thus, equity trading is continuous and very transparent; corporate-bond trading is more scarce and inventory-intensive; and single name CDS are designed to allow credit-risk concentrations without lending the underlying bond Augustin et al (2013). 2016). A capital-structure signal can however take different lengths of time to reach each of the three markets even when all prices eventually converge to the same issuer value. The no demonstrative question, then, is not with certainty which market leads or lags, but whether the lead that may be measured is a permanent process of information discovery—or rather a mechanical by-product of liquidity short-cuts between correlated innovations and stale observations.

But for now, all of the existing evidence suggests that a single leader is not going to be enough. Credit default swaps (CDS) typically lead cash bonds in credit information (Blanco et al. 7) The fact that our sample period is restricted to post October 2005 helps, but equity and CDS contributions vary by event, issuer and information state (Norden and Weber 2009; Forte e Peña 2009; Kryzanowski et al. 2017; Ismailescu et al. 2023; Rubesam and Zimmermann 2025). Corporate characteristics are also important for equity–credit price discovery (Zhou et al. 2026). With the move towards a more electronic (Loon and Zhong 2014), transparent (Adrian et al. O'Hara and Zhou 2021a, b, 2025; (2017).

The econometric challenge is no less salient. Hasbrouck information shares are based on contemporaneous covariance decomposition (Hasbrouck 1995), Gonzalo–Granger component shares emphasize adjustment to the permanent component (Gonzalo and Granger 1995) and the two methods do not necessarily identify one and the same economic object (de Jong 2002; Baillie et al. It is not only East Asia however where investors engage in averaging down during bear markets (role both need a proper structures). If a large enough fraction of candidate systems fails the rank requirement, then even a study that simply reports near perfect leader classification for retained rank-two systems can become sample-wide limited in reliability.

This paper develops a controlled Monte Carlo design, where the contribution of permanent information, staleness mechanism and location to structural break are all specified. All supplementary results are thus incorporated directly in the main methods and results sections instead of referring to separate tables. As a result, the contribution is not an overall ranking for the estimator either. This is an unambiguous and impartial evaluation of the order in which states must be true if a substantive claim on equity–bond–CDS leadership is too pass muster.

Five contributions to the study. I hypothesize that the model first specifies what looks like an entire form of a data-generating process, with public specifications of the cointegrating matrix; adjustment matrices (weights associated with deviations from long-run equilibria); covariance structures and measurement-noise processes, by fixed seeds associated with random number generations. Secondly, it distinguishes between performance conditional on the market being up or down and unconditional. Optionally detects Johansen-rank failures under diverse

deterministic terms, adjustment strengths and noise levels Fourth, to refresh-time synchronization you explore independent, clustered-exact musty mold degranuler. Fifth, it combines techniques to calibrate unknown-break detection, bootstrap coverage and event-study power – all under a single auditable framework with reproducibility checks.

It is an econometric validation study using simulated data, not a lead-analysis statement about real issuers in the certain markets. Prospective empirical extension is therefore clearly delineated in Section 6.

Institutional Background and Related Literature

2.1 Common issuer, heterogeneous claims

Structural credit-risk models view equity and debt as contingent claims on the same process (Merton 1974) Capital-structure implications of corporate taxes, distress costs, agency conflicts and asymmetric information (Myers 1984; Myers and Majluf 1984). As such a financing event can serve as signals for operation, leverage, hierarchy and liquidity. Issuing equity dilutes existing shareholders while lowering default risk; secured debt borrowing enhances near-term liquidity but at the expense of lower expected recovery for prior unsecured creditors, and repurchasing expensive stock with debt leaves equity holders in a better position at greater costs to creditors. Primary bond issuance (Hotchkiss et al. 2025).

Such heterogeneous exposure implies that a particular shock to an issuer does not uniformly translate into positive or negative effects across different markets. If the news is issue-specific (maturity extension, covenant modification, collateral or recovery redistribution), it can lead to bond leadership. It is also plausible that CDS will lead for a subset of information such as tail-credit or rating. It is possible but as far as news important for equity operating and valuation it happens to be. Hence, the empirical anchor that matters to you is not a ranked hierarchy but conditional lead following.

2.2 Price Discovery, Liquidity and Unidirectional Asynchronous Observation

Hasbrouck information shares divide the variance of the per-share common efficient-price innovation into firm-specific shocks. This is also the reason lower and upper bounds should be reported at the same time as the mid-point (Hasbrouck 1995; Lehmann 2002; Zema 2022), as varying orderings create different allocations of assets -- especially when reduced form innovations are correlated. We do this by utilizing the orthogonal complement of the adjustment matrix so that we have now, as component shares (Gonzalo and Granger 1995), a measure of each cointegrated series contribution to the permanent component. This distinction is important because slow adjusting or noisy price markets can acquire large fractions of their economies in the absence of being a main source of new information (Putniņš 2013).

Because of the illiquidity of corporate-bond data and less-sparse trade, synchronization is central to inference. While transparency does reduce some transaction costs, the effects vary considerably by security liquidity, trade size, dealer balance sheets and market stress 17 Large sales eat up dealer balance sheets and increase death from superior future trades. 2006; Bao et al. 2011; Dick-Nielsen et al. 2012; Adrian et al. 2017; Kargar et al. 2021). Persistent equity or

CDS moves can be made to create the illusion of front running from carried-over prices. Table 1 summarizes the information-based advantages/disadvantages of claim-specific factors that motivated our decision to simulate each attack type.

Table 1. Institutional features that can generate changing price-discovery leadership

Market	Primary measure	Potential information advantage	Principal empirical friction
Equity	Return or quote midpoint	Continuous trading and broad information aggregation	Residual-claim convexity, market beta, short-sale constraints
Corporate bond	Clean-price return, yield, or spread	Issue-level maturity, covenant, seniority, collateral, and recovery information	Sparse trading, inventory costs, embedded options, and stale observations
CDS	Par spread or upfront points	Focused expression of default-risk beliefs with limited cash commitment	Dealer quotes, contract liquidity, collateral, and regulation

Notes: The table identifies economic channels; the Monte Carlo experiments isolate the statistical consequences of observation frequency, correlation, and structural instability.

