Research Square

Preprints are preliminary reports that have not undergone peer review.
They should not be considered conclusive, used to inform clinical practice,
or referenced by the media as validated information.

Cause-Specific Smooth Transition Hazards for Stock Exchange Delisting: Estimation, Calibrated Testing, and an Application to Bursa Malaysia

Ahmad Shauqi bin Haji Mohamad Zubir

shauqi@umt.edu.my

Universiti Malaysia Terengganu

Research Article

Keywords: Survival analysis, Competing risks, Smooth transition, Delisting, Lomax distribution, Calibrated test

Posted Date: August 25th, 2026

DOI: https://doi.org/10.21203/rs.3.rs-10784308/v1

License: This work is licensed under a Creative Commons Attribution 4.0 International License.
Read Full License

Additional Declarations: No competing interests reported.

Abstract

Firms leave a stock exchange for two main reasons. First, voluntary transactions such as takeovers, or second, involuntary removal following financial distress. We study these exits separately within a common cause-specific hazard framework. The model allows coefficients to change gradually over time through a logistic transition whose timing is estimated from the data. It can use Weibull, Gompertz, log-logistic, or Beta Transformed Lomax baselines. We extend the last family to censored regression data. The application covers 116 exits among 836 Main and ACE Market firms on Bursa Malaysia from 2016 to 2025. Each exit is classified from the firm's full announcement record. The results show two distinct processes. Involuntary removal is more likely for small, low-priced, thinly traded firms. Its estimated hazard changes around December 2024. Overall removal risk rises, and leverage becomes an important risk factor. Voluntary exits occur among adequately priced, performing firms that had thin trading seven months earlier, a pattern more consistent with investor neglect than financial distress. The simulation study produces two important warnings. First, when firms share a common entry date, duration dependence and gradual regime change are difficult to distinguish. In about 90% of replications, a distorted solution has a higher likelihood than the true parameters. Second, the standard chi-squared transition test rejects a true null 11.9% of the time when the nominal level is 5%.

1. Introduction

In 2025, Bursa Malaysia involuntarily removed nine listed companies. This was the highest annual number in a decade and more than four times the 2024 total. During most of the previous ten years, voluntary exits were more common as about seven to ten firms per year left through takeovers or selective capital reductions, whereas forced removals were rare. This pattern changed when voluntary privatisations declined and pandemic-era relief for financially distressed firms ended. The mix of exits then shifted towards involuntary removals. A single delisting is a corporate event, but 116 exits among 836 firms over a decade form a duration dataset. Once the exits are classified by cause and month, the data raise questions that standard survival models do not handle well.

Three issues make the analysis difficult. First, delisting is not a single type of event. Pooling takeovers with removals for financial distress can average opposite relationships and produce coefficients that represent neither process. We therefore use cause-specific competing-risks models [10] and classify each exit from primary records. Second, the sample covers a pandemic, temporary regulatory relief, and the later withdrawal of that relief, so coefficient stability cannot be assumed. Smooth-transition models allow parameters to move gradually between regimes at an estimated time [19]. Jonéus and Lyhagen [9] recently applied this idea to hazard regression, but only with a Weibull baseline and without addressing the structure of a listing panel. Third, standard inference is unreliable. When no transition exists, the transition's timing and speed are not identified [5]. In addition, if most firms enter the risk set on the same date, listing age and calendar time become nearly collinear. A model may then mistake duration dependence for a slow change over calendar time. Standard critical values do not account for either problem.

This study develops a framework that addresses these issues and applies it to Bursa Malaysia. We use a cause-specific proportional hazard model in which coefficients may change through a logistic transition at an estimated date. The baseline hazard can be Weibull, Gompertz, log-logistic, or Beta Transformed Lomax. Kottakkunnan [11] introduced the last family for complete lifetime samples and we extend it to censored regression data with staggered entry by expressing the likelihood through the integrated hazard. This application also returns the Lomax family to its original subject. Lomax [12] developed the parent distribution to study business failures. We estimate the model by profiling over the transition parameters. We test for a transition using simulations under the fitted null model, applying the same search procedure to the observed and simulated data. A Monte Carlo study evaluates the method before the empirical application. With dispersed entry dates, the estimator recovers the parameters well. With a common entry date, however, a distorted ridge solution outperforms the true parameters in about 90% of replications. The standard chi-squared transition test also rejects a true null 11.9% of the time at a nominal 5% level.

Empirically, the two exit types have different predictors. Involuntary removal follows a familiar distress profile [3, 18], which are lower size, lower share price, and more non-trading all increase the hazard. Voluntary exit is associated with adequately priced firms, positive momentum, and thin trading seven months before exit. This pattern suggests investor neglect rather than financial failure [17]. Together, these exits resemble the listing gap documented for the United States by Doidge et al. [7]. The involuntary model also identifies a change around December 2024. The overall hazard rises, and leverage changes from an unimportant factor to a strong risk factor. Although both components are individually significant, the joint six-parameter test illustrates why calibration matters. The standard chi-squared approximation gives $p = 0.073$, whereas the calibrated test gives $p = 0.173$. We therefore describe the shift as economically meaningful and statistically suggestive, not statistically significant.

This paper makes three contributions. First, it develops a cause-specific smooth-transition hazard model with interchangeable baseline families. The framework includes the first censored-regression application of the Beta Transformed Lomax and combines profile estimation with a simulation-calibrated test. Second, the simulations identify a practical condition for estimating smooth transitions in duration panels, that is, entry dates must be sufficiently dispersed. They also quantify the size distortion of the standard transition test. These findings apply to other common-origin cohorts, including bank charters, loan vintages, and startup generations. Third, the paper provides the first cause-classified analysis of delisting on an ASEAN exchange.

