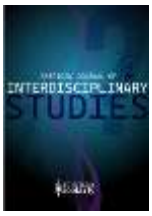

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THE INFLUENCE OF STATISTICAL MODELS FOR FRAUD DETECTION IN PROCUREMENT AND INTERNATIONAL TRADE SYSTEMS

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ABSTRACT

Procurement and international trade systems remain particularly vulnerable to complex fraud mechanisms that undermine transparency, equity, and fiscal integrity across both public and private sectors. These fraudulent behaviors – ranging from misinvoicing and threshold bunching to split purchasing and restricted competition – continue to distort market signals, inflate transaction costs, and erode trust in institutional governance. Addressing these vulnerabilities requires not only the deployment of advanced analytic tools but also an understanding of how interpretable statistical modeling frameworks can enhance the credibility, precision, and utility of fraud detection outcomes in practice. This study therefore seeks to explore the empirical relationships among internal control mechanisms, vendor concentration patterns, compliance cultures, and the intensity of transactional anomalies within enterprise procurement operations. Employing a quantitative, cross-sectional, multi-case research design, this investigation draws upon survey and administrative data from five enterprise environments that operate either enterprise resource planning (ERP) or e-procurement systems. A total of 268 respondent-level observations were analyzed, providing a robust dataset for comparative evaluation. To anchor the analytical framework, a structured literature review of 37 peer-reviewed studies was conducted, synthesizing theoretical constructs from fraud risk modeling, internal audit research, and information system governance. The resulting conceptual model integrates both organizational and transactional dimensions, encompassing variables such as internal control strength, vendor risk exposure, concentration ratios, transaction anomaly intensity, compliance culture maturity, audit cadence, and a composite fraud-risk outcome index. The analytical sequence proceeded in a methodologically rigorous manner: preliminary reliability and validity diagnostics were performed to assess construct consistency; descriptive analyses established baseline patterns; followed by zero-order and partial correlation matrices to determine intervariable associations. Subsequently, fixed-effects regression modeling was implemented to isolate within-organization effects, employing ordinary least squares (OLS) for continuous outcomes, logistic regression for binary fraud event classifications, and negative binomial models for count-based incident outcomes. Moderation terms were prespecified to evaluate interaction effects between structural controls and transactional anomalies. The empirical findings reveal that transaction anomaly intensity constitutes the most powerful positive correlate of the composite fraud-risk score, confirming that irregular purchasing and invoicing behaviors remain key indicators of systemic exposure. Conversely, internal control robustness and compliance culture strength demonstrate significant negative relationships with fraud risk, illustrating their preventive potential. The results further indicate that vendor concentration, a proxy for limited competitive pressure – exerts a positive and statistically significant effect on risk, highlighting the importance of diversification strategies.

KEYWORDS

Procurement Fraud, International Trade, Statistical Modeling, Internal Controls, Anomaly Detection, Vendor Concentration;

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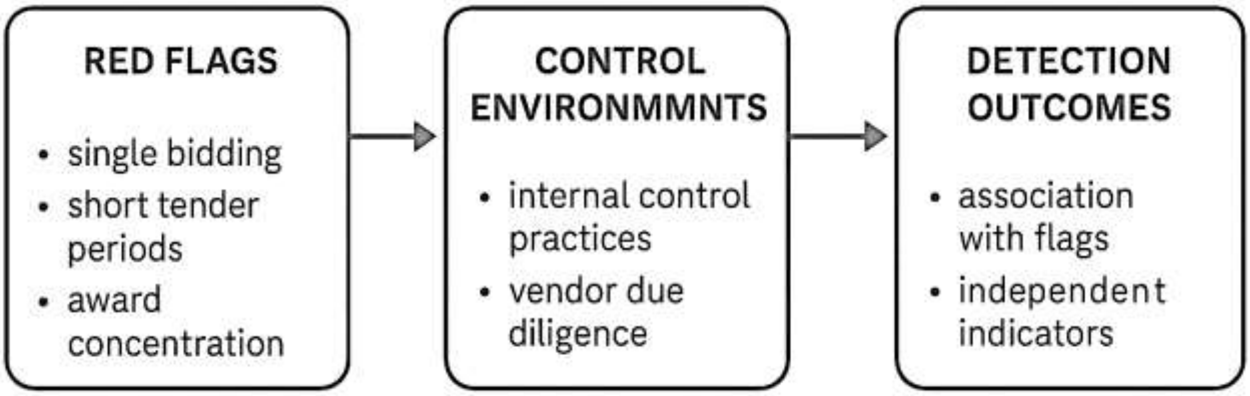
INTRODUCTION

Fraud in procurement and international trade systems is commonly defined as intentional deception for unlawful gain that subverts rules of competition, misallocates resources, and erodes trust in markets and public institutions. In international trade, fraud often manifests through misinvoicing, carousel schemes, and trade-based money laundering (TBML), while public and private procurement face bid-rigging, collusive tendering, and invoice manipulation. Statistical models spanning descriptive statistics, correlation analysis, and regression play a central role in screening large volumes of transactional, vendor, and contract data to identify anomalies associated with fraud risk. Within international trade, TBML scholarship shows that abnormal price deviations in import–export declarations can be detected using benchmarking and econometric profiling, demonstrating the international salience of quantitative methods (Zdanowicz, 2009). In procurement, data-driven "red-flag" indicators such as single-bid tenders in otherwise competitive markets, short tender periods, contract award concentration, and repeated awards to the same supplier have been operationalized statistically and validated using large administrative datasets (Fazekas & Kocsis, 2017; Fazekas, Tóth, et al., 2016). Across the broader fraud-analytics literature, classification and regression approaches logistic regression, support vector machines, and decision trees consistently emerge as robust baselines for detection tasks, with logistic regression often performing competitively relative to more complex learners when misclassification costs are carefully considered (Ngai et al., 2011; Perols, 2011). This research adopts a quantitative, cross-sectional, multi–case study design to evaluate how such statistical models influence (i.e., improve or shape) fraud detection outcomes in procurement and international trade contexts using Likert-scale measures of control practices, data quality, and detection effectiveness. The introduction sets up a program that is methodologically grounded in classical statistics and regression modeling while remaining attentive to domain-specific manifestations of fraud risk (Ngai et al., 2011; Perols, 2011; Zdanowicz, 2009).

