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Capital Structure Adjustment in ASEAN-6: Evidence and Inferential Limits

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ABSTRACT

Purpose – This study estimates the speed of book-leverage adjustment among non-financial listed firms in the six largest ASEAN economies, and examines whether it varies with economic uncertainty, with institutional quality, and across the pandemic period.
Design/methodology/approach – From an initial panel of 53,892 firm-year observations covering 4,689 firms from 2010 to 2024, we estimate a dynamic partial-adjustment model by system GMM on a final estimation sample of 32,264 observations from 3,816 firms, with wild cluster bootstrap inference over six country clusters.
Finding/Results – Firms close about fifteen per cent of the gap to target each year, a half-life of 4.4 years, stable from 0.132 to 0.205 across the instrument configurations that pass the diagnostics, four target specifications, six leave-one-country-out samples, and six country subsamples. Adjustment was four to five percentage points faster in 2020 and 2021, with no pre-trend, no difference across pandemic-exposure sectors, and a randomisation p-value of 0.013. The uncertainty effect is not estimated with a stable direction, and institutional moderation is not identified.
Originality/Value – The study shows how country-level effects reported confidently elsewhere become fragile when identification, and not only inference, is examined with six clusters. The market-leverage model remains unresolved.

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1. Introduction

Studies that combine firm-level panels with country-level explanatory variables face an arithmetic that is rarely made explicit. A variable measured for each country and year is replicated across every firm in that country, so a panel of thirty thousand firm-year observations may contain thirty thousand independent realisations of firm size and only six of institutional quality, yet standard errors clustered on firms treat the second number as though it were the first. The result is a literature in which effects of uncertainty and governance on speed of adjustment are reported with a confidence that reflects the number of firms rather than the number of countries.

The six largest ASEAN economies make this problem unusually visible while also offering the regional evidence the literature lacks. Indonesia, Malaysia, the Philippines, Singapore, Thailand, and Vietnam differ sharply in institutional quality, and that diversity is what makes the region attractive for testing whether institutions moderate financing behaviour. Yet it is concentrated rather than distributed. From 2010 to 2024 Singapore's governance score ranges from 1.502 to 1.620, above the maximum recorded anywhere else; the remaining five span 0.69 while the gap to Singapore is 1.22, and ninety per cent of the variance lies between countries.

This study examines the speed of adjustment of book leverage in these six economies. The initial panel contains 53,892 firm-year observations covering 4,689 non-financial listed firms. After the data requirements set out in Section 3.1 and traced in Table 2, the final estimation sample contains 32,264 firm-year observations from 3,816 firms, and every table below reports the sample on which it is estimated. The partial-adjustment model is estimated by system generalised method of moments with forward orthogonal deviations, the instrument matrix collapsed and its depth restricted, and every specification choice reported rather than assumed. Inference on country-level variables uses the wild cluster bootstrap with Webb weights over six country clusters, supplemented by checks that speak to identification rather than to inference.

Two findings are supported, and both refer to book leverage. Firms close approximately fifteen per cent of the gap to target each year, a half-life of about four and a half years, stable from 0.132 to 0.205 across the instrument configurations that pass the diagnostics, four target specifications, six leave-one-country-out samples, and six country subsamples, and satisfying the pooled OLS and fixed-effects bounds throughout. Adjustment was also four to five percentage points faster in 2020 and 2021, with no differential adjustment in the seven preceding years, no effect in two placebo periods, and a return to the prior pattern by 2022.

Two findings are not supported, and the manner of their failure is what this study contributes beyond the regional evidence. They fail differently, and the difference matters. Uncertainty varies meaningfully over time within each economy, so the design does contain variation from which an effect could in principle be estimated, but the data do not deliver a stable answer: the implied direction reverses within the dynamic approach when a single macroeconomic control is added, and the two estimation approaches then point in opposite directions. We therefore describe the effect of uncertainty as not estimated with a stable direction. Institutional quality is different in kind, and we describe its moderating role as not identified. The institutional moderation term does reach conventional significance under small-cluster inference, at a bootstrap p-value of 0.036, yet excluding Singapore triples the coefficient and raises the p-value to 0.506, aggregating to the seventy-eight country-year cells at which the country-level variables vary reverses the sign, and the crossing of the threshold is traceable to a lending-rate series whose availability determines how many Singaporean observations enter the sample.

Three contributions follow. First, the study provides multi-country evidence for a region studied mainly one country at a time, using an estimator that satisfies the bounding criterion and both overidentification tests, and reporting the full specification grid so the estimate can be assessed rather than accepted. Second, it documents that the pandemic-period acceleration was uniform across sectors, financing constraints, and leverage positions, which points toward a common policy and macroeconomic environment rather than differential exposure, and it therefore reports a period association rather than a causal effect. Third, it shows what happens to two conventionally reported country-level effects when inference and identification are both taken seriously with six clusters.

Section 2 reviews the literature and sets out the hypotheses. Section 3 describes the data, the model, the instrument strategy, the construction of target leverage, and the approach to inference. Section 4 reports the results, Section 5 discusses them by source of identifying variation, and Section 6 concludes.

2. Literature Review & Hypothesis Development

2.1 Dynamic trade-off theory and the speed of adjustment

The modern theory of capital structure begins with the irrelevance proposition of Modigliani & Miller (1958) and its tax correction (Modigliani & Miller, 1963). The trade-off view weighs the tax shield against distress costs, with non-debt tax shields substituting for the interest deduction (DeAngelo & Masulis, 1980) and agency conflicts adding further costs (Jensen & Meckling, 1976; Myers, 1977), while the pecking-order account rests on asymmetric information (Myers, 1984; Myers & Majluf, 1984). Tangibility, the market-to-book ratio, size, and profitability are robustly correlated with leverage across the major industrialised economies (Rajan & Zingales, 1995; Titman & Wessels, 1988), tests of the competing accounts find support for elements of both (Fama & French, 2002; Frank & Goyal, 2003), and Harris & Raviv (1991) survey the resulting body of theory.

The dynamic version supplies the framework used here. When rebalancing is costly the firm optimally allows leverage to drift within a range (Fischer et al., 1989), firms rebalance intermittently (Leary & Roberts, 2005), and a dynamic model reproduces cross-sectional patterns that appear to contradict the trade-off view when read statically (Strebulaev, 2007). Lemmon et al. (2008) document a large and persistent firm-specific component in leverage, so any credible specification must control for unobserved firm heterogeneity.

Within this framework the speed of adjustment becomes the central quantity, and estimates vary widely with the estimator and sample: about one third of the gap per year for United States firms (Flannery & Rangan, 2006), and slower speeds under long-differencing estimators (Huang & Ritter, 2009). Öztekin & Flannery (2012) report a mean of 21.11 per cent across thirty-seven countries with country estimates from 4 to 41 per cent, and further heterogeneity across firms, countries, and the business cycle is documented by Faulkender et al. (2012), Drobetz et al. (2015), and Lemma & Negash (2014). Within ASEAN the evidence remains largely national, covering Indonesian state-owned enterprises (Soekarno et al., 2015), Malaysian firms (Ting et al., 2016), Indonesian and Malaysian manufacturers (Memon et al., 2019), and Vietnam (Le et al., 2025). A multi-country regional estimate from a single harmonised specification is still missing.

H1: Non-financial listed firms in the ASEAN-6 adjust their leverage partially toward a target, so that the estimated speed of adjustment lies strictly between zero and one.

2.2 Economic uncertainty and the speed of adjustment

The willingness to rebalance need not be constant across economic conditions, and heightened uncertainty is of particular interest because its net effect is theoretically ambiguous. Bernanke (1983) shows that uncertainty raises the value of waiting before committing to irreversible decisions, and Bloom (2009) documents that uncertainty shocks depress investment and hiring, which applied to financing implies slower adjustment. The opposing argument is that uncertainty raises the cost of carrying excess leverage, which should push firms to correct deviations more urgently to preserve financial flexibility.

The empirical record mirrors this tension. Uncertainty is usually measured by the index of Baker et al. (2016) or the World Uncertainty Index of Ahir et al. (2022). Çolak et al. (2018) report that uncertainty slows adjustment and that strong institutions offset part of the effect, and Bajaj et al. (2021) agree for India, while Aftab et al. (2024) find the opposite for Pakistan and Bajaj et al. (2024) show that the direction depends on the economic state. Related work documents effects on debt maturity, cash holdings, investment, and emerging-market leverage (Chakradhar & Gupta, 2024; Datta et al., 2019; Demir & Ersan, 2017; Kang et al., 2014; Makololo & Seetharam, 2020).

H2: The speed of adjustment varies with economic uncertainty.

2.3 Institutional quality as an exploratory moderator

The institutional environment plausibly conditions how uncertainty transmits to financing behaviour. Where property rights are secure and contracts are enforced, information asymmetries are lower and external funding tends to remain accessible when uncertainty rises, which suggests that the drag imposed by uncertainty should be weaker in better-governed countries. Öztekin & Flannery (2012) provide the strongest evidence for a link between institutions and speed of adjustment, and Çolak et al. (2018) report a moderating role for institutional strength.

We treat this hypothesis as exploratory rather than as a conventional test, and state the reason in advance. Governance as measured by the Worldwide Governance Indicators (Kaufmann et al., 2011) is highly persistent and almost entirely cross-sectional: ninety per cent of its variance lies between countries, and one economy occupies a range no other approaches. Öztekin & Flannery (2012) can regress estimated speeds on institutional features across thirty-seven country-level observations; a regional sample cannot.

H3 (exploratory): Institutional quality moderates the relationship between uncertainty and the speed of adjustment.

2.4 The pandemic period

The COVID-19 pandemic produced an abrupt and widely shared disruption, and several studies examine its consequences for leverage adjustment. Vo et al. (2022) report changes in speed of adjustment across an international sample, and Tarkom & Huang (2023) examine the readjustment of leverage during the pandemic. The period is attractive empirically because it supplies time variation rather than cross-country variation, and time variation is comparatively abundant in a panel of fifteen years.