2. Literature review and hypotheses development

2.1 Two ways to leave a stock exchange

Firms leave stock exchanges for different reasons, yet some studies treat all exits as one event. One literature examines corporate failure. Shumway [18] showed that duration models with time-varying covariates can outperform static classifiers based on accounting ratios. Campbell et al. [3] found that

low prices, small size, and thin trading predict financial distress a year or more ahead. A second literature studies voluntary exits, particularly going-private transactions. These firms leave because an acquirer or controlling shareholder chooses to take them private, not because the exchange removes them. Going-private transactions are common when the benefits of public visibility no longer justify listing costs [17], when a majority owner buys out minority shareholders [6], or when concentrated ownership reduces the need for public equity [1]. Macey et al. [14] describe the suspension and removal process, while Doidge et al. [7] show how changes in these exit flows contributed to the decline in U.S. listings.

Because the two exit types are economically different, pooling them can mislead. A variable may increase one hazard but decrease the other, so the pooled coefficient describes neither process well. Cause-specific competing-risks analysis is the standard solution [10], but it has rarely been applied to exchange delisting and, to our knowledge, not to Malaysia. Bursa Malaysia also has a formal distress regime that should be modelled explicitly. Main Market firms that trigger Practice Note 17 and ACE Market firms that trigger Guidance Note 3 must regularise their condition within a specified period or face suspension and removal. Paragraph 8.03A separately covers firms with inadequate operations. These involuntary exits leave a regulatory record that differs clearly from the shareholder circulars and takeover documents associated with voluntary withdrawals. Section 3 uses this difference to classify each exit from company announcements.

2.2 When covariate effects refuse to stay constant

Most duration models assume that regression coefficients remain constant over calendar time. This assumption is doubtful for a sample that includes the pandemic, temporary regulatory relief, and the withdrawal of that relief. Time-series researchers address comparable changes with smooth-transition models, in which parameters move between regimes along a logistic path. The data determine both the timing and speed of the change [19], and linearity can be tested against a transition alternative [13]. Jonéus and Lyhagen [9] recently adapted this approach to survival analysis by allowing coefficients in a Weibull hazard model to change smoothly across unknown regimes.

We extend their framework in three ways required by listing data. First, we allow several interchangeable baseline families rather than fixing the Weibull. Section 2.3 explains why. Second, our likelihood directly accommodates right censoring, staggered entry, and monthly changing covariates. Third, we modify the inference procedure. Under the null of no transition, the transition's location and speed are unidentified, so standard asymptotic theory fails [5]. Listing panels create an additional identification problem. When most firms enter the risk set at the same time, listing age and calendar time are nearly collinear, allowing duration dependence to mimic a gradual calendar-time change. Our simulations show that both problems materially affect results. Standard chi-squared critical values reject too often, whereas a null distribution simulated under the fitted model restores the intended test size. To our knowledge, neither problem has previously been documented for smooth-transition hazards in financial listing data.

2.3 The baseline family, from Gompertz to a transformed Lomax

A parametric hazard model requires a baseline distribution. Common choices include the Gompertz [8] and Weibull [20] distributions. Cox [4] offers a semiparametric alternative that does not specify the baseline shape. A fully parametric model is useful here because its complete likelihood supports the simulation-based inference in Section 2.2 and provides an interpretable form of duration dependence. The risk is misspecifying the baseline shape. Recent distribution research addresses this risk by adding parameters to simple families, producing more flexible hazard shapes. Marshall and Olkin [16] proposed an influential one-parameter extension, and Mahdavi and Kundu [15] developed a related exponential-type transformation. Kottakkunnan's (2026) Beta Transformed Lomax adds a shape parameter that allows increasing, decreasing, and unimodal densities and fits some lifetime datasets better than competing Lomax extensions.

However, neither literature yet provides the complete method needed here. The Beta Transformed Lomax has been studied only for complete samples, without censoring, covariates, or a regression structure. Conversely, the smooth-transition hazard model of Jonéus and Lyhagen [9] includes regression but uses only a Weibull baseline. We combine these ideas by placing interchangeable baseline families inside a smooth-transition hazard regression and writing a single likelihood for censored and left-truncated data. This allows us to test whether the substantive results change when the baseline switches among Weibull, Gompertz, log-logistic, and Beta Transformed Lomax. If the estimates remain stable, the conclusions are not artefacts of one parametric choice.

2.4 Hypotheses

The preceding discussion leads to four testable hypotheses. Section 2.1 suggests that the two exit routes should depend on different firm characteristics. Involuntary removal should be associated with financial distress, whereas voluntary withdrawal should be associated with viable firms that receive little investor attention.

H1: Involuntary and voluntary delisting have different predictors. Involuntary exit is more likely among small, low-priced, thinly traded, and highly leveraged firms. Voluntary exit is more likely among adequately priced firms with non-negative momentum but thin trading.

The second hypothesis follows from Bursa Malaysia's institutional history. Pandemic-era relief delayed the classification of distressed firms. When the relief ended, previously delayed removals may have occurred within a short period. If so, the involuntary hazard should change late in the sample, while the voluntary hazard should remain more stable.

H2: The level of the exit hazard and the effects of firm characteristics change during the sample period, with a late-sample shift concentrated in involuntary delisting.

The third hypothesis concerns inference. Under the null, the transition parameters are unidentified, and the panel's entry pattern can weaken identification further. The standard likelihood ratio test should therefore reject a true null more often than its nominal level implies.

H3: In listing-survival panels, the chi-squared approximation makes the transition test reject too often. Simulating the null distribution under the fitted no-transition model corrects this distortion.

The fourth hypothesis concerns the baseline distribution. If the covariates and transition structure contain the main economic information, changing the baseline family should not materially change the estimates.

H4: Within sampling error, the covariate effects and transition timing are stable across Weibull, Gompertz, log-logistic, and Beta Transformed Lomax baselines.

Joint support for these hypotheses would show that Bursa Malaysia delisting consists of two distinct processes, one of which changed over time. It would also provide applied researchers with a flexible estimation method and a more reliable transition test.

