Leadership Practices and Innovation-Related Outcomes in Higher Education Institutions: An Audit-Ready Meta-Analysis

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ABSTRACT

Higher education institutions (HEIs) increasingly depend on innovation to navigate digital transformation, resource constraints, and rising performance accountability. Although leadership is widely viewed as a managerial lever for enabling innovation, empirical evidence in HEIs remains fragmented across overlapping leadership labels and unevenly defined innovation outcomes. This study synthesises quantitative evidence on the association between leadership practices and innovation-related outcomes in HEIs using a random-effects meta-analysis. A multi-database search of Web of Science, Scopus, ProQuest, CNKI, and Google Scholar identified 16 independent studies that met pre-defined inclusion and auditability criteria, retaining only extractable zero-order correlations with verifiable sample sizes. Leadership constructs were consolidated into three mechanism-based practice families (change/energising/developmental; enabling/condition-building; strategic orchestration/ambidexterity), and innovation outcomes were organised using a tiered framework spanning behavioural/process enactment and innovation performance/output. Effect sizes were synthesised on Fisher's z and back-transformed to r for interpretation. The pooled association between leadership practices and innovation-related outcomes was positive ($r \approx 0.52$, 95% CI ≈ 0.43–0.59), indicating that stronger leadership practice exposure is linked to higher innovation outcomes in HEIs. Heterogeneity was substantial ($I^2 > 90\%$), suggesting meaningful contextual variation across institutional settings and study designs. Subgroup analyses across leadership families, outcome tiers, and an institutional configuration proxy (China vs non China) were directionally consistent but did not provide decisive contrast evidence under modest subgroup sizes. Findings provide an evidence-based correlational benchmark for education leadership research and support practical governance guidance for university leaders seeking to strengthen innovation conditions.

Keywords: leadership practices; innovation-related outcomes; higher education institutions; mechanism-based coding; meta-analysis

INTRODUCTION

Higher education institutions (HEIs) face rising innovation pressures under environmental turbulence, including digital transformation, shifting knowledge production, constrained resources, and intensified performance accountability(Jameson et al., 2022; Sharma et al., 2024; Adeoye et al., 2025). In knowledge intensive universities, innovation is a governance challenge: novel ideas must be generated, selected, coordinated, and implemented across loosely coupled professional communities (Meyer, 2002; Langseth, Jacobsen, & Haugsbakken, 2023; Chen & Chen, 2013). From an innovation management perspective, the core problem is therefore how innovation-related activities can be governed through organisational arrangements that structure experimentation, knowledge recombination, and implementation capacity in professionally complex settings (Román-Cortéz et al., 2025; Jameson et al., 2022; Yassin et al., 2021).

Leadership is a salient managerial lever for shaping these innovation governance conditions in HEIs (RománCortéz et al., 2025; Jameson et al., 2022; Nong et al., 2025). Given professional autonomy, distributed expertise, and coordination demands across disciplinary and administrative boundaries, leadership influence is less likely to operate through command-and-control authority and more through practices that structure attention, legitimise experimentation, allocate resources, and reduce coordination frictions (Purcell et al., 2021; Mrayyan et al., 2024; Bendermacher et al., 2020). Accordingly, this study treats leadership as managerial practices and behavioural mechanisms relevant to innovation governance rather than as a catalogue of leadership labels or personality-driven styles (Soukupová & Kotková Stříteská, 2024; Awuah-Gyawu et al., 2025; Bian & Wang, 2024).

Despite strong theoretical plausibility, cumulative empirical inference remains difficult (Alvesson & Jonsson, 2018; Point et al., 2017; Tan et al., 2025). The higher-education leadership–innovation literature is fragmented by construct proliferation and label overlap, inconsistent operationalisation of innovation-related outcomes, and substantial variation in reported associations across contexts, samples, and outcome types (Jernigan & Carbonneau, 2025; Lajçi, 2025; Chiavaroli et al., 2020). Consequently, it remains unclear which leadership practices show directionally robust associations with innovation-related outcomes in HEIs, where along the innovation process such associations are more consistently observed, and whether effect magnitudes generalise across institutional environments—limiting audit-ready inference for innovation governance bodies (Reichenpfader et al., 2015; Evans et al., 2023; Yayeh et al., 2025).

Existing reviews of leadership in higher education frequently emphasise leadership styles (most prominently transformational leadership) and outcomes such as job satisfaction, commitment, organisational climate, or learning-oriented processes (Cummings et al., 2010; Toprak et al., 2023; Kim & Jeong, 2020). While informative, two limitations constrain their value for innovation management (Lyons et al., 2020; Restivo et al., 2022; Fosah & Llahana, 2025). First, many syntheses remain descriptive or conceptual rather than effectiveness-oriented, and therefore do not provide pooled association estimates, heterogeneity diagnostics, or structured comparative tests consistent with contemporary meta-analytic standards (Spaic et al., 2025; Jayasekara et al., 2006; Beaudry, 1989). Second, innovation-related outcomes are often peripheral rather than focal dependent variables, leaving unresolved which leadership mechanisms are most consistently associated with innovation-related outcomes in knowledge-intensive HEIs (Ardianti et al., 2024; Pearson et al., 2007; Dwyer, 2011).

To position the present study without overstating novelty, we conducted structured scoping searches across five major sources (Web of Science Core Collection, Scopus, ProQuest, CNKI, and Google Scholar) (Arksey & O'Malley, 2005; Peters et al., 2020; Tricco et al., 2018). Using conjunctive topic-collision criteria requiring simultaneous alignment on correlation-based synthesis, HEI context, leadership-as-governance mechanisms, innovation-related dependent variables, and an explicit purpose of aggregating leadership–innovation effect sizes (Pearson's r), no directly overlapping quantitative synthesis was identified (see Appendix A1; Table A1) (Moher et al., 2009; Haddaway et al., 2015; Page et al., 2021). This supports the need for an effectiveness oriented meta-analysis bounded by auditability and outcome comparability (Valentine et al., 2010; Gurevitch et al., 2018; Borenstein et al., 2021).

A further barrier to cumulative inference is outcome boundary discipline (Borsboom et al., 2009; Shepherd & Suddaby, 2017); Gerring, 2011). In higher education research, "innovation" is frequently conflated with adjacent domains such as entrepreneurial orientation, commercialisation, technology transfer, third-mission performance, or broad attitudinal and behavioural outcomes (Wu et al., 2025; Kopelyan, 2018; Bolzani et al., 2019). These constructs are treated as out-of-boundary for the present synthesis (Walker & Avant, 2005; Miles & Huberman, 1994). We therefore apply a disciplined outcome framework (Scheme A) distinguishing three tiers of innovation-related outcomes: Tier 1 behavioural/process enactment, Tier 2 innovation-enabling capability/process capacity, and Tier 3 innovation performance/output outcomes when operationalised explicitly as innovation results rather than generic performance indicators (Crossan & Apaydin, 2010; Adams et al., 2006; Damanpour & Aravind, 2012). Outcomes such as change readiness, organisational citizenship behaviour, generic performance, satisfaction, or climate are treated as out-of-boundary (West, 1997; Anderson et al., 2014; De Jong & Den Hartog, 2010).

Because HE systems differ in governance arrangements, managerial autonomy, resource allocation, accountability regimes, and norms regarding authority and risk-taking, leadership–innovation associations are also plausibly context-contingent (Stensaker et al., 2014; Gumport, 2000; Avolio et al., 2009). We therefore conduct an effectiveness-oriented meta-analysis of leadership practices and innovation-related outcomes in higher education using English- and Chinese-language searches (Moher et al., 2009; Higgins et al., 2022; Wang et al., 2024). The final audit-ready evidence base comprises eligible journal studies reporting reviewer verifiable leadership–innovation associations with traceable effective sample sizes, enabling correlation-based synthesis on the Fisher's z scale, and it spans the institutional configuration proxy (China vs non-China) to test effect transportability under an innovation-governance lens (Borenstein et al., 2021; Valentine et al., 2010; Alkhaledi et al., 2024).

This study contributes to innovation management scholarship in four ways. First, it benchmarks the pooled association between leadership practices and innovation-related outcomes in HEIs (Crossan & Apaydin, 2010; Anderson et al., 2014). Second, it moves beyond label-based comparisons by organising leadership constructs into mechanism-based leadership practice families reflecting functionally distinct innovation governance mechanisms (Santoso et al., 2025; Mokhtar et al., 2019; Gosling et al., 2016). Third, it examines whether pooled associations vary across innovation outcome tiers, clarifying where along the innovation chain associations appear more consistently observed (Damanpour, 2017; Jantz, 2012; Ortiz-Avram et al., 2023). Fourth, it tests an institutional contingency boundary condition by examining whether pooled associations differ systematically across China vs non-China; given modest subgroup evidence in some cells, moderator analyses are interpreted as exploratory and non-significant subgroup tests are not interpreted as evidence of equivalence (Borenstein et al., 2021; Hedges & Pigott, 2001; Lakens, 2017).

Accordingly, we address the following research questions:

RQ1 (overall association): What is the pooled association between leadership practices and innovation-related outcomes in HEIs?

RQ2 (leadership family differences): Do pooled associations differ across mechanism-based leadership practice families reflecting distinct innovation governance mechanisms?

RQ3 (outcome tier differences): Do pooled associations differ between more proximal behavioural/process outcomes and more distal performance/output outcomes along the innovation chain?

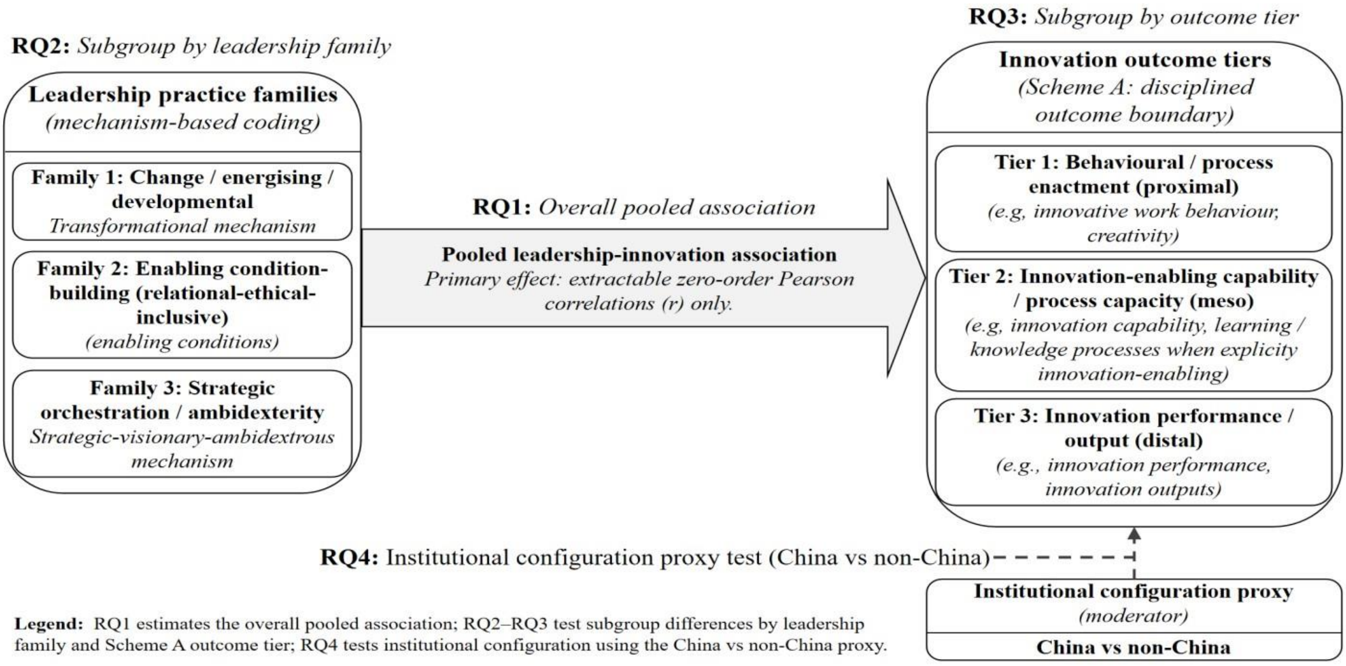
RQ4 (institutional contingency): Do pooled associations differ systematically between the institutional configuration proxy (China vs non-China), as a boundary-condition test of effect transportability?

Conceptual Framework and Coding Architecture

This chapter specifies an innovation-management-oriented coding framework for correlation-based synthesis of leadership practices and innovation-related outcomes in higher education institutions (HEIs), and for structured heterogeneity testing across leadership mechanisms, Scheme A outcome tiers, and institutional environments (Steel et al., 2021; Habersang & Reihlen, 2024; Jyoti, 2025). The framework is an auditable coding architecture (not a new theory) designed to prevent construct drift through two disciplined choices: (1) a tiered innovation outcome framework (Scheme A) restricting eligible outcomes to Tier 1 behavioural/process enactment, Tier 2 innovation-enabling capability/process capacity, and Tier 3 innovation performance/output defined explicitly as innovation results; and (2) mechanism-based leadership practice families that consolidate diverse leadership labels into interpretable families aligned with innovation governance mechanisms (Wright, 2025; Chen et al., 2025; Riess, 2022). Conceptually adjacent but non-equivalent outcomes (e.g., change readiness, organisational citizenship behaviour, generic performance, satisfaction, or climate) are treated as out-of-boundary for the primary synthesis (Anderson et al., 2014; West, 1997).