Figure 1: Procurement Fraud Statistics 2022

Procurement systems public and private aggregate significant financial flows, rendering them vulnerable to collusion and corruption. The literature has converged on a set of empirically tractable risk indicators ("red flags") that can be quantified from tender and contract data, including single bidding in competitive markets, excessive use of non-open procedures, short bid submission windows, and recurrent awards to the same firms (Abdul, 2021; Doyle et al., 2007). These indicators are grounded in a theory of restricted competition and have been replicated across jurisdictions, reinforcing their external validity (Doyle et al., 2007; Rezaul, 2021). Audit analytics complements these risk indicators with continuous monitoring of transactional data; for example, Benford-law-based screens and digit-distribution tests have been proposed for invoices and vendor payments, though scholars caution about false positives and emphasize appropriate contextualization and robustness checks (Diekmann, 2010; Mubashir, 2021). In international trade, the motivation for quantitative detection is similarly strong: statistical profiling of customs data can flag pricing patterns consistent with TBML and value-transfer schemes that exploit trade documents to move illicit funds (Diekmann, 2010; Rony, 2021). As data availability and e-procurement platforms expand, red-flagging through statistical models becomes a scalable first line of defense that guides investigative resources to the riskiest tenders or transactions (Danish & Zafor, 2022). This study is motivated by the practical need to synthesize these streams procurement red flags, audit analytics, and trade-data profiling within a unified statistical framework using descriptive statistics, correlation matrices to assess construct co-movement, and regression models that quantify associations between controls, data conditions, and observed fraud-risk outcomes (Fazekas, Cingolani, et al., 2016).

Figure 1: Statistical Modeling For Fraud Detection In Procurement And International Trade



The objective of this study is to rigorously examine how core statistical techniques descriptive statistics, correlation analysis, and regression modeling shape and quantify fraud detection in procurement and international trade systems within a cross-sectional, multi–case design. Specifically, the study seeks to develop and administer a structured instrument that operationalizes key constructs central to fraud detection (internal controls, vendor risk and concentration, transaction anomaly intensity, compliance culture, and audit/monitoring cadence) alongside an outcome construct capturing fraud risk through perceived exposure and observed suspicious events. The first objective is to establish reliable and valid composite indices for these constructs using a five-point Likert scale and to summarize the sample and case context through comprehensive descriptive statistics that profile central tendencies, dispersion, and distributional characteristics. The second objective is to estimate the magnitude and direction of associations among constructs via zero-order and partial correlations, thereby clarifying baseline co-movement patterns between red-flag intensity, control environment strength, and fraud-risk measures. The third objective is to evaluate the incremental explanatory power of regression models ordinary least squares for continuous outcomes, logistic for binary outcomes, and negative binomial for count

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outcomes relative to descriptive and correlational screening, while controlling for organizational size, sector, trade exposure, and case effects. The fourth objective is to test prespecified moderation relationships, such as whether stronger internal controls and more frequent monitoring attenuate the association between vendor risk or anomaly intensity and fraud risk, using mean-centered interaction terms and standard diagnostics for multicollinearity and model fit. The fifth objective is to conduct robustness and sensitivity analyses, including alternative operationalizations of anomaly intensity, influence checks, heteroskedasticity-robust standard errors, and leave-one-case-out validation, to assess stability of coefficients and conclusions across cases. The sixth objective is to document practical, measurement-focused outputs codebooks, scoring rules, and reproducible scripts that allow the same statistical workflow to be reapplied to additional cases or refreshed cross-sections. Collectively, these objectives are designed to yield a transparent, statistically grounded assessment of how interpretable modeling workflows contribute to fraud detection in procurement and trade contexts without relying on speculative assumptions or case-specific heuristics.

LITERATURE REVIEW

The literature on fraud detection in procurement and international trade converges on a core premise: systematic, data-driven scrutiny of organizational processes and transaction records is essential for identifying patterns consistent with opportunism, collusion, and misrepresentation. In procurement, studies emphasize market-structure signals and process irregularities such as single-bid tenders in ostensibly competitive markets, repeated awards to a narrow set of suppliers, unusually short bidding windows, and off-contract spending while international trade scholarship highlights misinvoicing behaviors, abnormal unit prices, suspicious routing, and documentation inconsistencies indicative of value-transfer schemes. Across both domains, research has progressively moved from ad hoc red-flag checklists toward formal statistical modeling that quantifies associations among risk indicators, control environments, and observed fraud-related outcomes. Descriptive statistics are used to profile prevalence and dispersion of red flags across organizations and sectors; correlation analysis clarifies baseline co-movements among factors like vendor concentration, anomaly intensity, and internal control strength; and regression frameworks estimate the independent contribution of each factor while adjusting for organizational size, sector, and trade exposure. The operational backbone of this work typically blends administrative microdata (e.g., tender, invoice, and customs declarations) with organization-level measures of control practices, monitoring cadence, and compliance culture, often captured through structured Likert-scale instruments. Methodologically, the literature underscores requirements for reliability (internal consistency of multi-item constructs), construct validity (clear mapping from theory to measurement), and rigorous diagnostics in modeling (multicollinearity checks, heteroskedasticity-robust inference, influence analysis). It also notes challenges inherent to cross-sectional fraud analytics, including class imbalance, context-dependent interpretation of red flags, and data quality constraints that affect inference and comparability across cases. While complex machine learning approaches feature in some strands, statistical baselines especially interpretable regression models remain prominent because they provide transparent effect estimates that can guide control design and audit prioritization. Against this backdrop, the present review synthesizes findings from procurement and trade, organizes determinants and indicators into coherent constructs suitable for survey and transactional measurement, and delineates the analytical path descriptive, correlational, and regression-based that will be applied in the empirical component of this study.