It is nonetheless not a clean experiment. Every economy and virtually every firm experienced the period simultaneously, so no untreated comparison group exists at the country level, and the period coincided with monetary easing, emergency lending facilities, regulatory forbearance, debt restructuring programmes, fiscal transfers, and large movements in asset values. We therefore frame the hypothesis as an association with the period rather than as an effect of pandemic uncertainty, and construct a within-period comparison based on sector exposure in order to probe the interpretation.

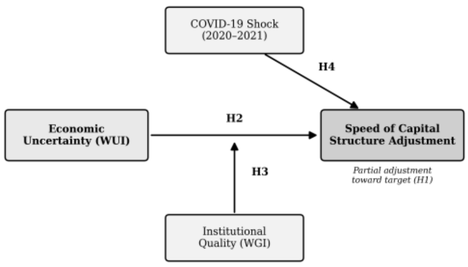
H4: The speed of adjustment during 2020 and 2021 differed from that in other periods of the sample.

2.5 Identification with a small number of country clusters

A fifth strand of literature is methodological and bears directly on the first four. Cameron & Miller (2015) show that conventional cluster-robust standard errors are unreliable when the number of clusters is small, and the wild cluster bootstrap has become the standard remedy. MacKinnon & Webb (2018) analyse the behaviour of the wild bootstrap when few clusters are treated, and Roodman et al. (2019) show that a two-point weight distribution admits only two raised to the power G distinct bootstrap samples, a binding constraint with six clusters, and recommend the six-point distribution below about ten to twelve clusters.

The bootstrap corrects the size of a test. It does not create identifying variation that the design lacks. This distinction organises the present study, which reports each result according to the number of independent realisations behind it rather than the number of observations.

Figure 1. Research framework



Source: authors.

3. Methodology

3.1 Data sources and sample construction

The analysis draws on an unbalanced panel of listed non-financial firms in Indonesia, Malaysia, the Philippines, Singapore, Thailand, and Vietnam over 2010 to 2024. Firm-level accounting and market data come from OSIRIS (Bureau van Dijk), country-level uncertainty from the World Uncertainty Index (Ahir et al., 2022), institutional quality from the Worldwide Governance Indicators (Kaufmann et al., 2011), and the macroeconomic controls from the World Development Indicators.

Banks and insurers are excluded, because their leverage reflects regulatory capital requirements rather than the forces the partial-adjustment framework captures. The initial panel contains 53,892 firm-year observations from 4,689 firms. A residual 367 observations in the estimation sample, or 1.1 per cent, carry GICS 4020 and comprise payment processors, consumer-finance providers, and capital-markets firms rather than deposit-taking or underwriting institutions; excluding them leaves the estimated speed at 0.1454 against 0.1455, so they are retained and the sensitivity reported.

Table 2 traces the construction of the estimation samples. Requiring a consecutive lagged observation removes 5,992 firm-years and requiring the full set of target determinants a further 15,636, leaving a final estimation sample of 32,264 firm-year observations from 3,816 firms for the baseline and interaction models; this second reduction is driven almost entirely by the availability of market capitalisation. Adding the lending rate removes 2,752 more and leaves 29,512, and that reduction is not random, because the lending rate is unavailable for Singapore, the Philippines, and Vietnam in some years, which Section 3.7 shows matters for the governance interaction. The market-leverage sample contains 32,167 observations.

All firm-level ratios are winsorised at the first and ninety-ninth percentiles, which limits the influence of outliers without discarding observations. The panel is unbalanced and contains 1,303 internal year gaps, and 1,068 firms, or 22.8 per cent of the sample, have at least one gap. This bears directly on the choice of transformation discussed in Section 3.5.

3.2 Variable definitions

Book leverage is total debt divided by total assets, and market leverage is total debt divided by the sum of total debt and market capitalisation; book leverage is the primary dependent variable and market leverage is examined in Section 4.6. Both the market-to-book ratio and market leverage contain market capitalisation, the first in the numerator and the second in the denominator, so any specification using market leverage as the dependent variable while treating the lagged market-to-book ratio as an exogenous instrument embeds a mechanical link between the moment conditions and the outcome.

Five firm-level determinants enter the target-leverage equation, all lagged one year so that the target for period t uses information available at the end of period t minus one: the natural logarithm of total assets, earnings before interest and taxes over total assets, net property, plant, and equipment over total assets, the market value of assets over their book value, and depreciation and amortisation over total assets. Four of these are the determinants Rajan & Zingales (1995) establish as robustly correlated with leverage across international data, and the non-debt tax shield follows DeAngelo & Masulis (1980).

The operational definitions follow Faulkender et al. (2012), with two differences: total assets are not deflated to a common price base, because the sample spans six currencies, and research and development expenditure is omitted because it is not consistently reported across the six markets. Lagged industry median leverage, which Faulkender et al. include, is added in one of the alternative target specifications in Table 6.

Economic uncertainty is the country-year value of the World Uncertainty Index, constructed from the frequency with which the word uncertainty and its variants appear in Economist Intelligence Unit country reports. Institutional quality is the simple average of the six Worldwide Governance Indicators. Both are standardised to zero mean and unit variance across the estimation sample, so interaction coefficients are interpretable as the effect of a one-standard-deviation change. Four macroeconomic controls enter the interaction models: real GDP growth, inflation, the lending interest rate, and stock-market capitalisation as a share of GDP.

3.3 The partial-adjustment model

Firms are assumed to have an unobservable target that varies with firm characteristics and the economic environment. Target book leverage for firm i in period t is a linear function of the lagged determinants and a set of time effects,

$$BL*_{i,t} = \beta' X_{i,t-1} + \delta_t + \eta_{i,t} \quad (1)$$

where X denotes the five firm-level determinants, δ_t denotes year effects, and η is a disturbance. Firms do not move to the target immediately, because rebalancing is costly. Instead they close a constant proportion λ of the gap in each period,

$$BL_{i,t} - BL_{i,t-1} = \lambda(BL*_{i,t} - BL_{i,t-1}) + \varepsilon_{i,t} \quad (2)$$

The parameter λ is the speed of adjustment, bounded by zero, where leverage follows a random walk, and one, where rebalancing is immediate and costless. Substituting (1) into (2) yields the reduced-form dynamic panel equation taken to the data,

$$BL_{i,t} = (1-\lambda)BL_{i,t-1} + \lambda\beta' X_{i,t-1} + \lambda\delta_t + \mu_i + \nu_{i,t} \quad (3)$$

where μ_i is an unobserved firm effect and ν is an idiosyncratic error. The coefficient on lagged leverage, denoted ρ, equals one minus the speed of adjustment, and the implied half-life of a deviation from target is

$$half-life = ln(0.5)/ln(\rho) \quad (4)$$

Two properties of equation (3) shape the estimation strategy. First, the lagged dependent variable is correlated with the firm effect by construction, so pooled ordinary least squares is biased upward for ρ and the within transformation downward: Nickell (1981) shows the inconsistency is of order one over T, and with an average of 8.45 observations per firm it is not negligible. The two estimators therefore bound the true value, and the bounding criterion of Bond (2002) is applied to every dynamic panel estimate reported below. Second, the firm effect must be removed, and the instruments addressing the resulting correlation must be selected explicitly rather than by default.

The hypotheses about uncertainty and institutional quality concern whether the speed of adjustment itself varies with the economic environment, and this is examined through two approaches. The dynamic approach interacts lagged leverage with the standardised country-level variables directly in equation (3), so the estimated speed becomes a function of uncertainty and governance without a target being estimated separately. The deviation approach estimates the target from equation (1), constructs the deviation from target, and regresses the change in leverage on that deviation and its interactions,

$$\Delta BL_{i,t} = \lambda DEV_{i,t} + \theta_1(DEV_{i,t} \times U_{s,t}) + \theta_2(DEV_{i,t} \times G_{s,t}) + \theta_3(DEV_{i,t} \times U_{s,t} \times G_{s,t}) + \varphi' Z_{s,t} + \alpha_i + \delta_t + \omega_{i,t} \quad (5)$$

where DEV is the deviation from target, U and G are the standardised uncertainty and governance measures, and Z denotes the macroeconomic controls. In this parameterisation θ_1 greater than zero indicates that uncertainty accelerates adjustment, whereas in the dynamic approach a positive coefficient on the interaction with lagged leverage indicates the opposite, and the two sign conventions are therefore kept distinct throughout Section 4.4. Following the requirement that interaction terms be accompanied by their constituent effects, U, G, and their product also enter equation (5) directly; Section 4.4 reports that including or excluding these main effects moves the interaction coefficients by less than one thousandth.

The dynamic approach carries the primary weight, because it requires no first-stage target and therefore does not propagate first-stage estimation error into the interaction terms. The deviation approach is retained because it permits the event study of Section 4.5 and because the comparison between the two is itself informative about robustness.

3.4 Instrument strategy

The model is estimated by system generalised method of moments (Arellano & Bond, 1991; Arellano & Bover, 1995; Blundell & Bond, 1998). Because system GMM offers wide discretion over the instrument matrix, and because that discretion is often exercised silently, this section reports every choice made.

Regressor classification. Lagged book leverage is the only endogenous variable and is instrumented with its own lags; the five firm-level determinants and the year indicators are strictly exogenous and enter as standard instruments in both equations. This classification was tested rather than asserted. Appendix Table A2 reports six alternatives that move lagged profitability, and then profitability with the lagged market-to-book ratio, into the endogenous set. Hansen rejects in five of the six and improves on the baseline in none, while the implied speed ranges from 0.146 to 0.176 across all seven specifications.

Instrument depth and count. The instrument matrix is collapsed and the lagged dependent variable is instrumented with its third and fourth lags only. Roodman (2009) shows that combining these two containment techniques makes the instrument count invariant in T and preserves the power of the Hansen test. The specification uses 21 instruments against 3,816 groups, but we do not rest the argument on that ratio, since Roodman also shows it does not by itself safeguard the test. It rests on applying both techniques, on reporting the full grid in Appendix Table A1, and on the level of the p-values: Roodman warns that a value as low as 0.25 warrants concern, and every specification carrying a substantive claim exceeds it.