Figure 1 summarises the coding architecture: leadership practice families are linked to Scheme A outcome tiers, and institutional environment is examined as a pragmatic boundary condition for effect transportability, operationalised as the institutional configuration proxy (China vs non-China) (Volberda et al., 2012; Faraj & Yan, 2009; Ilyas et al., 2023). Effect sizes are synthesised on Fisher's z under random-effects models and back-transformed to correlations (r) for interpretation; the framework supports subgroup testing without implying causal direction (Borenstein et al., 2021; Valentine et al., 2010; Gelman & Hill, 2007)

Figure 1. Conceptual framework and coding architecture for the leadership–innovation meta-analysis in higher education institutions (HEIs).



Note. Leadership constructs were coded into three mechanism-based leadership practice families (Family 1: change/energising/developmental; Family 2 enabling/condition-building [relational–ethical– inclusive]; Family 3 strategic orchestration/ambidexterity). Innovation-related outcomes were coded using Scheme A tiers (Tier 1 behavioural/process enactment; Tier 2 innovation-enabling capability/process capacity; Tier 3 innovation performance/output). Institutional environment was operationalised as the institutional configuration proxy (China vs non-China). The diagram summarises the pre-specified coding logic for subgroup tests in a correlation-based synthesis and does not imply causal direction.

Innovation-related outcomes in higher education: a tiered outcome framework (Scheme A)

Innovation research distinguishes innovation as a process (generation, mobilisation, coordination, and implementation of novelty) from innovation as an outcome (realised innovation results) (Damanpour & Schneider, 2006; Crossan & Apaydin, 2010; Hussain et al., 2014). This distinction is consequential in HEIs because innovation is typically enabled through governance conditions (e.g., attention, coordination, norms, and resource access) rather than direct command of outputs (Hoidn & Kärkkäinen, 2014; Deem & Brehony, 2005; Yassin et al., 2021).

To preserve conceptual coherence and meta-analytic comparability, this study codes innovation-related outcomes using Scheme A tiers as an audit-ready device for correlation-based synthesis and tier-based heterogeneity testing (Moher et al., 2009; Borenstein et al., 2021; Tavasoli et al., 2025). Tiering structures outcomes by proximity to innovation enactment and does not imply causal ordering (Giancola & Viteritti, 2014; Schiepe-Tiska et al., 2021; Hagelskamp et al., 2013).

Tier 1 behavioural/process enactment outcomes. Tier 1 captures observable innovation-related behaviours and early-stage enactment, such as innovative work behaviour (IWB: idea generation, idea promotion/championing, and idea realisation/implementation) (Janssen, 2000; Alzawahreh, 2011; Hughes et al., 2018). Creativity-related measures (e.g., creativity, creative performance) and innovation-enactment interaction measures (e.g., innovation-related communication and collaboration for novelty) are coded as Tier 1 when they reflect participation in innovation processes rather than end-state results (Anderson et al., 2014; Rietzschel & Ritter, 2018; Lua et al., 2023).

Tier 2 innovation-enabling capability/process capacity outcomes. Tier 2 captures innovation-relevant capabilities and process infrastructures (e.g., innovation capability; innovation-related learning or knowledge integration capacity) and is eligible only when explicitly conceptualised and analysed as innovation-enabling capability/process capacity within Scheme A (Nisula & Kianto, 2013; Hull & Covin, 2010; Acharya, 2016).

Tier 3 innovation performance/output outcomes. Tier 3 captures realised innovation results (innovation performance, innovation outputs, or domain-specific innovation outcomes) only when operationalised explicitly as innovation-specific results rather than generic performance indicators (Rosenbusch et al., 2018; Bawa et al., 2023; Karna et al., 2016).

Coding rules for tier assignment (audit-ready). Outcomes were assigned using pre-specified decision rules: (i) Tier 1 if the measure reflects innovation behavioural/process enactment; (ii) Tier 2 if it reflects innovation enabling capability/process capacity and is explicitly framed as innovation enabling; and (iii) Tier 3 if it reflects innovation-specific performance/output results, excluding generic performance indicators (Moher et al., 2009; Jyoti, 2025; Faruq, 2025). Where multiple eligible outcomes were reported, the outcome most directly aligned with Scheme A boundaries was prioritised and documented in the audit trail (Rojon et al., 2021; Faruq, 2025; Moher et al., 2009).

Outcome boundary discipline reduces construct drift by excluding domains often conflated with “innovation” in higher education research (e.g., entrepreneurial orientation, commercialisation, technology transfer, and third-mission performance) (Compagnucci & Spigarelli, 2020; Clayton, 2019; Bogacz-Wojtanowska et al., 2022). Accordingly, only Scheme A outcomes were eligible for inclusion in the primary synthesis (Moher et al., 2009; Xu et al., 2021; Bouguettaya et al., 2025).

Leadership practices as innovation-governance mechanisms: a family-based coding logic

Leadership–innovation research in higher education is characterised by construct-label proliferation (e.g., transformational, inclusive, ethical, empowering, visionary, ambidextrous) and overlap in behavioural content and mechanism emphasis (Lee et al., 2020; Canavesi & Minelli, 2021; Crossan & Apaydin, 2010). Label-bylabel subgrouping would create sparse cells and underpowered contrasts, limiting mechanism-relevant inference for an IJIM audience (Naing et al., 2023; Zawacki-Richter et al., 2020; Rojon et al., 2021). Accordingly, this meta-analysis consolidates eligible leadership constructs into mechanism-based leadership practice families for synthesis and planned heterogeneity tests (Khosravi et al., 2019; Hao et al., 2023; ChoiLundberg et al., 2023). This family scheme is an ex ante meta-analytic coding device designed to maximise comparability, reduce fragmentation, and support mechanism-interpretable subgroup analyses under auditready standards; it is not proposed as a new leadership taxonomy (see Chapter 3; Appendix C2) (Habersang & Reihlen, 2024; Fendt, 2023; Moher et al., 2009).

Family assignment rule (audit-ready and reproducible). Leadership constructs were mapped into families using a hierarchical rule order that prioritises observable operationalisation over nominal labels (Conry-Murray et al., 2024; Ratajczyk et al., 2016; Madill et al., 2000): (1) scale item content and behavioural indicators, (2) construct definition reported in the study, (3) mechanism cues implied by the focal model, and (4) label-based assignment only as a last resort (Ni et al., 2022; Kinnunen et al., 2024). Ambiguous cases were resolved using a documented dominant-mechanism rule anchored in item content and recorded for independent reproducibility (Appendix C2) (Hila & Hauser, 2025; Conry-Murray et al., 2024; Ratajczyk et al., 2016). In HEIs, these mechanisms matter because leadership practices act as micro-level innovation-governance levers by shaping enabling conditions for experimentation and voice, coordinating and orchestrating resources, and supporting exploration–exploitation balancing in professionally autonomous communities of distributed expertise (Westerlund & Gaunt, 2021; Pursio et al., 2021; Al Abdulqader et al., 2025).

Family 1: change/energising/developmental. Practices emphasising meaning-making and motivational energising under uncertainty (e.g., vision articulation, intellectual stimulation, developmental support), legitimising experimentation and mobilising discretionary engagement (McKimm et al., 2022; Vilaine et al., 2025; Mansur & Jia, 2019).

Family 2: enabling/condition-building. Practices emphasising relational and procedural infrastructure (e.g., inclusion, ethical conduct, empowerment, voice and fairness), lowering interpersonal and procedural costs of experimentation by strengthening safety, trust, autonomy support, and voice (Nembhard & Edmondson, 2006; Dunn, 2025; Vilaine et al., 2025).

Family 3: strategic orchestration/ambidexterity. Practices emphasising strategic framing and coordination architectures (e.g., direction setting, portfolio orchestration, balancing exploration and exploitation), aligning priorities and orchestrating resources so dispersed ideas are more consistently coordinated and implemented toward innovation outcomes (Kassotaki, 2022; Wenke et al., 2020; Hodgkinson et al., 2014).

Canonical leadership taxonomies remain valuable, but the focal HEI literature contains multiple variants that do not map cleanly onto a single shared taxonomy; mechanism-based families preserve theoretical meaning while reducing fragmentation, enabling auditable contrasts under modest k (Appendix C2) (Lee et al., 2020; Pietsch et al., 2020; Crossan & Apaydin, 2010).

Sources of heterogeneity in leadership–innovation relationships

This meta-analysis treats heterogeneity as theory-relevant variance motivating pre-specified subgroup and moderator tests across: (i) leadership mechanisms (mechanism-based leadership practice families; Appendix C2), (ii) innovation outcome tiers (Scheme A; Appendix C3), and (iii) institutional configuration proxy (China vs non-China) (Fourné et al., 2019; Batra et al., 2022; Higgins et al., 2003). Together these dimensions define the coding architecture for structured heterogeneity testing in a correlation-based synthesis, with subgroup findings interpreted as theory-informing where category sparsity or within-group dispersion is substantial. Mechanistically, the three families represent distinct innovation-governance functions: change mobilisation and developmental momentum under uncertainty (Family 1), enabling conditions that support experimentation and voice (Family 2), and direction-setting plus orchestration across exploration and exploitation (Family 3) (Mukhopadhyay & Bouwman, 2019; Addo, 2022; Paschoiotto et al., 2024). Institutional configuration is operationalised solely by study-reported sampling location and interpreted as a pragmatic proxy rather than a cultural classification; for coding consistency, Taiwan is classified under China for the binary contrast

(Talukder, 2025; Moher et al., 2009; Tran & Freel, 2022). Baseline expectation (RQ1). The meta-analysis estimates an overall pooled leadership–innovation association in HEIs as a benchmark under the correlation-based synthesis design. Leadership mechanism heterogeneity (RQ2). Planned subgroup comparisons test whether pooled associations differ across leadership mechanisms (Family 1, Family 2, Family 3) under audit-ready standards. Outcome-tier heterogeneity (RQ3). Planned subgroup synthesis tests whether pooled associations vary across

Scheme A outcome tiers under the disciplined outcome boundary (Appendix C3). Mechanism–tier alignment along the innovation process. Family coding (Appendix C2) and Scheme A tier coding (Appendix C3) jointly structure heterogeneity tests by mechanism and outcome proximity while maintaining bounded interpretation and without implying causal sequencing.

Reproducible coding protocol for leadership family assignment

Leadership family assignment was implemented ex ante using the hierarchical rule order in Section 2.2 and executed as a dominant-mechanism classification anchored in scale-item content and behavioural indicators. To enable independent replication, Appendix C2 reports the construct-to-family mapping table, if–then decision rules, and worked boundary-case examples with study-specific evidence pointers recorded in the audit layer.

Institutional context as a boundary condition: institutional configuration proxy (China vs non-China) (RQ4)

Institutional arrangements shape decision rights, incentives, resource flows, and tolerance for experimentation; leadership–innovation associations in HEIs may therefore be context-bounded (Kurzhals et al., 2020; Mora et al., 2023; Mele et al., 2014). Because primary studies rarely report institutional features in a comparable, meta analytically harmonisable manner, this study operationalises institutional context using the institutional configuration proxy (China vs non-China) to enable an audit-ready boundary-condition test of effect transportability; the proxy is not interpreted as geography being causal (Polanin et al., 2020; López-Nicolás et al., 2021; Hill et al., 2023). Coding was based solely on study-reported sampling location as a binary grouping rule, with Taiwan classified under China, consistent with the pre-specified protocol (Chapter 3; Appendix C) (Shamseer et al., 2015; Wang et al., 2022; Strawbridge et al., 2025).

Research questions and mapping to meta-analytic tests

This meta-analysis maps four research questions to pre-specified synthesis and heterogeneity tests: RQ1 benchmarks the overall pooled leadership–innovation association; RQ2 tests family-level heterogeneity by comparing pooled associations across leadership practice families; RQ3 tests tier-level heterogeneity by comparing pooled associations across Scheme A outcome tiers; and RQ4 tests institutional contingency using the institutional configuration proxy (China vs non-China) as a boundary condition for effect transportability.

Pre-specified analytic anchors (audit-ready). The primary synthesis uses extractable zero-order Pearson correlations (r) only (Crocetti, 2016; Zhang et al., 2025; Higgins et al., 2003). Random-effects models are estimated on Fisher's z using REML with Hartung–Knapp–Sidik–Jonkman (HKSJ) adjustment and back transformed to r for interpretation (Röver et al., 2015; van Aert & Jackson, 2019; Veroniki et al., 2018). Moderator contrasts are interpreted as theory-informing where subgroup cells are sparse; non-significant subgroup tests are not treated as evidence of equivalence (Viechtbauer et al., 2015; Rubio-Aparicio et al., 2019; Guolo & Varin, 2017).