Fraud Mechanisms in Procurement and International Trade

Public procurement and international trade expose large, complex contracting and documentation processes to strategic manipulation. In procurement, fraud mechanisms often hinge on limiting competition or distorting the allocation rule of auctions. Bid-rigging cartels coordinate entry and bids to raise prices or steer awards across contracts, frequently exploiting tender formats that can be gamed through predictable rules or repeated interactions. Such collusion can be subtle, for example via "reauction" strategies in which bidders synchronize behavior across failed tenders to achieve a focal outcome when a project is rebid, or via coalition behavior in average-bid mechanisms that

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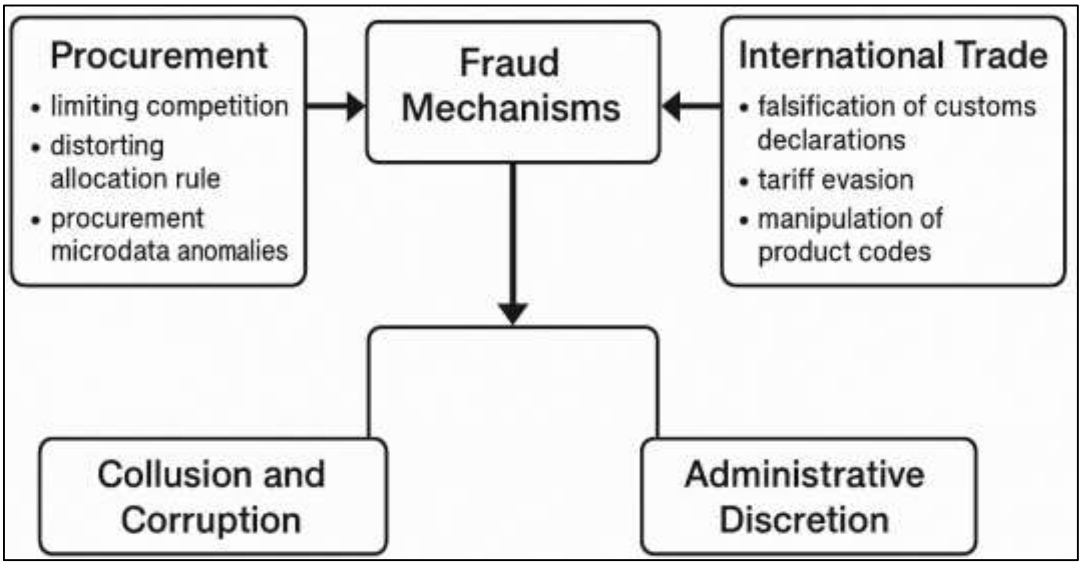
rewards bids clustered around a trimmed average (Auriol, 2006; Conley & Decarolis, 2016; Imhof et al., 2018). These practices are consistent with a broader theoretical and empirical understanding of corruption and waste in public purchases: corruption alters the selection of suppliers and the terms of trade, while weak oversight and misaligned incentives amplify the rents available to insiders (Danish & Kamrul, 2022). The procurement environment thereby enables both explicit collusion and opaque favoritism that masquerades as administrative discretion. Analytical work on procurement design and the political economy of purchasing underscores that corruption in procurement is not merely a series of isolated acts but a set of mechanisms that exploit information asymmetries, repeated contact between officials and suppliers, and the complexity of tender rules (Jahid, 2022).

Figure 2: Conceptual Model Of Fraud Mechanisms In Procurement And International Trade



A second cluster of mechanisms arises from process irregularities and pattern anomalies visible in procurement microdata. Collusive rings may coordinate who enters a tender and how bids are spaced, leaving statistical fingerprints across tenders (e.g., recurring coalitions, bid clustering around algorithmic thresholds, or suspicious patterns in the identity of winners and runners-up). In markets with frequent re-bids after failures to meet reserve prices, cartels can use the first round to signal and the second to implement the allocation, producing discontinuities in participation and markups (Auriol, 2006; Javorcik & Narciso, 2008). Even without outright collusion, corruption mechanisms exploit discretionary exemptions, short solicitation windows, and documentation pathways that restrict competition or enable directed awards. These operational channels tie to measurable "red flags" at scale: unusually low effective competition, repeated awards to a narrow set of firms, and bid dispersion patterns inconsistent with competitive benchmarks (Ismail, 2022). Empirical approaches develop screens that combine such features entry behavior, distance of bids from benchmarks, variance structures across contracts to prioritize suspicious tenders for review. The overarching idea is that procurement fraud is mechanized through predictable manipulation of participation, pricing rules, and administrative choices; therefore, it can be studied by mapping those manipulations to robust statistical indicators of abnormality (Javorcik & Narciso, 2008; Hossen & Atiqur, 2022).

In international trade, fraud mechanisms center on falsification or manipulation of customs declarations to evade tariffs, launder value, or circumvent regulatory controls. A canonical pathway is tariff evasion through under-invoicing, over-invoicing, or misclassification, where importers report prices or product codes that reduce duty liability or obscure the true nature of the goods. The incidence of such evasion is not random: differentiated products, which are harder to price

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objectively, present greater scope for discretionary valuation and thus higher evasion potential, while higher statutory tariffs increase the marginal gain from manipulation (Kamrul & Omar, 2022; Mishra et al., 2008). At the operational level, these mechanisms manifest as systematic gaps between reported import values and plausible comparator benchmarks, as well as asymmetries between partner-reported exports and domestic imports along finely defined product lines. Customs enforcement, institutional quality, and the credibility of audit and penalty regimes shape the feasibility and profitability of these practices. In this sense, international trade fraud is structurally linked to procurement corruption (Razia, 2022): both exploit administrative discretion and information frictions, both respond to economic incentives embedded in rules and rates, and both leave measurable statistical traces in transactional data. By articulating how these traces arise from valuation discretion in differentiated products to elasticity of evasion with respect to tariffs the literature clarifies why descriptive profiling, correlational evidence, and regression modeling provide a coherent toolkit for detecting and characterizing fraud mechanisms across procurement and trade (Javorcik & Narciso, 2008; Mishra et al., 2008).

