

State Ownership and the Formal-Oversight Paradox: Audit-Committee Adoption and Disclosure Enforcement

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Abstract

State ownership and audit committees are often modeled as independent governance mechanisms, although firms may organize accountability through different monitoring bundles. Using 2,335 firm-year observations for 467 nonfinancial companies listed on HOSE and HNX that satisfy the study's governance, ownership, financial, audit, and enforcement-data requirements during 2020–2024, this study separates the extensive margin of audit-committee entry from the intensive margin of committee architecture and links both to subsequent regulator-recorded disclosure enforcement. Among 415 firms without a validated audit committee in 2020, 49 adopted one by 2024. Adjusted cumulative adoption is 5.5% for state-linked firms versus 14.3% otherwise. At the same time, a ten-percentage-point increase in state ownership is associated with approximately 21% fewer expected next-year recorded violations. Observed audit-committee architecture barely attenuates this ownership gradient, and continuing committees do not display a broad state-linked reduction in size, meetings, independence, or expert leadership. Within-firm estimates reverse sign, but identifying variation comes from only two outcome-informative firms whose state share changes. The evidence therefore supports a bounded governance-regime interpretation rather than a causal state-ownership effect: formal board-centered oversight and recorded enforcement can move differently when monitoring is organized through alternative institutional configurations.

JEL Classification: G34; M41; M42; G38

1 Introduction

Corporate governance research often estimates ownership structure, board composition, audit committees, external audit, and internal control as separate monitoring mechanisms. That simplification is useful for estimating marginal associations, but it can obscure how governance is assembled in practice. Monitoring arrangements are jointly selected, their marginal value can depend on surrounding mechanisms, and the same formal structure can have different organizational meanings across ownership regimes (Rediker and Seth 1995; Aguilera et al. 2008; Misangyi and Acharya 2014). State ownership makes this interdependence particularly visible because the state can simultaneously be an owner, an appointing principal, a policy actor, and a source of administrative accountability. The relevant question is therefore not only whether state ownership is associated with better or worse outcomes, but also how state-linked firms allocate monitoring across formal board-centered and other institutional channels.

This issue is unusually observable in Vietnam after the Law on Enterprises 2020 and its implementing governance framework. The reform more clearly formalized the contemporary board-centered audit-committee model for public companies, but the architecture remained in an early implementation stage throughout the 2020–2024 observation window. The limited number of audit committees in the data is therefore an institutional feature rather than a researcher-imposed small-sample design. The analysis covers all nonfinancial firms listed on HOSE and HNX that satisfy the stated governance, ownership,

financial, audit, and enforcement-data requirements. It does not use a larger exchange-wide headline count because that number is not the relevant empirical population for constructs that require consistent measurement.

The empirical pattern is counterintuitive. State-linked observations are less likely to operate with a validated audit committee, yet they also record fewer regulator-sanctioned disclosure violations. A simple model in which more formal board oversight mechanically implies better compliance therefore does not describe the data. Nor does a simple mediation account in which state ownership changes audit-committee capacity and that capacity then carries the ownership association to enforcement. Once audit-committee entry is separated from committee characteristics among firms that already have a committee, state linkage predicts lower formal entry but does not broadly predict smaller committees, fewer meetings, less independence, or less expert leadership. More importantly, adding observed committee architecture to the subsequent-enforcement model barely changes the state-ownership gradient.

The paper calls this coexistence the formal-oversight paradox. The label is descriptive rather than theoretical: it names the observed combination of less entry into one formal architecture and fewer subsequent recorded enforcement events. It does not claim that state ownership is intrinsically superior, that audit committees are ineffective, or that the state directly substitutes for board monitoring. Its purpose is to sharpen a configuration problem. If firms embedded in one ownership regime can rely less on a newly salient formal mechanism without exhibiting more recorded enforcement, audit-committee presence cannot be treated as a universal scalar measure of monitoring strength.

The study contributes in four ways. First, it moves the state-ownership question from a directional good-versus-bad debate toward governance architecture. State ownership is treated as a persistent institutional regime marker whose relevance may be expressed partly through where monitoring authority is located. This complements evidence that ownership identity affects reporting incentives, transparency, risk, performance, and governance in emerging markets while avoiding the assumption that a single state-ownership coefficient summarizes all governance channels (Claessens et al. 2000; Shleifer and Vishny 1997; Phung and Mishra 2016; Vo 2018).

Second, the study separates the extensive margin of governance formalization from the intensive margin of committee design. Audit-committee existence is an organizational choice, whereas size, independence, meetings, expertise, chair experience, and legal expertise describe the architecture of an operating committee. Prior research shows that committee effectiveness depends on these dimensions and on process rather than on the committee label alone (Klein 2002; Abbott et al. 2004; Bédard et al. 2004; Beasley et al. 2009; Free et al. 2021). Recent RQFA research similarly treats board and committee characteristics as economically meaningful features rather than interchangeable checkboxes (Vafeas and Vlittis 2024; Noureldeen et al. 2024).

Third, the analysis treats failed mechanism evidence as informative. A transmission account predicts more than a statistically significant ownership coefficient: state ownership should be systematically

related to the proposed audit-committee channel, and conditioning on that channel should meaningfully attenuate the ownership gradient. Neither condition receives broad support. This falsification redirects interpretation toward alternative governance configurations rather than forcing an indirect-effect narrative that the observed variables do not sustain.

Fourth, the paper makes the identification boundary visible. State ownership is highly persistent. Correlated-random-effects and conditional fixed-effects estimates reverse the pooled sign, but the within-firm state-share estimand is supported by only two firms that both change state share and contribute outcome information. The result is retained as adverse evidence rather than promoted as a causal finding. This distinction follows the broader endogeneity literature: more elaborate estimators do not create identifying variation that the data do not contain (Wintoki et al. 2012; Bell and Jones 2015; Wooldridge 2019).

The enforcement outcome requires a parallel boundary. Regulator-recorded sanctions are externally validated events, but they combine underlying noncompliance with detection, regulatory attention, processing, and sanctioning. A lower count can therefore reflect better compliance, lower underlying risk, different visibility, or different enforcement exposure. Enforcement studies and disclosure-regulation reviews caution against equating observed cases with the latent universe of misconduct (Dechow et al. 1996, 2011; Karpoff et al. 2008; Dyck et al. 2010; Leuz and Wysocki 2016). The manuscript accordingly uses recorded, sanctioned, or observed enforcement language throughout.

The resulting claim is deliberately bounded. Persistent state linkage is associated with lower entry into the post-reform audit-committee architecture and with fewer subsequent recorded disclosure-enforcement events, while observed audit-committee architecture does not explain that association. The study does not estimate a causal state-ownership treatment effect or directly observe the alternative monitoring channels that would be required to establish substitution. The remainder of the paper develops the institutional setting, theory, hypotheses, research design, results, and implications under that constraint.

Note

The figure distinguishes a persistent ownership regime, entry into the board-centered audit-committee architecture, and subsequent recorded enforcement. Solid and dashed pathways describe the empirical sequence and configuration possibilities. No arrow is interpreted as an identified causal treatment effect.

2 Institutional setting: ownership, audit committees, and disclosure enforcement in Vietnam

Vietnam combines concentrated ownership, continued state participation, developing-market enforcement, and a rapidly evolving public-company governance framework. These features matter because they change both the agency problem and the available channels of monitoring. Concentrated owners can reduce manager-shareholder conflicts while increasing principal-principal conflicts; state

ownership can add policy, employment, strategic, and administrative objectives to the conventional shareholder-value objective (Jensen and Meckling 1976; Fama and Jensen 1983; La Porta et al. 1999, 2000). Evidence from Vietnam documents material relationships between ownership and agency costs, earnings management, risk, board characteristics, reporting quality, investment outcomes, conservatism, and disclosure (Nguyen et al. 2020, 2021a; Nguyen et al. 2021b; Nguyen et al. 2022; Dinh et al. 2023; Pham and Vu 2024). Complementary Vietnamese evidence links governance structure to governance disclosure, annual-report quality, earnings management, and financial-information transparency (Ha 2022; Tran and Dang 2021; Tran and Ha 2023; Truong et al. 2022; Truong and Nguyen 2024).