METHODOLOGY

Search strategy and data sources

We implemented a multi-database, multi-stage search to identify quantitative empirical studies reporting extractable associations between leadership practices and innovation-related outcomes in higher education institutions (HEIs) (Page et al., 2021; Bramer et al., 2017; Ab Wahab et al., 2024). The protocol prioritised audit-ready extraction of comparable effect sizes under a disciplined outcome boundary (Scheme A) and a mechanism-based coding model (leadership practice families) (Higgins et al., 2022; Hedges & Olkin, 2014; Kebede et al., 2024). Searches covered English- and Chinese-language evidence via Web of Science Core Collection, Scopus, ProQuest Dissertations & Theses Global, CNKI, and Google Scholar (Rethlefsen et al., 2021; Liu, 2025; Lin et al., 2025).

Scoping (topic-collision) searches. Before the inclusion search, we conducted scoping (topic-collision) searches across Web of Science Core Collection, Scopus, ProQuest, CNKI, and Google Scholar to assess whether any directly overlapping quantitative synthesis had already aggregated leadership–innovation relationships in HEIs (Tricco et al., 2018; Aboramadan et al., 2022; Mehboob & Haque, 2024). Candidate records had to jointly align with: (a) correlation-based quantitative effect-size synthesis, (b) higher education context, (c) leadership conceptualisation oriented toward innovation-governance mechanisms, (d) innovation related dependent variables, and (e) explicit aggregation of leadership–innovation effect sizes as Pearson correlations (r) synthesised on Fisher's z (Higgins et al., 2022; Rafique et al., 2022; Zhang et al., 2025). Scoping informed novelty positioning and search design but did not contribute to the effect-size evidence base (Tricco et al., 2018; Cooper et al., 2019; Liu, 2025).

Inclusion searches. The inclusion search was completed on 1 January 2026 across Web of Science Core Collection, Scopus, ProQuest Dissertations & Theses Global, CNKI, and Google Scholar (Page et al., 2021; Ab Wahab et al., 2024; Bramer et al., 2017). Two complementary query sets were implemented (Bramer et al., 2017; Rethlefsen et al., 2021; Musenze & Mayende, 2023). Query Set 1 combined explicit innovation-oriented leadership labels (e.g., "innovation leadership", "innovative leadership", "leadership for innovation") with higher education context terms (e.g., "higher education", universit*, college*, "tertiary education", HEI*). Query Set 2 expanded the leadership block to theory-relevant innovation-governance mechanisms commonly used in HEIs (e.g., transformational, empowering, participative, inclusive, ethical, visionary, ambidextrous, entrepreneurial, innovation-supportive leadership) while retaining the higher education context block; where supported, a population block (e.g., faculty, academic staff, lecturer*, professor*, university staff, administrative staff) was applied. No time limits were applied (database inception to the final search date)

(AlHusseini et al., 2019; Cooper et al., 2019; Higgins et al., 2022). Full reproducible database-specific search strings and filters are reported in Appendix A (Tables A1–A2) (Rethlefsen et al., 2021; Page et al., 2021; Sarwar et al., 2022).

Google Scholar coverage. Google Scholar was used as a supplementary coverage source; we screened the top 150 relevance-ranked records per query set on the fixed search date using a clean-browser setting (not logged in; cache cleared; English interface) (Gehanno et al., 2013; Haddaway et al., 2015; Rethlefsen et al., 2021).

Grey literature. Dissertations retrieved via ProQuest were screened using the same HEI-context requirements, Scheme A boundaries, and audit-ready extractability standards as journal studies, but were not retained in the final audit-ready synthesis pool (k = 16) (Paez, 2017; McAuley et al., 2000; Page et al., 2021).

Supplementary search procedures. To enhance coverage beyond database retrieval, we conducted backward reference checking for all studies retained after full-text eligibility assessment and for relevant non-colliding reviews identified during scoping (Greenhalgh & Peacock, 2005; Booth, 2016; Tricco et al., 2018). We also conducted targeted manual checks of selected journals spanning innovation management and higher education research (Booth, 2016; Bramer et al., 2017; Al-Husseini et al., 2019). These supplementary procedures were used to reduce the likelihood of missing eligible studies not captured by database indexing.

Study selection and eligibility criteria

Eligibility criteria were specified a priori and applied consistently across English- and Chinese-language studies (Page et al., 2021; Higgins et al., 2022; Rafique et al., 2022). Study selection proceeded through a three-stage workflow (title/abstract screening → full-text eligibility assessment → audit-ready extractability screening) (Page et al., 2021; Cooper et al., 2019; Bramer et al., 2017). The screening workflow and counts are summarised in the PRISMA-style flow diagram (Figure 2) (Page et al., 2021; Rethlefsen et al., 2021; Tricco et al., 2018).

PRISMA audit chain (locked anchors) (Page et al., 2021; Cooper et al., 2019; Rethlefsen et al., 2021). A total of 1,427 records were identified from the inclusion searches (Page et al., 2021). After removing 261 duplicates, 1,166 records remained for screening. Title/abstract screening excluded 1,050 records, retaining 116 for fulltext assessment (Page et al., 2021; Higgins et al., 2022). Full-text eligibility assessment excluded 31 articles, leaving 85 for audit-ready extraction (Page et al., 2021; Cooper et al., 2019). Audit-ready extraction excluded 69 records, yielding 16 independent studies for meta-analysis (k = 16) (Cooper et al., 2019; Hedges & Olkin, 2014; Higgins et al., 2022).

Study design and empirical requirements. Eligible studies employed quantitative empirical designs capable of estimating leadership–outcome associations (e.g., correlation/regression, SEM/PLS-SEM, multilevel models, experimental or quasi-experimental designs) and reported extractable effect-size information with a corresponding effective sample size (Higgins et al., 2022; Hedges & Olkin, 2014; Gurevitch et al., 2018). Purely qualitative studies, conceptual/theoretical papers, editorials/commentary pieces, bibliometric mapping studies, and narrative or non-quantitative systematic reviews were excluded (Page et al., 2021; Cooper et al., 2019; Booth, 2016).

Context and sample. Only studies conducted in HEIs (universities, colleges, or explicitly defined higher education institutions) were eligible (Ab Wahab et al., 2024; Al-Husseini et al., 2019; Lin et al., 2025). Samples comprised HEI employees (faculty/academic staff, administrative staff, or university employees). Studies conducted in K–12/school contexts and non-HEI organisational settings were excluded. Student-only samples were excluded unless students evaluated leadership behaviours of academic leaders within an HEI organisational structure (Cooper et al., 2019; Hunter et al., 2007; Higgins et al., 2022).

Independent variable: leadership practices. Studies were eligible if leadership was operationalised as an explanatory construct and a baseline leadership → innovation-related outcome association was empirically tested and extractable (Hedges & Olkin, 2014; Gurevitch et al., 2018; Kebede et al., 2024). Eligible leadership constructs were coded into mechanism-based leadership practice families using the reproducible protocol in Appendix C2 (Table C2.1; Rules C2.3) (Cooper et al., 2019; Booth, 2016; Hedges & Olkin, 2014).

Outcome scope and boundary discipline (Scheme A). Outcomes were restricted to innovation-related outcomes consistent with Scheme A and coded into three tiers: Tier 1 behavioural/process enactment; Tier 2 innovation enabling capability/process capacity (eligible only when explicitly operationalised as sustained innovation enabling capacity rather than generic organisational functioning); and Tier 3 innovation performance/output (eligible only when operationalised explicitly as innovation-specific performance rather than general effectiveness) (Crossan & Apaydin, 2010; Damanpour & Aravind, 2012; Anderson et al., 2014). Out-ofboundary outcomes (e.g., job satisfaction, commitment, wellbeing, generic performance not explicitly innovation-related, organisational citizenship behaviour, and adjacent domains such as entrepreneurial orientation, commercialisation, technology transfer, or third-mission performance) were excluded (Anderson et al., 2014; Arnold et al., 2021; Al-Husseini et al., 2019).

Meta-analytic feasibility and audit-ready extractability. Studies were required to provide auditable, extractable effect information linking leadership practices to eligible innovation-related outcomes (Higgins et al., 2022; Cooper et al., 2019; Page et al., 2021). The primary synthesis was based exclusively on extractable zero-order Pearson correlations (r) with a corresponding effective sample size (N) and a traceable evidence location (page/table/figure) (Hedges & Olkin, 2014; Hunter & Schmidt, 2004; Viechtbauer, 2010). Where correlations were not reported, baseline standardised effects could be converted only as a pre-specified contingency (e.g., β-to-r) with evidence pointers and an audit trail; this contingency rule was not triggered in the final audit-ready synthesis set (Peterson & Brown, 2005; Hunter & Schmidt, 2004; Higgins et al., 2022). Studies reporting non auditable statistics (e.g., latent-only associations not identifiable as zero-order effects, Fornell–Larcker matrices without zero-order correlations, or irreconcilable sample inconsistencies) were excluded at the auditready extraction stage (Hair et al., 2020; Higgins et al., 2022; Cooper et al., 2019). Eligibility criteria and Scheme A boundary rules are summarised in Table 1(Page et al., 2021; Tricco et al., 2018).

Table 1. Eligibility criteria and Scheme A boundary rules for study inclusion (Stage-3 audit-ready protocol)

Criterion	**Include**	**Exclude**	**Audit note (rule intent/examples)**
Study design	Quantitative empirical designs reporting testable associations (e.g., correlation/regression, SEM/PLS-SEM, multilevel, quasi/experimental)	Conceptual/theoretical papers; editorials; bibliometric mapping; narrative/non-quantitative reviews; purely qualitative studies	Must report a leadership–innovation association eligible for synthesis
Context	Higher education institutions (universities/colleges/HEIs)	Non-HEI settings; K–12/school contexts	HEI setting must be explicitly stated
Sample/unit	HEI employees (faculty/academic staff/administrative staff); students only when rating academic leaders within an HEI organisational structure	Student-only studies not evaluating leadership; non-organisational units	Unit must map to an HEI organisational context
IV (leadership practices)	Leadership operationalised as a focal explanatory predictor with an extractable leadership–innovation association	Leadership as DV; descriptive/background only; leadership included only as mediator/moderator without an extractable baseline association	Leadership must enter as a focal predictor for the focal effect

Outcomes (Scheme A)	Innovation-related outcomes within Scheme A tiers: Tier 1 behavioural/process enactment; Tier 2 innovation enabling capability/process capacity; Tier 3 innovation performance/output	Satisfaction/commitment/wellbeing; generic performance; OCB; entrepreneurship/commercialisation/tech transfer; third-mission outcomes not operationalised as innovation	Outcome must be explicitly innovation-related and Scheme Acompliant
Effect-size feasibility	Extractable zero-order Pearson correlation (r) with sample consistent effective N (N_effect) (required for primary synthesis)	No extractable effect; missing N; nonauditable statistics for zero-order association	Converted effects were pre-specified contingency only (not activated in final pool)
Stage-3 audit-ready extractability	Evidence pointer (page + table/figure) + extractable r + sample-consistent N_effect	Latent-only associations not auditable as zero-order	Reviewer verifiable extraction under the Stage-3 audit ready gate

Note. Scheme A restricts outcomes to innovation-related behaviours/process enactment (Tier 1), innovation enabling capability/process capacity (Tier 2), and explicitly innovation-specific performance/output outcomes (Tier 3). In the final audit-ready evidence base (k = 16), all primary effects were extracted as zero-order Pearson correlations (r) with N_effect; conversion procedures were pre-specified as contingency only and were not applied.

Screening procedure and final sample

Record consolidation and de-duplication. Records retrieved from the five inclusion-search sources (Web of Science Core Collection, Scopus, ProQuest Dissertations & Theses Global, CNKI, and Google Scholar) were exported and merged into a master dataset (Bramer et al., 2017; Booth, 2016; Page et al., 2021). Duplicate removal followed a two-step, audit-ready protocol: (i) exact-title matching to flag candidate duplicates and (ii) manual bibliographic verification (e.g., authors, year, outlet, DOI/URL) to confirm identity and prevent erroneous deletions (Bramer et al., 2016; Haddaway et al., 2015; Page et al., 2021). Numerical closure is locked across the PRISMA audit chain: 1,427 records retrieved; 261 duplicates removed; 1,166 unique records screened (Page et al., 2021; Rethlefsen et al., 2021).

Three-stage screening (locked PRISMA audit chain). Screening proceeded sequentially from conceptual eligibility to reviewer-verifiable extractability (Cooper et al., 2019; Higgins et al., 2022; Gurevitch et al., 2018). Stage 1 (title–abstract screening) applied minimal eligibility criteria (quantitative empirical design, HEI context, leadership as an explanatory predictor, and Scheme A–compliant innovation-related outcomes), retaining 116 records and excluding 1,050 (Page et al., 2021; Tricco et al., 2018). Stage 2 (full-text eligibility assessment) confirmed full eligibility and the feasibility of extracting a baseline leadership–innovation association with a traceable effective sample size (N_effect), preferably as a zero-order Pearson correlation (r), retaining 85 records and excluding 31(Hedges & Olkin, 2014; Hunter & Schmidt, 2004; Higgins et al., 2022). Convertible baseline statistics were permitted as a pre-specified contingency but were not activated in the final audit-ready pool. Stage 3 (audit-ready extractability screening) enforced traceability: each retained effect required an explicit evidence pointer (page/table/figure/cell) and a computable primary effect (r transformed to Fisher's z with the corresponding sampling variance, Var(z)) (Hedges & Olkin, 2014; Viechtbauer, 2010; Borenstein et al., 2009). Records were excluded when effects were not auditable as zero-order associations (e.g., latent-only reporting or discriminant-validity matrices), when effective N inconsistencies were irreconcilable, or when construct referents were not codable with sufficient clarity under the pre-specified coding manuals (Hair et al., 2020; Higgins et al., 2022; Cooper et al., 2019). After Stage 3, 16 independent studies met all eligibility and auditability requirements and were retained (k = 16) (Page et al., 2021).