A second and opposite danger applies. Roodman notes that a low degree of overidentification may also weaken the Hansen test, and the preferred specification carries only two degrees of freedom. We therefore report two more heavily overidentified members of the same lag family: lags three to five yield 0.841 on three degrees of freedom and lags three to six yield 0.899 on four, with implied speeds of 0.1453 and 0.1446 against 0.1455 in the preferred specification.

Choice of lag depth. Appendix Table A1 reports the full grid of fourteen configurations, and Figure 2 plots it. Configurations beginning at the first lag reject Hansen and Sargan at every depth. Those beginning at the second lag pass at one degree of freedom but collapse once a single overidentifying restriction is added, with the p-value falling from 0.665 to 0.016. Those beginning at the third lag are monotonically consistent: Hansen rises from 0.495 to 0.899 as restrictions are added and the implied speed varies only between 0.145 and 0.151. Those beginning at the fourth lag produce coefficients above the pooled OLS value of 0.900, outside the bounds a consistent estimator should satisfy (Bond, 2002), and are reported but not used.

Inference and diagnostics. All GMM estimates are two-step with the Windmeijer (2005) correction. For every specification we report the Arellano-Bond tests for first-order and second-order serial correlation, the Hansen test with its degrees of freedom, the Sargan test, and the Difference-in-Hansen test for the level-equation moment conditions and for the instrument subsets based on the dependent variable. One feature should be disclosed: the two-step weighting matrix is singular in every specification, arising from exact linear dependence among moment conditions involving the year indicators rather than from a high instrument count, so estimation uses a generalised inverse, which as Roodman (2009) notes does not affect consistency.

3.5 The choice of transformation

The firm effect is removed by forward orthogonal deviations rather than by first differencing (Arellano & Bover, 1995). Because a previous version of this paper attributed more to that choice than the evidence supports, the practical motivation and the empirical validation are separated here.

Practical motivation. First differencing discards the observation on either side of every gap, whereas forward orthogonal deviations express each observation as a deviation from the mean of all future available observations and lose only the final observation for each firm. Given 1,303 gaps affecting 22.8 per cent of firms, the transformed equation preserves 28,448 observations against 27,913, a difference of 535. In the system estimator the reported count is 32,264 under both transformations, so the gain is real but modest.

What the transformation does not do. Table 5 decomposes the change in the Hansen diagnostic into the part attributable to the transformation and the part attributable to instrument depth. Holding instruments at lags one to two, replacing first differencing with forward orthogonal deviations moves the Hansen p-value from 0.007 to below 0.001, so the transformation alone worsens the diagnostic. Holding the transformation at first differencing, restricting instruments to lags three and four moves it from 0.007 to 0.515, and at that depth both transformations satisfy Hansen and Sargan. The diagnostic problem was one of instrument depth, and the transformation is neither its cause nor its cure.

A second and independent reason. Across every configuration examined, the two transformations differ in the sign they assign to lagged profitability: first differencing yields 0.039 to 0.042, forward orthogonal deviations minus 0.022 to minus 0.030. A negative association between profitability and leverage is among the most consistently replicated regularities in the literature (Myers & Majluf, 1984; Rajan & Zingales, 1995), and the bias-corrected least squares dummy variable estimator, which uses no instruments, also returns minus 0.036. Three estimator families agree on the sign; only first-difference GMM does not.

Most importantly, the parameter of interest is insensitive to these choices within the set of specifications that survive the diagnostics. Across the seven specifications in Table 5, the fourteen in Appendix Table A1, and the seven in Appendix Table A2, spanning two transformations, six lag structures, and two regressor classifications, every configuration that satisfies both the overidentification tests and the pooled OLS and fixed-effects bounds yields an implied speed between 0.136 and 0.175. Those that fail are reported for completeness: configurations beginning at the fourth lag yield 0.060 to 0.072 with coefficients on lagged leverage above the pooled OLS bound, and configurations rejected by the Hansen test reach 0.176.

3.6 Construction of target leverage

Construction. Because the deviation enters every subsequent interaction, measurement error in the target propagates into all of them. The target is recovered from the estimated reduced form in equation (3). With ϱ the coefficient on lagged leverage and λ equal to one minus ϱ, the fitted value splits into a dynamic component and a non-dynamic component comprising the weighted determinants, the year effects, and the intercept; subtracting the dynamic component and dividing by λ yields the target, and the deviation is the target less observed lagged leverage. Year effects enter in all specifications.

A correction to an earlier construction. A previous version computed the target from the five determinant coefficients alone, excluding the intercept and the year effects. Because λ is approximately 0.146, an intercept of minus 0.0068 shifts the target by minus 0.047, and the resulting target had a mean of 0.2892 against an actual sample mean of 0.2416, an upward bias of 0.048 that made firms appear systematically underlevered. The construction adopted here reproduces the sample mean to within 0.007, at 0.2486.

Alternative target specifications. Table 6 reports four target equations estimated by the same preferred specification: the baseline with the five determinants and year effects, then adding GICS sector effects, country effects, and lagged industry median leverage within each country following Flannery & Rangan (2006) and Faulkender et al. (2012). Industry median leverage enters at 0.029 with a p-value of 0.045 and the Malaysia country effect at minus 0.010 below 0.001, so the additional determinants carry information, yet the implied speed ranges only from 0.1448 to 0.1474 and the deviation variable correlates at 0.977 or above between any pair of specifications.

The generated-regressor problem. Because the target is a fitted value, the deviation variable is a generated regressor, and Pagan (1984) shows that the resulting problem is one of inference rather than of consistency. We address it in three ways: the dynamic approach requires no target, so no first-stage error enters it, and every substantive conclusion is reported under both approaches; all inference on country-level terms in the deviation approach uses the wild cluster bootstrap of Section 3.7; and the sensitivity of the deviation variable to the target specification is reported in Table 6. Section 7 lists the residual issue among the limitations.

3.7 Inference with six country clusters

The interaction models use 32,264 firm-year observations, but the uncertainty and governance variables take only as many independent values as there are countries and years, and this shapes every claim about country-level effects in the paper.

The structure of the identifying variation. Table 8, Panel A decomposes the variance of both country-level series across the 90 country-year cells. For uncertainty the within-country standard deviation of 0.082 exceeds the between-country standard deviation of 0.035. For governance the pattern reverses sharply, at 0.758 between against 0.077 within, and Table 2 makes the consequence concrete: Singapore's entire range lies above the maximum observed anywhere else. Identification of governance moderation therefore rests on a comparison between one economy and five others, not on 32,264 observations. The two country-level variables pose different problems. Uncertainty supplies usable variation over time within each economy, so the question it raises is whether the data pin down a direction. Governance supplies a single contrast, so the question it raises is whether an effect can be identified at all.

Inference. Conventional cluster-robust standard errors are unreliable with few clusters, and with six the asymptotic approximation is not defensible (Cameron & Miller, 2015). We report wild cluster bootstrap-t p-values and confidence sets with the null imposed, clustering on country, using Webb six-point weights and 9,999 replications. Webb weights are preferred because a two-point distribution admits only 64 distinct bootstrap samples with six clusters, and because Roodman et al. (2019) recommend the six-point distribution below about ten to twelve clusters (see also MacKinnon & Webb, 2018). The bootstrap is applied to the deviation approach, which is estimated by linear regression with fixed effects. It is not applied to the dynamic approach, because imposing the null and re-forming the two-step GMM weighting matrix within each bootstrap replication is not supported by the available implementation, and we therefore rest no country-level significance claim on the standard errors reported for that approach. Analytic standard errors clustered on firms are reported for comparison only.

What the bootstrap cannot do. A bootstrap that respects the cluster structure corrects the size of the test; it cannot create identifying variation the design does not contain. We therefore supplement it with three checks that speak to identification: dropping one country at a time, dropping Singapore specifically, and aggregating to the 78 country-year cells at which the country-level variables actually vary.

Consequences for the hypotheses. Because the governance variation is concentrated in a single economy, H3 is treated as exploratory throughout, for the reason given in Section 2.3. Two terms are used consistently from here on. An effect is described as not identified when the design supplies no variation from which it could be estimated, which is the case for institutional moderation. An effect is described as not estimated with a stable direction when the variation exists but the data do not pin down a sign, which is the case for uncertainty. Neither term is a statement that the effect is absent in the world.

4. Result and Discussion

4.1 Descriptive statistics

Table 1 reports summary statistics. Mean book leverage is 0.242 with a standard deviation of 0.186 and an interquartile range from 0.082 to 0.366, and market leverage is higher at 0.318. The country-level series show the pattern that governs the rest of the paper: uncertainty has country means clustering tightly from 0.077 in Vietnam to 0.196 in Thailand with within-country standard deviations from 0.064 to 0.095, while governance shows the opposite structure, with country means from minus 0.352 in Vietnam to 1.560 in Singapore against within-country standard deviations of only 0.038 to 0.079.

Table 2, Panel A traces the construction of the estimation samples, Panel B shows that no single market dominates, since Malaysia contributes 25.5 per cent of firm-years and the Philippines 5.4 per cent, and Panel C reports the country means of both country-level series.