The Law on Enterprises 2020 is central to the empirical window. Together with Decree No. 155/2020/ND-CP and Circular No. 116/2020/TT-BTC, it clarified the contemporary public-company framework for a board-centered audit committee (National Assembly of Vietnam 2020; Government of Vietnam 2020; Ministry of Finance of Vietnam 2020). The legal change is not used as a natural experiment. Firms were not randomly assigned to adopt an audit committee, and governance planning could predate the effective date. The reform instead defines a common institutional period in which first observed entry into a newly salient architecture can be followed consistently.

Under the board-centered model, the audit committee is a specialized body of the board. The legal framework requires at least two members, an independent director as chair, and responsibilities relating to financial-statement integrity, internal control and risk management, internal audit, external-auditor independence, and legal compliance. This design differs from the traditional supervisory-board architecture, where formal supervision is located outside the board. The study therefore observes an architectural choice rather than an additional committee layered onto an otherwise identical organizational form.

The timing has a direct empirical implication. Formal adoption can require charter amendments, shareholder approval, recruitment or reassignment of independent directors, committee rules, reporting lines for internal audit, and coordination with the external auditor. Only four post-effective decision years are observed. The scarcity of audit committees by 2024 is consequently consistent with early institutional diffusion and organizational conversion, not evidence that the study discarded most committee firms after observing their outcomes. The empirical strategy uses sparse-event methods and exposes the number of adoptions instead of inflating support by treating already-adopted firms as new decisions.

State-linked firms require additional care because the absence of a board audit committee need not imply an absence of oversight. State owners can exercise monitoring through representation, ownership-accountability systems, internal control, audit, inspection, and administrative channels. Those channels may be weak or strong, and the present data do not measure each of them. The theoretical implication is narrower: state linkage creates a plausible setting in which monitoring may be organized differently, so formal audit-committee presence should not be interpreted in isolation.

This ambiguity also cautions against normative claims. State ownership may provide long horizons, resources, and additional oversight, but it may also produce political objectives, soft-budget expectations, or weaker minority-shareholder discipline. Vietnamese findings are correspondingly heterogeneous. State holdings have been related to risk, corporate social disclosure, earnings management, governance, and performance in ways that depend on the outcome and conditioning set (Vo 2018; Hoang et al. 2019; Nguyen et al. 2021b; Doan 2024). The study uses this heterogeneity to motivate an architecture question rather than imposing an ex ante beneficial or harmful state-ownership narrative.

Finally, the enforcement construct is institutionally specific. FDV counts disclosure violations that enter the formal regulatory record. These events are more externally verifiable than researcher-scored disclosure indices, but they remain the end product of detection and sanctioning. The same observed count can arise from different combinations of underlying misconduct and enforcement intensity. This measurement boundary is especially important when ownership or governance affects firm visibility, information access, or the likelihood that problems are surfaced.

Table 1
Institutional and empirical sequence

Period / instrument	Institutional development	Implication for the design
2020	Law on Enterprises No. 59/2020/QH14 enacted	Contemporary board-centered audit-committee architecture becomes more explicit; not random assignment
2020	Decree No. 155/2020/ND-CP and Circular No. 116/2020/TT-BTC	Public-company governance requirements and model regulations are elaborated
2021–2024	First four post-effective annual decision years observed	Short adoption window; committee scarcity is an institutional support constraint
2020–2024	Ownership, governance, audit, financial, and enforcement data observed	Permits baseline adoption, intensive-margin, and temporally ordered enforcement analyses
<i>Note. The legal sequence defines the institutional opportunity set but does not create a treatment-control contrast or difference-in-differences design.</i>		

3 Theoretical framework

3.1 Governance bundles and ownership-conditioned monitoring

Agency theory explains why monitoring becomes valuable when information asymmetry and divergent incentives separate decision management from residual claims. It does not imply that every monitoring mechanism should be maximized simultaneously. Monitoring is costly, control rights are distributed

across actors, and ownership identity changes both the conflicts to be controlled and the information channels available to do so (Jensen and Meckling 1976; Fama and Jensen 1983; Shleifer and Vishny 1997). The relevant unit is therefore the governance system rather than any single checklist item.

Governance-bundle research makes that interdependence explicit. Boards, owners, committees, auditors, and internal controls can substitute for, complement, or condition one another, so the marginal value of one mechanism depends on what surrounds it (Rediker and Seth 1995; Aguilera et al. 2008; Misangyi and Acharya 2014). Board structure itself is endogenous to monitoring and advising needs (Boone et al. 2007; Adams et al. 2010; Coles et al. 2008). This perspective changes the meaning of a negative state-ownership–audit-committee association. The association is consistent with alternative allocation, but it does not establish substitution unless the alternative input is observed and shown to intensify as audit-committee reliance falls.

The study therefore uses the term alternative monitoring regime to describe an ownership-conditioned configuration, not to assert that unobserved state channels are necessarily effective substitutes. That distinction is important for theory. A state-linked firm can have fewer board-centered formal mechanisms because of alternative channels, because of entrenched governance preferences, or because it faces different organizational demand. The data can distinguish some observable implications of these accounts but cannot identify all of their underlying mechanisms.

3.2 Formal adoption versus substantive committee architecture

Audit-committee research reinforces the difference between formal existence and substantive capacity. Committee independence, financial expertise, activity, status, internal-control involvement, and auditor interaction are each associated with reporting outcomes, but they measure distinct dimensions (Klein 2002; Abbott et al. 2004; Bédard et al. 2004; DeFond et al. 2005; Krishnan 2005; Dhaliwal et al. 2010; Krishnan et al. 2011). Evidence on full independence and board/committee quality further cautions against equating formal existence with effective oversight (Bronson et al. 2009; Karamanou and Vafeas 2005). Process research further shows that agenda control, information access, member standing, relationships with management, and auditor coordination affect whether formal oversight becomes substantive challenge (Beasley et al. 2009; Bruynseels and Cardinaels 2014; Lisic et al. 2016; Free et al. 2021; Livne et al. 2024).

This literature implies two different governance margins. The extensive margin asks whether a firm enters the board-centered audit-committee architecture. The intensive margin asks how an already operating committee is configured. Coding size, meetings, or independent membership as zero before committee existence mechanically mixes the two decisions. The current design therefore isolates first adoption among baseline non-adopters and estimates committee characteristics only among continuing audit committees. This distinction is also consistent with evidence that reporting failures can be followed by governance responses rather than governance being predetermined independently of prior

risk (Farber 2005). This separation is especially important in a short post-reform window in which non-adoption remains common.

The distinction also generates a direct mechanism test. If state ownership is associated with fewer sanctions because it produces a systematically different audit-committee capacity pathway, state ownership should predict substantive committee architecture among continuing committees and the state-ownership coefficient should materially attenuate after the relevant committee variables are introduced. A failure of either implication weakens the proposed mediation account. This is a falsification logic rather than causal mediation: no sequential ignorability assumption is invoked, and no natural indirect effect is reported.

3.3 Enforcement as behavior plus observability

Governance can influence both behavior and what becomes observable. Financial-reporting research shows that restatements, enforcement actions, and fraud cases are selected manifestations of underlying reporting problems, while disclosure regulation determines how information reaches markets (Healy and Palepu 2001; Dechow et al. 1996, 2011; Karpoff et al. 2008; Leuz and Wysocki 2016). Better monitoring may deter violations, but it can also reveal problems that would otherwise remain undetected. Conversely, low observed enforcement may reflect good compliance or low detection intensity.

The empirical outcome therefore does not measure latent reporting quality. It measures the burden of regulator-recorded disclosure enforcement. The theory predicts associations with the observed process and uses timing and alternative outcomes to test consistency; it does not assign a single behavioral mechanism to the sign of the coefficient. This measurement discipline is central to avoiding overclaiming when the dependent variable is sparse and institutionally filtered.

4 Literature review, hypotheses, and research questions

4.1 State linkage and audit-committee entry

Formation research treats specialized accounting oversight as an organizational response to monitoring, contracting, and certification demand. Later effectiveness studies show why boards may value specialized audit oversight, but existence remains conceptually upstream from effectiveness. In a state-capital setting, the marginal demand for this board-centered structure can be lower when ownership-accountability channels are organized differently, or higher when minority-shareholder credibility requires visible delegation. The sign is therefore an empirical question unless the institutional configuration yields a directional prediction.