Hard anchors for numerical closure (PRISMA audit chain).

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“A total of 1,427 records were identified from the inclusion searches.”

“After removing 261 duplicates, 1,166 records remained for screening.”

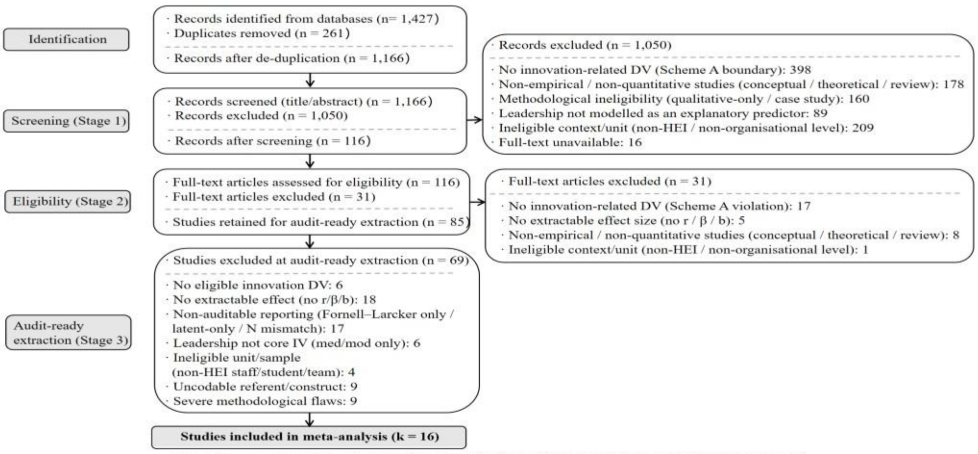
“Title/abstract screening excluded 1,050 records, retaining 116 for full-text assessment.”

“Full-text eligibility assessment excluded 31 articles, leaving 85 for audit-ready extraction.”

“Audit-ready extraction excluded 69 records, yielding 16 independent studies for meta-analysis (k = 16).”

Numerical closure reporting. Figure 2 reports the PRISMA-informed workflow including the audit-ready extractability stage, and Table 2 provides stage-wise counts and primary exclusion drivers for numerical closure. Exclusions were coded using a single primary driver (first-trigger rule) to prevent double counting (Appendix B) (Page et al., 2021; Booth, 2016; Tricco et al., 2018). Counts refer to records/studies rather than unique effect sizes; effect-size dependence is handled at extraction and synthesis (Section 3.4).

Figure 2. PRISMA-informed study identification and selection with an audit-ready extractability gate (final meta-analytic sample: k = 16).



Three-stage screening with audit-ready effect extraction (r transformed to Fisher’s z; random-effects models).

***Note.* The workflow comprises title/abstract screening (Stage 1), full-text eligibility assessment (Stage 2), and the Stage 3 audit-ready extractability gate. The Stage 3 gate requires reviewer-verifiable evidence pointers and computable primary inputs (r, Fisher’s z, and Var(z)) linked to a sample-consistent effective sample size (N_effect). Counts refer to records/studies (not unique effects); within-study multiplicity is addressed in dependence-robust robustness analyses as specified in the Methods(TannerSmith & Tipton, 2014; Hedges et al., 2010; Cheung, 2019).**

Table 2. PRISMA-informed screening and selection outcomes (numerical closure).

Stage	**Eligibility focus**	**Records retained**	**Records excluded**	**Primary exclusion driver (single-code)**
Deduplication	Merge records across sources; remove verified duplicates	1,166	261	Duplicates confirmed across sources
Stage 1: Title/abstract screening	Quantitative empirical; HEI context; leadership as explanatory predictor; Scheme A–compliant innovation-related DV	116	1,050	Non-eligible design/context; Scheme A boundary violation; leadership not explanatory; full text unavailable

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Stage 2: Fulltext eligibility assessment	Confirm eligibility and feasibility of extracting a baseline leadership–innovation association with a sampleconsistent N_effect (preferably zero-order Pearson r for the primary synthesis).	85	31	DV outside Scheme A; no extractable baseline association or missing N_effect; ineligible context/unit
Stage 3: Audit-ready extractability gate	Reviewer-verifiable evidence pointer; computable primary inputs (r, Fisher's z, Var(z))	16	69	Non-auditable reporting; N_effect mismatch; uncodable construct referent

Note. Exclusions were coded using a single primary driver (first-trigger rule) to prevent double counting (Appendix B). Counts refer to records/studies (not unique effects). The final audit-ready evidence base retained journal articles only (k = 16). Within-study multiplicity is addressed in dependence-robust robustness analyses as specified in the Methods.

Data extraction and coding scheme (audit-ready protocol)

Audit-ready coding architecture. Data were extracted using a pre-specified, audit-ready coding protocol to support correlation-based synthesis with full traceability (Cooper et al., 2019; Higgins et al., 2022; Rethlefsen et al., 2021). The protocol operationalised the Chapter 2 architecture by jointly coding: (i) leadership mechanisms via mechanism-based leadership practice families (Appendix C2), (ii) innovation outcomes via Scheme A tiers under the disciplined boundary (Appendix C3), and (iii) the institutional configuration proxy (China vs non-China) for boundary-condition testing (Appendix C) (Cooper et al., 2019; Rousseau, 2006; Arnold et al., 2021). Coding followed a three-layer structure: (1) a master study record (bibliographic and sample/context descriptors), (2) an effect-level record (eligible leadership–innovation associations), and (3) an audit layer (evidence pointers; effect-source/model-basis decisions; ambiguity-resolution notes; contingency rule flags) to enable reviewer-verifiable reproducibility (Cooper et al., 2019; Higgins et al., 2022; Gurevitch et al., 2018).

Inclusiveness–verifiability trade-off (Stage 3 gate) (Cooper et al., 2019; Higgins et al., 2022; Gurevitch et al., 2018). All Stage-2-eligible records were retained in an auxiliary log, including records later excluded at Stage 3 due to non-auditable reporting formats or irreconcilable statistic provenance (Page et al., 2021; Booth, 2016; Cooper et al., 2019). Dependence-robust and sensitivity checks drawing on the multi-effect library are reported in the Results chapter.

Coding dimensions and primary-effect rules. Leadership was coded as (a) the construct label reported in each study and (b) its mapped mechanism-based leadership practice family (Family 1; Family 2; Family 3) using the reproducible protocol in Appendix C2 (Hunter & Schmidt, 2004; Colquitt et al., 2013; Anderson et al., 2014). Innovation outcomes were coded to Scheme A tiers (Appendix C3); Tier 2 assignment followed a locked eligibility rule: learning- or knowledge-related constructs were coded as Tier 2 only when operationalised explicitly as innovation-enabling capability/process capacity (Crossan & Apaydin, 2010; Damanpour & Aravind, 2012; Anderson et al., 2014).

Effect-size extraction and contingency rules (locked). Effect sizes were extracted preferentially as zero-order Pearson correlations (r) with sample-consistent N_effect (Hedges & Olkin, 2014; Hunter & Schmidt, 2004; Viechtbauer, 2010). Where r was not reported, baseline standardised effects were eligible only as a prespecified contingency when they represented the closest minimally adjusted leadership–innovation association and could be converted using auditable rules with an evidence pointer; heavily controlled or mediation partialled paths were not selected when a baseline association was available (Peterson & Brown, 2005; Hedges & Olkin, 2014; Higgins et al., 2022). Conversion procedures were contingency-only and were not activated in the final audit-ready synthesis pool, because all retained primary effects were directly extractable as zero-order correlations.

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Multiplicity handling and auditability (Tanner-Smith & Tipton, 2014; Hedges et al., 2010; Cheung, 2019). When multiple eligible effects were reported within a study, all candidates were recorded at the effect level; one primary effect was selected for the main synthesis using a pre-defined hierarchy favouring baseline and minimally adjusted estimates, while additional eligible effects were retained for dependence-aware robustness analyses (Tanner-Smith & Tipton, 2014; Hedges et al., 2010; Cheung, 2019). Every extracted value was linked to an explicit evidence pointer (page number, table/figure label, and exact cell/row location), and key decisions (construct-to-family mapping, outcome-tier assignment, effect-source/model-basis prioritisation, and primary effect selection) were recorded in the audit layer (Cooper et al., 2019; Higgins et al., 2022; Rethlefsen et al., 2021).

Institutional configuration proxy coding (locked). Institutional context was coded using the institutional configuration proxy (China vs non-China) based solely on study-reported sampling location (Appendix C); Taiwan was coded under China to maintain a consistent China vs non-China contrast, consistent with the prespecified protocol (Rousseau, 2006; Arnold et al., 2021; Cooper et al., 2019).

Table 3 summarises the coding dimensions and operational definitions applied across study-level and effect level records; Appendix C2 and Appendix C3 provide the audit-ready coding manuals for family assignment and Scheme A tier rules, respectively.

Table 3. Audit-ready coding framework and operational definitions.

Coding dimension	**Coding rule (audit-ready definition)**
Bibliographic and context descriptors	Study identifiers and synthesis descriptors (e.g., year, outlet, publication type, sampling location).
Leadership construct and practice family	Reported leadership construct label mapped to a mechanism-based leadership practice family (Appendix C2).
Leadership role in model	Leadership coded as eligible when modelled as a focal explanatory predictor; moderator/mediator/auxiliary-only roles are eligible only if a baseline leadership–innovation association is extractable.
Outcome tier (Scheme A)	Innovation-related outcomes coded as Tier 1 behavioural/process enactment; Tier 2 innovation-enabling capability/process capacity (explicitly innovation-enabling); or Tier 3 innovation performance/output (explicitly innovation-specific) (Appendix C3).
Outcome operationalisation	Measurement indicator used for the focal innovation-related dependent variable.
Institutional configuration proxy	Sampling context coded as the institutional configuration proxy (China vs non-China) based on study location (Appendix C).
Effect source and model basis	Evidence source and analytic basis (e.g., correlation matrix vs baseline regression/SEM output); zero-order Pearson correlation (r) preferred for the primary synthesis.
Effect-size metric and N_effect	Extracted statistic (zero-order Pearson correlation (r); contingency-only baseline convertible statistic if applicable) with N_effect recorded as the effective sample size for that association.
Dependence flag	Primary effect (one-study–one-effect) versus additional eligible within-study effects retained for dependence-robust robustness analyses.

Audit layer (traceability)	Evidence pointers (page/table/figure/cell) and decision notes supporting construct mapping, tier assignment, effect selection, and auditability checks.

Note. Leadership constructs were consolidated into three mechanism-based leadership practice families (Appendix C2). Outcomes were coded into Scheme A tiers, with Tier 2 restricted to explicitly innovation enabling capability/process capacity constructs (Appendix C3). The institutional configuration proxy (China vs non-China) was coded from sampling location, with Taiwan coded under China for the binary contrast (Appendix C).

Effect-size metric and transformation

Common effect-size metric (zero-order Pearson correlation (r)). The common effect-size metric was the zero order Pearson correlation (r) (Hedges & Olkin, 2014; Hunter & Schmidt, 2004; Borenstein et al., 2009). All extracted correlations were transformed to Fisher's z prior to synthesis (Hedges & Olkin, 2014; Borenstein et al., 2009; Viechtbauer, 2010):

$$z = \frac{1}{2} \ln\left(\frac{1+r}{1-r}\right)$$

The corresponding sampling variance was computed as:

$$Var(z) = \frac{1}{N\_effect - 3}$$

where N_effect denotes the sample-consistent effective sample size for the extracted leadership–innovation association (Hedges & Olkin, 2014; Borenstein et al., 2009; Viechtbauer, 2010).

Audit-ready extractability requirement. An association was eligible for synthesis only if it was reviewer verifiable as a zero-order correlation with (i) an explicit evidence pointer (page + table/figure identifier + exact cell/row where applicable) and (ii) a verifiable, sample-consistent effective n (Higgins et al., 2022; Cooper et al., 2019; Page et al., 2021). Stage 3 exclusions were driven by non-auditable reporting formats (e.g., latent only associations without extractable correlations; Fornell–Larcker tables reporting AVE without recoverable inter-construct r; HTMT-only outputs lacking extractable correlations) or irreconcilable sample-size inconsistencies; an auxiliary log documented the boundary between evidence availability and verifiability (Hair et al., 2020; Fornell & Larcker, 1981; Henseler et al., 2015).

