Business, Management and Economics Engineering
ISSN: 2669-2481 / eISSN: 2669-249X
2026 Volume 24 Issue 01

ENFORCEMENT CHANNELS AND REPEAT REGULATORY INVOLVEMENT IN INDIA: ENTITY-LEVEL EVIDENCE FROM SEBI, 2011–2025

Sameek GHOSH (Orcid id - 0000-0002-3561-5043)[1], Sougata RAY (Orcid id - 0000-0002-7793-0498)[2]
[1] DBS Global University, Dehradun, Uttarakhand, India
[2] IMI Bhubaneswar, Bhubaneswar, Orissa, India
Corresponding author: Sameek Ghosh; email: sameekghosh@gmail.com

Abstract

Purpose *– This study examines the recurrence of respondents in Securities and Exchange Board of India (SEBI) enforcement records and investigates whether enforcement channels and respondent characteristics are associated with the timing of later observed enforcement involvement.*

Research methodology *– Longitudinal respondent histories were constructed from public SEBI enforcement records for 2011–2025. After validation and matter de-duplication, 4,714 unique respondents were analysed using survival and recurrent-event models.*

Findings *– Of the respondents, 487 (10.3%) subsequently appeared in a distinct matter. Chairperson and Member channel cases exhibited a significantly higher recurrence hazard than Adjudication Officer cases (HR 2.35). The Member-channel effect varied over time, while Settlement Officer cases were not significantly different from Adjudication Officer cases. Network degree showed a weak negative association with recurrence.*

Research limitations *– The analysis relies exclusively on publicly observable SEBI enforcement records; therefore, recurrence represents later enforcement involvement rather than proven subsequent misconduct.*

Practical implications *– The findings may assist regulators in identifying patterns of repeated enforcement involvement and developing risk-based monitoring and supervisory approaches.*

Originality/Value *– The study introduces a longitudinal, respondent-level approach to SEBI enforcement research by integrating recurrence timing, enforcement channels, and network characteristics within a recurrent-event framework.*

Keywords: securities enforcement; regulation; survival analysis; network embeddedness; recurrence; India
JEL classification: C41; G18; K22; K42

1. Introduction

The importance of securities enforcement is supported by the fact that many regulatory actions have economic consequences. Empirical studies have shown that enforcement actions can impact the decisions of firms and their peers in relation to financial reporting and disclosure (Gilbert & Vaughan, 2001; Kedia & Rajgopal, 2011; Choi et al., 2024). Furthermore, market penalties for companies that have committed financial crimes have been shown to be heterogeneous according to recent research (de Batz & Kočenda, 2024). A related but different question that is difficult to answer in many of the emerging markets is in what frequency do

these companies that have entered into a public enforcement action reappear in another, distinct enforcement action. This question is related to, but not the same as, the question of whether the enforcement action was effective at deterring the behaviours that are prohibited by the law or regulation in question. For instance, Becker (1968) proposed that individuals will not exhibit behaviours that are prohibited by some standard if they calculate that the punishment for exhibiting those behaviours is likely to result in some level of penalties. More recent studies have expanded upon this concept to include additional factors that impact deterrence, such as the certainty with which those penalties will be encountered (Karpoff et al., 2008; Nagin, 2013; Buckenmaier et al., 2021). In the financial sphere, however, enforcement actions may not reflect all of the violations that occur by those companies; violations may go undetected, enforcement actions may take years to complete, and there may be multiple enforcement actions by a public regulator in relation to the same economic matter by a company (Ashton et al., 2021). Thus, an enforcement action that occurs later in time does not necessarily indicate that a company exhibited some behaviour after it was penalized for a previous behaviour.

Recent studies on financial misconduct have highlighted the value of utilizing the history of the respondents to such studies. Wang et al. (2023) reveal differences in the recidivism of Chinese firms based on the type of punishment applied to them. Khanna et al. (2021) investigate instances of repeated financial misconduct by executives of firms that had restated their financial results. Finally, Honigsberg et al. (2025) find that financial advisers with high risk of providing financial misconduct can migrate to other regimes and continue to exhibit such misconduct. These studies demonstrate the level of heterogeneity that exists among the regulated entities, but none of them provide a solution to the problem of measuring the misconduct that is documented in public legal documents.

The Securities and Exchange Board of India (SEBI) has published multiple categories of enforcement orders against market entities. Furthermore, the public archive of the regulator contains the legal documents that justify 8,032 of their enforcement orders between the years 2011 and 2020 (Zaveri Shah & Damle, 2022). This study expands upon the legal documents published by SEBI up until 31 December 2025. More importantly, the various metrics constructed in this study use only information that was publicly available to each of the respondents at the time of publication of each of their legal documents.

The paper makes four contributions. First, we provide a precision-first longitudinal panel of securities market enforcement in an emerging market with validation of the extraction of the market's respondents. Second, we treat the market's public-order and recidivism concepts separately from proven behavioural recidivism, treating all cases that occurred on the same day as one enforcement action. Third, it estimates index-year-stratified survival models and explicitly relaxes proportional hazards when diagnostics indicate time-varying associations. Fourth, we treat the network and delay measures as secondary measures rather than attempting to draw conclusions regarding causation from the observational data regarding the regulators' actions. Finally, while there is a robust association between Member channel index cases and enforcement actions, the association between the index cases and settlement, network, monetary sanction, and delay actions is weaker.