Research Design and Monte Carlo Data

3.1 Baseline three-market data-generating process

Let p_t denote the vector of latent equity, bond, and CDS price measures. Each replication follows the rank-two vector error-correction model in Equation (1), which contains one permanent common trend and two stationary spreads.

$$\Delta p_t = \alpha(s)\beta' p_{t-1} + \varepsilon_t, \varepsilon_t \sim \mathcal{N}(0, \Omega(\rho)) \quad (1)$$

Equation (1) is simulated after a 200-observation burn-in. The fixed cointegrating matrix in Equation (2) represents the equity–bond and bond–CDS spreads.

$$\beta = \begin{bmatrix} 1 & 0 \\ -1 & 1 \\ 0 & -1 \end{bmatrix} \quad (2)$$

For target Hasbrouck shares $s = (s^E, s^B, s^c)'$, define $q(s) = (\sqrt{s^E}, \sqrt{s^B}, \sqrt{s^c})'$ and let $N(s)$ be an orthonormal basis for the null space of $q(s)'$. Equation (3) constructs the adjustment matrix so that $q(s)'\alpha(s) = 0$ and $\beta'\alpha(s) = -0.50I_2$.

$$\alpha(s) = -\kappa N(s)[\beta' N(s)]^{-1}, q(s)'\alpha(s) = 0, \kappa = 0.50 \quad (3)$$

The three rotations assign 0.60 to equity, bonds, or CDS and 0.20 to each remaining market. Table 2 reports every row of α, its orthogonal complement α⊥, and the programmed Hasbrouck target. The values in Table 2 fully reproduce the baseline systems.

Table 2. Exact adjustment matrices and programmed permanent-information shares

Leader	Row	α_1	α_2	$\alpha_\perp$	Target H shares
Equity	1	-0.267949	-0.133975	0.774597	(0.6, 0.2, 0.2)
Equity	2	0.232051	-0.133975	0.447214	(0.6, 0.2, 0.2)
Equity	3	0.232051	0.366025	0.447214	(0.6, 0.2, 0.2)
Bond	1	-0.366025	-0.133975	0.447214	(0.2, 0.6, 0.2)
Bond	2	0.133975	-0.133975	0.774597	(0.2, 0.6, 0.2)
Bond	3	0.133975	0.366025	0.447214	(0.2, 0.6, 0.2)
CDS	1	-0.366025	-0.232051	0.447214	(0.2, 0.2, 0.6)
CDS	2	0.133975	-0.232051	0.447214	(0.2, 0.2, 0.6)
CDS	3	0.133975	0.267949	0.774597	(0.2, 0.2, 0.6)

Notes: Rows are ordered equity, bond, and CDS. In every design, $\beta'\alpha = -0.50 I_2$.

Observed prices include stationary AR (1) measurement noise as specified in Equation (4). This component represents transient quote and observation errors without introducing an additional common trend.

$$y_t = p_t + u_t, u_t = 0.40u_{t-1} + \eta_t, \eta_t \sim \mathcal{N}(0, 0.10^2 I_3) \quad (4)$$

The equicorrelation covariance matrix in Equation (5) varies contemporaneous innovation correlation at ρ = 0, 0.5, and 0.8. Equation (5) is the basis for the ordering-uncertainty experiment reported later in Table 6 and Fig. 4.

$$\Omega(\rho) = (1-\rho)I_3 + \rho \mathbf{1}\mathbf{1}' \qquad (5)$$

3.2 Price-discovery estimators and evaluation measures

The Johansen trace test is applied at the 5% level. The baseline uses an unrestricted intercept and zero lagged differences, while Table 5 evaluates alternative deterministic specifications. Hasbrouck and component shares are estimated only when rank two is selected. The common-trend impact matrix is defined in Equation (6).

$$C = \beta_\perp (\alpha_\perp' \beta_\perp)^{-1} \alpha_\perp' \qquad (6)$$

For an ordering-specific Cholesky factor L of Ω, Equation (7) gives market m's Hasbrouck information share. All six orderings are evaluated, and the analysis reports lower bounds, upper bounds, and midpoints.

$$H_m = \frac{(\psi L e_m)^2}{\psi \Omega \psi'} \qquad (7)$$

Equation (8) defines the Gonzalo–Granger component share. Hasbrouck and component shares are evaluated against their own programmed targets rather than treated as interchangeable statistics.

$$CS_m = \frac{|\alpha_{\perp,m}|}{\sum_{j \in \{E,B,C\}} |\alpha_{\perp,j}|} \qquad (8)$$

Conditional accuracy in Equation (9) uses the number of valid rank-two fits in the denominator, whereas unconditional accuracy uses all programmed replications. Reporting both prevents accurate retained estimates from concealing identification failures.

$$Acc_{cond} = \frac{\sum_{r=1}^{R} I(rank_r = 2) I(\hat{L}_r = L)}{\sum_{r=1}^{R} I(rank_r = 2)}, Acc_{uncond} = \frac{\sum_{r=1}^{R} I(rank_r = 2) I(\hat{L}_r = L)}{R} \quad (9)$$

Bias, RMSE, and the Monte Carlo standard error are calculated using Equations (10)– (12). These measures are reported with the full rank distribution in Tables 4–10.

$$Bias_m = R_v^{-1} \sum_{r=1}^{R_v} (\hat{s}_{m,r} - s_m) \quad (10)$$

$$RMSE_m = \left[R_v^{-1} \sum_{r=1}^{R_v} (\hat{s}_{m,r} - s_m)^2 \right]^{1/2} \quad (11)$$

$$MCSE(\hat{p}) = \left[\hat{p}(1 - \hat{p})/R \right]^{1/2} \quad (12)$$

3.3 Asynchronous Bond Observations, Structural Breaks, And Event Tests

The bond-led design is subjected to independent, clustered, and endogenous missing-update mechanisms at target stale rates of 25%, 50%, and 75%. The clustered mechanism is a two-state Markov process with P ($stale_t = 1 \mid stale_{t-1} = 1$) = 0.85. Under endogenous staleness, Equation (13) makes large latent bond innovations more likely to trigger an observed update.

$$Pr(update_t = 1) = logit^{-1}\left[a + 1.25 \frac{|\Delta p_t^B|}{sd(\Delta p^B)} \right] \quad (13)$$

Previous-tick sampling carries the last bond price forward. Refresh-time sampling retains only dates with a genuine bond update and aligns equity and CDS to those dates. Table 7 reports the complete 18-cell mechanism-by-rate-by-method design, including realized staleness, effective sample size, bias, RMSE, and leader recovery.