Determinants of Fraud Risk

Fraud risk in procurement hinges on institutional design features that shape incentives and information flows across the tendering lifecycle. Auction format, publicity requirements, and the credibility of oversight alter both the opportunity set for manipulation and the expected return to honest participation (Bandiera et al., 2009). When publicity is weak or uneven, incumbent suppliers can dominate information channels and depress entry, raising the scope for favoritism and coordinated outcomes. Conversely, stronger publicity rules that mandate broader announcement and documentation increase contestability and shift the rebate distribution toward lower prices paid by buyers by attracting additional qualified bidders. These entry effects are not merely mechanical; they change the strategic environment in which firms set bids, reducing the feasibility of steering awards via short windows, limited invitations, or obscure notices. A parallel determinant is the intensity and predictability of audits or inspections. Where the perceived probability of scrutiny rises from sporadic to systematic, measured losses from procurement projects fall, indicating that oversight modifies the calculus of both officials and suppliers by raising the expected cost of misreporting and collusion. Finally, it is important to distinguish between "active" waste (consistent with patronage, kickbacks, or directed awards) and "passive" waste (stemming from bureaucratic inefficiency) (Olken, 2007; Sadia, 2022). Although both inflate spending, active waste is intrinsically connected to corruption risk because it reflects deliberate deviations from competitive procurement in pursuit of private benefits; procurement systems that attenuate discretion and increase transparency therefore target the very margin along which active waste is generated. Together, these determinants publicity, entry, and audit intensity, alongside the composition of waste provide a structural account of how procurement design and oversight shape observable fraud risk (Bandiera et al., 2009; Fisman & Miguel, 2007).

Market structure and procedural thresholds further condition fraud risk by creating focal points for behavior around which strategic actors can coordinate. Publicity thresholds, for example, often entail additional announcement or documentation requirements above specific contract values; tenders just below these thresholds may cluster if officials or favored suppliers attempt to avoid broader scrutiny. In markets with thin competition, repeated interactions between buyers and a small set of firms increase the predictability of rivals' actions, lowering the cost of tacit coordination and facilitating bid spacing or rotation. Entry barriers administrative complexity, short solicitation windows, and highly customized technical specifications can reinforce these dynamics by discouraging challengers who lack insider information or resources to bid quickly (Coviello & Mariniello, 2014; Sequeira, 2016). Auditing policy interacts with these features by raising the expected cost of exploiting them; when the audit probability is announced to be high and credible, measured discrepancies between reported and engineered costs decline, consistent with deterrence operating through changed expectations of detection. At the same time, distinguishing active from passive waste clarifies diagnostic interpretation of red flags: high prices or low rebates may result

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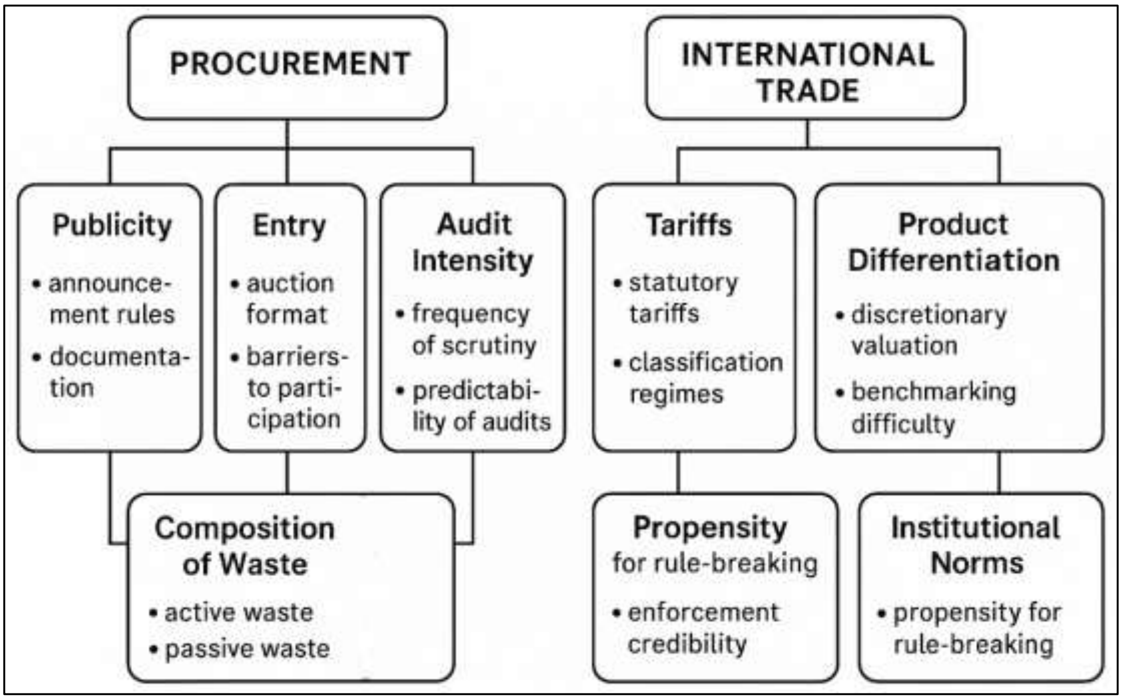
from poor aggregation or outdated catalogues (passive), whereas systematic patterns of single bidding, repeated awards to the same supplier, and clustering just below publicity thresholds are more consistent with active manipulation of process controls.