The current Vietnamese setting provides such a bounded prediction. State linkage is persistent and can embed alternative appointment and accountability relationships; the contemporary audit-committee architecture is newly salient and costly to establish. Conditional on observed baseline firm, ownership,

governance, audit, and industry characteristics, the study expects state-linked firms to enter the architecture less frequently during the short post-reform period. This prediction concerns regime-linked sorting, not the causal effect of increasing the state share.

H1. Among firms without a validated audit committee at the 2020 baseline, state-linked ownership is negatively associated with subsequent first entry into the post-reform audit-committee architecture.

4.2 State ownership and intensive committee design

A lower adoption rate does not imply that state-linked firms deliberately operate weaker committees once they adopt. Indeed, committee architecture can respond to different considerations than the entry decision. Size may supply human resources but increase coordination costs; meetings provide attention but not necessarily challenge; independence can improve monitoring distance; financial or legal expertise can improve interpretation; and chair experience can reflect accumulated knowledge or persistent appointment patterns (Badolato et al. 2014; Carcello et al. 2011; Vafeas and Vlittis 2024). Recent evidence on two-tier systems also illustrates that board and supervisory characteristics can relate differently to audit-reporting outcomes (Noureldeen et al. 2024).

Because the data do not identify a single theoretically necessary direction for each intensive attribute, these tests are framed as a research question rather than six directional hypotheses. The key issue is whether state ownership produces a broad, internally coherent pattern of less substantive committee capacity among firms that already maintain committees.

RQ1. Among firms with audit committees in both $t - 1$ and t , is state ownership systematically associated with changes in substantive audit-committee architecture—size, meetings, independence, expert leadership, chair experience, or legal expertise?

4.3 State ownership and subsequent recorded enforcement

State ownership has competing reporting implications. It can intensify oversight through ownership representation and public accountability, but it can also create multiple objectives and weaker market discipline. Emerging-market ownership structures further generate principal-principal conflicts that differ from diffuse-ownership agency models (Claessens et al. 2000; La Porta et al. 1999; Shleifer and Vishny 1997). Vietnamese research similarly reports non-uniform links between state participation, risk, disclosure, governance, and reporting outcomes (Vo 2018; Hoang et al. 2019; Nguyen et al. 2021b; Doan 2024).

The empirical record in the present data motivates a directional, prospective hypothesis that is explicitly about recorded enforcement. The model conditions on prior violations, thereby asking whether higher state ownership is associated with the subsequent count and occurrence of sanctions after observed enforcement history and other covariates are taken into account. The hypothesis remains associational

because persistent ownership can share unobserved determinants with both governance and enforcement.

H2. Higher state ownership is negatively associated with the subsequent annual count and occurrence of regulator-recorded disclosure violations, conditional on prior violation history and observed firm, board, ownership, audit, industry, and calendar characteristics.

4.4 Does audit-committee architecture explain the state-ownership gradient?

A mechanism claim requires evidence that changes when the proposed channel is introduced. If audit-committee architecture is the principal observed pathway, conditioning sequentially on existence, size, meetings, independence, expert leadership, chair experience, and legal expertise should materially reduce the magnitude of the state-ownership association with later sanctions. Stability of the coefficient would instead indicate that the observed committee variables do not account for the gradient, even if unobserved governance channels remain possible.

This attenuation design is intentionally modest. It does not interpret a sequence of conditional coefficients as causal mediation because state ownership and committee design are jointly selected. Its role is diagnostic: to determine whether the original simple transmission story is consistent with the observed architecture.

RQ2. Does conditioning sequentially on audit-committee existence, size, meetings, independence, expert leadership, chair experience, and legal expertise materially attenuate the association between state ownership and subsequent recorded disclosure violations?

5 Research design

5.1 Sample construction, timing, and support

The source panel combines annual reports, corporate-governance reports, audited financial statements, ownership and board disclosures, audit information, and formal disclosure-enforcement records. It contains 467 nonfinancial firms listed on HOSE and HNX that satisfy the study's governance, ownership, financial, audit, and enforcement-data requirements for each year from 2020 through 2024. The balanced panel therefore contains 2,335 firm-year observations. The study deliberately describes this eligibility-defined population rather than reporting a larger market-wide count that includes firms for which the required constructs cannot be measured consistently.

The sample boundary also reflects the institutional timing of the audit-committee construct. The Law on Enterprises 2020 more explicitly formalized the board-centered audit-committee architecture, and few listed companies had established and consistently disclosed the structure during the early post-reform years. Validated audit-committee counts are 52 in 2020, 76 in 2021, 89 in 2022, 96 in 2023, and 93 in

2024. Among the 415 firms without a validated committee at baseline, 49 first adopt during 2021–2024. These numbers are treated as substantive support constraints rather than as deficiencies to be hidden.

The strict temporal design links state ownership and controls measured at $t - 1$ to governance at t and disclosure enforcement at $t + 1$. It contains 1,400 observations across all 467 firms. One observation is lost because lagged board independence is unavailable. Across the full panel there are 208 recorded violations, 133 positive firm-years, and 74 firms with at least one violation. In the strict sample, only 50 firms contribute positive future-outcome information to conditional Poisson fixed-effects estimation. This sparse outcome motivates parsimonious count models and careful support reporting.

State ownership is also persistent. There are 622 state-positive firm-years and 135 firms with state ownership at least once. Only 27 firms vary in the exact lagged state share, and only two of those firms also contribute outcome information to the conditional fixed-effects state-share coefficient. The paper therefore distinguishes a broad between-firm configuration estimand from a poorly supported within-firm treatment-like estimand.

Table 2
Sample construction, timing, and identifying support

Audit item	Observed support	Identification implication
Source panel	2,335 firm-years; 467 firms; 2020–2024	Eligible HOSE/HNX nonfinancial firms with complete required constructs
Validated AC prevalence	52, 76, 89, 96, 93 firms in 2020–2024	Early institutional diffusion; committee support remains limited
Baseline non-AC risk set	415 firms; 49 first adoptions by 2024	Extensive-margin H1 population
Strict $t - 1/t + 1$ sample	1,400 observations; 467 firms	Common prospective timing for H2/RQ2
Full-panel enforcement support	208 violations; 133 positive firm-years; 74 positive firms	Sparse count outcome
State-positive support	622 firm-years; 135 firms ever state-linked	Persistent ownership regime is economically relevant
Exact state-share change	27 state-varying firms	Within-firm variation is thin
Joint within/outcome support	2 firms; 6 observations	Within-state FE coefficient is support-fragile
<i>Note.</i> AC = audit committee. The source panel includes all eligible firms satisfying the stated data requirements; no larger exchange-wide headline population is used. The 2020–2024 window reflects the early implementation of the contemporary board-centered architecture.		

5.2 Variables and measurement

State ownership (ST) is the proportion of equity held by the state. ST10 rescales the variable so that one unit represents ten percentage points. STATE_LINK equals one when state ownership is positive and is the primary exposure for H1 because the baseline adoption evidence is better described by a regime distinction than by a linear dose-response. A majority-state indicator is retained only as a functional-form sensitivity and is not treated as a regulatory threshold.

The primary enforcement outcome, FDV, is the annual count of disclosure violations formally sanctioned by the securities regulator and assigned to the year of the underlying violation in the project data. VD equals one when FDV is positive. The count ranges from zero to three in annual observations. The construct captures recorded enforcement burden rather than latent reporting quality, misconduct severity, or regulatory detection probability.

Audit-committee existence is validated using the project indicator and committee-member information. One raw observation combines AC = 0 with positive committee attributes; the validation rule sets AC to one when the raw indicator equals one or committee size is positive, without overwriting the underlying source fields. SIZEAC is the number of committee members, ACM the number of meetings, ACI the number of independent members, ACCAE expert-chair status, ACCEE chair experience, and NACEXP legal expertise. Intensive-margin models use only firms with an audit committee in both $t - 1$ and t so that structural zeros are not mistaken for low committee capacity.

The conditioning set is intentionally transparent rather than mechanically saturated. Firm characteristics include size, age, profitability, and leverage. External assurance is represented by Big Four auditor status and, in sensitivity analysis, modified audit opinion. Board controls include board size, board independence, and board financial expertise. Ownership controls include foreign, managerial, family, and concentrated ownership. Prior FDV absorbs observed persistence in enforcement exposure. ROA is winsorized at the 1st and 99th percentiles within year; the leverage measure is retained as coded in the source project. These variables address observable rival explanations but cannot eliminate unobserved selection. Their inclusion is consistent with prior work on corporate transparency, financial-reporting environments, and endogenous board design (Bushman et al. 2004; Beyer et al. 2010; Brown et al. 2011), while firm-level clustering follows the finance-panel guidance of Petersen (2009).