The structural-break experiment contains 600 observations and changes the programmed leader from CDS to equity at observation 300. The break date is unknown to the estimator. Candidate dates span the central 50% in five-observation increments. Equation (14) defines the Sup-Wald-like screening statistic; its critical value of 6.10 is calibrated against 1,000 stable null simulations.

$$W_{sup} = \max_{\tau \in \mathcal{T}} \left\{ T \log\left(\frac{|\hat{\Sigma}_0|}{|\hat{\Sigma}_1(\tau)|} \right) \right\} \quad (14)$$

The supplementary event experiment is also incorporated into the main analysis. Each replication contains 40 treated and 40 control issuers and a bond-dominant signal. Three market-level tests are adjusted jointly by the Benjamini–Hochberg procedure (Benjamini and

Hochberg 1995). Equation (15) defines the bounded early-adjustment fraction used in Table 10 and Fig. 8.

$$AF_{m,e} = \frac{|CA_{m,e}[-1,+1]|}{|CA_{m,e}[-1,+1]| + |CA_{m,e}[+2,+20]|} \quad (15)$$

3.4 Bootstrap Design and Reproducibility Controls

Coverage is evaluated as calculated for 500 random successful outer systems with respect to an independent test set of 499 parametric resamples per system. This bootstrap is conditional on the selected rank-two cointegrating space: as a result, each resample is drawn from the fitted VECM with only adjustment matrix and innovation covariance re-estimated. Both percentile and simple 95% intervals are all compared with the target percentage. This new design incorporates the prior experiment of 100-system, 30-resample and provides substantially less simulation error (Swensen 2006).

Table 3 provides an overview of the entire scenario registry. Each scenario has a seed that is fixed and summary tables are only generated when files relative to the replication-level exist. It captures the chosen Python, package versions, test logs/failures and a SHA-256 completeness manifest. These controls enable computation-based verification rather than a narrative reproducibility claim.

Table 3. Pre-specified Monte Carlo scenario registry

Experiment	Scenario values	Replications	Primary outcome
Baseline leaders	Equity, bond, CDS; shares 0.60/0.20/0.20	500 per leader	Rank, bias, RMSE, conditional and unconditional accuracy
Rank diagnosis	Deterministic term, noise, adjustment strength	500 per condition	Full selected-rank distribution
Sample size	T=250, 500, 1,000	400 per condition	Rank, RMSE, accuracy, bound width
Innovation correlation	ρ=0.0, 0.5, 0.8	400 per condition	Ordering bounds and leader recovery
Bond staleness	IID, clustered, endogenous; 25%, 50%, 75%	400 per cell	Naive versus refresh-time bias and effective T
Bootstrap	500 outer systems; 499 resamples	500	Coverage, width, bias, RMSE
Unknown break	CDS-to-equity switch at 300 of 600	400 + 1,000 null	Detection, date error, regime accuracy
Event diagnostic	Null and effects 0.5, 1.0, 1.5	400 per effect	BH discovery, power, leader accuracy

Notes: Master seed 20260706. Summary aggregation follows completion of the replication-level files.

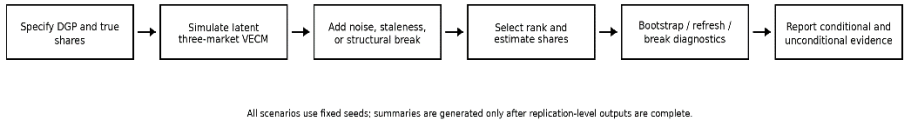


Fig. 1. Reproducible workflow for the integrated three-market Monte Carlo analysis.

The staged sequence from DGP construction through rank selection, estimator recovery, robustness tests and final evidence reporting is visualized in figure 1. This is significant because an invalid leader estimate cannot recover from a failed rank (and vice versa) or suffering parameters, and also mechanically stale observations.

Empirical Results From The Monte Carlo Experiments

4.1 Baseline identification and leader recovery

Table 4 shows the full baseline distribution, not just successful estimates. Rank two is selected in 347 of 500 equity-led samples, 347 of 500 bond-led samples, and rank two in 344 of the CDS-led samples. Rank three gets all samples (rank zero and rank one are never selected) For a rank two, Hasbrouck leader accuracy is 99.7%–100.0%, while the component-share accuracy is 99.7%–100.0%. Unconditional Hasbrouck accuracy is low, between 68.8%–69.4%, showing that estimator quality and sample-wide identification success are two distinct quantities.

The quantitative errors in Table4 are small after identification. Hasbrouck leader bias: −0.010 to 0.003; Hasbrouck RMSE: 0.073 to 0.076; component-share RMSE: 0.038 to 0.039 and ordering-bound width mean spans about 0.042 Near-perfect conditional recovery versus ~69% unconditional success recovery (contrast shown in Fig. 2).

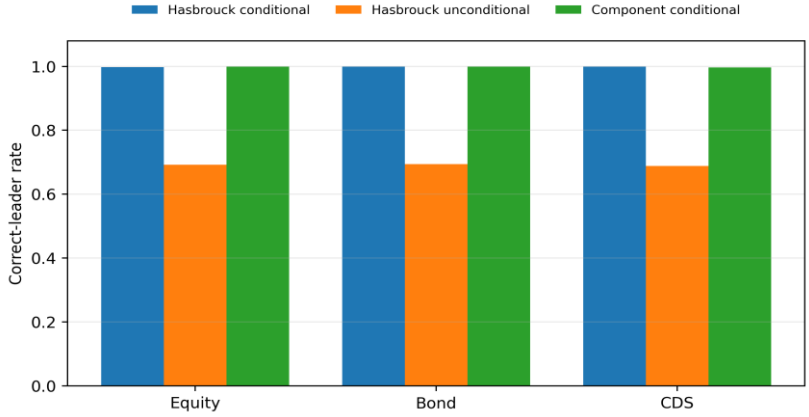


Fig. 2. Conditional estimator accuracy and unconditional sample-wide leader recovery.