2. Institutional setting and conceptual framework

2.1 SEBI enforcement channels and the timing problem

SEBI is India's principal securities-market regulator and publishes Adjudication Officer (AO) orders, Chairperson/Member orders and directions, and settlement orders. These repositories contain economically different procedures. AO proceedings commonly impose monetary penalties under adjudicatory powers; Member orders can include directions, restrictions, debarment, and other remedies; settlement resolves proceedings on agreed terms. The categories therefore capture enforcement channels rather than a one-dimensional scale of sanction severity.

Enforcement timing complicates longitudinal interpretation. Investigation, show-cause notice, adjudication, and final order can be separated by long intervals. A later public order may concern conduct that predates an earlier order. The full-sample outcome is therefore defined neutrally as repeat enforcement involvement in a later distinct observed matter. The term recidivism is reserved for settings in which underlying conduct dates establish post-sanction behavior.

2.2 Enforcement channels, timing, and repeat involvement

Deterrence theory implies that the expected cost of wrongdoing depends on detection probability, sanction magnitude, timing, and collateral consequences. Formal penalties may be smaller than reputational and contracting losses (Karpoff et al., 2008) and delayed formal sanctions need not have a simple monotonic relationship with deterrence (Buckenmaier et al., 2021). Because enforcement channel is endogenous to allegations, evidence, respondent status, statutory provisions, and regulatory objectives, channel coefficients in this study are interpreted as observational recurrence gradients rather than treatment effects.

H1. The hazard of later observed enforcement involvement differs across index enforcement channels after accounting for entry cohort.

2.3 Network embeddedness

Co-respondent relations can capture the relational structure of enforcement matters. Embeddedness may increase monitoring and reputational exposure, which can discourage misconduct, or may proxy for opportunity sets, repeated counterparties, and durable clusters of regulatory concern. Prior corporate-network evidence finds that network embeddedness can have a governance role in reducing fraud (Zhao & Zhu, 2023), but an enforcement co-respondent network is institutionally different from board or ownership networks.

H2. Index-date co-respondent network embeddedness is associated with the hazard of later observed enforcement involvement, with the direction determined empirically.

2.4 Enforcement delay

Long delays can weaken the temporal connection between conduct and consequence, but they can also reflect case complexity, investigation quality, or legal process. Delay is therefore both theoretically relevant and endogenous. The historical manually coded sample permits exact-link analyses of show-cause-to-order and investigation-end-to-order timing for a subset of index orders; missing timing information is not imputed.

H3. Enforcement delay is associated with the time to later observed enforcement involvement in the exact-link subsample.

3. Data and research design

3.1 Data sources and study window

The historical anchor is the CC BY 4.0 Mendeley Data archive Securities law enforcement in India, Version 2, contributed by Bhargavi Zaveri Shah and Devendra Damle. It contains metadata and source links for 8,032 SEBI enforcement orders from 2011–2020 and a manually coded 818-order subset (Zaveri Shah & Damle, 2022). The extension uses official SEBI AO, Chairperson/Member, and Settlement order repositories for 2021–2025. The analytical cutoff is fixed at 31 December 2025; 2026 orders are excluded.

Recent SEBI annual reports are used only as an independent context check because their fiscal-year process totals are not equivalent to calendar-year web listings. They are not merged into the respondent-level survival data (SEBI, 2024, 2025).

3.2 Precision-first respondent resolution

Resolution is deliberately precision-first. Names and identifiers are stored separately; no PAN, DIN, CIN, registration numbers, dates, amounts, addresses, docket fragments, option symbols, headings or text is permitted within the normalized name of the entity. Strings of multiple notices are only separated within the entity's normalized name if the structure of the string indicates such a separation. While stable keys based upon the PAN are utilized, they are only utilized in cases where the entity has a stable PAN and where no other, more precise, key can be constructed based upon its normalized name. No fuzzy matching is performed with either entity names or identifiers.

Of the certified V6 database, which contains a table of all orders (931 orders, 4,714 unique stable respondents, 5,696 rows), there were 200 orders that were sampled for validation. Three orders contained only non-entity table artefacts and were therefore discarded, leaving 197 orders that were to be resolved and validated against the documents themselves. The 95% lower confidence bound for this validation process is 98.1%, which meets the pre-specified threshold of achieving a 95% precision within the primary analysis. Although less certain extractions of titles and text are contained within these tables, they are not utilized within the primary analysis to increase the number of orders that are to be resolved.

3.3 Distinct matters and enforcement episodes

A repeat document is not necessarily a repeat economic matter. The matters are canonicalized in a deterministic fashion to normalize different formatting of companies (M/s. Pvt. Ltd. vs. Private Limited, for instance), different punctuation, addendum and corrigendum wrappers, and regulatory title wrappers. Within each of the respondents, all records with the same canonicalized matter within the 180-day window are collapsed; 90-day and 365-day windows are reported as robustness checks on this statistic.

The documents within a given time window are compared to the last date on which each of those records was observed with that same matter (rather than the previous record in the dataset); an A-B-A test prevents double-counting of respondents that experienced a return of that same matter. Any number of distinct economic matters that are observed on the same order date by a given respondent are bundled into a single record for that order date (since there is no ordering among the different matters); a 30-day washout of records of any type is applied as an additional robustness check on this statistic.