Table 3
Variable definitions and analytical roles

Code	Construct / operational definition	Analytical role
ST / ST10	State-owned equity share; ST10 = 10-percentage-point units	Focal ownership intensity
STATE_LINK	1 when ST > 0; 0 otherwise	Baseline state-regime indicator
FDV / VD	Annual count of regulator-sanctioned disclosure violations; VD = 1[FDV > 0]	Primary count / binary outcomes
AC	Validated audit-committee existence	Extensive-margin governance variable
SIZEAC	Number of audit-committee members	Intensive architecture
ACM	Annual audit-committee meetings	Intensive architecture
ACI	Number of independent audit-committee members	Intensive architecture
ACCAE	Expert-chair status	Intensive architecture
ACCEE	Audit-committee chair experience	Intensive architecture
NACEXP	Legal expertise on the audit committee	Intensive architecture
SIZE; AGE; ROA; LV	Scale, organizational age, profitability, leverage	Firm controls
BIG4; AO2	Big Four auditor; modified audit opinion	Assurance / alternative outcome
BSIZE; IB; BEXP	Board size, board independence, board financial expertise	Board controls
FO; MANOWN; FA; OWN	Foreign, managerial, family, and concentrated ownership	Ownership controls
<i>Note. Intensive audit-committee variables are interpreted only among continuing committees. The model does not combine them into an unvalidated effectiveness index.</i>		

5.3 Estimation strategy

H1 is estimated first as a baseline cumulative logit on the 415 firms without a validated audit committee in 2020. The dependent variable equals one if the firm first enters the architecture by 2024. Exposures and controls are frozen at 2020, which preserves temporal ordering. A discrete-time complementary-log-log hazard then retains the annual risk-set structure through the first adoption or 2024. The two estimators answer closely related extensive-margin questions without treating repeated years after adoption as new adoption decisions.

H2 uses Poisson pseudo-maximum likelihood (PPML) for the nonnegative next-year count with firm-clustered robust covariance. PPML targets the conditional mean and does not require the conditional

variance to equal the conditional mean for quasi-maximum-likelihood consistency when the mean is correctly specified (Gourieroux et al. 1984; Santos Silva and Tenreiro 2006; Cameron and Trivedi 2013). A binary logit for any next-year violation provides an occurrence check. Calendar and industry effects absorb common temporal and broad sectoral differences.

The core prospective conditional mean is summarized as: $E(FDV_{i,t+1} | X_{i,t}) = \exp[\gamma ST10_{i,t-1} + \delta FDV_{i,t-1} + \theta' Z_{i,t-1} + \lambda_t + \kappa_t]$. The focal coefficient γ is reported as an incidence-rate ratio for a ten-percentage-point increase in state ownership. The expression is an estimating equation, not a structural causal model.

RQ1 is estimated among 304 continuing-committee observations from 96 firms. Count-valued committee attributes use PPML, expert-chair status uses logit, and first-difference diagnostics ask whether annual changes in state ownership line up with annual changes in committee characteristics. These estimates distinguish persistent level differences from within-firm reconfiguration.

RQ2 adds observed audit-committee architecture sequentially to the H2 count model. The state IRR is compared after AC existence, size, meetings, independence, expert leadership, chair experience, and legal expertise enter. The purpose is to test whether the proposed channel explains the ownership gradient in the observed data. Because ownership and committee design are not randomized, attenuation is not interpreted as a natural indirect effect.

A correlated-random-effects (CRE/Mundlak) decomposition separates each firm's mean state ownership from its annual deviation (Mundlak 1978; Bell and Jones 2015; Wooldridge 2019). A conditional Poisson firm-fixed-effects model is retained as a source-of-variation diagnostic. These estimators remove or parameterize time-invariant heterogeneity in different ways, but they do not solve time-varying confounding. Their credibility depends on actual within-firm support, which is explicitly reported.

Additional analyses are threat-specific. Overlap weighting targets observed covariate imbalance (Li et al. 2018), the majority-state measure probes functional form, the state-by-current-AC interaction probes a boundary condition, next-year modified opinion provides a discriminant outcome, and leave-one-industry-out estimates assess influence. No instrumental-variable or system-GMM specification is introduced because the data do not provide a defensible exclusion restriction or an exogenous ownership shock. This restraint is preferable to presenting a technically elaborate but weakly identified design as a solution to endogeneity (Wintoki et al. 2012).

Because this revision involved generative editorial assistance beyond mechanical copyediting, the authors should retain the following disclosure if it accurately reflects their use: Generative AI was used to assist with language revision, manuscript organization, and journal-specific presentation of author-generated material. It was not used to generate, alter, or estimate the study data or empirical results. The authors verified the final text, citations, references, tables, figures, and interpretations and retain full responsibility for the manuscript.

6 Empirical results

6.1 Descriptive evidence

Table 4 documents the formal-oversight puzzle before conditioning. State ownership averages 12.0% in the full source panel. The raw audit-committee prevalence is 10.3% among state-positive observations and 19.9% among non-state observations. Recorded violations are also less frequent: 2.7% of state-positive observations versus 6.8% of non-state observations record at least one sanction. Continuous state share correlates -0.122 with validated AC existence, -0.076 with FDV, and -0.080 with violation occurrence. These associations describe the sample; they are not adjusted estimates.

The time series shows why the reform window matters. Validated audit-committee prevalence increases from 11.1% in 2020 to 16.3% in 2021, 19.1% in 2022, and 20.6% in 2023 before standing at 19.9% in 2024. The pattern is consistent with early diffusion rather than a mature long-run equilibrium. Figure 2 follows first adoption among the 415 baseline non-adopters and shows that the gap between state-linked and non-state firms accumulates over the post-reform years.

Table 4
Descriptive evidence for the formal-oversight paradox

Statistic / year	State-linked	Non-state / total	Interpretation
Source state ownership	622 firm-years; 135 firms ever state-linked	1,713 non-state firm-years	State regime is persistent but not rare
Raw AC prevalence	10.3%	19.9%	Formal AC architecture is less prevalent under state linkage
Raw violation occurrence	2.7%	6.8%	Recorded enforcement is also less prevalent
2020 AC prevalence	—	52/467 = 11.1%	Beginning of observation window
2021 AC prevalence	—	76/467 = 16.3%	Post-reform diffusion
2022 AC prevalence	—	89/467 = 19.1%	Post-reform diffusion
2023 AC prevalence	—	96/467 = 20.6%	Peak observed prevalence
2024 AC prevalence	—	93/467 = 19.9%	Architecture remains limited
Pearson $r(\text{ST}, \text{AC})$	—	-0.122	Descriptive only
Pearson $r(\text{ST}, \text{FDV})$	—	-0.076	Descriptive only
Pearson $r(\text{ST}, \text{VD})$	—	-0.080	Descriptive only
<i>Note. Raw rates do not adjust for industry, calendar time, prior violations, firm characteristics, board architecture, audit, or other ownership dimensions.</i>			

Note

The curves use the 415-firm baseline risk set and plot actual first adoption through 2024. They are descriptive cumulative shares, not Kaplan–Meier causal effects or a difference-in-differences design.

6.2 State linkage and post-reform audit-committee adoption

H1 receives support at the extensive margin. Among 299 baseline firms without state ownership, 42 adopt an audit committee by 2024 (14.05%). Among 116 state-linked baseline firms, seven adopt (6.03%). In the fully adjusted baseline logit, state linkage has an odds ratio of 0.300 (95% CI 0.113–0.797, $p = .016$). Average model-implied cumulative adoption is 14.31% under non-state status and 5.52% under state-linked status, a difference of 8.79 percentage points.

The discrete-time hazard yields a similar association. With baseline exposures and controls frozen at 2020 and firms followed until first adoption or 2024, the complementary-log-log ratio for state linkage is 0.324 (95% CI 0.123–0.858, $p = .023$). The result is therefore not created by collapsing four post-reform years into one cumulative indicator. Continuous state ownership is weaker in the baseline adoption logit: each additional ten percentage points has $OR = 0.837$ ($p = .105$). The contrast indicates that the observed data fit a state-regime distinction more clearly than a simple linear ownership dose-response.