Table 4. Complete baseline rank distribution and leader-recovery performance

True leader	R	r0	r1	r2	r3	H cond.	H uncond.	CS cond.	CS uncond.	H bias	H RMSE	CS RMSE	Mean width
Equity	500	0	0	347	153	0.997	0.692	1.000	0.694	-0.010	0.075	0.038	0.042
Bond	500	0	0	347	153	1.000	0.694	1.000	0.694	0.003	0.073	0.038	0.042
CDS	500	0	0	344	156	1.000	0.688	0.997	0.686	-0.010	0.076	0.039	0.042

Notes:H denotes the Hasbrouck midpoint; CS denotes component share. Conditional rates use valid rank-two fits; unconditional rates use all programmed replications.

4.2 Diagnosing low Johansen rank recovery

The rank problem is an exact over-selection problem. The baseline unrestricted intercept ranks three in 33.6% of the CDS-led samples. Table 5 shows that dropping the deterministic term boosts rank-two recovery to 86.4, leading with a linear trend reduces it to 37.0 and enhances rank-three selection (when misspecified) to 63.0 percent. Eliminating measurement noise raises rank-two recovery to only 71.0%, comparatively weaker adjustment and stronger stationary noise have small marginal effect of 46.4%. Thus the finite-sample trace-test size and, more importantly, the deterministic specification dominate ordinary estimator variance in this design (e.g., Johansen 1988, 1991; MacKinnon et al. 1999).

This diagnosis is confirmed by Figure 3, in which every specification places all probability on ranks two and three. The experiment does not detect the failure of cointegration; it treats an additional stationary direction as being statistically significant. Empirical applications should thus provide the entire rank distribution and sensitivity to economically warranted deterministic terms, rather than treating rank selection as a mere formality.

Table 5. Full Johansen rank-diagnostic results

Condition	R	det	Noise SD	κ	Rank 0	Rank 1	Rank 2	Rank 3
No deterministic term	500	-1	0.10	0.50	0.000	0.000	0.864	0.136
Unrestricted intercept	500	0	0.10	0.50	0.000	0.000	0.664	0.336
Linear trend	500	1	0.10	0.50	0.000	0.000	0.370	0.630
Intercept; no measurement noise	500	0	0.00	0.50	0.000	0.000	0.710	0.290
Intercept; weaker adjustment	500	0	0.10	0.20	0.000	0.000	0.680	0.320
Intercept; stronger noise	500	0	0.25	0.50	0.000	0.000	0.676	0.324

Notes: Each condition contains 500 CDS-led replications. The unrestricted-intercept row is the baseline.

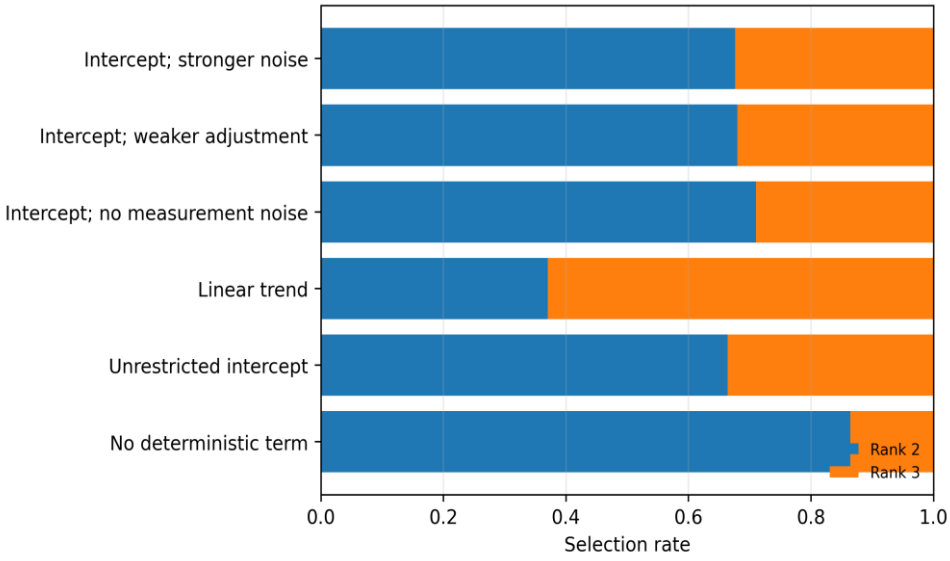


Fig. 3. Selected-rank distributions under alternative deterministic and noise specifications.

4.3 Sample Size and Contemporaneous Correlation

Rows in Table 6 under sample-size show that increasing T from 250 to 1,000 decreases leader RMSE from.111 to.061, tightens the mean Hasbrouck bound from.074 to.033, and raises conditional accuracy for leaders from 94.6% at T=250 to maximum value of 100%. Rank-two selection that does not improve monotonically: variable between 67.0% and 70.0%. Consequently, longer samples enhance share precision after identification but do not eliminate sensitivity to deterministic specification.

The contemporaneous correlation of innovation has a more prominent impact on identifying with the economy. Mean bound width increases from 0.050 to 0.821, conditional Hasbrouck accuracy remains around 99.6% to fall to around 67.7% and unconditional accuracy drop from around 70.8% down to a low of around 47.7%. As shown in Figure 4, a low RMSE at midpoint can be obtained even when ordering bounds are extremely wide; since weak innovations are strongly correlated, the midpoint alone is thus insufficient.

Table 6. Complete sample-size and innovation-correlation sensitivity results

Scenario	R	Rank 2	H cond.	H uncond.	CS cond.	Leader RMSE	Bound width
T=250	400	0.700	0.946	0.662	0.964	0.111	0.074
T=500	400	0.695	1.000	0.695	0.996	0.073	0.051
T=1000	400	0.670	1.000	0.670	1.000	0.061	0.033
rho=0.0	400	0.710	0.996	0.708	1.000	0.075	0.050
rho=0.5	400	0.705	0.876	0.618	0.887	0.063	0.565
rho=0.8	400	0.705	0.677	0.477	0.674	0.042	0.821

Notes: Sample-size scenarios use ρ=0. Correlation scenarios use T=500. Each row contains 400 replications.