Figure 3: Structural Determinants Of Fraud Risk In Procurement



These distinctions matter because statistical screens that treat all deviations from competitive benchmarks as equivalent can overstate risk in settings dominated by inefficiency rather than collusion; explicit attention to threshold effects, entry dynamics, and audit salience improves the alignment between indicators and true corruption exposure. In practice, procurement systems that raise publicity, lower procedural frictions for entry, and commit to credible audit regimes shift bidder behavior in ways that are detectable in transaction-level data and predictive of lower fraud risk (Coviello & Mariniello, 2014).

In international trade, fraud risk is driven by determinants that parallel procurement information frictions and enforcement credibility but operate through tariff schedules, product differentiation, and institutional norms that shape compliance. High statutory tariffs and complex classification regimes raise the private gain to misreporting, while differentiated products expand the scope for discretionary valuation because “true” prices are difficult to benchmark. Under these conditions, traders can under-invoice to reduce duty liability, over-invoice to transfer value, or misclassify goods to exploit lower rates, generating systematic gaps between reported values and plausible comparators. The elasticity of evasion with respect to tariff changes reveals that incentives embedded in policy directly affect manipulation; where tariffs fall exogenously, measured trade costs decline far less in high-corruption corridors than in comparable routes, implying that unofficial payments and misreporting offset policy gains. Cultural norms and baseline propensities for rule-breaking also influence behavior when formal enforcement is weak or absent: in environments that neutralize penalties, officials with backgrounds from high-corruption settings engage more readily in norm-violating actions, offering a revealed-preference lens on latent integrity constraints. For customs administrations, these determinants imply that raising detection credibility targeted inspections, post-clearance audits, and sanctions must be coupled with designs that reduce discretion (e.g., risk-based selectivity, standardized valuation databases) to address both the incentive and opportunity to misreport. For researchers, they underscore the relevance of tariff-level variation, product differentiation, and institutional norms as covariates when modeling fraud risk in trade data, ensuring that statistical associations are interpreted in light of policy-driven incentives and enforcement context rather than treated as free-floating anomalies (Coviello & Mariniello, 2014; Fisman & Miguel, 2007).

Data Sources for Fraud Detection

Transaction-level administrative data are the backbone for measuring procurement and trade-related fraud risk because they encode, at scale, the procedural and economic choices that actors make under specific rules. In public procurement, tender notices, bids, awards, supplier identifiers, contract values, timelines, and change orders collectively allow researchers to build red-flag indicators tied to competition (e.g., single bidding, short advertising windows), discretion (non-open procedures, direct awards), and market structure (winner concentration, buyer–supplier exclusivity). The maturation of e-procurement platforms has expanded both coverage and granularity, enabling designs that exploit within-jurisdiction policy or technology rollouts as quasi-experiments and supporting more reliable construction of standardized indicators across projects and agencies (Ferrantino et al., 2012; Lewis-Faupel et al., 2016). Crucially, the same datasets permit complementary statistical treatments descriptive profiling to summarize prevalence of red flags, correlation analysis to establish co-movement among risk factors and controls, and regression models to estimate the independent contribution of each indicator to observed outcomes (such as prices, delays, or protest rates) conditional on sector, buyer, and case fixed effects. A representative example is the use of platform adoption events to test whether increased transparency and reduced face-to-face discretion change procurement performance, leveraging the comparability and timestamps that administrative records provide (Lewis-Faupel et al., 2016).

A second strand formalizes indicator design by anchoring it to institutional thresholds and procedural rules that actors can game. Administrative data reveal “where to look” because many manipulation strategies like bunching contract values just below publicity or competitive-tender thresholds, or sequencing related awards to favored vendors produce distinctive distributional patterns. Researchers operationalize these patterns as screens: density tests around thresholds, measures of effective competition (bidders, unique entrants), and network-based concentration indices that capture repeated buyer–supplier ties. With sufficiently rich microdata, these screens can be embedded in regression frameworks to separate active manipulation from passive inefficiency and to quantify effect sizes while adjusting for confounders (buyer capacity, sector technology, regional cost indices). Evidence exploiting the introduction of discretionary thresholds shows clear bunching just below regulatory cutoffs, validating the threshold-screen approach and highlighting how rule design transmits into measurable anomalies (Palguta & Pertold, 2017). At a cross-country scale, paired “law and practice” datasets link indicator performance to institutional environment, underscoring that de jure rules alone are weak predictors of outcomes absent supportive practices and enforcement capacity; this motivates multi-indicator composites that mix process red flags with outcome measures like rebates, delivery times, and amendment frequency (Bosio et al., 2020; Palguta & Pertold, 2017).

In international trade, customs microdata (declarations at the HS-6 or HS-8 line with quantities, unit values, origin/destination, and regime codes) and mirror statistics (partner-reported exports vs. domestic imports) support two complementary indicator families. The first class centers on “value-gap” and “price-filter” screens that flag outliers in unit values relative to interquartile or historical benchmarks useful for detecting under- and over-invoicing in differentiated products and duty-sensitive lines. The second exploits systematic discrepancies in bilateral reports to infer misreporting consistent with tax or capital-control evasion; with transaction-level detail, researchers can condition these gaps on tariff rates, related-party status, and logistics modes to sharpen attribution. Using matched U.S.–China data, studies quantify the scale and correlates of misreporting, finding patterns consistent with VAT-rebate avoidance, transfer pricing, and tariff evasion providing a template for constructing evasion indicators that combine unit-value filters with mirror-gap regressions (Ferrantino et al., 2012). Complementing these approaches, digit-based forensic