Pre-specified extraction hierarchy and conversion rules. Primary effect rule (locked). The primary synthesis was based exclusively on extractable zero-order Pearson correlations (r) (Hedges & Olkin, 2014; Hunter & Schmidt, 2004; Borenstein et al., 2009). A pre-specified extraction hierarchy governed any contingency fallback decisions (Table 4): (1) reported zero-order r; (2) SEM/PLS construct-correlation tables treated as r only when explicitly labelled as inter-construct correlations and n was verifiable (Hair et al., 2020; Higgins et al., 2022; Cooper et al., 2019); (3) contingency-only conversion of baseline standardised coefficients (β/standardised paths) representing the closest minimally adjusted association using a pre-specified approximation approach (e.g., Peterson & Brown, 2005) (Peterson & Brown, 2005; Hunter & Schmidt, 2004; Borenstein et al., 2009); and (4) contingency-only conversion of test statistics (t/F) when the baseline association and required parameters (df and n) were traceable (Rosenthal, 1991; Borenstein et al., 2009; Hedges & Olkin, 2014). Mediation-conditioned paths, total effects, and heavily controlled specifications were not used as primary inputs when baseline associations were available (Laczo et al., 2005; Hunter & Schmidt, 2004; Cooper et al., 2019).

Locked outcome for the final pool: in the final audit-ready evidence base (k = 16), all primary effects were directly extractable as zero-order correlations; conversion rules were retained ex ante but not activated.

Table 4. Effect-size extraction hierarchy and contingency conversion rules (audit-ready protocol)

Priority	Eligible source (preferred → fallback)	Accepted input	Conversion rule (if needed)	Exclusion / red-flag rule	Rationale (reviewerfacing)
1	Zero-order Pearson correlation r	Scheme Acompliant leadership–innovation r with traceable n + evidence pointer	None	Exclude if not a true zero-order correlation or n not verifiable	Highest comparability; least modeldependent
2	SEM/PLS construct correlations (r only)	Accept only if explicitly labelled as construct correlation matrix (interconstruct correlations) explicitly reported as Pearson r, with verifiable N_effect	None	Exclude HTMT-only; exclude Fornell–Larcker AVE without recoverable r	Retains SEMformat evidence while preserving auditability
3	Baseline standardised coefficient (β/std. b)	Contingency-only if r unavailable and baseline/minimally adjusted	β → r using pre-specified approximation (e.g., Peterson & Brown, 2005)	Exclude mediationconditioned/total/heavily controlled when baseline available	Minimises modeldependent partialling while retaining eligible evidence under contingency rules
4	Convertible test statistics (t/F)	Contingency-only if baseline association clear and df + n reported	Standard conversion (t/F→ r → z)	Exclude if provenance unclear or n unverifiable	Avoids assumptionheavy conversions
5	Non-auditable statistics	Not eligible for synthesis	Not applicable	Always exclude latentonly without extractable statistics; irreconcilable n	Explicit safeguard for reviewerverifiable reproducibility

Note. This hierarchy was specified ex ante to preserve audit-ready extractability and minimise modeldependent inputs (Page et al., 2021; Higgins et al., 2022; Cooper et al., 2019). The primary synthesis prioritised extractable zero-order Pearson correlations (r) with a traceable evidence pointer and sample consistent N_effect. Contingency conversions (β/test-statistics → r → Fisher's z) were retained only as rulebound fallbacks for transparency and reproducibility. In the final audit-ready evidence base (k = 16), all primary effects were directly extractable zero-order correlations, so conversion steps were not activated.

Primary effect selection for the main synthesis (one-study–one-effect). Independence rule (locked). One focal effect per study was selected for each analytical comparison (RQ1–RQ4) (Tanner-Smith & Tipton, 2014; Cheung, 2019; Hedges et al., 2010). When multiple eligible effects were available, selection followed a fixed tie-break procedure prioritising: (i) Scheme A compliance + auditability (verifiable n and evidence pointer), (ii) zero-order and baseline/minimally adjusted estimates over model-dependent paths, and (iii) the outcome most directly aligned with Scheme A boundaries (Page et al., 2021; Higgins et al., 2022; Laczo et al., 2005). Remaining eligible effects were retained for dependence-aware robustness checks (Appendix F).

Conversion safeguards and sensitivity logic. Converted effects (if used) were treated as approximations and permitted only under the rule-bound contingency hierarchy, with sensitivity analyses excluding converted effects pre-specified (Appendix F) (Borenstein et al., 2009; Hedges & Olkin, 2014; Viechtbauer, 2010). Because the final audit-ready pool (k = 16) provided extractable zero-order correlations for all retained effects, conversion-based sensitivity checks were not activated in the primary analyses.

Analytical strategy and model specification

Random-effects synthesis (locked). Random-effects models were estimated on Fisher's z using REML with HKSJ adjustment and back-transformed to r for interpretation (DerSimonian & Laird, 1986; Viechtbauer, 2010; Röver et al., 2015).

Heterogeneity and moderator tests (RQ2–RQ4). Between-study heterogeneity was evaluated using Cochran's Q and REML-based τ^2 (Higgins et al., 2003; Borenstein et al., 2009; Hedges & Olkin, 2014). Consistent with the Chapter 2 coding architecture, heterogeneity was examined through pre-specified categorical moderators using mixed-effects subgroup models: mechanism-based leadership practice families (RQ2), Scheme A outcome tiers (RQ3), and the institutional configuration proxy (China vs non-China) (RQ4) (Viechtbauer, 2010; Borenstein et al., 2009; Cooper et al., 2019). Moderator inference used omnibus tests (Q_M) alongside subgroup pooled estimates and uncertainty intervals (Borenstein et al., 2009; Viechtbauer, 2010; Hedges & Olkin, 2014); non-significant contrasts were not interpreted as evidence of equivalence, particularly under sparse subgroup cells (Altman & Bland, 1995; Hoenig & Heisey, 2001; Greenland et al., 2016). For the institutional configuration proxy, Taiwan was classified under China (locked rule).

Analyses were conducted in R (version 4.5.0) using metafor (Viechtbauer, 2010; R Core Team, 2024).

Publication bias assessment and robustness checks

Publication-bias risk was assessed as a diagnostic layer using funnel-plot inspection and small-study diagnostics (Egger et al., 1997; Sterne et al., 2011; Borenstein et al., 2009). Given the audit-ready evidence base (k = 16), Egger-type regression and trim-and-fill sensitivity checks were implemented, but interpreted as supportive rather than definitive because asymmetry tests can be unstable under modest k and substantial heterogeneity (Terrin et al., 2003; Peters et al., 2007; Sterne et al., 2011). Accordingly, publication-bias assessment was triangulated with robustness evidence (e.g., influence and dependence-robust checks) rather than treated as proof of no bias (Ioannidis, 2008; Rothstein et al., 2005; Tanner-Smith & Tipton, 2014).

Robustness analyses evaluated sensitivity to influential studies and extractability decisions, including leaveone-out analyses and influence diagnostics (Viechtbauer & Cheung, 2010; Borenstein et al., 2009; Hedges & Olkin, 2014). Sensitivity analyses excluding converted effects were pre-specified but not activated because all retained primary effects were extractable as zero-order Pearson correlations (r) in the final audit-ready pool (k = 16) (Borenstein et al., 2009; Hedges & Olkin, 2014; Viechtbauer, 2010).

Ethics statement. This study synthesises previously published research and required no new human/animal data collection; ethical approval and informed consent were not required (Moher et al., 2015; Page et al., 2021).

RESULTS

Included studies and dataset profile (audit-ready evidence base)

The final audit-ready evidence base comprised k = 16 independent studies, each contributing one reviewer verifiable focal effect to the primary synthesis. Under the Stage-3 audit-ready extractability gate (Chapter 3), eligible effects had to be (i) traceable to an explicit evidence pointer (page + table/figure reference), (ii) linked to a sample-consistent effective sample size (N_effect), and (iii) computable as meta-analytic inputs (r, Fisher's z, and Var(z)). The primary effect metric was restricted to extractable zero-order Pearson correlations (r) only; the gate therefore excluded non-auditable reporting formats (e.g., latent-only reporting) and sample inconsistent statistics.

Table 5 summarises the study-level profile of the audit-ready dataset, including mechanism-based leadership practice families, Scheme A outcome tiers, and the institutional configuration proxy (China vs non-China). Institutional configuration was coded from study-reported sampling location; Taiwan was classified under China under the pre-specified binary grouping rule (Chapter 3; Appendix C). Tier 2 innovation-enabling capability/process capacity outcomes were not present in the final audit-ready pool (k = 0) under the combined Scheme A Tier 2 eligibility rule and Stage-3 evidence-pointer requirements, and all retained focal effects were directly extractable as zero-order correlations (k = 16; 100%), with no conversion-based effects required.

Table 5. Descriptive profile of the audit-ready meta-analytic evidence base (k = 16 studies)

Characteristic	**Category**	**k**	**Percent (%)**
Institutional configuration proxy (China vs non-China)	China	4	25
	Non-China	12	75
Leadership practice family	Family 1: change/energising/developmental	8	50
	Family 2: enabling/condition-building	6	37.5
	Family 3: strategic orchestration/ambidexterity	2	12.5
Outcome tier (Scheme A)	Tier 1 behavioural / process enactment	12	75
	Tier 3 innovation performance / output	4	25
Effect source type	**zero-order Pearson correlation (r)**	**16**	**100**

Note. Counts are reported at the study level (k), with one focal effect per study retained for the primary synthesis under the Stage-3 audit-ready extractability gate. Percentages may not sum to 100 due to rounding. Tier 2 outcomes were not retained in the final audit-ready pool (k = 0).

Overall, the dataset provides an audit-ready, mechanism-disciplined, and Scheme A-bounded evidence base for estimating the pooled leadership–innovation association (RQ1) and planned heterogeneity across leadership practice families, outcome tiers, and China vs non-China (RQ2–RQ4).

RQ1 — Overall pooled association of leadership practices with innovation-related outcomes in HEIs

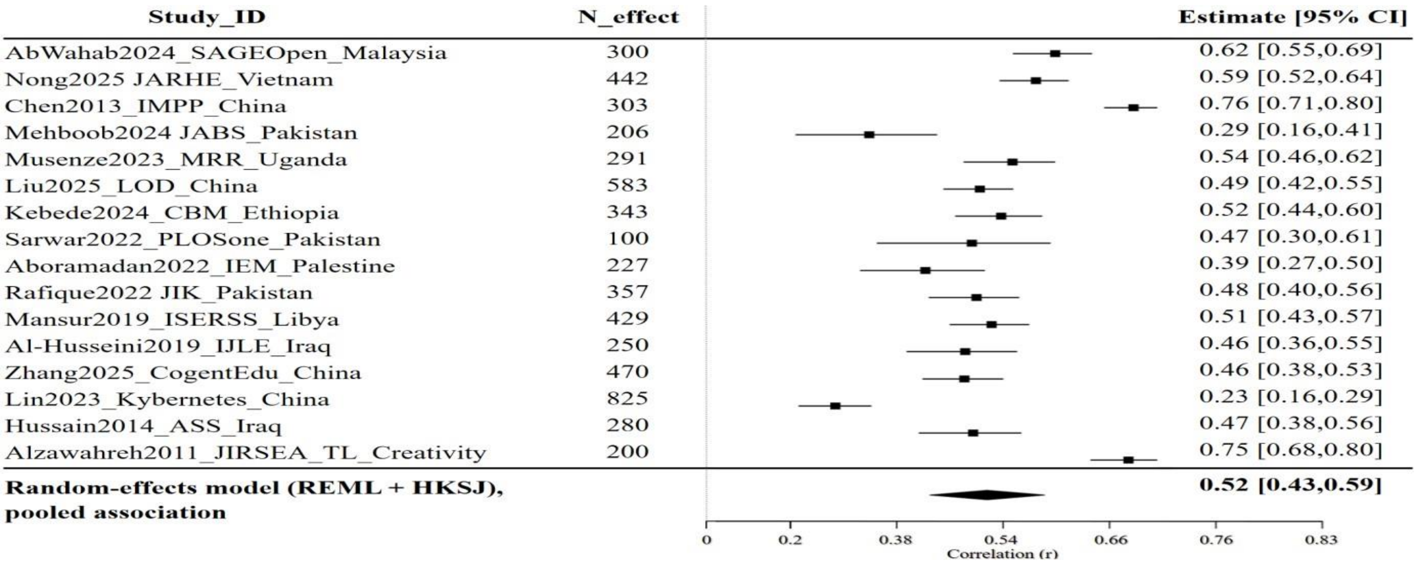
Using a one-study–one-effect rule (k = 16), correlations were synthesised on Fisher's z under random-effects models estimated via REML with HKSJ adjustment and back-transformed to r. The pooled association was r = 0.52, 95% CI [0.43, 0.59], $p < 0.001$ (Table 6). Between-study dispersion was substantial and statistically detectable ($Q(15) = 212.76$, $p < 0.001$), with high inconsistency ($I^2 = 92.69\%$) and non-trivial between-study variance ($\tau^2 = 0.0371$, REML) (Higgins et al., 2003; Borenstein et al., 2009; Viechtbauer, 2010). This heterogeneity motivates the planned moderator analyses (RQ2–RQ4) by mechanism-based leadership practice families, Scheme A outcome tiers, and the institutional configuration proxy (China vs non-China). The pooled estimate is an audit-ready correlational benchmark and does not imply causality.