Table 1. Descriptive statistics

Variable	N	Mean	SD	P25	Median	P75
Book leverage	32,264	0.2416	0.1864	0.0822	0.2149	0.3658
Market leverage	32,167	0.3183	0.2549	0.0870	0.2743	0.5121
Firm size (ln assets)	32,264	11.7410	1.7953	10.5023	11.5819	12.8171
Profitability (EBIT/TA)	32,264	0.0444	0.0952	0.0077	0.0445	0.0874
Tangibility	32,245	0.3017	0.2334	0.0999	0.2613	0.4616
Market to book	32,167	1.3297	1.1651	0.7774	0.9850	1.4012
Non-debt tax shield	32,264	0.0346	0.0340	0.0116	0.0257	0.0454
Dividend payer	32,264	0.6126	0.4872	0.0000	1.0000	1.0000
World Uncertainty Index	32,264	0.1418	0.0900	0.0651	0.1287	0.2062
Governance (WGI average)	32,264	0.1377	0.6474	−0.3311	−0.1593	0.3672
GDP growth (%)	32,264	4.1316	3.0569	3.1340	4.8176	5.5535
Inflation (%)	32,264	2.5179	2.0946	1.2304	2.3895	3.5258
Lending rate (%)	29,512	6.3465	2.5881	4.5311	5.2827	8.5200

Variable	N	Mean	SD	P25	Median	P75
Market cap to GDP (%)	32,264	96.7063	51.6996	47.3866	98.6998	120.3519

Source: authors' calculation using Stata.
Notes: estimation sample of 32,264 firm-year observations from 3,816 non-financial listed firms across six ASEAN economies, 2010 to 2024. All firm-level ratios are winsorised at the first and ninety-ninth percentiles.

Table 2. Sample construction and country composition

Step	Firm-years	Loss
(a) All firm-years, six countries, 2010 to 2024	53,892	—
(b) Book leverage available	53,892	0
(c) Consecutive lagged observation available	47,900	−5,992
(d) All target determinants available (H1 sample)	32,264	−15,636
(e) All macroeconomic controls available (interaction sample)	29,512	−2,752
(f) Market leverage and its lag available	32,167	−97

Table 3. Country composition of both estimation samples

Country	H1 firm-years	%	Interaction firm-years	%	Firms (H1)	%
Indonesia	5,674	17.59	5,674	19.23	756	19.81
Malaysia	8,223	25.49	8,223	27.86	873	22.88
Philippines	1,732	5.37	966	3.27	186	4.87
Singapore	4,661	14.45	3,381	11.46	464	12.16
Thailand	6,296	19.51	6,296	21.33	690	18.08
Vietnam	5,678	17.60	4,972	16.85	847	22.20
Total	**32,264**	**100.00**	**29,512**	**100.00**	**3,816**	**100.00**

Table 4. Governance and uncertainty by country, H1 sample

Country	WGI mean	WGI SD	WGI min	WGI max	WUI mean	WUI SD
Singapore	1.5595	0.0378	1.5023	1.6200	0.1198	0.0812
Malaysia	0.3348	0.0725	0.2061	0.4495	0.1520	0.0951
Indonesia	−0.1634	0.0793	−0.3602	−0.0816	0.1497	0.0757
Thailand	−0.3261	0.0642	−0.4423	−0.2174	0.1961	0.0806
Philippines	−0.3454	0.0596	−0.4242	−0.1932	0.1420	0.0899
Vietnam	−0.3521	0.0680	−0.5263	−0.2856	0.0771	0.0637

Source: authors' calculation using Stata.
Notes: the loss at step (d) is driven almost entirely by the availability of market capitalisation, which enters the market-to-book ratio, and the loss at step (e) entirely by the lending rate, which is unavailable for Singapore, the Philippines, and Vietnam in some years. In Panel C, Singapore's entire time-series range for governance lies above the maximum observed in any other country.

4.2 Partial adjustment

Table 3 reports the pooled estimates in its first row. The coefficient on lagged book leverage is 0.8545 with a standard error of 0.0203, implying an annual speed of adjustment of 0.1455 and a half-life of 4.4 years. The estimate is bounded as theory requires, since pooled ordinary least squares gives 0.9004, biased upward by the correlation between lagged leverage and the firm effect, and fixed effects gives 0.6126, biased downward by the Nickell (1981) transformation bias. Full diagnostics for this specification appear in column (1) of Table 4. H1 is supported.

The diagnostics support the specification. The Arellano-Bond test rejects first-order serial correlation below 0.001, as it must if the underlying disturbances are serially uncorrelated, and does not reject second-order correlation at 0.408. Hansen does not reject at 0.716 on two degrees of freedom, Sargan agrees at 0.579, and Difference-in-Hansen for the level-equation moment conditions does not reject at 0.687, using 21 instruments against 3,816 groups. The determinants carry the expected signs: size positive at 0.0035, profitability negative at minus 0.0219, the non-debt tax shield negative at minus 0.0479, the market-to-book ratio positive at 0.0020, and tangibility positive but not significant.

The closest comparison is Öztekin & Flannery (2012), who estimate the same model by system GMM across 105,568 firm-year observations from 37 countries and report a mean of 21.11 per cent with country estimates from 4 to 41 per cent, so our 14.55 per cent lies within that range and below its centre. Flannery & Rangan (2006) report 0.343 with firm fixed effects and 0.091 from a two-stage model on the same data, and our own bounds of 0.100 and 0.387 reproduce that range closely; Huang & Ritter (2009) report 11.5 to 21.1 per cent, and our estimate falls inside that range as well.

Table 5. Speed of adjustment across estimator families and countries, book leverage

Sample	Firm-years	Firms	Pooled OLS	Fixed effects	LSDVC	System GMM	ρ (SE)	95% CI for SOA	AR(2) p	Hansen p
ASEAN-6 pooled	32,264	3,816	0.0996	0.3874	0.2233	0.1455	0.8545 (0.0203)	0.106 to 0.185	0.408	0.716
Indonesia	5,674	756	0.0980	0.3780	—	0.1463	0.8537 (0.0539)	0.041 to 0.252	0.802	0.633
Malaysia	8,223	873	0.0942	0.3462	—	0.1434	0.8566 (0.0338)	0.077 to 0.210	0.324	0.428
Philippines	1,732	186	0.0929	0.3753	—	0.1878	0.8122 (0.0617)	0.066 to 0.309	0.967	0.990
Singapore	4,661	464	0.1459	0.4348	—	0.1542	0.8458 (0.0749)	0.007 to 0.301	0.378	1.000
Thailand	6,296	690	0.1037	0.4244	—	0.2047	0.7953 (0.0414)	0.123 to 0.286	0.804	0.947
Vietnam	5,678	847	0.0994	0.3936	—	0.1441	0.8559 (0.0347)	0.076 to 0.212	0.478	0.195

Source: authors' estimation using Stata.

Notes: the four estimator columns report implied speeds of adjustment, equal to one minus the coefficient on lagged leverage; ϱ is that coefficient under system GMM and the confidence interval is for the implied speed. All models use the same specification and the same sample. Standard errors are clustered by firm for pooled OLS and fixed effects and use the Windmeijer (2005) correction for system GMM. LSDVC is the bias-corrected least squares dummy variable estimator of Kiviet (1995) implemented for unbalanced panels by Bruno (2005), initialised from the Blundell-Bond estimator with the correction taken to order one over T; it is reported for the pooled sample only, its standard error is not computed because that requires a bootstrap, it provides no test of overidentifying restrictions, and two year indicators are absorbed in its within transformation, so its year controls are marginally less rich than those of the other three columns. Every system GMM column uses 21 instruments, Hansen carries two degrees of freedom, and the Arellano-Bond test for first-order serial correlation rejects below 0.001 in every row. Sargan p-values are 0.579 pooled, and 0.385, 0.159, 0.977, 0.999, 0.935 and 0.112 for Indonesia, Malaysia, the Philippines, Singapore, Thailand and Vietnam. Every system GMM estimate lies within its own pooled OLS and fixed-effects bounds, as Bond (2002) requires. Full diagnostics for the pooled specification appear in column (1) of Table 5.

4.3 Robustness of the speed of adjustment

The remaining rows of Table 3 estimate the same model on the same sample for each country. Every fixed-effects estimate lies between 0.346 and 0.435 and every system GMM estimate between 0.143 and 0.205, so the figures cited elsewhere as high are fixed-effects estimates and those cited as low are system GMM estimates. Each country estimate lies within its own bounds, and each of the six confidence intervals contains the pooled value of 0.1455.

Heterogeneity across countries cannot be distinguished from zero: the widest pair is Thailand at 0.2047 and Malaysia at 0.1434, a difference of 0.0613 against a pooled standard error of approximately 0.0534. The serial-correlation requirements are met in every subsample, since the Arellano-Bond test rejects first-order correlation below 0.001 throughout and does not reject second-order correlation anywhere, with p-values from 0.324 in Malaysia to 0.967 in the Philippines. The overidentification tests are where caution is warranted rather than confidence. Singapore and the Philippines return Hansen p-values of 0.9996 and 0.9898 and Sargan p-values of 0.999 and 0.977, which approach the implausibly perfect values Roodman (2009) identifies as the symptom of a test with little power in subsamples of 464 and 186 firms, while Vietnam returns 0.195 and 0.112, below the 0.25 threshold he recommends treating with concern. The country estimates are therefore reported to reconcile the magnitudes across estimator families, and not as six independently credible parameters.

Flannery & Hankins (2013) demonstrate the same pattern within a single dataset: holding the data fixed and varying only panel length, they report 13 per cent under pooled OLS, 25 per cent under fixed effects at thirty years, and 44 per cent under fixed effects at ten years, while the system GMM estimate remains between 15.4 and 18 per cent. Our own estimates of 10.0, 38.7, and 14.6 per cent, at an average of 8.45 observations per firm, reproduce that configuration closely.

A fourth estimator sharpens the point. The bias-corrected least squares dummy variable estimator of Kiviet (1995), implemented for unbalanced panels by Bruno (2005), corrects the fixed-effects bias directly rather than instrumenting around it, and on this sample it raises the coefficient on lagged leverage from 0.6126 to 0.7767, an implied speed of 0.2233 that lies inside the bounds and closer to system GMM than to fixed effects. The two estimators that correct for dynamic panel bias produce 0.146 and 0.223, and neither approaches the range of 0.30 to 0.49 an uncorrected fixed-effects estimator produces. We retain system GMM, because Bruno motivates the corrected estimator for panels of ten to twenty units whereas ours has 3,816.