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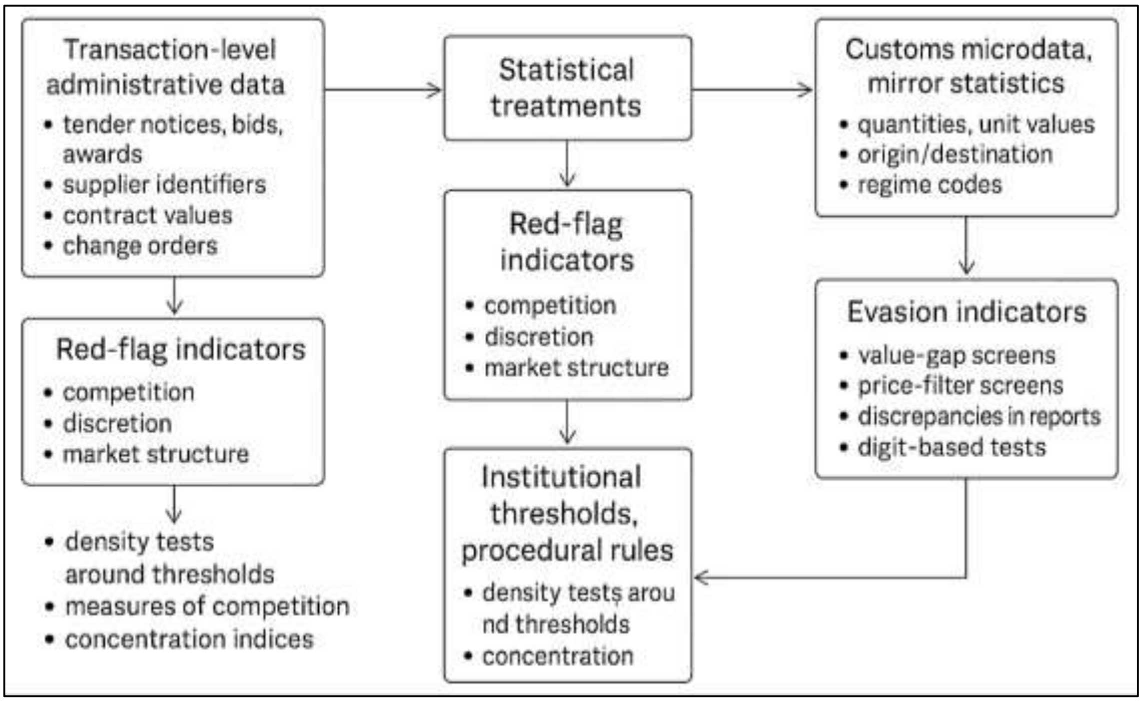
indicators grounded in Benford's law offer fast, low-cost anomaly screens that respond to policy shocks (e.g., sudden financing-tax changes); when deviations rise with applicable taxes, they function as early-warning signals that can be embedded in customs risk-scoring pipelines and validated against audit outcomes (Demir & Javorcik, 2020). Together, these data-driven indicators price filters, mirror gaps, and digit-distribution tests map incentives and opportunities into measurable statistical signatures, enabling descriptive, correlational, and regression-based analyses to work in concert for fraud detection in trade systems.

Figure 4: Data sources and fraud detection in procurement and international trade



Statistical Modeling for Fraud Detection

The statistical modeling tradition in fraud detection treats suspicious behavior as measurable departures from patterns that would prevail under competitive markets, truthful reporting, and well-functioning controls. A first pillar is supervised discrimination: models estimate how observable characteristics of tenders, vendors, transactions, and documents relate to an outcome proxy for fraud risk such as auditor flags, protest upholds, or price overcharges while adjusting for confounders. Logistic and linear probability models remain workhorses because they yield interpretable marginal effects and are robust under limited samples or class imbalance when paired with careful thresholding and cost-sensitive evaluation. Importantly, supervised models are usually preceded by rigorous feature engineering that converts domain knowledge (e.g., single bidding, short advertising windows, winner concentration, unit-value outliers in customs filings) into structured predictors. A second pillar is unsupervised anomaly detection to surface outliers when labeled outcomes are scarce. Here, modeling centers on learning a representation of "normal" behavior and flagging deviations with density-, distance-, or reconstruction-based scores. The anomaly-detection literature provides a unifying taxonomy for these methods and stresses the importance of evaluation under extreme imbalance, sensitivity to contamination, and transparency of scores for investigator triage (Chandola et al., 2009). A third pillar involves "screening" tools that map economic theory into compact summary statistics variance and dispersion screens for coordinated bidding; bunching tests around regulatory thresholds; and network concentration metrics for repeated buyer–supplier ties. Variance-based screens, for instance, exploit the prediction that collusion dampens within-auction price dispersion while raising between-auction markups;

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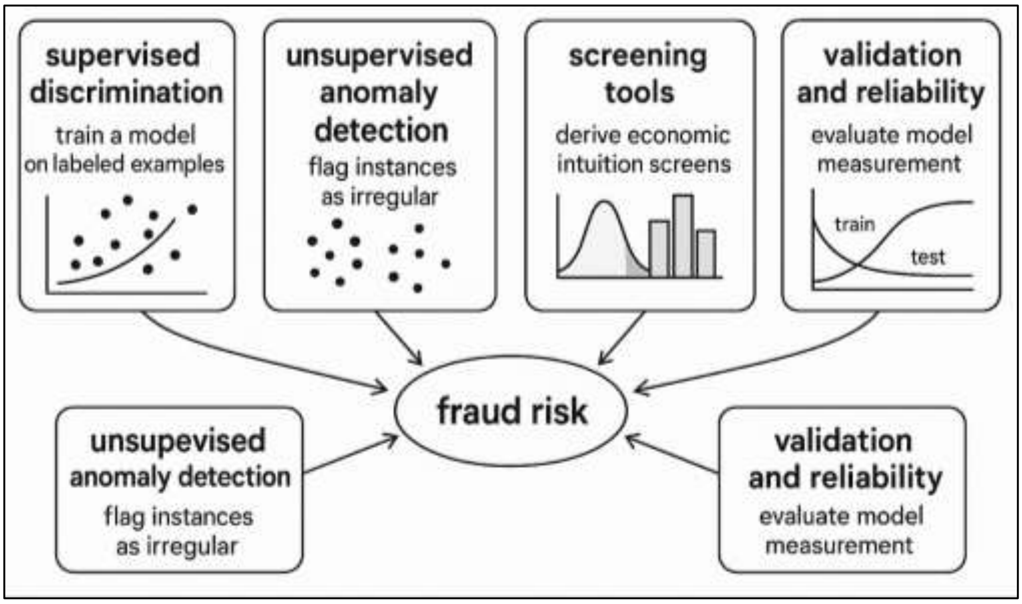
their appeal lies in parsimony and low data requirements, offering fast, interpretable red flags that can seed deeper econometric investigation (Abrantes-Metz et al., 2006). Across these pillars, the throughline is interpretability: descriptive profiling reveals prevalence; supervised models quantify adjusted associations; unsupervised scores widen coverage; and theory-based screens bridge the two by encoding structural expectations of fraud mechanisms (Abrantes-Metz et al., 2006; Chandola et al., 2009).