Figure 3 translates the main models into economic magnitude. The state-linked/non-state adoption gap is large relative to the low baseline adoption rate. The prospective enforcement model implies an approximately 21.4% reduction in expected next-year recorded violations for a ten-percentage-point increase in state share. Both quantities remain conditional observational associations; neither is a counterfactual policy effect.

Table 5
State linkage and post-reform audit-committee adoption

Model / estimand	State contrast	Ratio	95% CI	p	Support / magnitude
Unadjusted cumulative adoption	Non-state	—	—	—	42/299 = 14.05%
Unadjusted cumulative adoption	State-linked	—	—	—	7/116 = 6.03%
Adjusted baseline logit	STATE_LINK	OR 0.300	0.113–0.797	.016	N = 415; 49 events
Adjusted predicted adoption	Non-state status	—	—	—	14.31%
Adjusted predicted adoption	State-linked status	—	—	—	5.52%; difference = -8.79 pp
Discrete-time cloglog hazard	STATE_LINK	0.324	0.123–0.858	.023	1,546 at-risk firm-years; 49 events
Baseline continuous logit	+ 10 pp ST	OR 0.837	0.675–1.038	.105	Linear dose-response sensitivity

Note. All adoption models use baseline non-adopters and 2020 exposures/controls. The estimates are associational and do not identify the causal effect of state ownership on adoption.

Note

The first two quantities are adjusted cumulative audit-committee adoption probabilities from the baseline model. The third is the percentage change implied by the strict PPML state-ownership IRR for a ten-percentage-point increase in state share. These are model-based associations, not treatment effects.

6.3 State ownership and intensive audit-committee architecture

RQ1 asks whether the lower extensive-margin adoption rate is accompanied by broadly weaker committee design among firms that already maintain committees. The answer is no. In the 304 continuing-committee observations from 96 firms, state ownership does not precisely predict committee size, meeting frequency, independent membership, expert-chair status, or legal expertise. Chair experience is positively associated with state ownership in levels, but the corresponding first-difference estimate is imprecise.

This asymmetry matters. A persistent level association with chair experience can reflect stable firm sorting, appointment history, or ownership-linked retention patterns rather than an annual response to changes in state ownership. None of the first-difference p-values is below .05. The intensive evidence therefore does not support a broad story in which state-linked firms systematically adopt smaller, less active, less independent, or less expert committees once the architecture exists.

Table 6
State ownership and intensive-margin audit-committee architecture

Outcome	Level ratio	95% CI	p	First-difference p	Interpretation
AC size	IRR 1.018	0.992– 1.046	.175	.554	No precise state-linked difference
AC meetings	IRR 0.942	0.852– 1.041	.244	.456	No precise state-linked difference
Independent members	IRR 0.989	0.945– 1.035	.635	.728	No precise state-linked difference
Expert chair	OR 0.889	0.638– 1.240	.489	.443	No precise state-linked difference
Chair experience	IRR 1.186	1.061– 1.326	.003	.828	Persistent level association; no change response
Legal expertise	IRR 1.111	0.900– 1.372	.328	.273	No precise state-linked difference
<i>Note. N = 304 continuing-committee firm-years from 96 firms. Ratios are scaled to the state-ownership contrast used in the current PB03 analysis. First-difference p-values test annual reconfiguration and do not support a broad intensive-margin response.</i>					

6.4 State ownership and subsequent recorded disclosure enforcement

H2 is supported in the prospective pooled specifications. In the strict PPML model, a ten-percentage-point increase in lagged state ownership is associated with an IRR of 0.786 (95% CI 0.641–0.964, $p = .021$). On the multiplicative scale, this corresponds to approximately 21.4% fewer expected next-year recorded violations. The binary model returns the same point ratio, OR = 0.786 (95% CI 0.637–0.970, $p = .025$), indicating that the result is not driven only by repeated counts among positive observations.

A baseline-to-postperiod design gives the same direction. Using 2020 state ownership to predict the 2021–2024 cumulative enforcement count yields IRR = 0.802 ($p = .010$); predicting whether any postbaseline violation occurs yields OR = 0.737 ($p = .004$). These prospective specifications improve temporal ordering but do not eliminate stable unobserved differences between state-linked and non-state firms.

The source-of-variation audit is therefore critical. In the CRE/Mundlak PPML, the firm-mean state component remains below one (IRR = 0.822, $p = .029$), while the annual within-firm deviation is above one (IRR = 4.116, $p < .001$). Conditional Poisson firm fixed effects also produce a positive within direction (IRR = 2.520, $p = .002$). The apparent precision of those within estimates is misleading if support is ignored: only 27 firms vary in lagged state share, only 50 contribute positive future-outcome information, and only two firms—six observations—satisfy both conditions.

The reversal is therefore adverse evidence against a causal interpretation of the pooled negative gradient. It is not evidence that increasing state ownership harms compliance. The broad panel is informative about persistent ownership configurations; it is not capable of identifying what would happen if the same firm experienced an exogenous ten-percentage-point increase in state share. This is precisely the distinction between endogeneity diagnostics and causal identification.

Table 7
State ownership and subsequent recorded disclosure enforcement

Model / estimand	State contrast	Ratio	95% CI	p	Support
Strict PPML count	+ 10 pp ST at $t - 1$	IRR 0.786	0.641– 0.964	.021	1,400 obs; 467 firms
Strict binary logit	+ 10 pp ST at $t - 1$	OR 0.786	0.637– 0.970	.025	1,400 obs; 467 firms
2020→2021–2024 cumulative count	+ 10 pp baseline ST	IRR 0.802	0.679– 0.948	.010	467 firms
2020→2021–2024 any violation	+ 10 pp baseline ST	OR 0.737	0.597– 0.909	.004	467 firms
CRE between-firm component	+ 10 pp firm-mean ST	IRR 0.822	0.689– 0.980	.029	1,400 obs
CRE within-firm component	+ 10 pp annual deviation	IRR 4.116	1.963– 8.631	< .001	27 varying firms; only 2 outcome-informative
Conditional Poisson firm FE	+ 10 pp ST	IRR 2.520	1.386– 4.583	.002	149 obs / 50 outcome-positive firms; focal variation from 2 firms

Note. The within-state estimates are adverse identification diagnostics rather than treatment effects. State ownership is highly persistent; the data primarily identify persistent between-firm ownership configurations.

6.5 Mechanism falsification and robustness

RQ2 provides the central mechanism result. The baseline state-ownership IRR is 0.786. It becomes 0.794 after adding audit-committee existence, 0.796 after size, 0.797 after meetings, 0.796 after independence, 0.798 after expert-chair status, 0.792 after chair experience, and 0.793 in the full observed audit-committee architecture model. The total movement is less than one IRR percentage point. Figure 4 makes that stability visible.

The lack of attenuation is informative because it contradicts the simple channel the study initially considered. If observed audit-committee architecture were the principal mechanism linking state ownership to lower recorded enforcement, the state coefficient should move materially toward one when the channel is conditioned on. It does not. Combined with the weak intensive-margin evidence in Table 6, this result rejects the observed pathway state ownership → audit-committee capacity → fewer recorded violations as the main explanation in the current data.

The remaining diagnostics reinforce a bounded interpretation rather than a stronger causal claim. Overlap weighting produces a state-linkage IRR of 0.575 but with a wide 95% CI of 0.266–1.241 ($p = .159$), so the observed-overlap estimate is imprecise. The state-by-current-AC interaction ratio is 1.483 (95% CI 1.023–2.149, $p = .037$), but the strict-sample support cells are uneven: 336 state/no-AC, 41 state/AC, 803 non-state/no-AC, and 220 non-state/AC observations. The interaction is therefore a support-limited boundary diagnostic, not a universal moderation result.

A majority-state sensitivity produces IRR = 0.086 (95% CI 0.013–0.549, $p = .009$), but that binary split is not a threshold design and should not be interpreted as identifying a 50% ownership rule. Next-year modified audit opinion has OR = 0.914 ($p = .150$), providing no precise parallel association on this alternative audit outcome. Leave-one-industry-out IRRs range from 0.730 to 0.836; 22 of 23 estimates retain $p < .05$, while dropping industry 1 yields IRR = 0.836 with $p = .105$. The sign is not driven by one industry, but precision is not invariant to every deletion.