Variable	Operational definition	Role
Later repeat involvement	Indicator that a respondent appears on a later order date in a distinct observed matter before censoring	Primary event
Duration	Days from first observed enforcement episode to first later-date distinct episode or 31 Dec 2025	Survival time
Member	Index enforcement channel is Chairperson/Member; AO is reference	Primary covariate
Settlement	Index enforcement channel is Settlement; AO is reference	Primary covariate
Index network degree	Number of co-respondent neighbors observed by the index date; modeled as log(1+degree)	Secondary covariate
Enforcement delay	Chronologically valid investigation-end-to-order or SCN-to-order interval from exact URL match	Conditional covariate
Monetary sanction	Monetary vs non-monetary coding in exact-link historical subset	Conditional covariate

Table 1. Outcome and covariates. All full-sample variables are derived from the certified public-order panel; delay and monetary-sanction variables are conditional exact-link measures from the manually coded historical subset.

3.4 Survival and recurrent-event models

Let T_i denote days from respondent i's first observed enforcement episode to its first later-date distinct enforcement episode. Respondents with no later episode are right-censored on 31 December 2025. Following Cox (1972), the proportional-hazards model uses Member and Settlement indicators as covariates and stratifies the baseline hazard by index year. This allows each entry cohort to have its own baseline hazard and reduces confounding from changes in order coverage and channel composition over 2011–2025.

The primary Cox model includes Member and Settlement indicators with AO as reference. A secondary model adds log(1+index-date network degree). Schoenfeld-residual diagnostics use linear, logarithmic, and rank-time transformations. Because the diagnostics indicate non-proportionality, a start/stop Cox model estimates separate channel and network associations for 0–1 year, 1–3 years, and more than 3 years after the index episode.

A secondary Andersen–Gill counting-process model (Andersen & Gill, 1982) uses all later-date recurrent episodes. Covariance is clustered by respondent, and centered index year is included as a cohort adjustment. The patched robust covariance was independently validated: four censored intervals ending before the first recurrent-event failure time had undefined software score residuals but zero event-time contribution; setting only those validated zero contributions to zero yields finite clustered sandwich standard errors without changing coefficients.

3.5 Network construction and no-lookahead rule

The co-respondent graph is built chronologically. For each date, the edges created by matters observed on that date are added to the network before the features of the respondents whose index episode occurs on that date are recorded. Thus, index-date degree incorporates only information that is contemporaneous or prior to the index date; no later orders can impact that degree statistic. The degree, weighted degree, and component size statistics are exported for that network; however, the main model uses the log(1+degree) transformation of degree.

3.6 Conditional timing and sanction analyses

The analyses of delay and sanction types are limited to matching URLs to the 818-order. Any negative values for the elapsed time between the show-cause and order are discarded. The investigation end to order and monetary sanction analyses are performed only as low power conditional analyses and are not generalized to the full sample.

3.7 Reproducibility and AI-assisted workflow

All of the calculations performed in this study were executed in Python within Google Colab. The reproducibility package includes the certified input for the V6 analysis, the corrected V7 analysis notebook, the V7.1 patch for the clustered covariance analysis, the exported CSV files from the analyses, the figures created from the analyses, the validation records, and the SHA-256 hashes of each component of the reproducibility package. OpenAI ChatGPT was used to assist in the development of the code that was utilized for the analysis, in the debugging of any resulting issues, in the testing of the reproducibility of the analysis, and in the drafting of the manuscript. No synthetic observation was utilized in the empirical path of this investigation, and any identities that were not able to be resolved were simply excluded from the investigation as a whole. The human author performed a review of the analysis, its sources, its outputs, and the manuscript that was created as a result of the investigation.

4. Results

4.1 Sample composition and recurrence

After procedural exclusions and the primary 180-day matter collapse, the analytical panel contains 5,388 respondent–matter rows from 893 retained order URLs and 4,714 unique respondents. Of these, 487 respondents (10.3%) have a first later-date repeat episode during observed follow-up. The unadjusted repeat share is 16.0% after a Member-channel index episode, compared with 5.7% after AO and 3.6% after Settlement. Median observed follow-up is 3.43 years. These are descriptive public-order recurrence rates and do not adjust for different entry cohorts.

Index channel	Respondents	First later-date repeats	Repeat share
Overall	4714	487	10.3%
AO	2131	121	5.7%
MEMBER	2194	352	16.0%
SETTLEMENT	389	14	3.6%

Table 2. Primary 180-day sample and unadjusted later-date recurrence. Respondents with no repeat are right-censored on 31 December 2025.