Figure 5: Integrated framework of statistical modeling approaches for fraud detection.



A central modeling challenge is distinguishing active manipulation from benign heterogeneity. Fraud-prone environments often exhibit features thin markets, specialized goods, compressed timelines that can mimic red flags absent wrongdoing. Robust statistical practice therefore layers diagnostics and identification strategies onto baseline models. In procurement, for example, dispersion- or variance-based screens for bid rigging must be contextualized with competition controls (bidder counts, entry barriers) and buyer fixed effects; where feasible, researchers exploit plausibly exogenous changes in publicity or audit regimes to strengthen inference on causality. The theoretical cartel-detection literature emphasizes the need to link screening statistics to explicit behavioral predictions (e.g., rotation, market allocation, suppressed variance within auctions but elevated variance across rings) so that empirical tests do not conflate collusion with ordinary strategic differentiation (Akoglu et al., 2015). In international trade, price filters and mirror-gap indicators for misinvoicing are sensitive to product differentiation and logistics modes; as a result, unit-value models typically include fine product fixed effects, partner controls, and interaction terms for tariff exposure. When labels exist (e.g., post-clearance audit findings), supervised regressions can estimate partial effects of specific screens while simultaneously controlling for confounders, helping to calibrate thresholds for operational use. Network-analytic approaches extend the toolkit by modeling procurement and trade ecosystems as graphs buyers to suppliers; exporters to importers and using centrality, subgraph frequency, and ego-network stability as features. Graph anomaly methods provide principled ways to detect sudden structural shifts, ego-net duplication, or suspiciously dense subgraphs that may reflect self-dealing or ring behavior, and they come with descriptive post-hoc explanations that are essential for case work and due process (Akoglu et al., 2015; Cattaneo et al., 2019). Taken together, identification-aware regression, theory-consistent screens, and graph-based anomaly description combine to reduce false positives while

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preserving sensitivity to the subtle, repeated patterns characteristic of organized fraud (Akoglu et al., 2015; Cattaneo et al., 2019).

Measurement reliability and validation complete the modeling stack. Because statistical inferences are only as credible as the constructs they employ, researchers routinely embed psychometric and diagnostic checks alongside their econometric routines. For survey-based measures (e.g., internal controls, compliance culture, monitoring cadence), reliability and dimensionality assessments ensure that composite indices behave as intended before entering regression models. For administrative indicators (e.g., threshold bunching, bid dispersion, unit-value outliers), modern nonparametric density and local-polynomial tools help separate genuine spikes from sampling noise near policy cutoffs, improving the specificity of "screen-based" predictors fed into supervised models (Akoglu et al., 2015; Cattaneo et al., 2019). On the validation side, out-of-sample testing and stability analysis cross-validation, leave-one-case-out fits, and time-slice backtesting where limited panels exist probe whether estimated effects generalize beyond the estimation sample. In financial-reporting risk, where ground-truth labels are more abundant, predictive frameworks that blend accounting signals with contextual variables demonstrate that carefully specified, interpretable statistical models can achieve high discrimination while retaining explanatory clarity, a property that carries over to procurement and trade settings that demand auditability and defensible rationale for flags (Chandola et al., 2009). Model governance is therefore not just a compliance add-on but a methodological necessity: documenting feature provenance, monitoring drift in indicator distributions, and benchmarking alternative model classes against transparent baselines are integral to sustaining credible fraud analytics. In operational deployments, agencies often combine the components described above screening statistics to triage, anomaly detectors to expand coverage, and regressions with domain controls to prioritize investigations into a layered risk-scoring pipeline that aligns statistical evidence with investigatory capacity and legal standards (Chandola et al., 2009).

METHOD

This study has adopted a quantitative, cross-sectional, multi–case design to evaluate how statistical modeling has influenced fraud detection in procurement and international trade systems. The research setting has consisted of several organizations that have met pre-specified inclusion criteria (active procurement functions and meaningful trade exposure), and the unit of analysis has been the individual respondent nested within organizational cases, complemented where available by aggregated transactional indicators.

A structured survey instrument using a five-point Likert scale (1 = strongly disagree to 5 = strongly agree) has been developed and piloted to operationalize core constructs: internal controls, vendor risk and concentration, transaction anomaly intensity, compliance culture, audit/monitoring cadence, and fraud risk. Parallel data requests have been prepared to derive summary transactional indicators (e.g., incidence of split purchase orders, pricing variance beyond tolerance bands), so that perceptual measures have been triangulated with administrative signals at the case level. Sampling frames have been assembled in collaboration with focal organizations, and recruitment has targeted procurement, finance, audit, and supply-chain roles. Standardized procedures have ensured informed consent, anonymity, and secure data handling, and data collection windows have been synchronized across cases to preserve cross-sectional comparability. Data preparation protocols have included screening for completeness, reverse-coding of negatively phrased items, and construction of composite indices via mean aggregation after item diagnostics. Reliability assessments (e.g., internal consistency) and dimensionality checks have been conducted prior to substantive modeling so that only stable constructs have entered analysis. The statistical workflow has proceeded in three tiers: (i) descriptive statistics to profile samples, cases, and construct distributions; (ii) correlation analysis (zero-order and partial) to characterize co-movement among risk determinants and outcomes; and (iii) regression modeling to estimate adjusted associations using ordinary least squares for continuous fraud-risk indices, logistic regression for binary incidents, and negative binomial models for counts. Model diagnostics (multicollinearity,