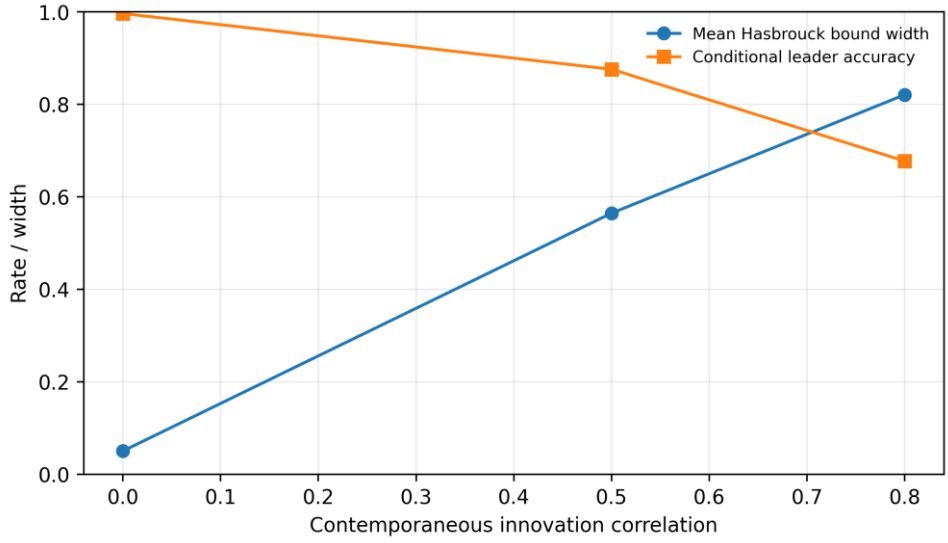


Fig. 4. Contemporaneous innovation correlation, ordering uncertainty, and conditional leader accuracy.

4.4 Independent, clustered, and endogenous bond staleness

The entire staleness experiment with 18 cells is in Table 7. At 25% IID staleness, naive and refresh-time estimates are close, suggesting that mild independent missingness is not blatantly harmful. At 75% IIDstaleness, however, although naive carry forward sampling gets a bond-share bias of −0.214 and unconditional leader recovery of 39.3%, refresh-time sampling brings down the bias to −0.096 and RMSE from 0.248 to 0.134 (systematic recovery conditional on the observed sample was previously estimated at about 500), while improving unconditional recovery to expected 58% (effective sample size reduced from around n = 500) per observation).

The most damaging mechanism is clustered staleness. For naive bias we reach−0.277, RMSE becomes 0.304 and unconditional recovery is 27.3% at the cutoff of 751%. These values are improved to −0.114, 0.145, and 51.5% with refresh-time sampling. Endogenous Staleness generates smaller naive bias more likely to be observed for large latent innovations yet refresh-time sampling still holds recovery benefits. The bias patterns are summarized in Figure 5. Data is from before October 2023 The paired previous-tick and refresh-time specifications are supported by evidence because synchronization mitigates mechanical bias but at a very high cost to precision.

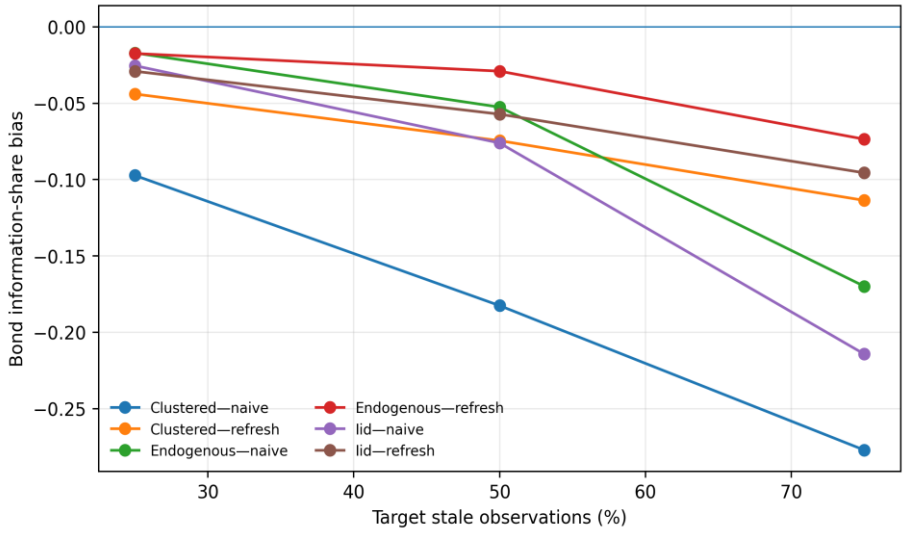


Fig. 5. Bond information-share bias under independent, clustered, and endogenous staleness.

Table 7. Complete staleness experiment under naive and refresh-time sampling

Mechanism	Target	Method	R	Actual stale	Rank 2	Cond. acc.	Uncond. acc.	Bond bias	Bond RMSE	Effective T
Clustered	25%	Naive	400	0.244	0.710	0.908	0.645	-0.097	0.144	500.0
Clustered	25%	Refresh	400	0.244	0.698	0.968	0.675	-0.044	0.093	378.0
Clustered	50%	Naive	400	0.495	0.680	0.669	0.455	-0.183	0.220	500.0
Clustered	50%	Refresh	400	0.495	0.662	0.909	0.603	-0.075	0.118	252.4
Clustered	75%	Naive	400	0.748	0.730	0.373	0.273	-0.277	0.304	500.0
Clustered	75%	Refresh	400	0.748	0.685	0.752	0.515	-0.114	0.145	125.8
Endogenous	25%	Naive	400	0.250	0.693	0.989	0.685	-0.017	0.074	500.0
Endogenous	25%	Refresh	400	0.250	0.700	0.996	0.698	-0.017	0.072	374.9
Endogenous	50%	Naive	400	0.500	0.670	0.974	0.652	-0.053	0.096	500.0
Endogenous	50%	Refresh	400	0.500	0.680	0.989	0.672	-0.029	0.081	249.8