3. Data and methodology

3.1 Sample, exits, and a classification protocol built on announcements

The unit of observation is the firm-month. We build a monthly panel of all ordinary shares listed on Bursa Malaysia's Main and ACE Markets between January 2016 and December 2025. Prices, trading activity, and balance-sheet variables come from LSEG Datastream and Worldscope. We clean the initial set of 902 listed securities in two stages. First, we exclude the LEAP Market because it has a lighter disclosure regime, a qualified-investor base, and different transfer and withdrawal rules. Its exits therefore arise from a different process. Second, we remove convertible preference shares and other non-ordinary instruments that the source records as equities. We also remove one firm whose announcement record was mixed with another issuer's record after a stock code was reused. These exclusions remove 66 securities. The final sample contains 836 firms and 89,046 firm-months (Table A1, Supplementary Appendix).

We obtain exit dates and causes from primary records. For each firm that left the exchange, we retrieve its complete announcement history from the Bursa Malaysia portal [2] and classify the exit from those documents. Involuntary markers include Practice Note 17 or Guidance Note 3 classification, regularisation deadlines, paragraph 8.03A notices of inadequate operations, winding-up petitions, receivership or liquidation, and suspension followed by removal. Voluntary markers include takeover offers, compulsory acquisition notices, selective capital reductions, schemes of arrangement, and requests to withdraw the listing. A keyword screen gives each firm an initial label, with more weight placed on documents near the delisting date. We then review every label against the firm's full record.

Ambiguous cases are resolved by reading the complete documents rather than applying an automatic rule.

The protocol identifies 116 exits in the cleaned sample (Table A2, Supplementary Appendix). Of these, 108 occur within the estimation window. 62 are voluntary and 46 are involuntary. The remaining eight exits were completed in early 2026, after the panel ends, so we treat those firms as censored in December 2025. Voluntary exits consist mainly of 38 takeovers and 23 selective capital reductions, plus one court-supervised restructuring that ended in a negotiated withdrawal. Involuntary exits include 36 removals under Practice Note 17 or Guidance Note 3, eight removals following winding-up or a direct removal order, and two special purpose acquisition companies that failed to complete a qualifying acquisition on time. Figure 1 shows the annual pattern. Voluntary exits dominate until 2021 and then decline, while involuntary exits rise to nine in 2025, the highest annual total in the sample.

3.2 Covariates

Table 1 defines the covariates. We use a compact set drawn from the distress and going-private literatures. Size and price capture firm value, momentum captures recent returns, the share of zero-return trading days measures market neglect, average turnover provides a related volume measure, and leverage captures balance-sheet risk. We also include two control indicators. The first marks firm-months without annual accounting data in Worldscope, treating missing coverage as information rather than dropping the observation. The second marks firms listed after January 2016, for which the panel observes the full listed life. We standardise all continuous covariates to a mean of zero and a standard deviation of one within the estimation sample. The coefficients can therefore be compared as the effect of a one-standard-deviation change.

Table 1
Covariate definitions.

Variable	Definition	Timing
Size	Natural logarithm of market capitalisation	t − 1
Price	Natural logarithm of the month-end share price	t − 1
Momentum	Cumulative return over months t − 12 to t − 2	t − 2
Zero-return share	Fraction of trading days in the month with a zero return	t − 1 (involuntary); t − 7 (voluntary)
Turnover	Natural logarithm of average daily turnover in the month	t − 1
Leverage	Total debt over total assets, most recent annual report	t − 1
No accounting coverage	Indicator: no Worldscope annual accounts available	t − 1
Listed after January 2016	Indicator: listing date inside the sample window	
Transition variable, s	Calendar month rescaled to [0, 1], January 2016 to December 2025	current month

The timing of the covariates is important for identification. In the involuntary model, covariates are lagged by one month, so they represent the latest information available before a possible removal. In the voluntary model, the zero-return share is measured seven months before the event rather than one month before it. This longer lag avoids a mechanical relationship. Takeover and going-private processes often include an offer period or suspension that itself reduces trading. Seven months generally places the measure before that process begins. Section 5.4 applies the same longer lag to the involuntary model as a robustness test. The transition variable s measures calendar time on a scale from 0 to 1, where January 2016 is 0 and December 2025 is 1. All firms share the same value of s in a given month.

3.3 A cause-specific hazard with a smooth transition and interchangeable baselines

Let a be the listing age of firm i in calendar month m, measured in months since its listing date. Each firm is observed from panel entry until delisting or censoring, and each firm-month contributes one record. A firm can exit voluntarily or involuntarily. Each cause has its own hazard, which is the conditional risk of that exit at a given listing age. When a firm exits through one cause, it is treated as censored for the other cause, as in the standard cause-specific approach [10]. For cause k, the model is

$$\lambda_k(a \mid x_{im}, s_m) = \lambda_{0k}(a) \exp\{x_{im}'\beta_k + G(s_m; \gamma_k, c_k)\, \tilde{x}_{im}'\psi_k\} \quad (1)$$

Here, x contains the covariates in Table 1. A selected subset of x contains only the covariates whose effects may change over time. The shift vector ψ measures the size and direction of those changes. G is the logistic transition function proposed by Teräsvirta [19]:

$G(s; \gamma, c) = [1 + \exp\{-\gamma(s - c)\}]^{-1}, \gamma > 0, c \in (0, 1)$ (2)

The location parameter c identifies the calendar date when the transition is half complete. The speed parameter γ determines how quickly the change occurs. A large γ produces an abrupt break near c, while a small γ produces a gradual change across the sample. When all shift parameters are zero, the model reduces to a constant-coefficient hazard. Following Jonéus and Lyhagen [9], the transition changes the effects of covariates rather than changing time itself. This preserves the baseline hazard as a measure of duration dependence. Including a constant among the changing terms allows the overall hazard level to change as well.