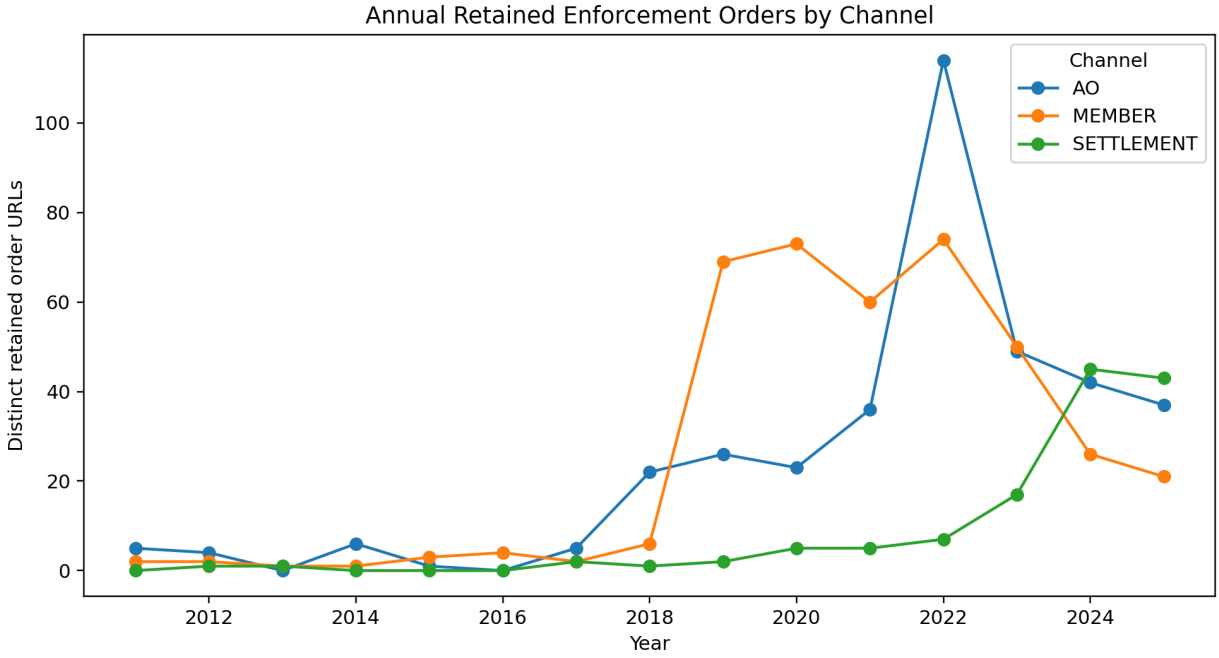

Figure 1. Annual retained enforcement orders by channel after the final matter and procedural filters. Counts are descriptive and should not be equated with the annual number of violations.

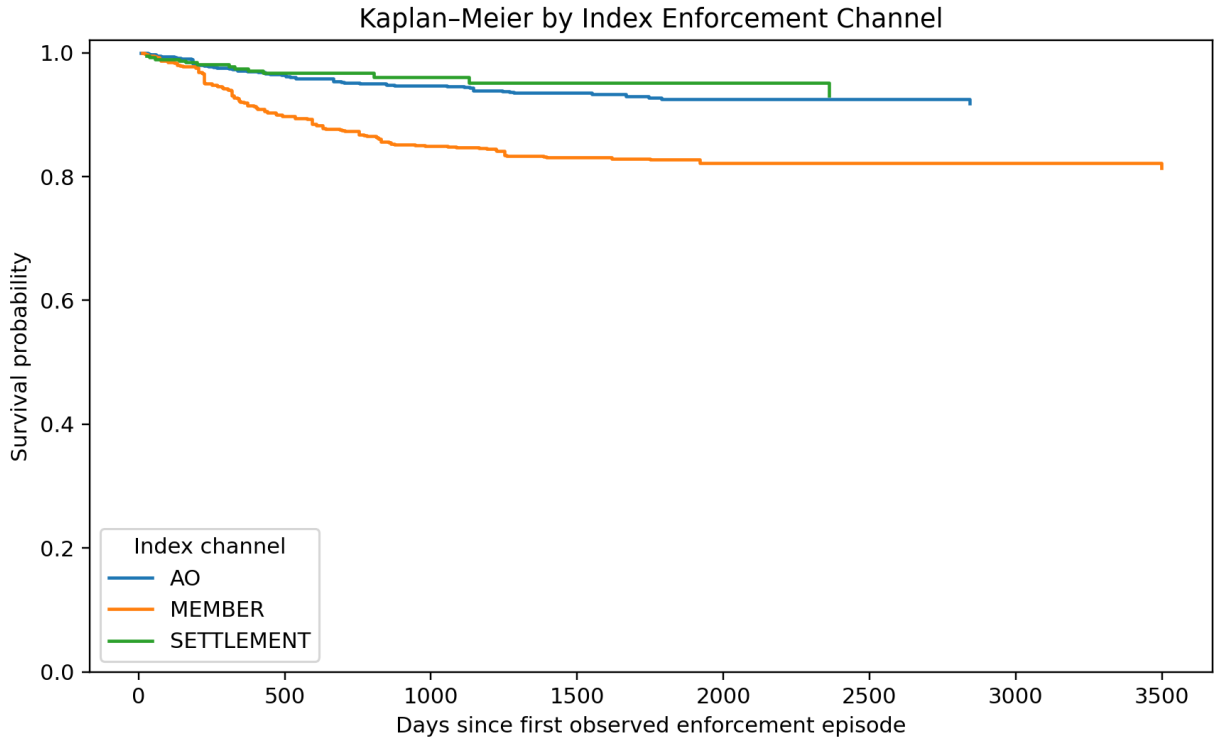

Figure 2. Kaplan–Meier recurrence-free survival by index enforcement channel. The event is first later-date involvement in a distinct observed matter.

The Kaplan–Meier estimates reinforce the descriptive channel gradient. At one year, cumulative later-repeat probabilities are 2.9% for AO, 8.1% for Member, and 2.5% for Settlement. At five years, the corresponding estimates are 7.5%, 17.3%, and 4.8%. Overall cumulative repeat probability is 5.4% at one year and 12.1% at five years.

4.2 Primary Cox estimates

In the preferred index-year-stratified Cox model, Member-channel index cases have a higher hazard of later observed enforcement involvement than AO cases (HR=2.345, 95% CI [1.868, 2.944], p<0.001). Settlement cases are not statistically distinguishable from AO (HR=0.865, 95% CI [0.481, 1.556], p=0.629). Adding baseline network degree leaves the Member estimate nearly unchanged (HR=2.312) and yields a modest negative association for log(1+degree) (HR=0.906, 95% CI [0.829, 0.990], p=0.028).