Taken together, the robustness analyses clarify threats rather than accumulate favorable p -values. The strongest evidence concerns a persistent state-linked configuration: less entry into a newly formalized audit-committee architecture and fewer subsequent recorded enforcement events. The weakest evidence concerns within-firm changes in state ownership, where support is exceptionally thin. The mechanism evidence is negative: the observed audit-committee variables do not explain the ownership gradient.

Table 8
Mechanism falsification and threat-specific diagnostics

Test / model	State estimate	95% CI / range	p	Inferential role
Baseline strict PPML	IRR 0.786	0.641–0.964	.021	Reference state gradient
+ AC existence	IRR 0.794	—	.028	Minimal attenuation
+ AC size	IRR 0.796	—	.032	Minimal attenuation
+ AC meetings	IRR 0.797	—	.033	Minimal attenuation
+ AC independence	IRR 0.796	—	.028	Minimal attenuation
+ Expert chair	IRR 0.798	—	.032	Minimal attenuation
+ Chair experience	IRR 0.792	—	.014	Minimal attenuation
Full observed AC architecture	IRR 0.793	—	.020	Observed AC channel does not explain gradient
Overlap-weighted state linkage	IRR 0.575	0.266–1.241	.159	Observed-selection sensitivity; imprecise
State × current AC	IRR 1.483	1.023–2.149	.037	Support-limited boundary diagnostic
Majority state	IRR 0.086	0.013–0.549	.009	Functional-form sensitivity only
Next-year modified opinion	OR 0.914	—	.150	Alternative audit outcome; imprecise
Leave-one-industry-out	IRR 0.730–0.836	23 deletions	22/23 < .05	Influence range; one deletion p = .105
<i>Note. The attenuation sequence compares nested observational specifications and is not causal mediation. Interaction and majority-state estimates are sensitivity diagnostics, not policy thresholds.</i>				

Note

Points show the state-ownership ratio and 95% confidence interval across the sequential conditioning set. The reference line is one. Similar point estimates across the sequence indicate that observed audit-committee architecture barely attenuates the state-ownership gradient.

7 Discussion

7.1 Interpreting the formal-oversight paradox

The results are easier to understand as an allocation problem than as a ranking of governance quality. State-linked firms enter the board-centered audit-committee architecture less frequently, yet higher state

ownership is associated with fewer subsequent recorded sanctions. Once an audit committee exists, the data do not show a broad state-linked pattern of lower committee size, independence, activity, expert leadership, or legal expertise. Observed committee architecture also does not account for the enforcement gradient. The three facts are jointly inconsistent with a simple model in which formal audit-committee capacity is the principal channel connecting state ownership to recorded compliance.

A governance-bundle interpretation is more consistent with the evidence. State linkage can mark a persistent institutional arrangement in which accountability is distributed across ownership, boards, internal control, audit, inspection, and other channels. The current data observe only part of that portfolio. They therefore support the proposition that the marginal meaning of a newly formalized board mechanism depends on the surrounding ownership regime, but they do not identify which unobserved channel substitutes for or complements the audit committee. This distinction preserves the theoretical insight without overclaiming a mechanism that is not measured.

The result also sharpens the formal-versus-substantive distinction. The adoption gap is clear at the extensive margin, while continuing-committee differences are mostly imprecise. This suggests that ownership regime is more strongly related to whether the new architecture is used than to a generalized thinning of its observable internal features after adoption. The positive level association with chair experience further cautions against treating state-linked committees as uniformly weak.

7.2 Contribution to finance, accounting, and governance research

For corporate-governance research, the main contribution is a source-of-variation distinction. A pooled ownership coefficient describes persistent differences across firms when the ownership structure itself barely changes. It should not be narrated as the effect of changing ownership within the same firm. The sign reversal in CRE and fixed effects makes that point concrete: statistical precision does not rescue an estimand supported by only two jointly informative state-changing firms. This is a general issue in governance research where ownership and board structures are slow-moving endogenous institutions (Adams et al. 2010; Wintoki et al. 2012).

For accounting and auditing research, the study demonstrates why formal committee existence, committee capacity, and enforcement outcomes should not be collapsed into one governance-quality scale. Audit committees coordinate with management, internal functions, and auditors, and their contribution depends on information, authority, expertise, and process (Beasley et al. 2009; DeFond and Zhang 2014; Livne et al. 2024). RQFA studies likewise show that board activity and two-tier characteristics are associated with reporting and audit information in ways that depend on the specific governance dimension being measured (Vafeas and Vlittis 2024; Noureldeen et al. 2024).

For quantitative finance and accounting, the methodological contribution is not a new estimator. It is disciplined alignment among estimand, variation, and support. PPML matches the sparse count outcome; baseline and hazard models isolate first adoption; continuing-committee models isolate the

intensive margin; CRE reveals whether an association is between or within firms; and fixed effects are interpreted only after auditing the firms that actually identify the focal coefficient. This ordering is more informative than presenting a battery of estimators as interchangeable robustness checks.

The Vietnam setting functions as a theory-revealing boundary condition rather than a country gap. Concentrated ownership, meaningful state participation, and the early implementation of the 2020 governance framework make alternative monitoring configurations visible within a common national environment. Analytical generalization therefore concerns settings in which ownership regimes embed different accountability structures while a formal board mechanism is diffusing; it does not imply that the Vietnamese coefficient should replicate mechanically elsewhere.

7.3 Practical implications

Regulators and exchanges should avoid using audit-committee existence alone as a universal score of oversight quality. A board-centered committee is an important accountability mechanism, but formal adoption and substantive monitoring capacity are separate. Evaluation should consider the broader accountability portfolio: board structure, committee composition and process, internal control, external audit, ownership oversight, and the institutional responsibilities attached to different ownership regimes.

For state-capital agencies and boards, the results suggest distinguishing the decision to enter the audit-committee architecture from the way an operating committee is staffed and used. The data do not justify slower adoption as a preferred policy, nor do they justify a numerical ownership threshold. They indicate that formalization and substantive capacity can evolve on different margins during institutional transition.

For investors and auditors, fewer sanctions should not be equated mechanically with higher reporting quality. Detection intensity, regulator attention, firm visibility, and self-reporting practices affect which failures enter the enforcement record. Ownership and audit-committee signals are more informative when interpreted together with prior sanctions, auditor indicators, internal-control information, and the institutional context in which monitoring is performed.

7.4 Limitations and future research

Four limitations define the boundary of inference. First, state ownership is highly persistent. The broad sample supports a persistent-regime association, but only two outcome-informative firms identify within-firm state-share change. A credible causal ownership design would require a stronger source of exogenous variation, such as well-defined divestment or ownership-reallocation events with adequate pre- and post-event support.

Second, regulator-sanctioned violations are observed enforcement events rather than a complete measure of misconduct. The data cannot separately identify deterrence, detection, regulatory attention, or sanctioning. Future work could link enforcement outcomes to restatements, internal-control

deficiencies, self-reported corrections, auditor communications, or regulator review intensity to distinguish these processes more directly.

Third, the post-2020 audit-committee window is short. This is the institutional reason committee support is limited, but it also restricts precision and external validity. Longer panels can reveal whether the adoption gap persists after the architecture matures, whether early adopters differ from later adopters, and whether committee attributes change once formal adoption becomes routine.

Fourth, the study does not observe the full alternative monitoring bundle in state-linked firms. Administrative oversight, state-capital representation, internal audit quality, inspection, controller authority, and informal information channels are potential components, but treating them as present merely because a firm is state-linked would be an untested assumption. Future data that measure those channels can turn the current configuration interpretation into a direct mechanism test.

These limitations are not reasons to discard the empirical pattern. They determine the level of claim the evidence can sustain. The present study can show that formal audit-committee entry, intensive committee architecture, and recorded enforcement do not align in the simple way implied by a single-mechanism model. It cannot identify a universal state-ownership treatment effect or the unobserved institutional channel that generates the configuration.

8 Conclusion

This study asks why state-linked firms can rely less on a newly formalized audit-committee architecture without exhibiting more subsequent recorded disclosure enforcement. Using 2,335 firm-year observations from 467 eligible Vietnamese listed nonfinancial firms during 2020–2024, it establishes three facts. State linkage at the 2020 baseline is associated with substantially lower first audit-committee adoption through 2024. Among continuing committees, state ownership does not systematically predict broad intensive weakening. Higher state ownership is also associated with fewer subsequent recorded violations in pooled and prospective specifications, while observed audit-committee architecture barely attenuates that association.