Three further checks confirm the stability of the pooled estimate. Leave-one-country-out estimation yields 0.132 when Thailand is dropped and 0.157 when Malaysia is dropped, and every one of the six satisfies the diagnostics, with Hansen p-values from 0.541 to 0.954 and second-order serial correlation p-values from 0.285 to 0.734. Every lag configuration in Appendix Table A1 that satisfies the diagnostics yields between 0.145 and 0.175, and the four target specifications in Table 6 yield 0.1448 to 0.1474. The speed of adjustment is therefore stable within the system GMM family and not stable across estimator families.

Table 6. Instrument strategy and diagnostics for every GMM specification

	(1) H1 baseline	**(2) Uncertainty and governance**	**(3) Pandemic period**	**(4) Market leverage**
Dependent variable	Book leverage	Book leverage	Book leverage	Market leverage
Macro controls include lending rate	—	No	Yes	—
Observations	32,264	32,264	29,512	32,167
Number of groups	3,816	3,816	3,739	3,811
Number of instruments	21	33	28	24
Instruments per group	0.006	0.009	0.007	0.006
Implied SOA	0.1455	0.1496	0.1456	0.0717
AR(1) p-value	0.000	0.000	0.000	0.000
AR(2) p-value	0.408	0.360	0.364	0.808
Hansen p-value	0.716	0.431	0.735	0.128
Hansen degrees of freedom	2	8	4	4
Sargan p-value	0.579	0.308	0.582	0.068
Diff-in-Hansen, level instruments	0.687	0.895	0.869	0.078
Diff-in-Hansen, lagged leverage subset	—	0.876	0.572	—

Source: authors' estimation using Stata.

Notes: all four models use forward orthogonal deviations with the instrument matrix collapsed, instruments restricted to the third and fourth lags, and no predetermined regressors, and are two-step with the Windmeijer (2005) correction. The only GMM-instrumented firm-level variable is lagged leverage and its interactions where present; the five lagged determinants and the year indicators are strictly exogenous instruments in both equations. Column (3) retains the lending rate, because omitting it there moves the Sargan test from 0.582 to 0.001 and the implied normal-period speed to 0.091, below the pooled OLS lower bound. The Difference-in-Hansen tests for the instrument subsets are computed from a specification in which the lagged dependent variable and its interactions enter separate gmm() groups, which raises the instrument count without materially changing the estimate.

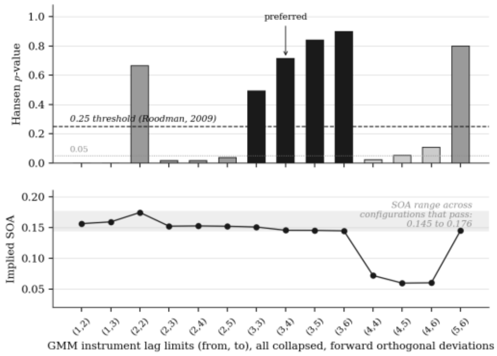
Table 7. Transformation and lag depth, decomposed

Lag limits	**Transformation**	**Inst.**	**Hansen df**	**Hansen p**	**Sargan p**	**AR(2) p**	**SOA**	**L.prof coef.**
1 to 2	First differences	21	2	0.007	0.000	0.491	0.1569	+0.0388
1 to 2	Forward orthogonal	21	2	0.000	0.000	0.413	0.1564	−0.0216
3 to 4	First differences	21	2	0.515	0.333	0.480	0.1358	+0.0419
3 to 4	Forward orthogonal	21	2	0.716	0.579	0.408	0.1455	−0.0219

Source: authors' estimation using Stata.

Notes: holding the instrument set at lags one to two, replacing first differencing with forward orthogonal deviations moves the Hansen p-value from 0.007 to below 0.001, so the transformation alone does not repair the diagnostic; holding the transformation at first differences, restricting the instruments to the third and fourth lags moves the p-value to 0.515, so instrument depth does. Forward orthogonal deviations are retained because they preserve 535 observations in the transformed equation and yield the negative coefficient on lagged profitability that the literature consistently reports.

Figure 2. Hansen p-value and implied speed of adjustment across the full lag grid



Source: authors' estimation using Stata.

Notes: all configurations use forward orthogonal deviations with the instrument matrix collapsed. The dashed line marks the 0.25 threshold that Roodman (2009) recommends. The shaded band in the lower panel marks the range of implied speeds of adjustment across configurations that satisfy the diagnostics.

Table 8. Alternative target-leverage specifications

Panel A. Estimation of the target equation

Target specification	Additional determinants	N	Inst.	Hansen p	AR(2) p	Implied SOA
(1) Baseline	none	32,264	21	0.716	0.408	0.1455
(2) Sector	GICS sector effects	31,692	31	0.789	0.403	0.1448
(3) Sector and country	sector and country effects	31,692	36	0.813	0.402	0.1456
(4) Full	+ lagged industry median leverage	31,692	37	0.823	0.404	0.1474

Table 9. Properties of the fitted target

	Spec (1)	Spec (2)	Spec (3)	Spec (4)	Actual leverage
Mean	0.2486	0.2485	0.2485	0.2483	0.2416
Standard deviation	0.0551	0.0604	0.0669	0.0676	0.1864
10th percentile	0.1788	0.1735	0.1636	0.1643	—
Median	0.2464	0.2464	0.2472	0.2465	0.2149
90th percentile	0.3213	0.3271	0.3350	0.3366	—

Source: authors' estimation using Stata.

Notes: the target is computed as the non-dynamic component of the fitted value divided by the adjustment coefficient, and therefore includes the intercept and the year effects. An earlier construction that omitted both produced a target with a mean of 0.2892 against an actual mean of 0.2416, an upward bias of 0.048 equal to the omitted intercept divided by the adjustment coefficient. The construction adopted here reproduces the sample mean to within 0.007. The deviation variable correlates at 0.977 or above between any pair of the four specifications.

4.4 Uncertainty and institutions are not identified

This section reports the paper's central methodological result: the two country-level hypotheses cannot be tested with the precision the sample size superficially suggests, and the evidence that would ordinarily be read as support does not survive checks that speak to identification rather than to inference.

The structure of the variation. For governance the between-country standard deviation is 0.758 against a within-country standard deviation of 0.077, so any test of institutional moderation is a comparison between one economy and five others whatever the nominal sample size.

Inference. Table 7, Panel A contrasts analytic and small-cluster inference. Under analytic standard errors clustered on firms the triple interaction carries a t-statistic of 3.90 and a p-value of 0.011; under the wild cluster bootstrap over six clusters the p-value rises to 0.036, the deviation-uncertainty interaction moves from 0.188 to 0.196, and the governance interaction from 0.106 to 0.673. The uncertainty interaction therefore cannot be distinguished from zero at any conventional level, and the governance term produces a disjoint 95 per cent confidence set once the lending rate is included, a signature of weak identification that a single interval would conceal. H2 is not supported.

Why statistical significance is not evidence here. The triple interaction survives the bootstrap at the five per cent level, and three checks in Table 8 show that this should not be read as support for H3. Removing Singapore triples the coefficient from 0.0043 to 0.0137 while the bootstrap p-value rises from 0.036 to 0.506 and the governance term reverses sign, and Panel B confirms that Singapore alone produces this, since dropping any other economy leaves the triple interaction between 0.0036 and 0.0051. Aggregating to the 78 country-year cells at which the country-level variables actually vary reverses the sign of both terms. And the source of the significance is the sample rather than the specification: changing the control set alone moves the p-value only from 0.061 to 0.056, whereas adding 2,544 observations, of which 1,280 are Singaporean, carries it to 0.036 while the coefficient falls by a third.

The dynamic approach adds a second reading, and the two must be compared on a common scale because their sign conventions are opposite. Table 7, Panel B estimates the same hypotheses without an estimated target, and Figure 3 plots the uncertainty interaction across specifications. Read as an effect on speed of adjustment, the deviation approach implies that uncertainty accelerates adjustment under both control sets, at 0.0054 and 0.0044, while the dynamic approach implies acceleration in the preferred specification, at minus 0.0017, and the opposite once the lending rate is added, at 0.0094. The implied direction is therefore not stable, and no estimate is distinguishable from zero, with bootstrap p-values of 0.196 and 0.322 in the deviation approach and p-values of 0.848 and 0.141 in the dynamic approach. The triple interaction is more consistent, implying faster adjustment in all four cells, but its only crossing of a conventional threshold is the one the checks in Table 8 dismantle.

The two results therefore fail for different reasons. Uncertainty varies within each economy over time, so the design supplies variation from which an effect could be estimated, and what the data withhold is a stable direction rather than the possibility of estimation. Governance varies almost entirely between six economies and one of them occupies a range no other approaches, so what the design withholds is identification itself. We describe the effect of uncertainty as not estimated with a stable direction, and institutional moderation as not identified. Neither statement claims that uncertainty or institutions are irrelevant to financing behaviour.

Table 10. Uncertainty and governance: analytic versus small-cluster inference

Panel A. Deviation approach, fixed effects, target specification (3)

Term	Preferred (N = 31,692) Coef.	t	Analytic p	Bootstrap p	With lending rate (N = 29,148) Coef.	t	Analytic p	Bootstrap p
Deviation from target	0.3723	29.08	0.000	—	0.3861	21.05	0.000	—
Deviation × uncertainty	0.0054	1.53	0.188	0.196	0.0044	1.40	0.221	0.322
Deviation × governance	0.0125	1.97	0.106	0.673	0.0237	1.86	0.122	0.687
Deviation × uncertainty × governance	0.0043	3.90	0.011	0.036	0.0065	4.59	0.006	0.061

Table 11. Dynamic approach, system GMM, forward orthogonal deviations, lags 3 to 4

	Preferred (N = 32,264) Coef.	SE	p	With lending rate (N = 29,512) Coef.	SE	p
Lagged leverage	0.8504	0.0174	0.000	0.8421	0.0218	0.000
Lagged leverage × uncertainty	−0.0017	0.0088	0.848	+0.0094	0.0064	0.141
Lagged leverage × governance	0.0172	0.0086	0.046	0.0177	0.0257	0.491
Lagged leverage × uncertainty × governance	−0.0114	0.0062	0.064	−0.0086	0.0078	0.272
Implied SOA	0.1496			0.1579		
Hansen p (df)	0.431 (8)			0.186 (8)		
Sargan p	0.308			0.049		

Source: authors' estimation using Stata.