Model	Covariate	HR	95% CI	p
Year-stratified Cox	Member vs AO	2.345	1.868–2.944	<0.001
Year-stratified Cox	Settlement vs AO	0.865	0.481–1.556	0.629
Cox + network	Member vs AO	2.312	1.844–2.899	<0.001
Cox + network	Settlement vs AO	0.810	0.450–1.457	0.482
Cox + network	log(1+degree)	0.906	0.829–0.990	0.028
Andersen–Gill robust	Member vs AO	2.362	1.867–2.987	<0.001
Andersen–Gill robust	Settlement vs AO	0.722	0.396–1.316	0.288

Table 3. Primary first-event Cox estimates and recurrent-event robustness. Cox models are stratified by index year; Andersen–Gill standard errors are clustered by respondent and include centered index year.

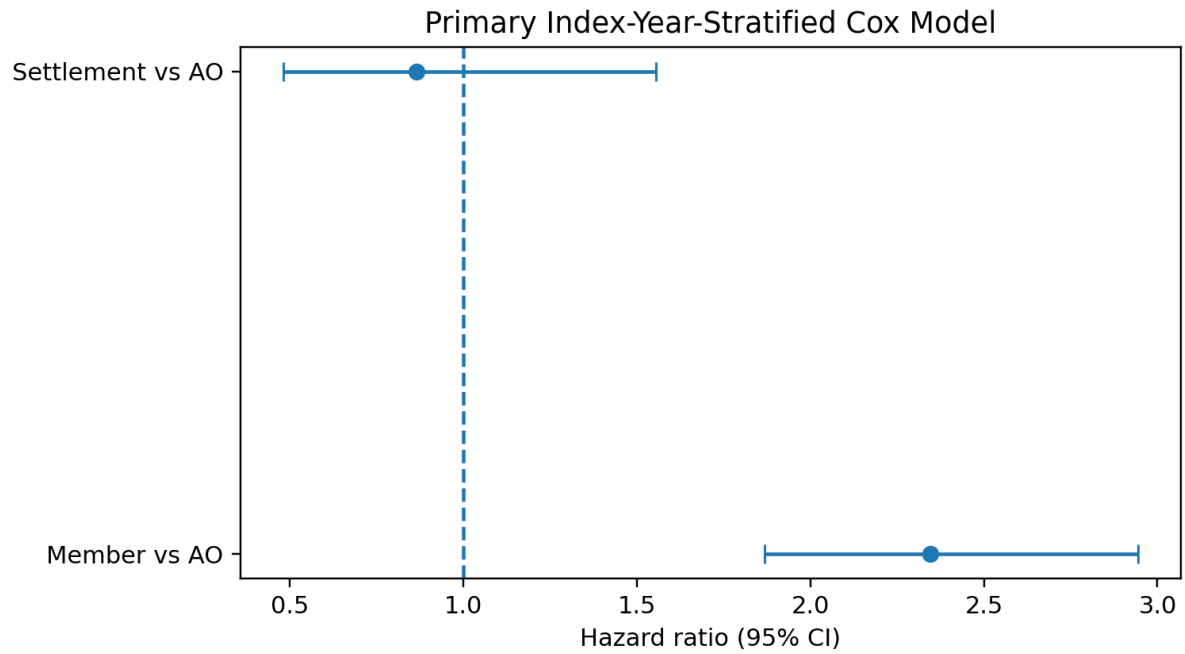

Figure 3. Preferred index-year-stratified Cox hazard ratios. AO is the reference enforcement channel.

4.3 Proportional hazards and time-varying associations

Schoenfeld-residual diagnostics indicate that a constant hazard-ratio interpretation is incomplete. For the network-adjusted stratified model, the linear-time Member diagnostic is significant (p=0.009) and the corresponding global Fisher test is p=0.021; the rank-time Member diagnostic is also significant (p=0.044). Settlement shows time variation under the log-time transformation (p=0.028). The piecewise start/stop model is therefore more informative about the temporal profile of the associations.

Variable	Period	HR	95% CI
Member vs AO	0–1 year	3.381	2.457–4.652
Member vs AO	1–3 years	1.874	1.294–2.716
Member vs AO	>3 years	0.778	0.423–1.429
Settlement vs AO	0–1 year	1.245	0.581–2.670
Settlement vs AO	1–3 years	0.487	0.147–1.610
Settlement vs AO	>3 years	0.403	0.082–1.982
log(1 + index-network degree)	0–1 year	0.991	0.871–1.127
log(1 + index-network degree)	1–3 years	0.846	0.735–0.974
log(1 + index-network degree)	>3 years	0.837	0.644–1.087

Table 4. Time-varying hazard ratios from the index-year-stratified start/stop Cox model.

The Member-versus-AO association is concentrated early in follow-up. The hazard ratio is 3.38 during the first year and 1.87 during years 1–3, while the estimate after three years is 0.78 with a confidence interval spanning one. Settlement estimates are imprecise in every band. Network degree is approximately neutral during the first year, modestly negative during years 1–3, and imprecise thereafter. This pattern argues against describing the primary Member coefficient as a permanent sanction effect.