We select the baseline from four families. Each family is expressed through its integrated hazard $H_0(a)$, the quantity required by the likelihood. After absorbing the scale parameter into the regression constant, the Weibull, Gompertz, and log-logistic integrated hazards are

$H_0(a) = a^{\alpha}, H_0(a) = (e^{ba} - 1)/b, H_0(a) = \ln[1 + (a/\lambda)^{\kappa}]$ (3)

These three expressions correspond to the Weibull, Gompertz, and log-logistic families, respectively, with family-specific shape and scale parameters. The fourth option is the Beta Transformed Lomax introduced by Kottakkunnan [11]. It extends the Lomax distribution originally developed for business failure data [12], following the broader parameter-extension approach of Marshall and Olkin [16] and Mahdavi and Kundu [15]. Let F denote the Lomax distribution function with shape κ and scale λ. The transformed distribution and its integrated hazard are

$F_{BTL}(a) = [\beta/(\beta - 1)][1 - \beta^{-F(a)}], F(a) = 1 - (1 + a/\lambda)^{-\kappa}$ (4)

The transformation parameter β satisfies $\beta > 0$ and $\beta \neq 1$. The integrated hazard is the negative logarithm of the corresponding survival function. Previous work developed this family for complete samples. Once its integrated hazard is available, however, it can be inserted directly into the likelihood in Section 3.4. The same likelihood then handles censoring, staggered entry, and covariates without a separate derivation. To our knowledge, this is the first censored-regression application of the family. The main methodological point is that expressing models through the integrated hazard makes baseline families interchangeable.

3.4 Estimation, calibrated inference, and what tests what

Each firm-month contributes the change in the integrated hazard during that month. Let η be the linear predictor for one exit cause, including both the constant and changing coefficient terms in Eq. (1). Let d equal 1 when the firm exits through that cause in month m and 0 otherwise. The cause-specific log likelihood is

$$\ell(\theta) = \Sigma_{i,m} \{ d_{im}[\ln \lambda_0(a_{im}) + \eta_{im}] - \exp\{\eta_{im}\}[H_0(a_{im}) - H_0(a_{im} - 1)] \} \quad (5)$$

Right-censored observations contribute only the survival term. Firms that were already listed in January 2016 enter the sample after their listing began and contribute hazard increments from their current listing age. This treatment accounts for left truncation. Covariates update each month. We maximise the likelihood separately for each exit cause, using a separate parameter set for each model.

We estimate the transition by profiling over its speed and location (γ, c), which are often weakly identified. The coarse search evaluates c at 0.15, 0.55, and 0.90 with γ = 16. Each fit starts from both the previous solution and the no-transition estimates. A fine search then evaluates locations within 0.12 of the best coarse value and γ in {8, 32, 64}. At every grid point, a quasi-Newton algorithm maximises the remaining parameters. We calculate standard errors for the regression coefficients from a finite-difference Hessian at the profiled optimum while holding γ and c fixed. These conditional standard errors are used only for individual coefficients, not for testing the transition as a whole.

Testing the joint null that all transition shifts are zero requires special treatment. Under this null, γ and c are not identified, so the likelihood ratio statistic does not follow the usual chi-squared distribution [5]. The score-based approximation of Luukkonen et al. [13] is also difficult to apply to a hazard model with staggered entry and censoring. Section 4 shows that the resulting distortion is large. We therefore simulate the null distribution under the fitted no-transition model. For each replication, we keep every firm's covariate history, entry month, and censoring time unchanged, but redraw the exit indicator. We then refit the transition and no-transition models using the same search grid applied to the observed data. The calibrated p-value equals one plus the number of simulated likelihood ratios at least as large as the observed ratio, divided by one plus the number of replications. Matching the search procedures is essential because a broader profile search tends to produce larger test statistics.

The method connects directly to the four hypotheses. H1 compares the cause-specific coefficient estimates from Eq. (5). H2 concerns the shift parameters and the transition timing in Eq. (2), especially in the involuntary model. H3 concerns the size of the transition test and is examined through the simulations in Section 4. H4 compares estimates after changing only the baseline family in equations (3) and (4), while keeping the covariates, transition specification, and search procedure fixed.

4. Simulation study

We conduct three simulation experiments before applying the model to the Bursa Malaysia data. The first asks whether the estimator works with the staggered entry pattern observed in the sample. The second shows how the estimator fails when all subjects enter together and identifies a warning sign that can be recognised in empirical results. The third measures how often the standard transition test rejects a true null and provides calibrated critical values for the application. All experiments use a Weibull baseline and the search procedure described in Section 3.4, so they evaluate the same method later used for the data.

4.1 Design

Each simulated panel contains 900 firms observed monthly for up to 120 months and one standardised covariate per firm. The hazard follows equations (1) to (3), with α = 1.2, an intercept of -7, a covariate effect of 0.4, a level shift of 1, a slope shift of 0.5, c = 0.75, and γ = 12. The transition is moderate and centred about three quarters of the way through the sample. It produces roughly 300 exits per replication, similar in scale to the empirical data. Entry timing is the experimental treatment. In the staggered-entry design, 60% of firms are present in month one and the remainder enter uniformly over the next eight years, approximating the Bursa Malaysia panel. In the cohort-entry design, every firm enters in month one, as in a loan vintage or a wave of new charters. The search grid includes γ values of 8, 16, 32, and 64, but not the true value of 12. The method must therefore recover the transition without selecting the exact true speed from the grid.

4.2 Recovery under staggered entry

The left side of Table 2 summarises 224 staggered-entry replications. The estimates show little bias. The mean shape parameter is 1.24 compared with the true value of 1.2. The covariate effect is 0.39 compared with 0.4, the level shift is 1.16 compared with 1, the slope shift is 0.59 compared with 0.5, and the location is 0.78 compared with 0.75. Its median of 0.82 mainly reflects the spacing of the fine grid. The speed parameter is recovered only approximately, with its median at the grid point below the true value. This is a common and relatively harmless form of weak identification. Nearby combinations of speed and location can trace similar transition paths. Overall, the model performs well when entry dates are dispersed.