Notes: bootstrap p-values are wild cluster bootstrap-t with the null imposed, Webb weights, and 9,999 replications, clustered on the six countries. Analytic inference in Panel A clusters on firms and is reported for comparison only. The bootstrap is not applied to the dynamic approach in Panel B, for the reason given in Section 3.7, so no country-level significance claim rests on the standard errors reported there. The 95 per cent bootstrap confidence sets in the preferred column of Panel A are minus 0.0024 to 0.0250 for the uncertainty interaction, minus 0.0570 to 0.0402 for the governance interaction, and 0.0015 to 0.0104 for the triple interaction; with the lending rate included they are minus 0.0074 to 0.0252, minus 0.1033 to 0.0448 together with 0.0602 to 0.0923, and minus 0.0013 to 0.0111. The governance set in the second column is disjoint, and the triple-interaction set moves from entirely positive to containing zero when a single macroeconomic control is added.

Table 12. Identification checks

Panel A. Variance decomposition of the country-level variables

	Overall SD	Between-country SD	Within-country SD	Within / between
World Uncertainty Index	0.0883	0.0350	0.0822	2.35
Governance (WGI average)	0.7003	0.7582	0.0772	0.10

Table 13. Panel B. Leave-one-country-out, deviation approach

Country dropped	N	Deviation	× uncertainty	× governance	× unc. × gov.
Indonesia	26,090	0.3727	0.0075	0.0109	0.0042
Malaysia	23,524	0.3835	0.0021	0.0128	0.0036
Philippines	29,962	0.3730	0.0042	0.0103	0.0051
Singapore	27,038	0.3572	0.0072	−0.0316	0.0142
Thailand	25,418	0.3629	0.0058	0.0158	0.0043
Vietnam	26,428	0.3687	0.0069	0.0126	0.0039
Full sample	**31,692**	**0.3723**	**0.0054**	**0.0125**	**0.0043**

Table 14. Panel C. Excluding Singapore (five-country bootstrap) and aggregation to 78 country-year cells

	Excluding Singapore: Coef.	Bootstrap p	95% bootstrap set	78 cells: Coef.	SE	p
Deviation from target	—	—	—	0.1580	0.0262	0.002
Deviation × uncertainty	0.0075	0.708	−0.0398 to 0.0573	−0.0270	0.0254	0.335
Deviation × governance	−0.0293	0.732	−0.1329 to 0.0988	0.0212	0.0094	0.074
Deviation × uncertainty × governance	0.0137	0.506	−0.0519 to 0.0237	−0.0276	0.0186	0.199

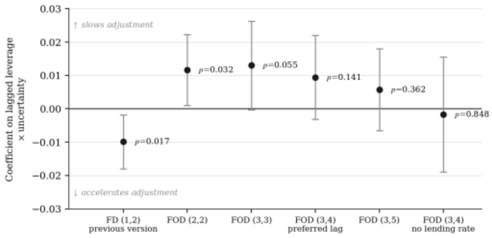
Table 15. Panel D. Sensitivity to sample composition, triple interaction

Cell	Macro controls	Sample	N	Coef.	Bootstrap p	95% set contains zero
A	with lending rate	narrow	29,148	0.0065	0.061	Yes
B	without lending rate	narrow	29,148	0.0064	0.056	Yes
C	without lending rate	full	31,692	0.0043	0.036	No

Source: authors' estimation using Stata.

Notes: Panel A decomposes the variance of both country-level series across the 90 country-year cells. Panel B shows that Singapore alone reverses the sign of the governance term and triples the triple interaction. In Panel C the left-hand block re-estimates the model on five countries and bootstraps over five clusters, and the right-hand block aggregates the data to the 78 country-year cells at which the country-level variables actually vary. Panel D isolates the source of statistical significance: cell A to cell B changes the control set but not the sample and leaves the estimate essentially unchanged, whereas cell B to cell C changes only the sample and shifts the bootstrap p-value across the conventional threshold; the 2,544 additional observations are concentrated in Singapore, which recovers 1,280 firm-years.

Figure 3. The uncertainty interaction across specifications, dynamic approach



Source: authors' estimation using Stata.

Notes: bars are 95 per cent analytic confidence intervals, reported for comparison only. Under this parameterisation the interaction enters the coefficient on lagged leverage, so a positive value indicates that uncertainty slows adjustment, the opposite of the convention in equation (5).

4.5 The pandemic period

Adjustment was faster in 2020 and 2021 than in any other period, and this is the most robust of the macro-level results. Table 9, Panel A reports the difference across four target specifications and two macroeconomic control sets: the estimated increase ranges from 0.046 to 0.050 with p-values below 0.001 in every case, and is 0.046 under the earlier target construction as well. H4 is supported, subject to the qualification set out below.

Table 9, Panel B and Figure 4 report the event study with 2019 omitted. None of the seven pre-pandemic interactions is significant, with p-values from 0.103 to 0.926, while the 2020 and 2021 interactions are 0.0458 and 0.0576 with p-values of 0.003 and 0.001 and the 2022 and 2023 interactions return to 0.0096 and 0.0103 with p-values above 0.49. The 2024 interaction turns negative at minus 0.0291 with a p-value of 0.049, so adjustment returned to and then fell slightly below the pre-pandemic pace, which is the opposite of a persistent acceleration. Two placebo periods, 2014 to 2015 and 2016 to 2017, are not significant, and Panel E generalises this to every two-year window the sample admits. The temporal break is therefore sharp, isolated, and reversed within a year.

Table 9, Panel C and Figure 5 address the concern that a period indicator provides no untreated comparison group. Sectors are split by pandemic exposure and both groups accelerate, the low-exposure group from 0.3765 to 0.4426 and the high-exposure group from 0.3802 to 0.4222, with a difference in differences of minus 0.0240 and a p-value of 0.344. The same absence of heterogeneity holds between dividend payers and non-payers and between firms above and below target.

The pandemic indicator raises the same question this paper asks of the country-level variables, and Table 9, Panel E answers it. The indicator is defined at the year level and repeated across thousands of firms, so standard errors clustered on firms may overstate its precision. Two features of the specification limit the concern. Year effects are included, so the level of every year shock is absorbed and the estimated term is the interaction between the deviation and the pandemic period, which varies at the firm-year level. What remains at the year level is the timing of the split itself, and Panel E therefore re-estimates the same coefficient under nine inference procedures.

The point estimate is invariant at 0.0484. The standard error rises from 0.0099 under firm clustering to 0.0179 under year clustering, which confirms that firm clustering overstates precision, yet the coefficient remains significant at 0.019 with thirteen year clusters, at 0.011

with six country clusters, at 0.002 with seventy-eight country-year clusters, and at 0.005 under two-way clustering on firm and year. Under the wild cluster bootstrap with Webb weights the result holds when clustering on country, at 0.016 with a confidence set of 0.0145 to 0.0726, and on country-year, at 0.009 with a confidence set of 0.0157 to 0.0776.

One procedure does not reject. Clustering on year, the wild cluster bootstrap returns a p-value of 0.153 and a confidence set from minus 0.0486 to 0.1218 that contains zero. We report this rather than select among procedures, and we note the reason it is likely to be conservative here: only two of the thirteen year clusters carry the pandemic label, and MacKinnon & Webb (2018) show that the wild cluster bootstrap loses power severely when few clusters are treated. That is the configuration in which a randomisation test is preferable, because it is exact and requires no asymptotic approximation. Reassigning the pandemic label to each of the seventy-eight two-year windows available in the sample, the actual 2020 to 2021 estimate is the largest of all seventy-eight, giving a two-sided p-value of 0.013. We therefore describe H4 as supported, with the qualification that the evidence rests on eight of nine inference procedures rather than on all of them, and that the exception is the procedure with the least power against this design.

This uniformity shapes the interpretation. An acceleration concentrated in the hardest-hit sectors would point toward pandemic exposure, whereas one common across sectors, financing constraints, and leverage positions points toward a common environment. The period coincided with monetary easing, emergency lending facilities, regulatory forbearance, debt restructuring programmes, fiscal transfers, and large movements in asset values, and this design cannot separate them from uncertainty. We therefore describe speed of adjustment as unusually high during the pandemic period, and do not describe pandemic uncertainty as its cause.

Three further qualifications belong here. Table 9, Panel D shows that debt fell relative to lagged assets by 0.0198 in 2020 and 0.0299 in 2021 against 0.0062 in 2019, while the decline in asset growth continued a trend visible from 2013, so the fall in leverage originates in the numerator. The fitted target itself moved by 0.106 between 2020 and 2021, so part of the measured convergence in 2021 reflects a target moving toward firms. And the effect is not evenly divided between the two pandemic years, since eight of the ten largest placebo windows in Panel E include 2021, which is consistent with the event study, where the 2021 interaction of 0.0576 exceeds the 2020 interaction of 0.0458. The dynamic approach places the pandemic effect at 0.0123 with a standard error large enough that the interaction is not individually significant.