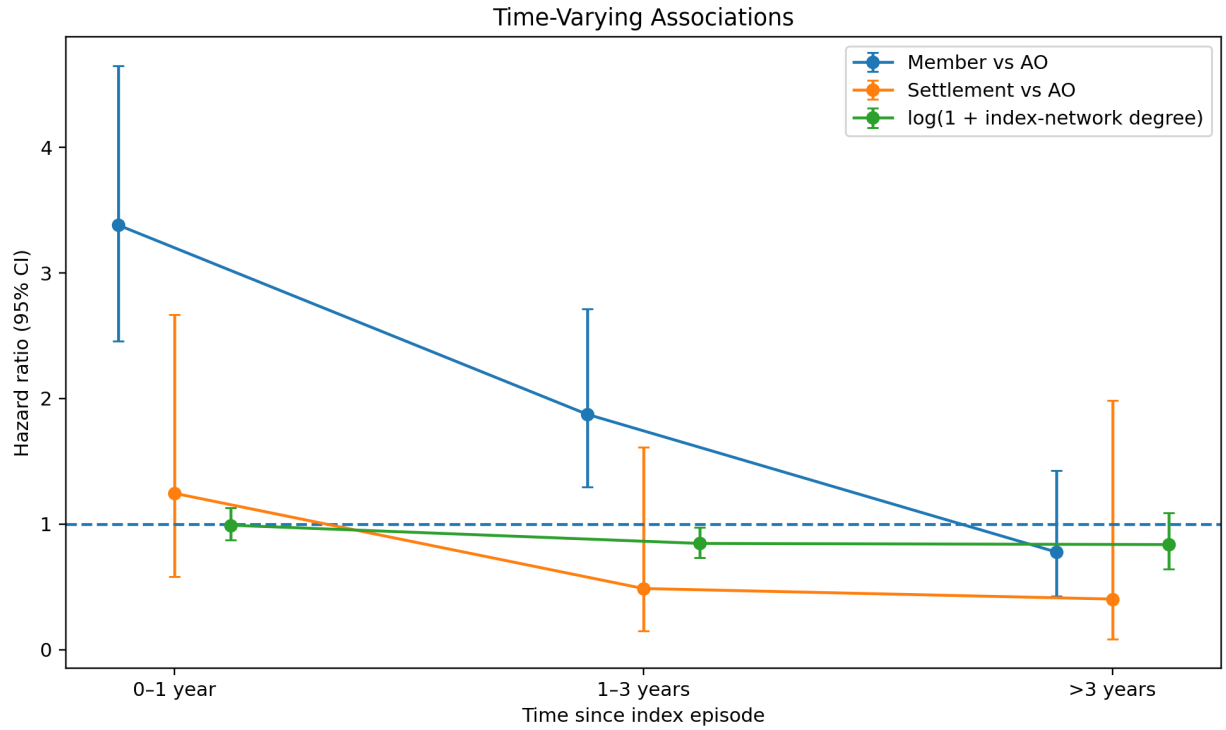

Figure 4. Time-varying hazard ratios with 95% confidence intervals. The dashed line denotes HR=1.

4.4 Robustness and recurrent events

The positive Member-channel association is robust to alternative matter definitions. It is HR=2.727 under a 90-day matter window, 2.345 under the primary 180-day window, 2.005 under a 365-day window, and 2.299 when a 30-day recurrence washout is added; all p-values are below 0.001. Restricting index cohorts to 2018–2025, 2018–2024, 2021–2024, or 2021–2025 yields Member hazard ratios between 2.434 and 2.548, again all p<0.001. Settlement estimates are not stable across specifications: the 90-day window gives a lower hazard (HR=0.559, p=0.047), while the other matter-window and calendar-period estimates are statistically indistinguishable from AO.

Specification	N	Events	Member HR	p	Settlement HR	p
90-day	4714	556	2.727	<0.001	0.559	0.047
180-day	4714	487	2.345	<0.001	0.865	0.629
365-day	4714	435	2.005	<0.001	0.902	0.745

Specification	N	Events	Member HR	p	Settlement HR	p
180-day + 30-day washout	4714	481	2.299	<0.001	0.720	0.304
Index years 2018–2025	4443	475	2.437	<0.001	0.966	0.911
Index years 2021–2025	3067	192	2.548	<0.001	1.372	0.331

Table 5. Matter-definition, washout, and calendar-period sensitivity. All models are stratified by index year.

The Andersen–Gill model uses 5,337 risk intervals and 623 later-date recurrent events. With respondent-clustered robust covariance, the Member-versus-AO hazard ratio is 2.362 (95% CI [1.867, 2.987], p<0.001), while Settlement remains non-significant (HR=0.722, p=0.288). The recurrent-event result therefore supports the first-event channel gradient without changing its observational interpretation.

4.5 Network, sanction type, and enforcement delay

Network evidence is weaker than the channel result. In the network-adjusted Cox model, a one-unit increase in log(1+index degree) is associated with HR=0.906 (p=0.028), but unadjusted recurrence does not decline monotonically across degree quartiles: the observed repeat shares are 9.5%, 8.8%, 12.5%, and 10.5% from the lowest to highest quartile. A network-channel interaction model finds no meaningful Member×degree interaction but a negative Settlement×degree interaction; because the Settlement group is smaller and the descriptive degree gradient is non-monotonic, the interaction is treated as exploratory rather than a central result.

Conditional predictor	N	Events	HR	95% CI	p
Monetary vs non-monetary sanction	146	17	2.392	0.909–6.292	0.077
Investigation-end-to-order delay (1 SD log)	126	10	3.741	0.759–18.438	0.105
SCN-to-order delay	34	0	Not estimated	—	—

Table 6. Conditional exact-link sanction and delay analyses. These low-event subsamples are not used to make full-sample deterrence claims.