Table 2
Simulation experiments 1 and 2: parameter recovery under staggered and cohort entry.

		Staggered entry			**Cohort entry**		
Parameter	**True**	**Mean**	**SD**	**Median**	**Mean**	**SD**	**Median**
Shape α	1.2	1.236	0.096	1.227	2.106	1.036	1.397
Intercept β_0	−7.0	−7.202	0.383	−7.189	−7.594	0.757	−7.480
Covariate effect β_1	0.4	0.391	0.111	0.396	0.219	0.337	0.352
Level shift ψ_0	1.0	1.161	0.348	1.157	−3.019	5.123	0.744
Slope shift ψ_1	0.5	0.594	0.242	0.585	0.674	0.359	0.655
Location c	0.75	0.775	0.096	0.820	0.503	0.371	0.725
Replications		224			250		
Mean events		289.8			329.7		

4.3 The common-entry ridge

The cohort-entry results are very different. When every firm enters in the same month, listing age and calendar time are the same variable apart from scale. Duration dependence and a slow transition can then explain the same increase in the observed hazard. This creates a ridge in the likelihood surface along which the two effects trade off. Table 2 shows the result. The estimated shape parameter averages 2.11 instead of the true 1.2, with a standard deviation near one. The level shift averages − 3.02, with a standard deviation above five. The estimated location is spread across the unit interval, and 42% of replications end at a grid boundary. This is not an optimisation failure. In 89.2% of replications, the distorted estimate has a higher likelihood than the true parameter vector evaluated on the same data. The sample itself prefers the ridge solution, so better numerical searching cannot solve the problem.

This failure has a recognisable empirical signature. An implausible duration parameter combined with an extreme or boundary transition estimate. Section 5.2 finds this pattern in an exploratory voluntary-exit model. The main design lesson is that smooth calendar-time transitions in duration models require dispersed entry dates. Firms of different listing ages must be observed at the same calendar date to separate age effects from time effects. Panels built from a single starting cohort, such as bank-charter waves, loan vintages, or startup generations, cannot estimate gradual calendar transitions reliably from the cohort alone.

4.4 The size of the naive test and the calibrated alternative

The third experiment simulates data under the no-transition model fitted to the involuntary sample. We keep every observed covariate path, entry month, and censoring time, redraw only the exit indicators, and fit both models to each of 201 replications using the same search procedure as the empirical analysis. The simulated likelihood ratio has a median of 7.8, compared with 5.3 under a chi-squared distribution with six degrees of freedom. Its 95th percentile is 14.9 rather than 12.6, and its maximum is 21.0. Using the standard 12.6 critical value rejects a true null in 11.9% of replications, more than twice the intended 5% rate. This distortion is the practical effect of the Davies (1987) problem, in which the search maximises over parameters that the null does not identify. The calibrated test uses the simulated distribution and raises the effective 5% critical value to about 14.9.

5. Results

5.1 Two doors, two firms

Table 3 reports the final estimates. The involuntary model includes a smooth transition, while the voluntary model has constant coefficients for the reason explained in Section 5.2. The involuntary estimation sample contains 87,493 firm-months and 45 events. The voluntary sample requires seven months of covariate history and contains 81,393 firm-months and 55 events.

Table 3
Cause-specific hazard estimates, Weibull baseline.

Covariate	Coefficient	SE	z
Panel A. Involuntary hazard: base regime			
ln α	−0.316	0.232	−1.36
Constant	−10.462	1.089	−9.61
Size	−1.020	0.281	−3.63
Price	−1.059	0.193	−5.48
Momentum	0.021	0.310	0.07
Zero-return share (t − 1)	1.800	0.429	4.19
Leverage	0.005	0.031	0.15
No accounting coverage	−0.612	0.651	−0.94
Listed after January 2016	−1.491	1.181	−1.26
Panel A, continued. Transition shifts ψ (c = December 2024, γ = 16)			
Level	3.882	1.476	2.63
Size	0.226	0.825	0.27
Zero-return share	−1.757	0.996	−1.76
Leverage	0.804	0.326	2.47
Panel B. Voluntary hazard: no transition			
ln α	−0.240	0.230	−1.04
Constant	−6.480	0.934	−6.93
Size	0.311	0.206	1.51
Price	0.481	0.184	2.62
Momentum	0.065	0.025	2.61
Zero-return share (t − 7)	0.808	0.162	4.98
Turnover	0.121	0.077	1.56
Leverage	−0.040	0.159	−0.25
No accounting coverage	0.433	1.092	0.40

The involuntary results match the distress profile reported by Shumway [18] and Campbell et al. [3]. A firm that is one standard deviation smaller than average has a log hazard 1.02 points higher. A one-

standard-deviation decrease in log price adds 1.06 points, and a one-standard-deviation increase in the share of zero-return days adds 1.80 points, the largest effect in the model. All three estimates are precise. Momentum adds little after size and price are controlled as its coefficient is 0.02 with a z-statistic of 0.07. This suggests that involuntary removal depends on a firm's current price and trading position, not the speed at which it reached that position. In the early regime, leverage also has almost no effect, with a coefficient of 0.005 and a z-statistic of 0.15. Section 5.2 shows that this changes later. The Weibull shape below one indicates that, after controlling for observed characteristics, firms that have remained listed longer face a lower baseline hazard.

Voluntary exit has a different set of predictors. The strongest is the share of zero-return days measured seven months before exit. Its coefficient is 0.81, with a z-statistic close to five. Firms therefore become thinly traded well before a bid appears, consistent with the investor-neglect mechanism described by Mehran and Peristiani [17]. Price has a positive coefficient of 0.48, and momentum has a positive coefficient of 0.07 with a z-statistic of 2.6. Firms that exit voluntarily are therefore adequately priced and performing rather than failing. Size is positive but estimated imprecisely, while leverage and accounting coverage have little effect. Together, the two panels support H1. The two exit types have nearly separate predictors. Thin trading matters for both, but at different horizons and for different types of firms. Pooling the exits would average a size effect of -1.02 with an effect of + 0.31 and produce a coefficient that represents neither group well.