Together, these findings reject the simple observed pathway state ownership → audit-committee capacity → fewer violations. The more defensible interpretation is that state ownership marks a persistent governance regime in which firms allocate monitoring differently. The formal-oversight paradox is therefore a configuration result—less entry into one formal architecture alongside fewer recorded enforcement events—not evidence that audit committees are ineffective or that state ownership is causally beneficial.

The central identification caveat remains decisive. The negative pooled and between-firm gradients coexist with positive within-firm estimates, but only two outcome-informative firms change state share. The study therefore treats within-firm estimates as diagnostics that expose weak support rather than as treatment effects. Its contribution lies in showing how governance architecture, ownership regime, and

enforcement observability must be analyzed jointly and with explicit attention to where the identifying variation actually comes from.

Declarations

Author Contribution

N.H.P.N. developed the initial research idea and research design, collected and curated the data, conducted the empirical analyses, prepared the tables and figures, and drafted the manuscript. H.X.T. contributed to the conceptual development, methodological validation, interpretation of the findings, research supervision, and critical revision of the manuscript. Both authors contributed to the final review and editing of the manuscript, approved the final version, and accept accountability for the work.

Data availability

The analytical dataset and annotated Stata 17 code supporting the reported analyses will be made available to editors and reviewers in anonymized form during peer review and, subject to source-use and licensing requirements, deposited in an appropriate repository upon acceptance. The underlying annual reports, audited financial statements, governance disclosures, and public enforcement records are available from the listed companies, HOSE, HNX, and relevant Vietnamese regulatory sources.

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Figures

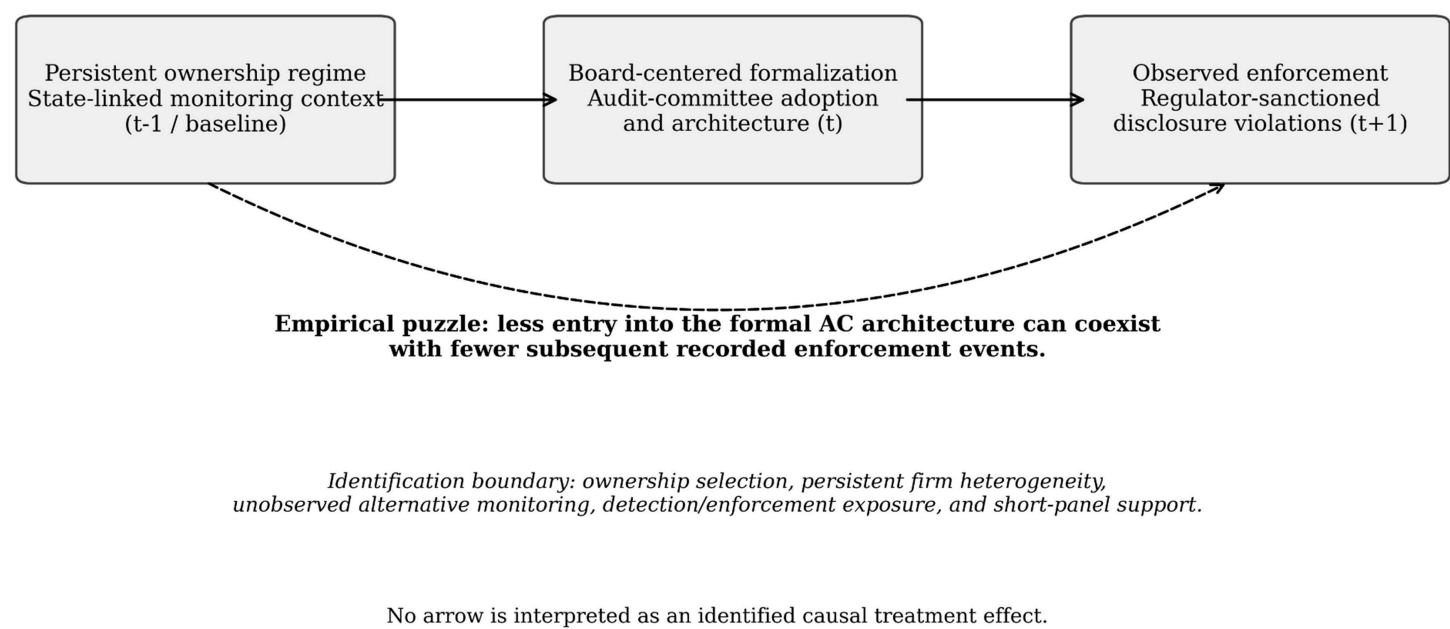


Figure 1

Alternative monitoring regimes and the identification boundary

Note. The figure distinguishes a persistent ownership regime, entry into the board-centered audit-committee architecture, and subsequent recorded enforcement. Solid and dashed pathways describe the empirical sequence and configuration possibilities. No arrow is interpreted as an identified causal treatment effect.

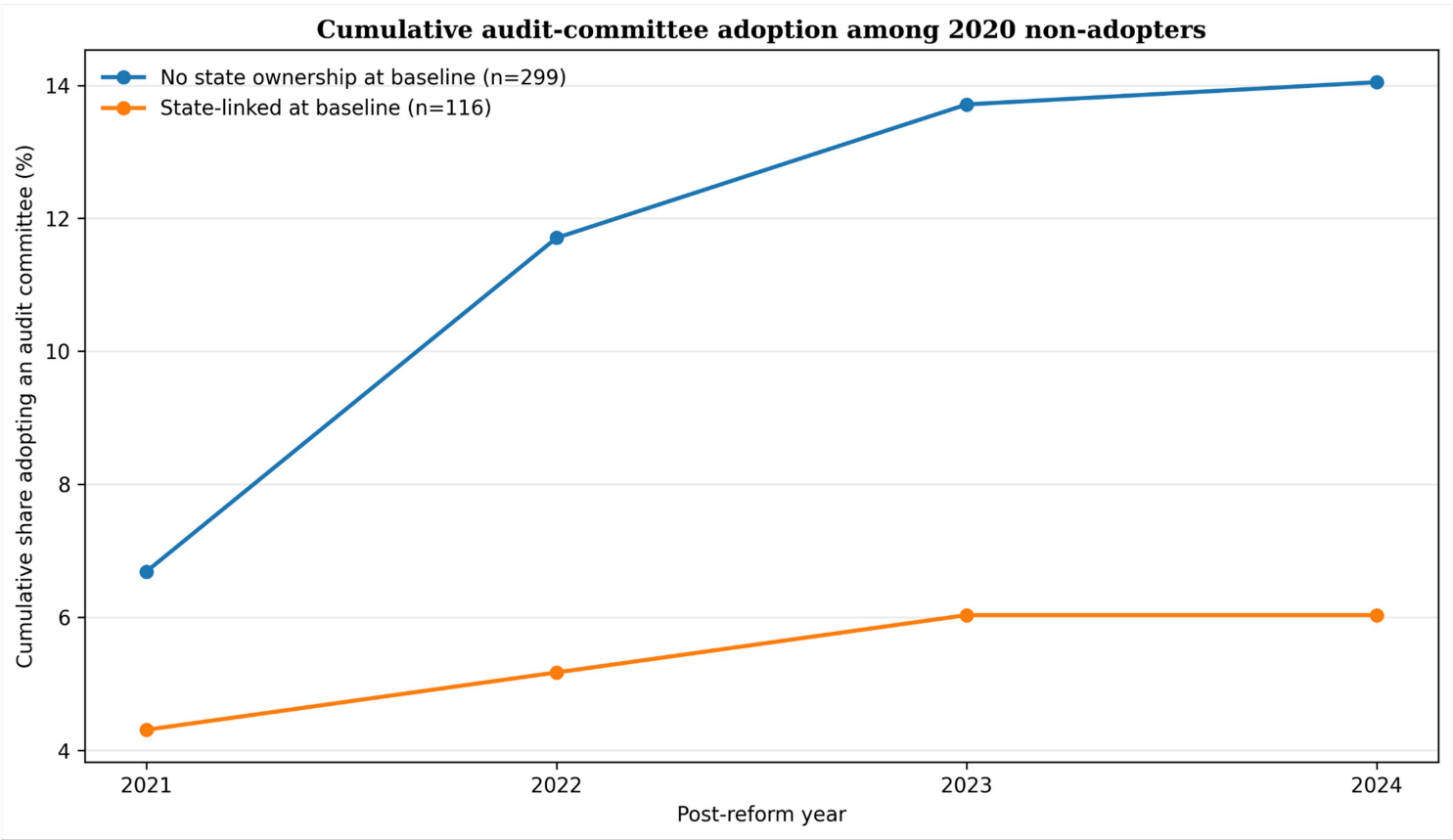


Figure 2

Cumulative audit-committee adoption among 2020 non-adopters

Note. The curves use the 415-firm baseline risk set and plot actual first adoption through 2024. They are descriptive cumulative shares, not Kaplan–Meier causal effects or a difference-in-differences design.