Mechanism	Target	Method	R	Actual stale	Rank 2	Cond. acc.	Uncond. acc.	Bond bias	Bond RMSE	Effective T
Endogenous	75%	Naive	400	0.748	0.695	0.730	0.507	-0.170	0.203	500.0
Endogenous	75%	Refresh	400	0.748	0.670	0.903	0.605	-0.074	0.110	126.1
Iid	25%	Naive	400	0.248	0.657	0.996	0.655	-0.025	0.083	500.0
Iid	25%	Refresh	400	0.248	0.655	1.000	0.655	-0.029	0.081	376.1
Iid	50%	Naive	400	0.497	0.675	0.941	0.635	-0.076	0.120	500.0
Iid	50%	Refresh	400	0.497	0.677	0.970	0.657	-0.057	0.102	251.3
Iid	75%	Naive	400	0.748	0.728	0.540	0.393	-0.214	0.248	500.0
Iid	75%	Refresh	400	0.748	0.710	0.817	0.580	-0.096	0.134	126.0

Notes: Each cell contains 400 bond-led replications. Refresh-time sampling retains only dates with a genuine bond update.

4.5 Bootstrap interval coverage

The coverage estimate is made greatly more precise by the expanded bootstrap experiment given in Table 8. Panel A: Coverage for equity (92.4%), bonds (93.4%), and CDS (94.6%) of outer systems, with 499 resamples per each successful system Even with average widths identical to those of the percentile intervals, basic intervals cover only 85.8%–90.8%. We depict that percentile intervals converge towards the nominal 95% line (Fig.6), but equity and bond coverage are still slightly low.

These results explain the specificity of percentile band in current Gaussian rank-conditional design, while emphasizing that no nominal bootstrap interval are globally calibrated. For example, empirical work on heteroscedasticity and serial dependence should compare different block, sieve and stationary bootstrap procedures (Politis and Romano 1994).

Table 8. Complete coverage results for nominal 95% parametric-bootstrap intervals

Market	Requested R	Successful R	B	Pct. Cover	Pct. width	Basic cover	Basic width	Bias	RMSE
Equity	500	500	499	0.924	0.235	0.858	0.235	0.005	0.067
Bond	500	500	499	0.934	0.235	0.886	0.235	0.003	0.064
CDS	500	500	499	0.946	0.286	0.908	0.286	-0.007	0.075

Notes: All 500 outer systems retained all 499 bootstrap estimates. Coverage is evaluated against the programmed Hasbrouck midpoint.

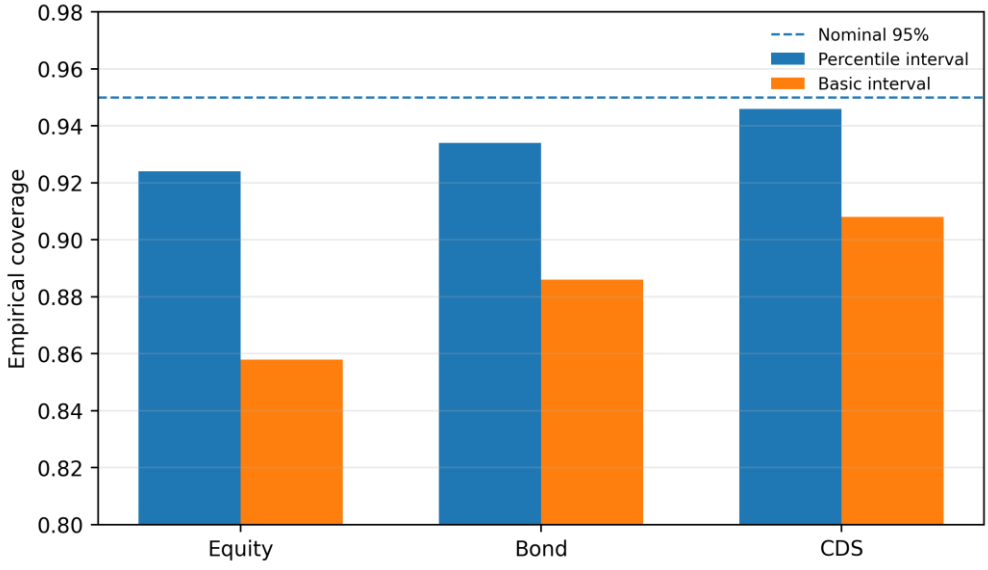


Fig. 6. Percentile and basic bootstrap coverage relative to the nominal 95% rate.

4.6 Unknown Structural Breaks and Regime Recovery

Null calibration and recovery are reported in Table 9. 95th percentile of stable-null statistic = 5.849; minimum critical value (implemented) = more conservative value, 6.10 In all replications, the detector identifies 56.8% of programmed breaks and locates the date within ±30 observations in 33.0%. The median absolute date error is 25 observations among detections.

Pre-break CDS leaders are accurate 99.4% of the time (conditional on a detected break and a valid rank in the relevant segment), post-break equity leaders 97.5%, and both regimes are identified jointly with an accuracy of 98.1% in all cases where each regime is valid. False stable dominance is 13.2% full sample The main challenge is in detecting and dating the break rather than estimating leadership once stable regimes are already separated, as shown by Figure 7. This result supports parameter-stability tests and regime-specific estimates (Andrews 1993; Bai and Perron 1998; Gregory and Hansen 1996).

Table 9. Unknown-break calibration, detection, and regime-recovery results

Outcome	Value
Null-calibration replications	1000
90th percentile	5.296
95th percentile	5.849
99th percentile	7.027
Implemented critical value	6.10

Outcome	Value
Break-detection rate	0.568
Detection within ±30 observations	0.330
Median absolute date error among detections	25.0
Pre-regime conditional accuracy	0.994
Post-regime conditional accuracy	0.975
Both regimes correct, conditional	0.981
Full-sample false stable dominance	0.132
Full-sample rank-two rate	0.738

Notes: The detector uses an estimated break date; the programmed midpoint is not supplied to the estimator.