5.2 The December 2024 shift, tested honestly

The involuntary transition is centred at c = 0.90, which corresponds to December 2024. Its estimated speed, γ = 16, implies that the move from the early to the late regime occurs mainly during the final year of the sample. Table 3 shows three important changes. First, the overall hazard increases by 3.88 log points when the transition is complete (z = 2.63), consistent with the 2025 increase in Fig. 1. Second, leverage changes from almost irrelevant to an important risk factor. Its effect rises by 0.80 (z = 2.47), so one standard deviation of additional leverage multiplies the late-regime removal hazard by about 2.2. Third, the effect of the zero-return share falls by 1.76 (z = -1.76), leaving little effect in the late regime. These estimates describe a change in removals after pandemic-era relief ended. The later removals were more strongly associated with balance-sheet weakness and less strongly associated with trading inactivity.

The joint evidence for this shift is weaker than the individual coefficients suggest. The likelihood ratio for the six shift parameters is 11.53. A standard chi-squared test with six degrees of freedom gives p = 0.073, which would be significant at the 10% level often used in applied research. Section 4.4 shows that this test rejects a true null more than twice as often as intended in this design. The calibrated result in Table 4 is therefore more reliable. 34 of 201 null replications have a likelihood ratio at least as large as 11.53, giving p = 0.173. The higher hazard level, stronger leverage effect, and timing remain economically coherent. However, the transition does not pass a correctly sized 5% test. We therefore describe it as economically meaningful and statistically suggestive rather than statistically significant. This result demonstrates the practical importance of H3.

Table 4
Inference on the involuntary transition: naive and calibrated.

Quantity	Simulated null	$\chi^2(6)$ reference
Observed likelihood ratio	11.53	11.53
Median	7.8	5.3
90th percentile	13.1	10.6
95th percentile	14.9	12.6
Maximum	21.0	
Replications	201	
Replications with ratio ≥ observed	34	
p-value	0.173	0.073
Empirical size of nominal five percent test		0.119

We find no evidence of a transition in the voluntary model. The profile search places the location at the lower grid boundary, c = 0.15, and the likelihood ratio is only 3.34. The information criterion also favours the constant-coefficient model. An earlier exploratory model appeared to identify a voluntary transition in 2018, together with a Weibull shape parameter near three. This combination matches the common-entry ridge described in Section 4.3. We therefore follow the simulation evidence and report the voluntary hazard without a transition.

5.3 Family invariance

Table 5 re-estimates the involuntary transition under all four baseline families. The substantive results change very little. The size coefficient ranges from − 1.02 to -1.09, the price coefficient from − 1.03 to -1.06, the zero-return share from 1.76 to 1.80, and the leverage shift from 0.65 to 0.81. Every family places the transition late in the sample. The Weibull and log-logistic models estimate December 2024, while the Gompertz and Beta Transformed Lomax models estimate May 2024. A seven-month difference within a ten-year panel does not change the interpretation in Section 5.2. The maximised log likelihoods differ by less than one point. The Weibull has the lowest information criterion mainly because it uses one baseline shape parameter, compared with three for the Beta Transformed Lomax.

Table 5
Family invariance: the involuntary equation under four baselines.

Size	Weibull	Gompertz	Log-logistic	Beta Transformed Lomax
	−1.020	**−1.091**	**−1.022**	**−1.090**
Price	−1.059	−1.031	−1.059	−1.033
Zero-return share (t − 1)	1.800	1.764	1.798	1.766
Level shift ψ	3.882	2.817	3.910	2.774
Leverage shift ψ	0.804	0.647	0.805	0.649
Transition month	Dec 2024	May 2024	Dec 2024	May 2024
Speed γ	16	32	16	32
Log likelihood	−233.01	−233.76	−233.05	−233.72
Baseline parameters	1	1	2	3
AIC	496.0	497.5	498.1	501.4

This comparison has two implications. Substantively, it shows that the results in Sections 5.1 and 5.2 are not caused by choosing the Weibull baseline, which supports H4. Methodologically, it shows that the baseline families are genuinely interchangeable within the model. The Beta Transformed Lomax, previously used for complete lifetime samples, can be applied to censored and left-truncated competing-risks regression with time-varying covariates and a smooth transition once its integrated hazard is supplied. The more flexible family does not improve fit in this sample, but that negative result is useful: it shows that the additional baseline parameters are unnecessary here.

5.4 Robustness

The main concern about the December 2024 estimate is reverse causation. Trading often becomes illiquid in a firm's final months, so a transition estimated from one-month lags might reflect the 2025 removals themselves rather than a broader change. The zero-return share is the variable most exposed to this concern. We therefore re-estimate the involuntary model using its value seven months earlier, as in the voluntary model. The longer history requirement removes six events, but the location remains c = 0.90. The transition becomes sharper, with γ = 64. The distress profile also remains: the zero-return coefficient is 1.00, the size and price effects are slightly larger, and the likelihood ratio falls to 7.5, consistent with the loss of information. Moving price and momentum to the same seven-month lag also leaves the location unchanged and gives a likelihood ratio of 10.3. These tests indicate that terminal illiquidity does not determine the estimated transition date. The Supplementary Appendix reports the complete estimates.

Other robustness checks concern the sample and the exit labels. Excluding the LEAP Market and non-ordinary securities matters because the uncleaned source data treat several withdrawals of convertible

preference shares as company exits. Models fitted to the uncleaned data partly reflect these false events. Classification creates a different risk. One firm had an announcement record that was mixed with another issuer's record after a stock code was reused. We identified the problem because the announcements contradicted the market data and excluded the firm. We also censor the eight exits completed in early 2026 rather than assigning them assumed dates within the sample. This ensures that only exits completed in 2025 explain the 2025 increase. Each choice makes the analysis more conservative.