Table 6. Overall pooled association between leadership practices and innovation-related outcomes in higher education institutions (random-effects meta-analysis)

k	Pooled_r	CI_lb	CI_ub	p_value	τ²	I² (%)	Q	Q_df	Q_p	Model
16	0.52	0.43	0.59	< 0.001	0.0371	92.69	212.76	15	< 0.001	Random-effects (REML) + HKSJ adjustment

Note. Effects were synthesised on Fisher's z using REML with the HKSJ adjustment and back-transformed to r. τ² denotes between-study variance; I² indicates the proportion of variability due to heterogeneity; Q tests the null hypothesis of homogeneity (Hedges & Olkin, 2014; Higgins et al., 2003; Borenstein et al., 2009).

Figure 3. Forest plot of study-level zero-order correlations and the pooled random-effects association (k = 16)



Note. **Squares represent study-level zero-order Pearson correlations (r) with 95% confidence intervals; the diamond represents the pooled association estimated using a random-effects model on Fisher's z (REML) with the Hartung–Knapp–Sidik–Jonkman (HKSJ) adjustment and back-transformed to r. Taiwan was coded under China for the institutional configuration proxy (China vs non-China) moderator (Appendix C).**

Heterogeneity as a rationale for theory-driven moderator tests

The overall synthesis showed substantial dispersion (Q(15) = 212.76, p < 0.001; I² = 92.69%; τ² = 0.037, REML), motivating the pre-specified moderator structure testing mechanism-based leadership practice families (RQ2), Scheme A outcome tiers (RQ3), and the institutional configuration proxy (China vs non-China; RQ4). Moderator analyses were implemented to examine theory-grounded boundary conditions rather than post hoc significance searching, and are interpreted cautiously given modest subgroup sizes and residual within-group dispersion (Higgins et al., 2022; Rubio-Aparicio et al., 2019; Viechtbauer, 2010).

Moderator and subgroup analyses (RQ2–RQ4)

Moderators were tested separately using mixed-effects subgroup models synthesised on Fisher's z with REML and HKSJ. Subgroup pooled associations and residual dispersion are reported in Table 7 (Panels A–C) and visualised in Figures 4A–4C; omnibus moderator tests (Q_M) are interpreted as contrast-evidence tests under modest k and dispersion, not as equivalence tests (Altman & Bland, 1995; Hoenig & Heisey, 2001; Viechtbauer, 2010).

Table 7. Subgroup and moderator analyses of the pooled leadership–innovation association (random-effects models)

Group	k	Pooled r	95% CI	τ^2	I^2 (%)
Panel A. Leadership practice family (RQ2)					
Family 1: change/energising/developmental	8	0.49	[0.35, 0.61]	0.037	92.78
Family 2: enabling/condition-building	6	0.50	[0.36, 0.61]	0.022	87.57
Family 3: strategic orchestration/ambidexterity	2	0.66	[-0.95, 1.00]	0.082	96.30
Omnibus between-group test (Q_M)			Q_M(2) = 1.42, p = 0.28		
Panel B. Outcome tier (RQ3)					
Tier 1: behavioural/process enactment	12	0.53	[0.42, 0.63]	0.050	94.76
Tier 3: innovation performance/output	4	0.46	[0.45, 0.48]	0.000	0.00
Omnibus between-group test (Q_M)			Q_M(1) = 0.517, p = 0.484		
Panel C. Institutional configuration proxy (China vs non-China) (RQ4)					
China	4	0.51	[0.06, 0.79]	0.097	98.05
Non-China	12	0.52	[0.44, 0.59]	0.023	86.44
Omnibus between-group test (Q_M)			Q_M(1) = 0.02, p = 0.91		

Note. Estimates are pooled correlations (r) with 95% confidence intervals, synthesised on Fisher's z and back transformed to r using REML with the HKSJ adjustment. k denotes the number of studies. τ^2 and I^2 (%) report residual within-subgroup dispersion. Omnibus between-group contrasts (Q_M) provide limited contrast evidence under modest subgroup sizes and residual dispersion and should not be interpreted as subgroup equivalence (Greenland et al., 2016; Altman & Bland, 1995; Hoenig & Heisey, 2001). Family 3 is exploratory ($k = 2$). For Tier 3, $\tau^2 = 0$ is a boundary estimate under small k and should be interpreted cautiously (Appendix F).

RQ2 — Leadership practice families (mechanism-based coding)

Pooled associations were positive across families: Family 1: $r = 0.49$ [0.35, 0.61] ($k = 8$), Family 2: $r = 0.50$ [0.36, 0.61] ($k = 6$), and Family 3: $r = 0.66$ [−0.95, 1.00] ($k = 2$) (Table 7, Panel A). The between-family contrast was not statistically detectable (Q_M(2) = 1.42, $p = 0.28$), and residual dispersion remained high within Family 1 and Family 2 ($I^2 > 87\%$).

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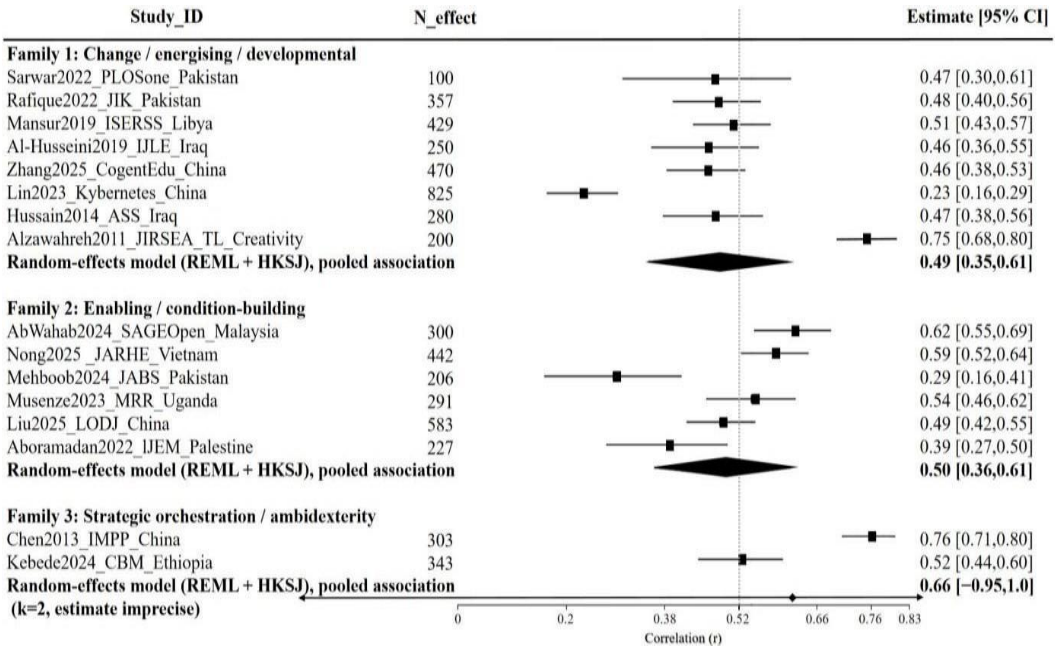
Figure 4A. Subgroup forest plot by leadership practice family (mechanism-based coding)

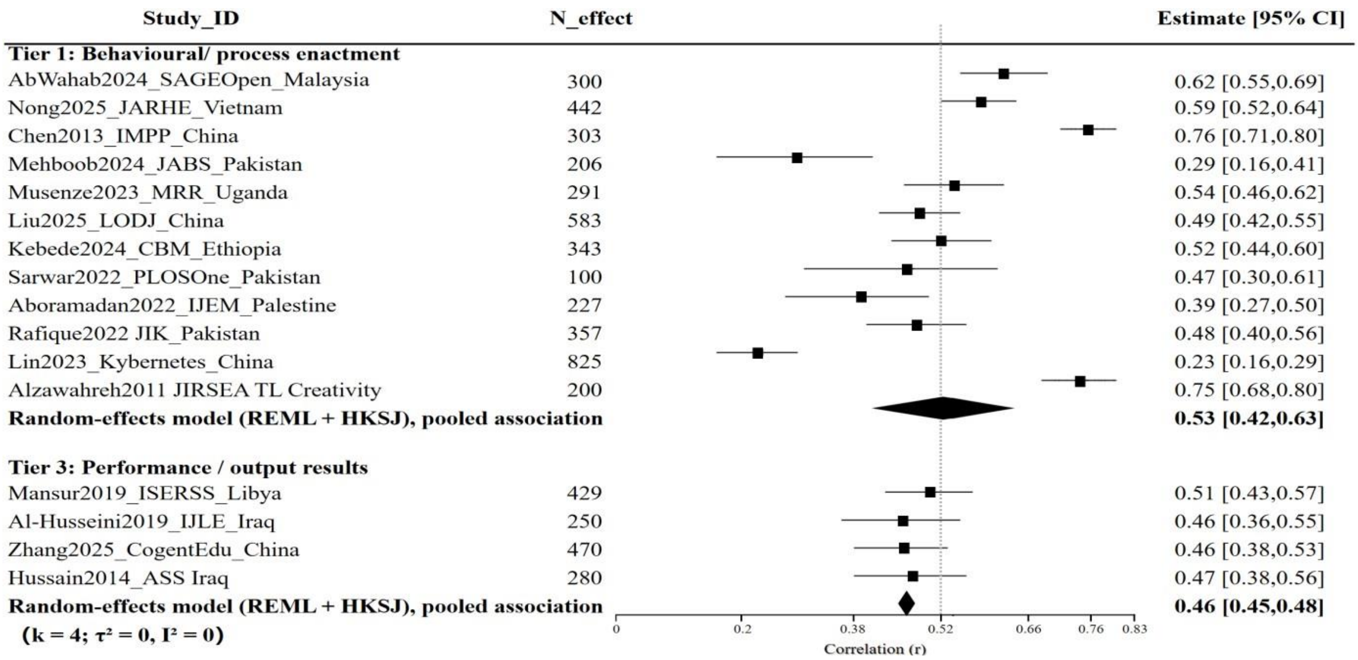


Note. Squares show study-level correlations (r) with 95% confidence intervals and diamonds show subgroup pooled estimates (REML + HKSJ). Family 3 is exploratory (k = 2).

RQ3 — Outcome tier under Scheme A boundary discipline

Leadership practices were positively associated with Tier 1 outcomes r = 0.53 [0.42, 0.63] (k = 12) and Tier 3 outcomes r = 0.46 [0.45, 0.48] (k = 4) (Table 7, Panel B). The between-tier contrast was not statistically detectable ($Q\_M(1) = 0.52$, $p = 0.48$); Tier 1 retained very high residual dispersion ($I^2 = 94.76\%$). For Tier 3, $\tau^2 = 0$ reflects a boundary estimate in this small subgroup and should not be overinterpreted as definitive homogeneity (Appendix F) (Veroniki et al., 2016; Röver et al., 2015).

Figure 4B. Subgroup forest plot by Scheme A outcome tier.



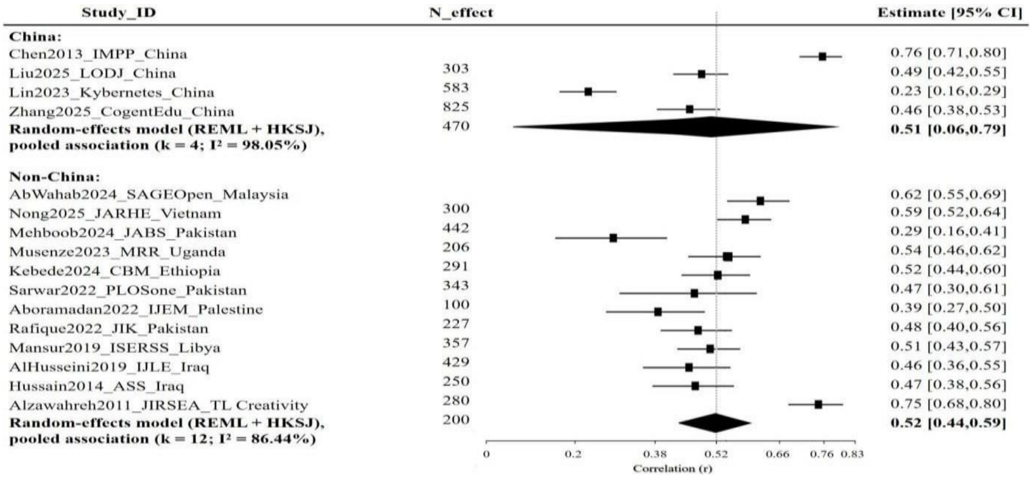
Note. Squares show study-level correlations (r) with 95% confidence intervals and diamonds show subgroup pooled estimates (REML + HKSJ). Tier 2 outcomes were not retained in the audit-ready pool (k = 0). For Tier

3 (k = 4), $\tau^2 = 0$ is a boundary estimate and should be interpreted cautiously (Raudenbush & Bryk, 2002; Veroniki et al., 2016; Viechtbauer, 2010).