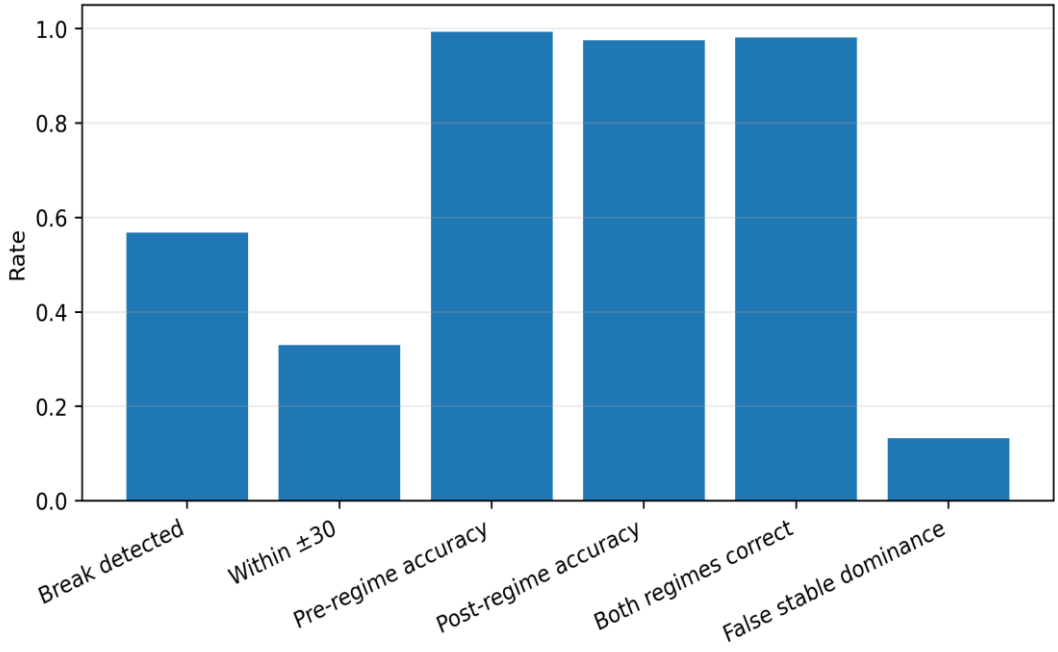


Fig. 7. Unknown-break detection and regime-specific leader-recovery performance.

4.7 Event-Study False Discovery and Power

Table 10 is the main results for the event-study diagnostic. The 5.25% probability of at least one Benjamini–Hochberg-adjusted discovery under the null hypothesis is consistent with correct control of type I error rates to a threshold near the intended level of 5%. For three markets, this 34.8% null leader accuracy is nearly the one-third random-classification benchmark. Modeling bond-test power at an effect scale of 0.5, shows a bond-test accuracy of only 44.0%, whereas leader accuracy is much higher for this same low effect lead (71.8%) When effect scale is 1.0: power = 99.0% and leader accuracy = 91.2%; with an g amp; of set at 1.5, power = one hundred% and leader accuracy = 97.5%.

The median bond adjustment fraction given by (15) rises monotonically from 0.229 without signal to 0.729 under the strongest signal. Thus, the joint patterns we hope to show are illustrated in desired ESS conditions in Figure 8: false discoveries exist only in null settings and, as expected, power and leader recovery are proportional to signal size. Weak effects are a situation in which you cannot reject the null but that is not evidence of no transmission.

Table 10. Event-study false-discovery control, power, and leader recovery

Effect scale	R	Any BH discovery	Bond power	Leader accuracy	Median bond AF
0.0	400	0.052	0.020	0.347	0.229
0.5	400	0.502	0.440	0.718	0.475
1.0	400	0.998	0.990	0.912	0.646
1.5	400	1.000	1.000	0.975	0.729

Notes: Each replication contains 40 treated and 40 control issuers. Three market-level tests are adjusted jointly.

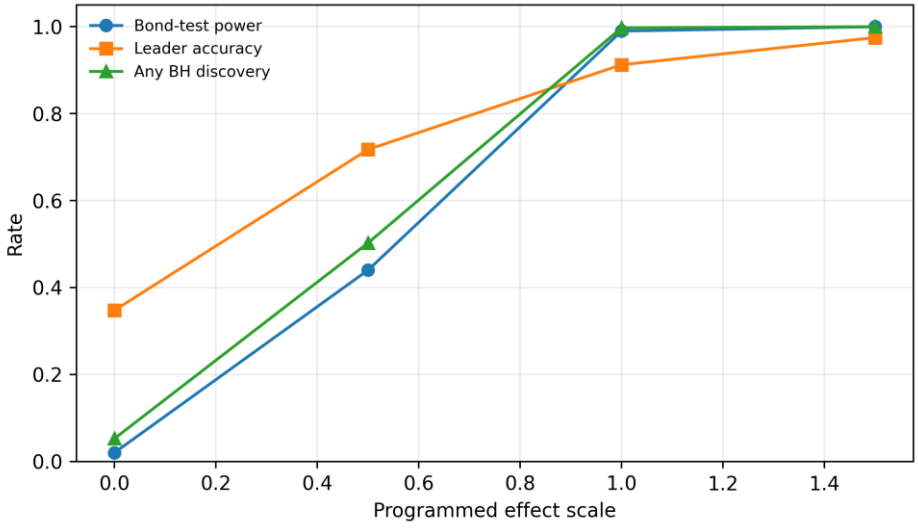


Fig. 8. Event-study power, adjusted discoveries, and correct-leader recovery.

4.8 Reproducibility Audit

Table 11 translates reproducibility from a claim statement into an empirical outcome. With all test cases returning zero failures, three unit tests and 20 automated output-consistency checks pass. It consists of 1,500 baseline replication rows; 7,200 staleness replication rows; 500 bootstrap outer per system and 499 resamples. Your environment and platform are recorded; all phone is validated with SHA-256 hashes of the archived files.

Table 11. Automated reproducibility audit

Audit item	Verified result
Unit tests	3 passed
Output-consistency checks	20 passed; 0 failed

Audit item	Verified result
Baseline replication rows	1500
Staleness replication rows	7200
Bootstrap outer rows	500
Bootstrap resamples per outer system	499
Estimated null 95th percentile	5.848548
Integrity manifest	SHA256SUMS.txt included
Software environment	Python, package versions, and platform recorded

Notes: All summary tables are generated only after replication-level outputs are complete.