Table 16. The pandemic period

Panel A. Speed of adjustment in 2020 and 2021 across target specifications

Target specification	Macro controls	N	Normal	Pandemic	Difference	p
(1) Baseline	with lending rate	29,512	0.3796	0.4255	+0.0459	0.000
(2) Sector	with lending rate	29,148	0.3773	0.4252	+0.0479	0.000

Target specification	Macro controls	N	Normal	Pandemic	Difference	p
(3) Sector and country	with lending rate	29,148	0.3780	0.4283	+0.0503	0.000
(4) Full	with lending rate	29,148	0.3795	0.4296	+0.0501	0.000
(3) Sector and country	without lending rate	31,692	0.3667	0.4151	+0.0484	0.000
(1) Manuscript-style target	with lending rate	29,512	0.3796	0.4255	+0.0459	0.000

Table 17. Panel B. Event study, deviation interacted with year, 2019 omitted

Year	Interaction	p	Year	Interaction	p
2012	−0.0240	0.282	2018	+0.0116	0.394
2013	−0.0175	0.357	2020	+0.0458	0.003
2014	+0.0017	0.926	2021	+0.0576	0.001
2015	−0.0029	0.868	2022	+0.0096	0.494
2016	+0.0105	0.536	2023	+0.0103	0.503
2017	+0.0290	0.103	2024	−0.0291	0.049

Table 18. Panel C. Within-period comparisons, placebo periods and heterogeneity

Comparison	Normal	Pandemic	Difference	p
Low-exposure sectors (staples, health care, comm. services, utilities)	0.3765	0.4426	+0.0660	0.004
High-exposure sectors (energy, industrials, cons. disc., real estate)	0.3802	0.4222	+0.0420	0.001
Difference in differences			−0.0240	0.344
Placebo period 2014 to 2015 (N = 29,148)			−0.0099	0.371
Placebo period 2016 to 2017 (N = 29,148)			+0.0118	0.292
Dividend payers versus non-payers			−0.0161	0.409
Firms above versus below target			+0.0136	0.698

Table 19. Panel D. Mechanism, year effects

Year effect	2019	2020	2021
Change in debt over lagged assets	−0.0062	−0.0198	−0.0299
Asset growth	−0.0462	−0.0789	−0.0633
Fitted target, year effect	+0.0587	+0.0509	−0.0550

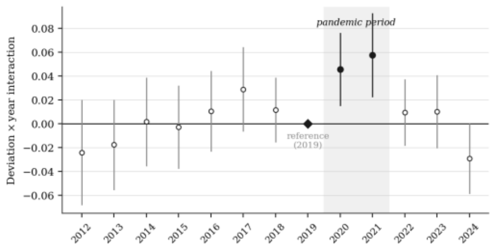
Table 20. Panel E. Inference for the pandemic-period result under alternative assumptions about the level of the shock

Inference procedure	Clusters	Coefficient	SE	t	p	95% set
Analytic, clustered on firm	3,535	0.0484	0.0099	4.90	0.000	—
Analytic, clustered on year	13	0.0484	0.0179	2.70	0.019	—
Analytic, clustered on country	6	0.0484	0.0121	3.98	0.011	—
Analytic, clustered on country-year	78	0.0484	0.0151	3.21	0.002	—
Analytic, two-way on firm and year	—	0.0484	0.0170	2.84	0.005	—
Wild cluster bootstrap, year	13	0.0484	—	2.70	0.153	−0.0486 to 0.1218
Wild cluster bootstrap, country	6	0.0484	—	3.98	0.016	0.0145 to 0.0726
Wild cluster bootstrap, country-year	78	0.0484	—	3.21	0.009	0.0157 to 0.0776
Randomisation over two-year windows	78 windows	0.0484	—	—	0.013	—

Source: authors' estimation using Stata.

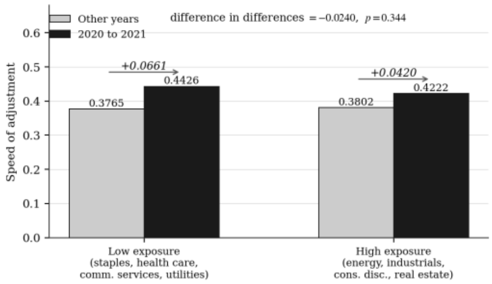
Notes: the sector-exposure comparison in Panel C uses 22,995 firm-years and the placebo rows 29,148. Panel C provides the within-period comparison group that a simple period indicator cannot. Both exposure groups accelerate and the difference between them cannot be distinguished from zero, which is consistent with a common policy and macroeconomic environment rather than with differential pandemic exposure. Panel D shows that debt fell faster in 2020 and 2021 than in any adjacent year while the decline in asset growth continued a trend visible from 2013, and that the fitted target itself moved by 0.106 between 2020 and 2021. Panel E re-estimates the specification of Panel A, row (5) under nine inference procedures; all rows share the same point estimate and only the standard error and the p-value change. Every row of Panel E is estimated with firm effects absorbed rather than differenced out, so its firm-clustered standard error of 0.0099 differs from the 0.0093 reported elsewhere through a degrees-of-freedom adjustment. Bootstrap rows use Webb six-point weights with 9,999 replications and the null imposed. The randomisation test reassigns the pandemic label to each of the seventy-eight two-year windows available in the estimation sample, holding the year effects in place so that only the interaction with the deviation is estimated; the reported p-value is the proportion of windows whose coefficient is at least as large in absolute value as the actual one. The deviation approach uses 31,692 firm-year observations from 3,535 firms, fewer than the 3,816 firms of the final estimation sample, because the deviation is defined only where the target can be constructed.

Figure 4. Event study of adjustment speed, 2019 omitted



Source: authors' estimation using Stata. Notes: bars are 95 per cent confidence intervals with standard errors clustered by firm.

Figure 5. Adjustment speed by sector exposure group



Source: authors' estimation using Stata.

4.6 Market leverage remains unresolved

Appendix Table A3 reports the systematic search across twenty-two configurations of the market-leverage model, spanning fourteen lag structures, two transformations, three regressor classifications, and two structural modifications.

Without a control for equity returns the second-order serial correlation test rejects in every configuration, with p-values from 0.040 to 0.053. A failure invariant to fourteen instrument configurations is structural rather than instrumental, and the mechanism is mechanical: market leverage carries market capitalisation in its denominator, and equity valuations have serially correlated dynamics that a first-order partial-adjustment model does not accommodate. Adding the annual equity return as an endogenous regressor confirms this. Holding the instrument set at the third and fourth lags, the second-order serial correlation p-value moves from 0.040 to 0.808 and the Hansen p-value from below 0.001 to 0.128.

The repair creates a second problem. The bounds for this specification are 0.0830 under pooled OLS and 0.3024 under fixed effects, and every configuration satisfying the serial-correlation and overidentification tests produces a speed below the lower bound, while the single configuration inside the bounds fails Hansen at 0.015. No configuration satisfies the Bond (2002) bounding criterion and the diagnostics simultaneously.

We therefore report the market-leverage model as an unresolved robustness check. The direction of partial adjustment is consistent with the book-leverage result, but the magnitude is not comparable, since a specification controlling for the equity return measures active rebalancing net of mechanical valuation movements. The previous version's conclusion that partial adjustment is robust to the choice between book and market leverage is withdrawn.

5. Discussion

5.1 Three sources of variation, three levels of confidence

The results in Section 4 are not equally strong, and the reason is not sample size. The same 32,264 observations support a precise estimate of the speed of adjustment, a well-isolated estimate of a temporal break, and no usable estimate of the two country-level effects. What separates them is the number of independent realisations behind each parameter: 3,816 firms, fifteen years with a sharp and reversible break, and six countries reducing for governance to a single contrast.

5.2 What the firm-level and time variation support

Non-financial firms in the ASEAN-6 close approximately fifteen per cent of the gap to target book leverage each year, so targeting is present and economically meaningful but slow enough that a firm displaced by a large financing decision carries the consequences for years. Placing this estimate in the international literature requires care, because Section 4.3 shows that estimator family moves the number more than country composition does: much of the apparent disagreement about how fast firms rebalance is a disagreement about estimators rather than about firms.

Adjustment was also unusually fast in 2020 and 2021. The temporal isolation of that break is strong: seven pre-pandemic years show no differential adjustment, the pattern reverses by 2022 and turns slightly negative by 2024, and the estimate for 2020 to 2021 is the largest of the seventy-eight two-year windows the sample admits. The evidence is weaker than firm-clustered standard errors alone would suggest, since clustering on year almost doubles the standard error and the wild cluster bootstrap at that level does not reject, but it survives seven of the eight remaining procedures in Table 9, Panel E. The mechanism is visible in the balance sheet, since debt fell relative to lagged assets by two to three percentage points in those years against less than one in 2019, while the contraction in asset growth continued a trend running since 2013.

What the design cannot establish is why. The sector-exposure test was built to address this and returns a null, as do splits by dividend payment and by leverage position, and an acceleration uniform across the dimensions along which pandemic exposure varied most is more consistent with a common environment than with differential exposure to uncertainty. A second caution belongs alongside: the fitted target itself shifted by 0.106 between 2020 and 2021, so part of what the model records as firms converging on targets is targets converging on firms.

5.3 What the country-level variation cannot support

The two hypotheses concerning uncertainty and institutional quality fail, and they fail for different reasons. Uncertainty has a within-country standard deviation of 0.082 against 0.035 between countries, so the design does contain variation from which an effect could be estimated. What it does not deliver is a stable answer: within the dynamic approach the implied direction reverses when a single macroeconomic control is added, the two estimation approaches then point in opposite directions, and no estimate is distinguishable from zero under small-cluster inference. Governance is different in kind, and the way it fails is the more instructive of the two. It attains conventional significance under small-cluster inference, yet removing Singapore triples the coefficient and raises the p-value to 0.506, aggregation to the level at which the country-level variables actually vary reverses its sign, and the crossing of the threshold is traceable to a lending-rate series that determines how many Singaporean observations enter the sample.

Each of these checks is available to any researcher working with a firm-level panel and country-level regressors, yet none is standard practice in this literature. The wild cluster bootstrap corrects the size of a test without adding identifying variation, so a corrected p-value does not by itself establish that a country-level parameter is estimable. When ninety per cent of the variance in a regressor lies between six units and one of those units occupies a range no other approaches, the appropriate description of the resulting estimate is that it is not identified, whatever its p-value. Öztekin & Flannery (2012) draw on thirty-seven country-level observations, so our results do not contradict theirs but show what happens when the same question is asked of six.