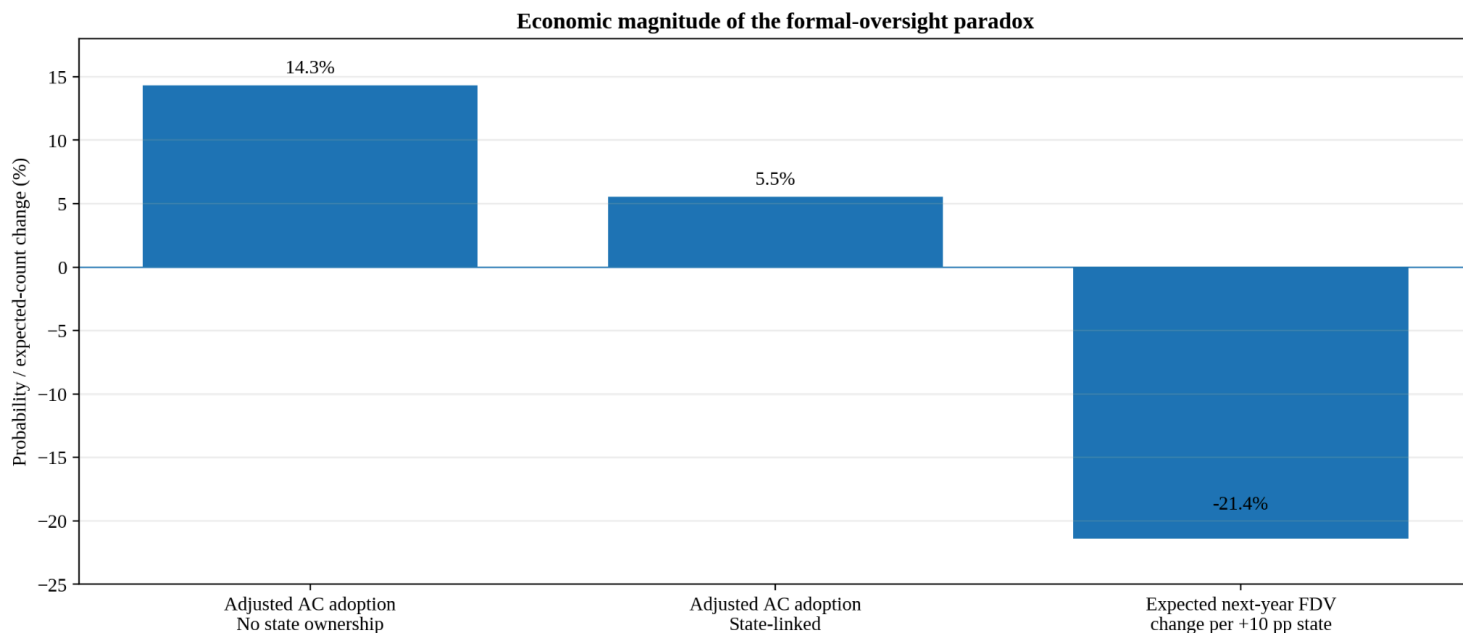


Figure 3

Economic magnitude of the formal-oversight paradox

Note. The first two quantities are adjusted cumulative audit-committee adoption probabilities from the baseline model. The third is the percentage change implied by the strict PPML state-ownership IRR for a ten-percentage-point increase in state share. These are model-based associations, not treatment effects.

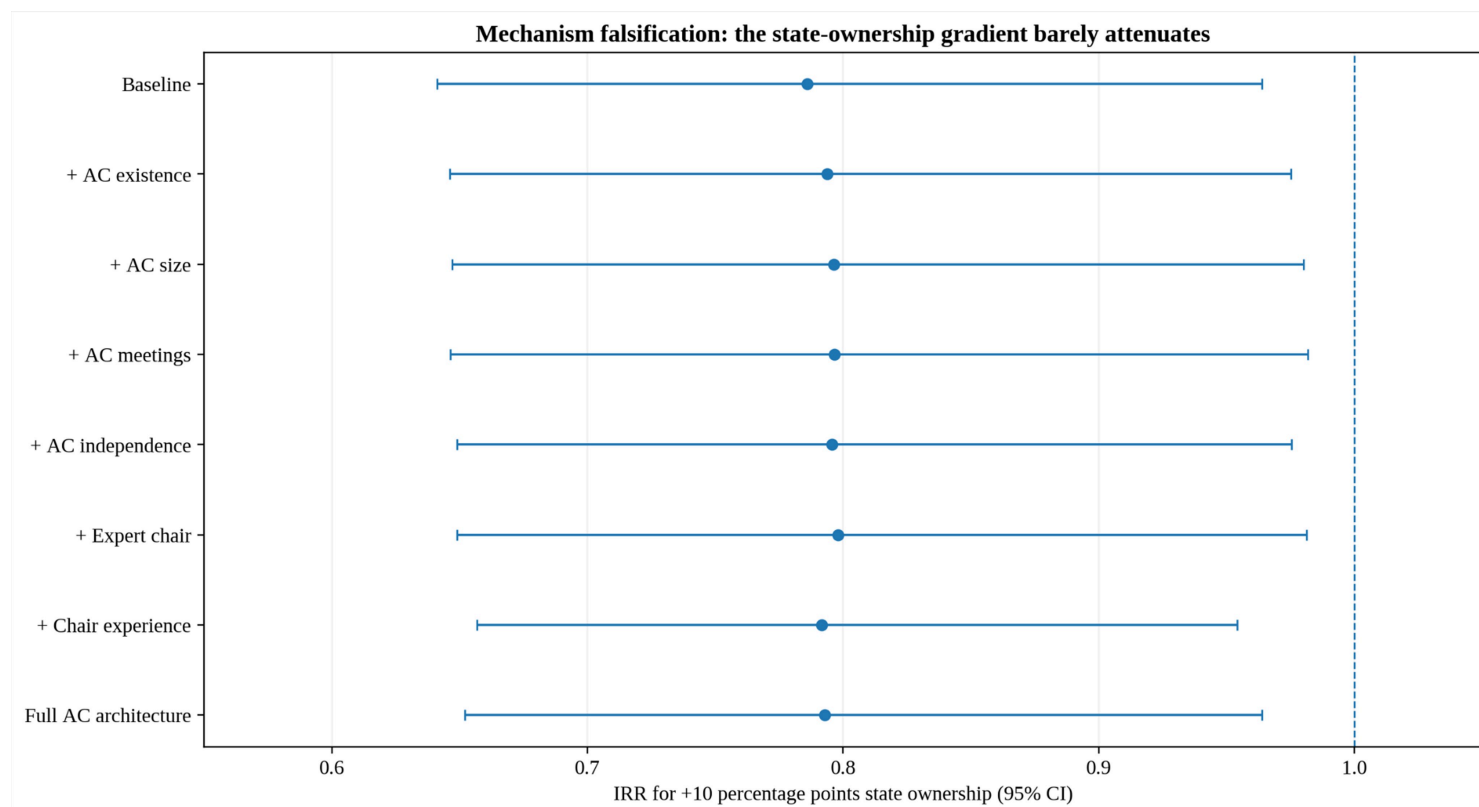


Figure 4

Mechanism falsification: attenuation of the state-ownership gradient after audit-committee controls

Note. Points show the state-ownership ratio and 95% confidence interval across the sequential conditioning set. The reference line is one. Similar point estimates across the sequence indicate that observed audit-committee architecture barely attenuates the state-ownership gradient