The conditional monetary-sanction estimate is HR=2.39 (95% CI 0.91–6.29, p=0.077) based on only 17 recurrence events, so the data do not establish that monetary sanctions either reduce or increase subsequent public enforcement involvement. The investigation-end-to-order delay estimate is HR=3.74 per one standard deviation of log delay, but the confidence interval is extremely wide (0.76–18.44, p=0.105) with only 10 events. The SCN-to-order matched subset contains 34 respondents and no later recurrence event, preventing estimation. H3 is therefore unresolved rather than rejected or supported.

5. Discussion

The main result is a robust but time-concentrated difference in later observed enforcement involvement across enforcement channels. Respondents whose first observed episode is in the Member channel have more than twice the later-event hazard of AO respondents in the preferred index-year-stratified model, and the association is preserved in recurrent-event and matter-definition robustness checks. The contrast is strongest within the first year and remains elevated through years 1–3, but it is no longer distinguishable from one thereafter. This temporal pattern is inconsistent with a simple permanent "sanction effect."

There are several ways in which the gradient within the Member channel may be created. The Member channel may tend towards more complex matters. Additionally, enforcement actions tend to arrive in waves; Choi et al. (2024) have found that enforcement actions related to financial misrepresentation often arrive in clusters. Thus, while the 30-day washout and the corrected matter de-duplication helps to rule out the impact of duplicated fines for the same matters, it does not rule out the possibility that enforcement tends to cluster in such ways. Therefore, while the gradient within the Member channel may appear in the data, it is more likely the result of selection effects rather than a failure of the sanctions to be directed at the Member channel.

The findings regarding the Settlement channel are even weaker. The model that is presented does not distinguish between Settlement and AO channels, the estimates for Settlement are based on imprecise model estimates, and the restrictions on the impact of Settlement channels remain non-significant. Thus, the finding of a negative estimate for Settlement for matters within a 90-day window is likewise not robust enough to make a claim regarding the effect of Settlement on the Member channel.

The network results also warrant restraint. A modest negative adjusted degree coefficient is consistent with a monitoring or reputational-governance interpretation, but the quartile recurrence pattern is non-monotonic and the effect is concentrated in the 1–3 year band. The enforcement co-respondent network may capture both governance and opportunity structures. Unlike board-interlock networks, it is created by regulatory co-involvement itself, so degree can reflect case architecture and regulator attention as much as enduring economic relationships. H2 therefore receives only weak, specification-dependent support.

The low-power conditional results demonstrate why delay should not be overinterpreted. Long elapsed times are substantively important, but exact-link coverage produces only 10 recurrence events for the investigation-end-to-order model and none for SCN-to-order delay. Large point estimates with very wide confidence intervals are a signal to collect better timing data, not a basis for policy claims. The same caution applies to the monetary-versus-non-monetary comparison. These results are retained because transparent non-findings are preferable to dropping theoretically relevant mechanisms after they fail to reach conventional significance.

The methodological contribution of this study is therefore also important. The archives contain documents, not panels. Without being able to resolve identities to matters, canonicalize matters, establish rules for episodes of the same day, and right-censor documents, the counts of documents can create false recurrences of regulatory behavior. The validation study shows that a smaller sample of high-precision regulator identities produces more valid recurrences than a

larger sample of aggressively resolved regulator identities. These findings are relevant to other regulatory agencies whose enforcement activities are published as legal documents.

Regulators can use these findings to more efficiently triage their regulatory correspondence with identified high-risk respondents. Such uses of the regulator panel should be subordinate to the due process protections of the entities with which they interact. Researchers can use these findings to design research studies that more precisely separate regulator identity from regulator behavior. For example, researchers could investigate whether certain regulators are more likely to employ specific regulatory strategies than others. Recent research has found regulatory leakage between financial advisers that are otherwise regulated by different regulators (Honigsberg et al., 2025). Future research should follow these regulators across their regulatory domains rather than restricting the research to the regulator from whom their regulatory activity was published.

6. Limitations

First, the orders are a partially observed outcome. The absence of an order does not necessarily equate to the absence of misconduct after that order, and there may be changes to the behavior of the regulators in the detection and publication of those orders (Ashton et al., 2021).

Second, the primary sample was intentionally precision-first rather than attempting to resolve every public order. While this avoids the introduction of false merges between cases having the same misconduct, it can introduce understatements of the true number of cases that recur with the same actor, especially in cases where that actor has a different spelling or document structure for their matters that remain unresolved.

Third, while the matter canonicalization software cannot perfectly represent the identity of every legal case, the use of the 90-day, 180-day, and 365-day window analysis, the A-B-A regression test for cases occurring on the same day, and similar measures helps to eliminate known sources of false recurrences in the data set.

Fourth, the enforcement channel is endogenous, and thus does not reflect the severity of the sanctions that were applied to the individual. The hazard ratios for the primary models do not reveal the effect of orders by Members, AO penalties, or settlements. Furthermore, the models for the application of monetary sanctions and delays in the publication of orders have especially limited statistical power.

Fifth, while there is a measure of the network of entities that are subject to the same orders as each AO, that network represents the enforcement network of the AOs, not any network relating to the ownership, communication, or ties to the governing body of each AO. Thus, the economic meaning of that network is ambiguous.