RQ4 — Institutional configuration proxy (China vs non-China)

The pooled association was positive in China r = 0.51 [0.06, 0.79] (k = 4) and non-China r = 0.52 [0.44, 0.59] (k = 12) (Table 7, Panel C). The between-context contrast was not statistically detectable ($Q\_M(1) = 0.02$, $p = 0.91$), with very high residual dispersion in China ($I^2 = 98.05\%$) and substantial dispersion in non-China ($I^2 = 86.44\%$). This comparison is interpreted as an institutional-configuration proxy rather than a cultural classification (Rousseau, 2006; Arnold et al., 2021; Page et al., 2021).

Figure 4C. Subgroup forest plot by institutional configuration proxy (China vs non-China).



Note. Squares show study-level correlations (r) with 95% confidence intervals and diamonds show subgroup pooled estimates (REML + HKSJ). The vertical dotted line indicates the overall pooled estimate across all included studies. Taiwan samples were coded under China for the China vs non-China contrast (Appendix C).

Synthesis across moderators. Across RQ2–RQ4, subgroup pooled estimates remained directionally positive, while omnibus tests were not statistically detectable in this audit-ready dataset; this pattern is interpreted as limited contrast evidence under modest k and residual within-group dispersion, not as evidence of "no moderation" (Altman & Bland, 1995; Hoenig & Heisey, 2001; Greenland et al., 2016).

Robustness checks and sensitivity analyses

Robustness checks were conducted under the same inference framework as the primary synthesis (random effects models on Fisher's z using REML with HKSJ adjustment). Sensitivity analyses evaluated single-study influence, protocol-contingency dependence (conversion), and within-study multiplicity beyond the one study–one-effect primary rule.

Leave-one-out re-estimation (excluding one study at a time) yielded consistently positive pooled estimates ranging from r = 0.49 to r = 0.53 (Appendix F, Tables F1–F2). No single study altered the direction of the pooled association.

All retained primary effects in the final audit-ready pool were directly reported zero-order Pearson correlations (r) (k = 16; 100%). Accordingly, the "pure-r" re-estimation is analytically equivalent to the primary model and reproduces the same pooled association (Appendix F, Table F3), confirming that the primary synthesis does not rely on conversion contingencies.

To assess within-study multiplicity, a multi-effect library (31 effects nested within 16 studies) was analysed using a three-level random-effects model (REML) on Fisher's z (back-transformed to r) (Cheung, 2014; Van den Noortgate et al., 2013; Tanner-Smith & Tipton, 2014). The pooled association remained substantively consistent with the primary synthesis ($r = 0.51$, 95% CI [0.43, 0.57], $p < 0.001$; vs $r = 0.52$ in Table 6)

(Appendix F, Tables F4–F5). A convergent dependence-robust check using robust variance estimation (CR2) produced a comparable estimate ($r = 0.51$, 95% CI [0.42, 0.58], $p < 0.001$; df ≈ 14.93) (Hedges et al., 2010; Tanner-Smith & Tipton, 2014; Tipton, 2015). Variance decomposition indicated heterogeneity was primarily between studies (τ^2_between = 0.0334) rather than within-study across effects (τ^2_within = 0.0047) (Cheung, 2014; Van den Noortgate et al., 2013; Raudenbush & Bryk, 2002).

Bridge to publication-bias diagnostics. Overall, robustness checks supported stability under single-study deletion, confirmed that conversion contingencies were not operative in the final audit-ready pool, and yielded consistent estimates under dependence-robust specifications; publication-bias and small-study effects diagnostics are therefore reported next (Section 4.6).

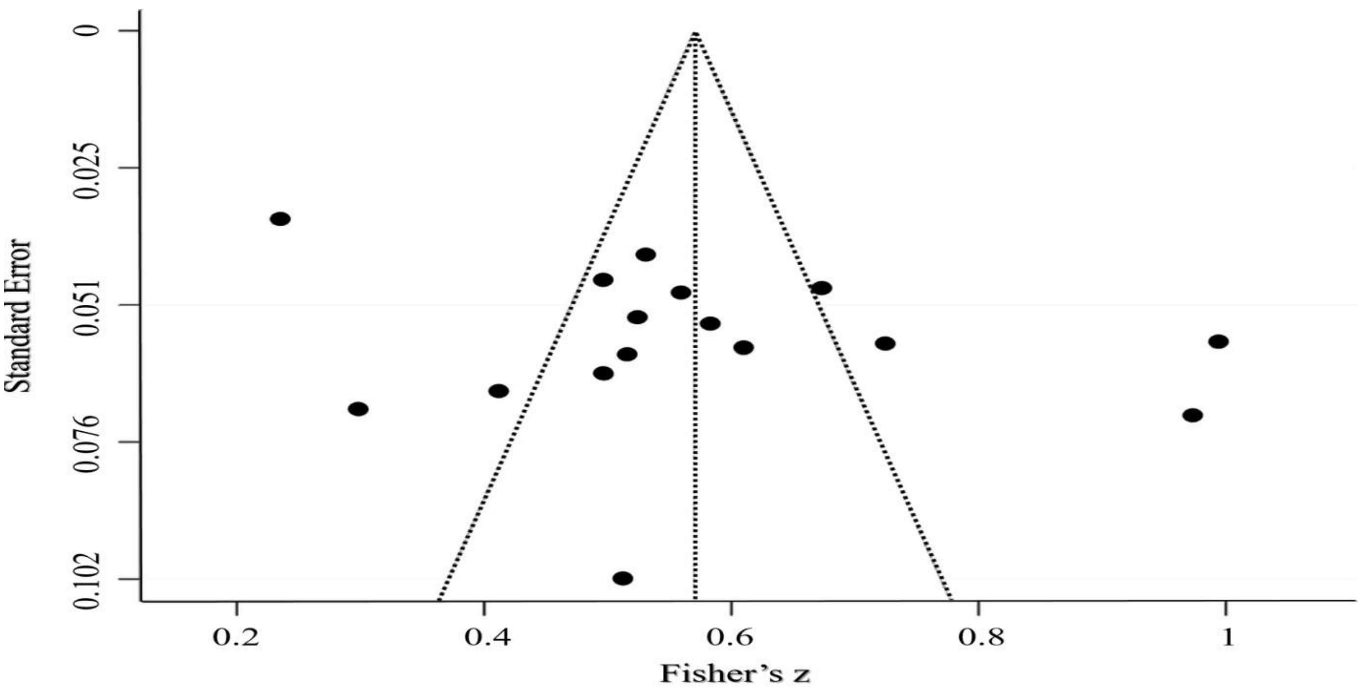
Publication bias and small-study effects

Publication-bias risk and small-study effects were assessed by triangulating funnel-plot inspection with asymmetry diagnostics (Egger et al., 1997; Sterne et al., 2011; Borenstein et al., 2009). Given the modest evidence base ($k = 16$) and substantial heterogeneity, these procedures are interpreted as supportive diagnostics rather than confirmatory tests (Sterne et al., 2011; Terrin et al., 2003; Carter et al., 2019).

Funnel plot (visual assessment)

Inspection of the funnel plot of Fisher's z-transformed correlations against standard errors (Figure 5) did not suggest pronounced or systematic asymmetry around the pooled random-effects estimate; visual assessment is interpreted conservatively under high dispersion (Sterne et al., 2011; Terrin et al., 2003).

Figure 5. Funnel plot for assessing small-study effects and potential publication bias ($k = 16$).



Note. Effects are plotted on the Fisher's z scale against standard errors; the vertical line denotes the pooled random-effects estimate.

Egger regression test and trim-and-fill sensitivity reference

Egger's regression test did not detect an asymmetry signal ($z = 0.58$, $p = 0.57$) (Egger et al., 1997; Sterne et al., 2011). Duval and Tweedie's trim-and-fill imputed $k_0 = 0$ potentially missing studies and produced an adjusted pooled estimate identical to the primary association (r = 0.52) (Table 8). These findings are treated as corroborative supportive diagnostics and do not rule out publication bias under limited power.

Table 8. Publication bias and small-study effects diagnostics (k = 16)

k	Pooled_r	Egger's z	Egger's p	Trim-and-fill imputed studies (k_0)	Trim-and-fill adjusted pooled r	Interpretation
16	0.52	0.58	0.57	0	0.52	No statistically detectable asymmetry signal under Egger's regression or trimand-fill; interpreted as supportive diagnostics given modest k and substantial heterogeneity.

Note. Egger's regression tests funnel-plot asymmetry as an indicator of small-study effects (Egger et al., 1997; Duval & Tweedie, 2000; Sterne et al., 2011). Trim-and-fill is reported as a sensitivity reference. Under k = 16 and substantial heterogeneity, "no detectable signal" is interpreted as supportive diagnostics rather than proof of no publication bias (Sterne et al., 2011; Terrin et al., 2003; Carter et al., 2019).

Chapter summary

This chapter synthesised audit-ready quantitative evidence on the leadership–innovation association in higher education institutions, retaining 16 independent studies (k = 16) with one reviewer-verifiable zero-order Pearson correlation (r) per study.

RQ1 (overall association). Leadership practices showed a positive pooled association with innovation-related outcomes (r = 0.52, 95% CI [0.43, 0.59], k = 16) under random-effects synthesis on Fisher's z (REML + HKSJ). Given substantial between-study dispersion, this pooled estimate is interpreted as an audit-ready correlational benchmark rather than a uniform relationship (Borenstein et al., 2009; Higgins et al., 2003; Cooper et al., 2019).

RQ2 (leadership practice families). Pooled associations were directionally positive across families—Family 1: r = 0.49 (k = 8), Family 2: r = 0.50 (k = 6), and Family 3: r = 0.66 (k = 2)—with a non-detectable between family contrast (Q_M(2) = 1.42, p = 0.28). Relative-magnitude inference is therefore exploratory under sparse cells (especially Family 3, k = 2) (Rubio-Aparicio et al., 2019; Hedges & Olkin, 2014; Viechtbauer, 2010).

RQ3 (outcome tiers). Associations were directionally positive for Tier 1 outcomes (r = 0.53, k = 12) and Tier 3 outcomes (r = 0.46, k = 4), with a non-detectable between-tier contrast (Q_M(1) = 0.52, p = 0.48); tier comparisons are interpreted cautiously under imbalanced k (Rubio-Aparicio et al., 2019; Viechtbauer, 2010).

RQ4 (institutional configuration proxy: China vs non-China). Associations were directionally positive in China (r = 0.51, 95% CI [0.06, 0.79], k = 4) and non-China settings (r = 0.52, 95% CI [0.44, 0.59], k = 12), with a non-detectable between-context contrast (Q_M(1) = 0.02, p = 0.91). This moderator is interpreted as an institutional-configuration proxy rather than a cultural classification, and context contrasts remain imprecise under small k and high within-group dispersion.

Robustness checks indicated that the pooled association was stable under leave-one-out deletion, did not depend on conversions (all retained primary effects were zero-order Pearson correlations (r)), and remained consistent under dependence-robust specifications using the multi-effect library. Publication-bias and small study effects diagnostics (funnel plot, Egger regression, and trim-and-fill) did not detect an asymmetry signal and are treated as supportive diagnostics given modest k and heterogeneity (Sterne et al., 2011; Terrin et al., 2003). The next chapter interprets these findings under the pre-specified innovation-governance lens without extending inference beyond the Chapter 4 evidence.

DISCUSSION AND CONCLUSIONS

This study examined whether leadership practices are associated with innovation-related outcomes in higher education institutions (HEIs), and whether these associations vary across mechanism-based leadership practice families, Scheme A outcome tiers, and the institutional configuration proxy (China vs non-China). Using an audit-ready evidence base (k = 16) and conservative random-effects synthesis on Fisher's z (REML + HKSJ) under Scheme A boundaries, the results provide a correlational benchmark and pre-specified moderator tests. Interpreted through the innovation-governance lens, the positive associations are compatible with leadership operating as a governance lever in professionally complex settings; however, substantial residual dispersion limits reliable subgroup ranking under modest k (Arnold et al., 2021; Rousseau, 2006; Mumford et al., 2017).

Leadership–innovation associations are directionally robust under heterogeneity (RQ1–RQ4)

This study examined whether leadership practices are associated with innovation-related outcomes in higher education institutions (HEIs), and whether these associations vary across mechanism-based leadership practice families, Scheme A outcome tiers, and the institutional configuration proxy (China vs non-China). Using an audit-ready evidence base (k = 16) and conservative random-effects synthesis on Fisher's z (REML + HKSJ) under Scheme A boundaries, the results provide a correlational benchmark and pre-specified moderator tests. Interpreted through the innovation-governance lens, the positive associations are compatible with leadership operating as a governance lever in professionally complex settings; however, substantial residual dispersion limits reliable subgroup ranking under modest k (Rubio-Aparicio et al., 2019; Viechtbauer, 2010; Hedges & Olkin, 2014).