6. Discussion and conclusion

6.1 What was found

The results for the four hypotheses are as follows. H1 is supported as involuntary and voluntary delisting have largely different predictors. Involuntary removal is associated with small size, low price, and trading inactivity, whereas voluntary exit is associated with thin trading seven months earlier among firms that are adequately priced and performing. H2 receives qualified support. The overall involuntary hazard and its sensitivity to leverage rise around December 2024, and both changes are individually significant, but the joint calibrated test gives $p = 0.173$. H3 is supported and affects the headline result. In this design, the standard chi-squared test rejects a true null in 11.9% of replications at a nominal 5% level; relying on it would incorrectly strengthen the evidence for the transition. H4 is also supported as Weibull, Gompertz, log-logistic, and Beta Transformed Lomax baselines produce substantively similar estimates.

The simulation study also produces two broader findings. First, smooth calendar-time transitions in duration panels require dispersed entry dates. If all subjects enter together, duration dependence and calendar-time change form a likelihood ridge. In about 90% of replications, the distorted ridge solution has a higher likelihood than the true parameters, so an improved optimiser cannot resolve the identification problem. Second, transition tests that search over unidentified profile parameters require higher critical values than the standard chi-squared table provides. Both findings apply to other duration settings with a common starting date, such as bank charters, loan vintages, startup generations, and patient cohorts recruited at the same time.

6.2 Implications

For researchers, the paper provides a practical toolkit. It includes a cause-specific smooth-transition hazard model with interchangeable baselines, a record-level likelihood that uses any baseline through its integrated hazard, a profile procedure that recognises the weak identification of the speed parameter, and a simulation-based calibration method. The essential rule for calibration is to apply the same transition search to the observed and simulated data. A simpler lesson is also useful, that is, re-estimating a parametric duration model under several plausible baselines is often inexpensive, and conclusions that remain stable across those models are more credible.

For Bursa Malaysia, the results show that delisting risk should not be treated as one number. The variables that predict involuntary removal are mostly different from those that predict privatisation, so one screening model would rank both types poorly. The simultaneous increase in the overall removal hazard and the effect of leverage after pandemic-era relief ended suggests that regulatory relief delayed some removals rather than preventing them. When those removals resumed, highly leveraged firms were especially exposed. Thin trading seven months before a voluntary exit may also provide minority shareholders with an early warning well before an offer is announced.

The findings also contribute to two related literatures. For smooth-transition models, the study identifies an entry-pattern problem that does not arise in a single time series: duration panels need subjects of different ages at the same calendar date to separate duration from time change. For research on new lifetime distributions, the study shows that a distribution need not remain limited to complete samples. Once its integrated hazard is available, it can be used as a regression baseline with censoring and covariates. More demanding applications then show whether the additional parameters improve fit.

6.3 Limitations and further work

The study has several limitations. First, the sample covers one exchange and about one hundred events. This is enough for the reported models and calibrated tests, but not enough to let every coefficient change, estimate several transitions for each cause, or detect much smaller shifts. Second, calendar time identifies when a change occurs, but not why it occurs. Future work could replace calendar time with observed policy or macroeconomic variables to test specific explanations. Third, although the classification process is replicable, its keywords reflect Malaysian regulation. Applying it to another exchange would require a new list of regulatory markers. Further theory could determine how much dispersion is required and which parameters benefit. An analytical approximation could also reduce the computational cost of the calibrated test.

Declarations

Conflict of interest.

The author has no conflict of interest.

Ethics approval.

Not applicable. The study uses publicly available corporate disclosures and licensed market data; no human participants or animals are involved.

Funding.

There is no funding involved in this research.

Author Contribution

Ahmad Shauqi: Conceptualization, Methodology, Software, Formal Analysis, Investigation, Writing – Original Draft, Writing – Review & Editing

Data availability.

Market and accounting data are from LSEG Datastream and Worldscope under licence and cannot be redistributed. The exit classification, the keyword protocol, and replication code for all estimates, simulations, and figures are available from the corresponding author on reasonable request.

References

1. Bharath, S.T., Dittmar, A.K.: Why do firms use private equity to opt out of public markets? Rev. Financial Stud. **23**(5), 1771–1818 (2010). https://doi.org/10.1093/rfs/hhq016
2. Bursa Malaysia. (n.d.). Company announcements [Online database]. Retrieved July 10: from (2026). https://www.bursamalaysia.com/market_information/announcements/company_announcement
3. Campbell, J.Y., Hilscher, J., Szilagyi, J.: In search of distress risk. J. Finance. **63**(6), 2899–2939 (2008). https://doi.org/10.1111/j.1540-6261.2008.01416.x
4. Cox, D.R., Methodological, B.:), 34(2), 187–202. https://doi.org/10.1111/j.2517-6161.1972.tb00899.x (1972)
5. Davies, R.B.: Hypothesis testing when a nuisance parameter is present only under the alternative. Biometrika. **74**(1), 33–43 (1987). https://doi.org/10.1093/biomet/74.1.33
6. DeAngelo, H., DeAngelo, L., Rice, E.M.: Going private: Minority freezeouts and stockholder wealth. J. Law Econ. **27**(2), 367–401 (1984). https://doi.org/10.1086/467070
7. Doidge, C., Karolyi, G.A., Stulz, R.M.: The U.S. listing gap. J. Financ. Econ. **123**(3), 464–487 (2017). https://doi.org/10.1016/j.jfineco.2016.12.002
8. Gompertz, B.: On the nature of the function expressive of the law of human mortality, and on a new mode of determining the value of life contingencies. Philos. Trans. R. Soc. Lond. **115**, 513–583 (1825). https://doi.org/10.1098/rstl.1825.0026
9. Jonéus, P., Lyhagen, J.: Estimation in a hazard regression model with smooth transitions over unknown regimes. J. Stat. Theory Appl. **25**(1) (2026). Article 30 https://doi.org/10.1007/s44199-026-00180-z
10. Kalbfleisch, J.D., Prentice, R.L.: The statistical analysis of failure time data (2nd ed.). Wiley. (2002). https://doi.org/10.1002/9781118032985
11. Kottakkunnan, F.: A new generalization of Lomax distribution: Estimation and modelling for medical data. J. Stat. Theory Appl. **25**(1) (2026). https://doi.org/10.1007/s44199-026-00171-0 Article 19