7. Conclusion

A precision-certified public-order panel shows that 487 of 4,714 SEBI respondents reappear on a later date in a distinct observed enforcement matter under the primary 180-day definition. Member-channel index cases have a substantially higher later-recurrence hazard than AO cases after stratifying by index year, and the association survives alternative matter windows, a 30-day washout, calendar-period restrictions, and an Andersen–Gill recurrent-event specification. The excess hazard is mainly concentrated in the first three years, especially the first year, of a firm's life rather than extending indefinitely into the future. Furthermore, there is no difference between the settlement and AO. There is a modest and specification-

dependent negative association between network embeddedness and misconduct, but the estimates for sanction-type and enforcement-delay are too imprecise to draw conclusions from them. Thus, both a substantive result and a lesson regarding measurement contribute to the study's findings.

Data availability

The historical dataset is publicly available through Mendeley Data as "Securities law enforcement in India," Version 2, DOI 10.17632/ppdk9pzfdp.2, under a CC BY 4.0 licence. The 2021–2025 extension is reconstructed from publicly accessible official SEBI enforcement repositories with a fixed cutoff of 31 December 2025. The submission package includes the certified processed analysis input, validation records, final statistical outputs, source URLs where available, code, and reproducibility documentation. Regulator-hosted source PDFs are referenced rather than redistributed.

Code availability

The supplementary reproducibility package contains the Google Colab analysis notebook, the V7.1 clustered-covariance patch, certified analysis input, final result CSVs and figures, QA records, and SHA-256 manifests. The empirical output is generated from code rather than manually entered coefficients.

Funding

This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

Declaration of competing interest

Declarations of interest: none.

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

During the preparation of this work the author used OpenAI ChatGPT to assist with code development, debugging, reproducibility checks, literature organization, and manuscript drafting. After using this tool, the author reviewed and edited the content as needed and takes full responsibility for the content of the published article.

References

Andersen, P. K., & Gill, R. D. (1982). Cox's regression model for counting processes: A large sample study. The Annals of Statistics, 10(4), 1100–1120. https://doi.org/10.1214/aos/1176345976

Ashton, J., Burnett, T., Diaz-Rainey, I., & Ormosi, P. (2021). Known unknowns: How much financial misconduct is detected and deterred? Journal of International Financial Markets, Institutions and Money, 74, 101389. https://doi.org/10.1016/j.intfin.2021.101389

Becker, G. S. (1968). Crime and punishment: An economic approach. Journal of Political Economy, 76(2), 169–217. https://doi.org/10.1086/259394

Buckenmaier, J., Dimant, E., Posten, A.-C., & Schmidt, U. (2021). Efficient institutions and effective deterrence: On timing and uncertainty of formal sanctions. Journal of Risk and Uncertainty, 62(2), 177–201. https://doi.org/10.1007/s11166-021-09352-x

Choi, H. M., Karpoff, J. M., Lou, X., & Martin, G. S. (2024). Enforcement waves and spillovers. Management Science, 70(2), 834–859. https://doi.org/10.1287/mnsc.2023.4711

Cox, D. R. (1972). Regression models and life-tables. Journal of the Royal Statistical Society: Series B (Methodological), 34(2), 187–202. https://doi.org/10.1111/j.2517-6161.1972.tb00899.x

de Batz, L., & Kočenda, E. (2024). Financial crime and punishment: A meta-analysis. Journal of Economic Surveys, 38(4), 1338–1398. https://doi.org/10.1111/joes.12580

Gilbert, R. A., & Vaughan, M. D. (2001). Do depositors care about enforcement actions? Journal of Economics and Business, 53(2–3), 283–311. https://doi.org/10.1016/S0148-6195(00)00043-6

Honigsberg, C., Hu, E., & Jackson, R. J. (2025). Regulatory leakage among financial advisors: Evidence from FINRA regulation of "bad" brokers. Journal of Financial Economics, 174, 104170. https://doi.org/10.1016/j.jfineco.2025.104170

Karpoff, J. M., Lee, D. S., & Martin, G. S. (2008). The cost to firms of cooking the books. Journal of Financial and Quantitative Analysis, 43(3), 581–611. https://doi.org/10.1017/S0022109000004221

Kedia, S., & Rajgopal, S. (2011). Do the SEC's enforcement preferences affect corporate misconduct? Journal of Accounting and Economics, 51(3), 259–278. https://doi.org/10.1016/j.jacceco.2011.01.004

Khanna, P., Khan, S. A., Krasikova, D. V., & Miller, S. R. (2021). Repeated engagement in misconduct by executives involved with financial restatements. Organizational Behavior and Human Decision Processes, 166, 194–203. https://doi.org/10.1016/j.obhdp.2020.08.003

Nagin, D. S. (2013). Deterrence in the twenty-first century. Crime and Justice, 42(1), 199–263. https://doi.org/10.1086/670398

Securities and Exchange Board of India. (2024). Annual Report 2023–24. SEBI.

Securities and Exchange Board of India. (2025). Annual Report 2024–25. SEBI.

Securities and Exchange Board of India. (2026). Enforcement orders [Public database]. Official SEBI enforcement repositories; analytical records restricted to orders dated through 31 December 2025. Accessed 17 August 2026.

Wang, Y., Ashton, J. K., & Jaafar, A. A. (2023). Financial statement fraud, recidivism and punishment. Emerging Markets Review, 56, 101033. https://doi.org/10.1016/j.ememar.2023.101033

[dataset] Zaveri Shah, B., & Damle, D. (2022). Securities law enforcement in India. Mendeley Data, Version 2. https://doi.org/10.17632/ppdk9pzfdp.2

Zhao, J., & Zhu, L. (2023). Does network embeddedness deter corporate fraud? Evidence from China. Emerging Markets Finance and Trade, 59(9), 2906–2927. https://doi.org/10.1080/1540496X.2023.2203807