5.4 Implications for research on capital structure in emerging markets

The results of this study carry three implications for empirical work that combines firm-level panels with country-level regressors in emerging markets. The first concerns how precision is reported. The same 32,264 observations support a speed of adjustment with a standard error of 0.0203 and a governance interaction whose identifying variation consists of six country means, one of which lies 1.22 above the next. The variance decomposition in Table 8, Panel A that makes this visible requires no data beyond what such a study already holds, so the number of independent realisations behind each parameter can be reported alongside the number of observations at no cost.

The second concerns the reporting of specification choices. Appendix Table A1 shows that the Hansen p-value moves from 0.016 to 0.899 across instrument depths that a reader would regard as equally defensible, while the implied speed of adjustment varies by about three percentage points among the configurations that satisfy the diagnostics. A single preferred configuration therefore conveys less about the reliability of a dynamic panel estimate than the grid from which it was drawn.

The third concerns disagreement between estimation routes. The deviation and dynamic approaches agree about partial adjustment and about the pandemic period, and they do not agree about the country-level interactions, where the implied direction depends on which macroeconomic controls enter. Divergence between two defensible routes to the same parameter is itself evidence about the strength of identification, and it is observable only when both routes are reported.

6. Conclusion and Suggestion

This study estimates the speed of book-leverage adjustment on a final estimation sample of 3,816 non-financial listed firms, drawn from an initial panel of 4,689 firms across the six largest ASEAN economies from 2010 to 2024. Two findings are supported, and both refer to book leverage. Firms adjust toward target leverage at approximately fifteen per cent per year, a half-life of about four and a half years, stable across the instrument configurations that pass the diagnostics, four target specifications, six leave-one-country-out samples, and six country subsamples, and satisfying the pooled OLS and fixed-effects bounds throughout. Second, adjustment was four to five percentage points faster in 2020 and 2021, with no differential adjustment in the seven preceding years and a return to the prior pattern by 2022. This second result is qualified in one respect: it survives firm, country, country-year, and two-way clustering, the wild cluster bootstrap at the country and country-year levels, and an exact randomisation test over every two-year window, but not the wild cluster bootstrap clustered on year, a procedure with little power when only two of thirteen clusters are treated. Section 4.6 reports that no market-leverage specification satisfies the serial-correlation, overidentification, and bounding criteria simultaneously, so we make no claim that the estimated speed generalises across leverage measures.

Two findings are not supported, and the manner of their failure is the study's methodological contribution. The effect of uncertainty on the speed of adjustment is not estimated with a stable direction, and its moderation by institutional quality is not identified. The distinction is deliberate. Uncertainty varies over time within each economy, so the design supplies usable variation and the failure lies in the answer: the implied direction reverses across macroeconomic control sets, and the two estimation approaches then point in opposite directions. The institutional moderation term attains conventional significance under small-cluster inference, yet loses it when Singapore is excluded, reverses sign under aggregation, and crosses the threshold only because one macroeconomic series determines how many Singaporean observations enter the sample. The identifying variation is a single contrast, and no adjustment to standard errors can change that.

The implications are confined to the two supported findings. For managers, the relevant fact is the pace rather than the existence of targeting: a half-life of four and a half years means a financing decision moving leverage well away from target remains visible in the balance sheet several years later, so the cost of a large deviation should be assessed over that horizon rather than the following year. The pandemic evidence adds that firms in these markets retained the capacity to rebalance under adverse conditions, and did so by reducing debt.

For researchers, the implication is a caution about how country-level results in regional finance studies should be weighed. Effects of macroeconomic and institutional variables are routinely reported from firm-level panels with confidence appropriate to the number of firms rather than of countries, and this study shows what happens to two such effects when that distinction is enforced. We deliberately do not draw the implications a positive uncertainty result would have supported: we do not conclude that firms should maintain leverage management when uncertainty rises, and we do not recommend that regulators reduce financing frictions, because those frictions are neither measured nor estimated here.

7. Limitations and Future Research

Seven limitations qualify the results. First, the country-level effects rest on six clusters, for the reasons set out in Section 5.3. Second, the deviation approach uses a generated regressor, and although Pagan (1984) shows the problem is one of inference rather than consistency, it is not fully resolved. Third, the market-leverage specification remains unresolved across twenty-two configurations, so the book-leverage result should not be described as robust to the choice of leverage measure. Fourth, the pandemic analysis has no untreated comparison group, and the sector-exposure test is a partial substitute that returns a null. Fifth, the target itself moved sharply in 2021, so part of the measured convergence that year is a moving target. Sixth, leverage is bounded between zero and one while the linear specification does not impose that bound, research and development expenditure is omitted from the target equation, and uncertainty is captured by a single text-based index. Seventh, the pandemic result is identified from a split defined at the year level, and although it survives eight of the nine inference procedures in Table 9, Panel E, thirteen years is a small number of independent time periods, so the caution the paper applies to six countries applies here in weaker form.

The most direct response is to move the source of variation. Uncertainty can be measured at the firm level, through earnings volatility, the dispersion of analyst forecasts, or exposure to volatile input and output markets, and such measures vary across thousands of firms rather than six countries, so a study designed around firm-level uncertainty exposure would test the same mechanism with identifying variation the present design lacks. Institutional quality is harder, because it is genuinely a country-level construct, but discrete within-country reforms offer a route that cross-sectional governance scores do not. Extending the country set helps only if it raises the number of independent realisations substantially.

Declaration of AI and AI-assisted technologies in the writing

During the preparation of this manuscript, the authors used AI-assisted technology to support language refinement, improve sentence clarity, enhance academic coherence, and organize several sections of the manuscript. The AI tool was used only as a writing assistance tool and did not replace the authors' intellectual contribution, research design, data collection, data analysis, interpretation of findings, or final scholarly judgment. All AI-assisted outputs were critically reviewed, revised, verified, and approved by the authors to ensure accuracy, originality, and alignment with the objectives of the study. The authors take full responsibility for the content, validity, and integrity of the final manuscript.

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Appendix

Table A1. Full lag grid, book leverage, forward orthogonal deviations

Lags	Instruments	Hansen df	Hansen p	Sargan p	AR(2) p	SOA
1 to 2	21	2	0.000	0.000	0.413	0.1564
1 to 3	22	3	0.000	0.000	0.410	0.1592
2 to 2	20	1	0.665	0.568	0.414	0.1745
2 to 3	21	2	0.016	0.000	0.412	0.1522
2 to 4	22	3	0.019	0.000	0.412	0.1526
2 to 5	23	4	0.039	0.000	0.412	0.1521
3 to 3	20	1	0.495	0.369	0.407	0.1508
3 to 4 (preferred)	**21**	**2**	**0.716**	**0.579**	**0.408**	**0.1455**
3 to 5	22	3	0.841	0.705	0.408	0.1453
3 to 6	23	4	0.899	0.775	0.408	0.1446
4 to 4	20	1	0.024	0.010	0.421	0.0721
4 to 5	21	2	0.052	0.026	0.424	0.0598
4 to 6	22	3	0.107	0.046	0.424	0.0603
5 to 6	21	2	0.801	0.761	0.409	0.1450

Source: authors' estimation using Stata. Notes: the lag-two family collapses as soon as a single overidentifying restriction is added, moving from 0.665 at one degree of freedom to 0.016 at two, with Sargan rejecting throughout. The lag-three family is monotonically consistent, with Hansen rising from 0.495 to 0.899 and the implied speed confined between 0.145 and 0.151. Configurations beginning at the fourth lag produce coefficients on lagged leverage above the pooled OLS bound of 0.900 and are reported but not used. The second-order serial correlation test does not reject in any of the fourteen configurations.

Table A2. Regressor classification tests

Specification	Transformation	Lags	Profitability and market-to-book	Inst.	Hansen df	Hansen p	SOA
A	FD	1 to 2	strictly exogenous	21	2	0.007	0.1569
B	FD	1 to 2	profitability predetermined	23	4	0.000	0.1762
C	FD	1 to 2	both predetermined	25	6	0.000	0.1743
D	FOD	2 to 2	both predetermined	22	3	0.175	0.1709
E	FOD	2 to 3	both predetermined	25	6	0.001	0.1563
F	FOD	2 to 4	both predetermined	28	9	0.002	0.1585
Preferred	**FOD**	**3 to 4**	**strictly exogenous**	**21**	**2**	**0.716**	**0.1455**

Source: authors' estimation using Stata. Notes: treating profitability and the market-to-book ratio as predetermined and instrumenting them with their own lags does not improve instrument validity, since the Hansen test rejects in five of the six alternative specifications. The implied speed ranges from 0.146 to 0.176 across all seven specifications. The strictly exogenous classification is retained because the alternatives perform worse, not because the assumption was left unexamined.

Table A3. Market leverage: a systematic search

Instrument lags	Equity return control	Inst.	Hansen p	Sargan p	AR(2) p	SOA	Within bounds	All criteria met
1 to 2, FD	No	21	0.027	0.002	0.053	0.1223	–	No
2 to 2	No	20	0.026	0.014	0.048	0.1407	–	No
3 to 3	No	20	0.435	0.380	0.042	0.1956	–	No
4 to 4	No	20	0.854	0.849	0.043	0.0966	–	No
2 to 3	Yes, endogenous	21	0.744	0.711	0.416	0.0360	No	No
3 to 4	Yes, endogenous	24	0.128	0.068	0.808	0.0717	No	No
3 to 5	Yes, endogenous	26	0.093	0.026	0.843	0.0798	No	No
3 to 6	Yes, endogenous	28	0.015	0.002	0.807	0.0944	Yes	No
4 to 5	Yes, endogenous	22	0.214	0.105	0.407	0.0702	No	No

Instrument lags	Equity return control	Inst.	Hansen p	Sargan p	AR(2) p	SOA	Within bounds	All criteria met
5 to 6	Yes, endogenous	22	0.661	0.615	0.317	0.0454	No	No

Source: authors' estimation using Stata. Notes: bounds for the equity-return specification are 0.0830 under pooled OLS and 0.3024 under fixed effects. All configurations use forward orthogonal deviations unless marked FD. Twenty-two configurations were examined and ten representative ones are tabulated; those treating the equity return as strictly exogenous are omitted because they fail Hansen below 0.001.