12. Lomax, K.S.: Business failures: Another example of the analysis of failure data. J. Am. Stat. Assoc. **49**(268), 847–852 (1954). https://doi.org/10.1080/01621459.1954.10501239
13. Luukkonen, R., Saikkonen, P., Teräsvirta, T.: Testing linearity against smooth transition autoregressive models. Biometrika. **75**(3), 491–499 (1988). https://doi.org/10.1093/biomet/75.3.491
14. Macey, J., O'Hara, M., Pompilio, D.: Down and out in the stock market: The law and economics of the delisting process. J. Law Econ. **51**(4), 683–713 (2008). https://doi.org/10.1086/593386
15. Mahdavi, A., Kundu, D.: A new method for generating distributions with an application to exponential distribution. Commun. Stat. - Theory Methods. **46**(13), 6543–6557 (2017). https://doi.org/10.1080/03610926.2015.1130839
16. Marshall, A.W., Olkin, I.: A new method for adding a parameter to a family of distributions with application to the exponential and Weibull families. Biometrika. **84**(3), 641–652 (1997). https://doi.org/10.1093/biomet/84.3.641
17. Mehran, H., Peristiani, S.: Financial visibility and the decision to go private. Rev. Financial Stud. **23**(2), 519–547 (2010). https://doi.org/10.1093/rfs/hhp044
18. Shumway, T.: Forecasting bankruptcy more accurately: A simple hazard model. J. Bus. **74**(1), 101–124 (2001). https://doi.org/10.1086/209665
19. Teräsvirta, T.: Specification, estimation, and evaluation of smooth transition autoregressive models. J. Am. Stat. Assoc. **89**(425), 208–218 (1994). https://doi.org/10.1080/01621459.1994.10476462
20. Weibull, W.: A statistical distribution function of wide applicability. J. Appl. Mech. **18**(3), 293–297 (1951). https://doi.org/10.1115/1.4010337

Figures

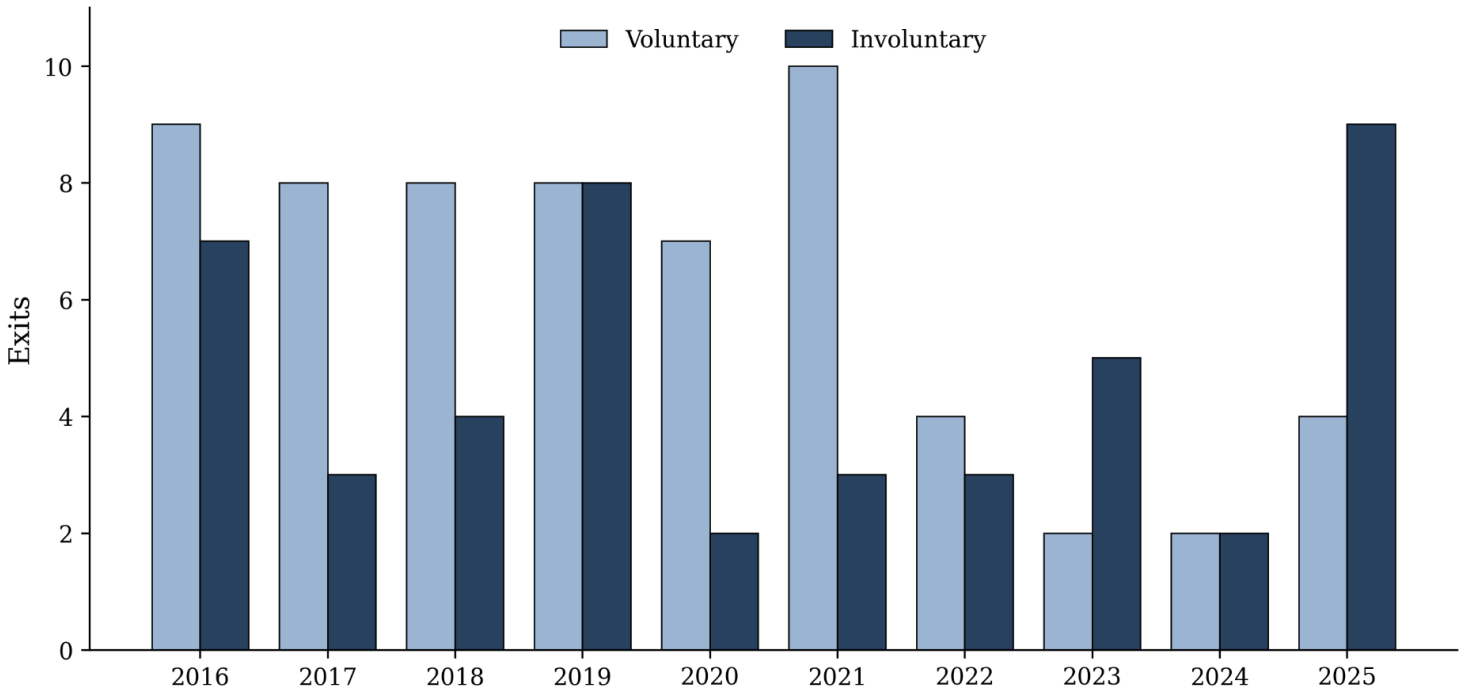


Figure 1

Delistings from Bursa Malaysia by year and cause, 2016 to 2025. Voluntary exits are take-overs, selective capital reductions, and other negotiated withdrawals; involuntary exits are removals initiated by the exchange. Eight further exits completed in early 2026 fall outside the window and are treated as censored. Source: [2].

Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- SupplementaryAppendix.docx