Discussion and Empirical Decision Rules

One of the most important evidences derived from integrated models is of PDI (price-discovery inference) respective to three-markets; The findings show that major threats are realized before ordinary share estimation occurs. After specifying rank two, the Hasbrouck midpoint and component shares return the programmed leader almost perfectly. This procedure is less reliable as deterministic choices can result in the Johansen test over-selecting rank three. Conditional performance should therefore never be reported without the sample-retention rate. A second difference is explained by the correlation experiment. An RMSE modest at a midpoint may have ordering bounds that are so wide there is no unique economic attribution. Thus, residual correlation should be reported with all economic reasonable Cholesky orderings or bounds whenever it is material. There should be an architectural rationale for preferring one ordering over another, and two such orderings are likely primitive.

The staleness experiments illustrate why corporate-bond synchronization is a fundamental identification rather than a data-cleaning detail. Data on clustered carry-forward may unduly change leader classification, and sampling over refresh-time can greatly lessen the distortion. Refresh time, on the other hand, throws away observations; hence this correction needs to be considered in conjunction with effective sample size, rank recovery and RMSE.

The break and event experiments contribute complementary lessons. It is hard to see structural instability, but it is proximate accurately when the leaders separate; while the regime-level leadership feeds into estimates after separation. Event-study inference is the method that controls for false positives in finding null effects, and it has predictable power as a function of effect size. This can mean that non-rejection could simply doing weakly better than anything else in power, while full-sample dominance can be telling you about a regime shift (that you would not see otherwise).

Table 12 converts these findings into pre-specified decision rules for empirical equity–bond–CDS applications.

Table 12. Evidence-based decision rules for empirical price-discovery research

Monte Carlo evidence	Required empirical reporting rule
Rank two is selected in about 69% of baseline samples	Disclose the full rank distribution and retained-sample fraction before interpreting shares
Conditional accuracy is near 100%, but unconditional accuracy is about 69%	Report both conditional and sample-wide evidence
Rank recovery changes sharply with deterministic terms	Justify intercept/trend choices and report sensitivity
High correlation widens Hasbrouck bounds to 0.821	Report all orderings or bounds; withhold a unique leader when bounds overlap materially
Clustered staleness produces severe negative bond bias	Report previous-tick and refresh-time estimates with effective T and RMSE
Percentile coverage is 92.4%–94.6%	Use conservative uncertainty statements and alternative resampling checks
Only 56.8% of breaks are detected	Pre-test stability, report estimated dates, and present regime-specific estimates
BH null discovery is 5.25%, but weak-signal power is limited	Pre-specify windows and interpret non-rejection cautiously
All audit checks pass	Archive code, seeds, replication files, environment, and integrity hashes

Prospective Real-Data Application

This can be extended to real data by replacing the simulated prices with issuer-linked equity, corporate-bond, and single-name CDS data. Equity measures were derived from CRSP or TAQ; bond transactions and security characteristics from additional TRACE and FISD; Compustat based accounting controls; five-year CDS quotes from a licensed vendor. Timestamps of events would distinguish between pre-sounding, launch, pricing, allocation, settlement and completion. We do not use these proprietary data in the present study.

The empirical workflow must freeze issuer–security links before constructing outcomes; model each bond, not just an average for the issuer; keep quote age, time since last trade, trade count and dispersion as staleness status variables; and run the subsample to be examined intraday. Local projections, event-time abnormal returns, matched controls, and policy-based identification are basic extensions but recommendations rather than analyses.

Limitations

The DGP is purposely low-dimensional and Gaussian. It has a common stochastic trend, a fixed cointegrating matrix between all three variables, no conditional heteroscedasticity and fixed error-correction strength within stable regimes. The grouped and internal carryforward

processes are more realistic than independent observations, but still fail to replicate all the institutional attributes of corporate-bond trading.

While the economic cointegrating vectors are fixed by the break detector, adjustment coefficients are allowed to vary. Cointegrating spaces and multiple breaks, along with time-varying parameters from real-market applications need also be accounted. The boot strap is a rank conditional and hence does not integrate uncertainty of rank selection. Different procedures (eg, rank-resampling, sieve, wild, and block methods) should be compared in future works.

Simulation evidence alone cannot tell us which market is the leader for actual issuers. It thus formalizes when or whether standard tools may credibly back up such a claim, and what sort of evidence should be considered fragile or not even identified.

Conclusion

We analyze three measures of price discovery from each market, all within experimental environments that replicate the key identification challenges inherent in studies across equity and corporate-bond and CDS settings. Given a valid rank-two system, the scheduled leader is almost perfectly identified by Hasbrouck midpoints and component shares. Use of rank three in about a third of baseline replications reduces the success across the sample. This over-selection can be largely accounted for by deterministic-term choice. Staleness widens ordering borders where high innovation is well correlated, but clustered and endogenous staleness attenuates bond leadership, with fraction of samples for which breakpoints are unknown being very large.

Refresh-time sampling can overcome this severe staleness bias, but at the same time reduces effective sample size. The expanded bootstrap experiment confirms that percentile intervals have superior calibration compared to basic intervals – but equity and bond coverage struggles to exceed 95% (table D-10). Effective control of false-discovery rates along with power that depends on effect size are confirmed by event-study results. Finally, the audit of reproducibility checks that each table and figure is able to be traced back to archived replication-level outputs. In essence, the heart of our message is procedural: check rank, bound organization-orderings, manage synchronization and parameter instability risks, conduct calibration for power and interval estimates—only then can one claim superior information leadership from equity, bond or CDS markets.

Statements and Declarations

Funding This research was not funded (or supported) externally.

Competing interests: No financial or non-financial competing interest's session.

Availability of data and code: The study included simulated data only. We provide the submission with an online reproducibility resource which includes its complete source code, fixed seeds, scenario registry, replication-level outputs and figures, tests, environment file and

SHA-256 integrity manifest. As an example, CRSP, TRACE, FISD, Compustat or any other rating-agency or CDS-vendor data are not simulated observations.

Ethics approval: Not applicable. No Human Participants, Animals, Personal Data or Confidential Records, the study uses computer generated data and does not involve human participants [12].

Consent for Publication: Not applicable.

Generative AI Used: An assistant with support for language editing, code scaffolding and document formatting. Numerical results were generated by the archived scripts and cross-checked against replication-level outputs, and the author is responsible for the final manuscript and software.

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