RQ1 (overall association). Leadership practices showed a positive pooled association with innovation-related outcomes (r = 0.52, 95% CI [0.43, 0.59], k = 16). Under the Stage-3 audit-ready extractability gate and the zero-order correlational metric, this pooled estimate is interpreted as an audit-ready benchmark association and does not imply causality (Borenstein et al., 2009; Shadish et al., 2002; Cooper et al., 2019).

RQ2 (mechanism-based leadership practice families). Pooled associations were directionally positive across families—Family 1: r = 0.49 (k = 8), Family 2: r = 0.50 (k = 6), and Family 3: r = 0.66 (k = 2)—with a nondetectable between-family contrast ($Q_M(2) = 1.42$, p = 0.28). Family-level ranking is therefore not supported under sparse cells (especially Family 3, k = 2) (Rubio-Aparicio et al., 2019; Hedges & Olkin, 2014; Viechtbauer, 2010).

RQ3 (Scheme A outcome tiers). Associations were directionally positive for Tier 1 behavioural/process enactment outcomes (r = 0.53, k = 12) and Tier 3 innovation performance/output outcomes (r = 0.46, k = 4), with a non-detectable tier contrast ($Q_M(1) = 0.52$, p = 0.48). No Tier 2 innovation-enabling capability/process capacity outcomes were retained (k = 0), reflecting evidence coverage and auditability constraints under Scheme A eligibility and Stage-3 traceability rather than evidence about Tier 2 relevance (Altman & Bland, 1995; Greenland et al., 2016; Page et al., 2021).

RQ4 (institutional configuration proxy: China vs non-China). Associations were directionally positive in China (r = 0.51, 95% CI [0.06, 0.79], k = 4) and non-China settings (r = 0.52, 95% CI [0.44, 0.59], k = 12), with a non-detectable contrast ($Q_M(1) = 0.02$, p = 0.91). Given small k and high within-group dispersion in China, this pattern supports directional transportability under the proxy without claims of equivalence or fine-grained contextual ranking (Schmidt et al., 1985; Rousseau, 2006; Arnold et al., 2021).

Across RQ2–RQ4, the evidence supports a stable sign (positive association), while subgroup differentiation remains empirically unsettled under modest subgroup sizes and residual dispersion (Rubio-Aparicio et al., 2019; Viechtbauer, 2010).

Governance-mechanism interpretation remains compatible with the evidence base

Implication 1: Leadership aligns with an innovation-governance lever interpretation in HEIs. Because pooled estimates are derived from reviewer-verifiable zero-order correlations under an audit-ready gate, the evidence supports interpreting leadership practices as innovation-governance levers that structure coordination, legitimise experimentation, and shape resource attention in HEIs (Arnold et al., 2021; Ocasio, 1997; Rousseau, 2006).

Implication 2: Mechanism-based leadership practice families organise "leadership for innovation" without supporting subgroup ranking. Positive pooled associations across Family 1–3 indicate that leadership–innovation links are not confined to a single labelled approach (Yukl, 1992; Anderson et al., 2014; Dinh et al., 2014). Given non-detectable contrasts and sparse coverage for Family 3, the family framework improves interpretability but does not justify "best family" claims under current k.

Implication 3: Directional generality is supported; contrast claims across Scheme A tiers and the institutional configuration proxy remain limited. Non-detectable contrasts by Scheme A outcome tier and the institutional configuration proxy (China vs non-China) are treated as limited contrast evidence under the available subgroup sizes and residual dispersion, not as evidence of equivalence (Altman & Bland, 1995; Hoenig & Heisey, 2001; Greenland et al., 2016). The evidence therefore shifts the agenda from whether leadership matters to identifying innovation-governance contingencies that may explain remaining dispersion (Arnold et al., 2021; Gulati et al., 2012; Rousseau, 2006), without extending inference beyond the Chapter 4 results.

Leadership as an innovation-governance system: If–Then decision rules aligned to RQ2–RQ4 (final, reviewer-friendly)

Chapter 4 demonstrates a directionally positive leadership–innovation association across mechanism-based leadership practice families (RQ2), Scheme A outcome tiers represented in the audit-ready pool (RQ3), and the institutional configuration proxy (China vs non-China) (RQ4), while omnibus subgroup contrasts are not statistically detectable in the current audit-ready pool; accordingly, the managerial contribution is framed as evidence-bounded governance decision rules that translate directional robustness into controllable levers without implying causal effects or subgroup rank-ordering (Rousseau, 2006; Cooper, 2014; Tranfield et al., 2003).

Rule set aligned to RQ2: Mechanism-family governance levers (procedural justice/decision rights/resource discretion)

RQ2–Rule 1 (Procedural justice + voice infrastructure).

If innovation depends on discretionary idea expression and professional risk-taking, then institutionalise procedural justice and voice: codified evaluation criteria, formal submission channels, transparent reasons-for decision, and predictable decision timetables (Colquitt et al., 2013; Detert & Burris, 2007; Edmondson, 1999).

RQ2–Rule 2 (Decision-rights architecture for coordination).

If innovation work spans disciplines, units, or professional groups, then specify decision rights and interfaces: who decides what (initiate, prioritise, resource, continue/terminate), where decisions sit (committee/unit/project), and how disputes escalate, supported by recurring cross-boundary forums and named integrator roles (Eisenhardt, 1989; Galbraith, 1973; Burton & Obel, 2018).

RQ2–Rule 3 (Resource discretion + accountability safeguards).

If innovation requires downstream implementation beyond ideation, then lock implementation safeguards: ring-fenced time/funding, explicit allocation rules, named accountable owners, and stage-gate reviews (continue/pivot/stop) (Cooper, 2014; Killen & Kjaer, 2012; Meskendahl, 2010). Safeguards are consistent with a directionally positive association and are not framed as subgroup superiority claims.

Rule set aligned to RQ3: Outcome-tier governance rules (Tier 1 vs Tier 3; Tier 2 absent under audit ready gate)

RQ3–Rule 4 (Tier 1 enactment: permission + interaction design).

If the objective is Tier 1 behavioural/process enactment, then combine permission structures—voice protection, low-penalty trial rules, procedural justice—with interaction design: boundary-spanning routines, cross-unit problem-solving forums, and shared artefacts/cadence (Tushman & Scanlan, 1981; Ferguson & Brannick, 2012; Faraj & Yan, 2009).

RQ3–Rule 5 (Tier 3 outputs: prioritisation + accountability chain).

If the objective is Tier 3 innovation performance/output, then strengthen portfolio governance: explicit innovation priorities, selection and resource-allocation rules (Cooper, 2014; Killen & Kjaer, 2012; Martinsuo, 2013), ownership of implementation milestones, and monitoring cadence tied to continuation/termination decisions.

RQ3–Rule 6 (Tier 2 evidence gap: audit-ready reporting rule).

If institutions aim to manage Tier 2 innovation-enabling capability/process-capacity constructs under Scheme A, then enforce audit-ready reporting: traceable zero-order r, consistent N_effect (Hair et al., 2021; Kline, 2023; Borenstein et al., 2009), and explicit construct referents, avoiding latent-only or mediation-only reporting that prevents extractability. Tier 2 coverage is absent in the current audit-ready pool (Tier 2 k = 0).

Rule set aligned to RQ4: Institutional configuration proxy (China vs non-China)

RQ4–Rule 7 (Transportable direction; local calibration).

If governance practices are transferred across China vs non-China settings, treat the association direction as transportable under this institutional configuration proxy, but calibrate expected magnitude locally (Schmidt et al., 1985; Rousseau, 2006; Johns, 2006) by tuning decision-rights allocation, accountability intensity, and resource discretion, and by monitoring outcomes, given high within-group dispersion and modest subgroup k (especially China).

To support implementation and facilitate reviewer-verifiable linkage to the results boundary, Table 9 consolidates Rules 1–7 into a one-page “If–Then → Governance instrument” toolkit, with each rule expressed using the same four controllable levers (procedural justice, decision rights, accountability, and resource discretion) and a short HEI example to aid operationalisation.

Table 9. Innovation-governance If–Then decision rules aligned to RQ2–RQ4 (audit-ready managerial toolkit)

Rule	**If (trigger condition)**	**Then (governance action)**	**Governance instruments (controllable levers)**	**Examples in HEIs (Instrument — Mechanism)**
RQ2–Rule 1	Innovation requires discretionary idea expression and risk taking in professional communities	Institutionalise procedural justice and voice infrastructure (codified criteria, formal channels, transparent reasons-for decision, predictable timetables).	Procedural Justice	Procedural Justice — Voice & Appeals Channel
RQ2–Rule 2	Innovation spans disciplines, units, or professional groups	Specify decision rights and coordination interfaces (who decides what; where decisions sit; escalation routes), supported by cross-boundary forums and integrator roles.	Decision Rights	Decision Rights — Cross Boundary Integrator Role

RQ2–Rule 3	Innovation requires downstream implementation commitments beyond ideation	Lock resource discretion and accountability (ring-fenced time/funding, allocation rules, accountable owners, stage-gate reviews: continue/pivot/stop).	Resource Discretion; Accountability	Resource Discretion — Innovation Seed Fund
RQ3–Rule 4	Objective is Tier 1 behavioural / process enactment outcomes	Combine permission structures (voice protection, low-penalty trial rules) with interaction design (boundary-spanning routines, forums, shared artefacts/cadence).	Procedural Justice; Decision Rights	Procedural Justice — Low-Penalty Pilot Policy
RQ3–Rule 5	Objective is Tier 3 innovation performance / output outcomes	Strengthen prioritisation and the accountability chain (explicit priorities, selection/allocation rules, milestone ownership, monitoring tied to continuation/termination).	Decision Rights; Accountability	Accountability — Innovation Portfolio Review Board
RQ3–Rule 6	Institution intends to manage Tier 2 innovation-enabling capability/process capacity under Scheme A	Impose an audit-ready reporting rule (traceable zero-order r, consistent N_effect, clear construct referents; avoid latent-only/mediation-only formats that break extractability).	Accountability; Procedural Justice	Accountability — Audit-Ready Reporting Template
RQ4–Rule 7	Practices are transferred across China vs non-China settings	Treat direction as transportable under the proxy, but calibrate magnitude locally by tuning levers and monitoring outcomes.	Decision Rights; Accountability; Resource Discretion	Decision Rights — Local Delegation Matrix

Note. Rules translate the directionally positive leadership–innovation associations observed across RQ2–RQ4 into implementable governance instruments. Governance instruments are restricted to four controllable lever families—Procedural Justice, Decision Rights, Accountability, and Resource Discretion—and listed as up to three keywords per rule. Examples provide HEI-specific institutional mechanism names in a consistent Instrument — Mechanism format. Tier 2 rules reflect an evidence-coverage constraint in the current audit ready pool (Tier 2 k = 0).

Taken together, the toolkit positions leadership as an innovation-governance system—formalising legitimacy for experimentation, coordination interfaces, and implementation safeguards—while maintaining the audit ready inference boundary that directional patterns inform actionable levers but do not warrant causal claims or “best family” prescriptions under modest subgroup coverage and high residual dispersion.

Synthesis (governance lens). Taken together, these If–Then rules frame leadership as an innovation governance system that institutionalises procedural justice and voice, clarifies decision rights for cross boundary coordination, and secures resource discretion plus accountability, without implying causal effects or subgroup rank-ordering beyond the Chapter 4 evidence boundary.

Inference boundary: evidence coverage and measurement diversity

Inference is bounded by the audit-ready evidence base. The evidence base is modest (k = 16), making moderator contrasts plausibly underpowered in sparse cells (e.g., Family 3; China subgroup under the institutional configuration proxy (China vs non-China)) (Rubio-Aparicio et al., 2019; Viechtbauer, 2010; Hedges & Olkin, 2014). Substantial heterogeneity indicates contextual dispersion not resolved by the tested categorical moderators. The predominance of cross-sectional survey designs limits causal interpretability and increases vulnerability to common-method artefacts (Podsakoff et al., 2003; Spector, 2006; Shadish et al.,

2002). Measurement heterogeneity across leadership and innovation operationalisations may inflate dispersion and reduce detectability of subgroup contrasts (Hunter & Schmidt, 2004; Borenstein et al., 2009; Aguinis et al., 2011); Tier 2 innovation-enabling capability/process capacity outcomes were absent in the audit-ready pool (k = 0) under Scheme A eligibility and Stage-3 traceability constraints (Page et al., 2021; Rethlefsen et al., 2021; Higgins et al., 2022).

CONCLUSION

Governance-lever interpretation under directional robustness

Across k = 16 independent studies retained under an audit-ready extractability gate, leadership practices show a positive association with innovation-related outcomes in HEIs (r = 0.52). Moderator analyses indicate directional robustness across mechanism-based leadership practice families, Scheme A tiers represented in the pool, and the institutional configuration proxy (China vs non-China), while subgroup contrasts remain constrained by modest k and residual dispersion. Leadership is therefore interpreted as a broadly relevant innovation-governance lever in correlational terms, with managerial meaning expressed as governance decision rules specifying coordination, legitimacy, and resource levers without implying causal effects or subgroup ranking (Arnold et al., 2021; Shadish et al., 2002; Rousseau, 2006).

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