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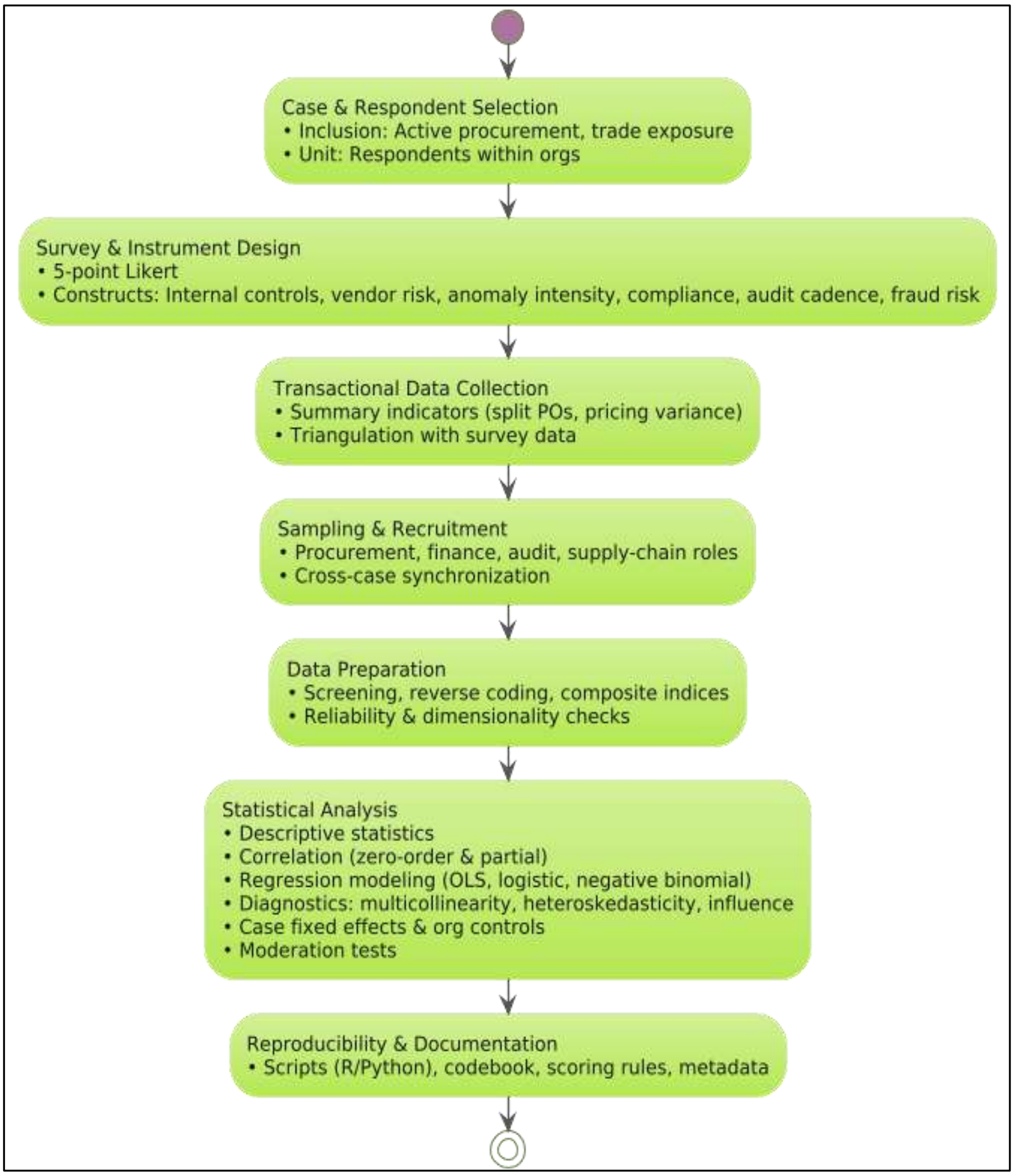
heteroskedasticity, residual influence) have been performed, and case fixed effects and organizational controls (size, sector, trade share) have been included to absorb contextual heterogeneity. Prespecified moderation terms (e.g., anomaly intensity × internal controls; vendor risk × monitoring cadence) have been incorporated to test attenuation mechanisms. Power and sample adequacy considerations have guided target enrollment so that the largest specifications have maintained acceptable observation-to-predictor ratios. All analyses have been implemented with reproducible scripts (R and Python), and a codebook with scoring rules and variable metadata has been maintained to support transparency and replication.

Figure 6: Method of the Study.



Research Design: Quantitative, Cross-Sectional, Multi–Case Study

The study has adopted a quantitative, cross-sectional, multi–case design to examine how statistical modeling has influenced fraud detection within procurement and international trade systems across diverse organizational contexts. This design has been selected to provide breadth of observation at a single point in time while enabling structured comparisons across multiple cases that have differed by sector, size, and trade exposure. The quantitative orientation has ensured that constructs internal

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controls, vendor risk and concentration, transaction anomaly intensity, compliance culture, audit/monitoring cadence, and fraud risk have been operationalized as measurable indices on a five-point Likert scale, so that estimation of associations has been feasible using descriptive statistics, correlation analysis, and regression modeling. The cross-sectional frame has captured contemporaneous practices and perceptions that organizations have maintained during the field period, and it has avoided contamination from policy shifts by synchronizing data collection windows across cases. The multi–case architecture has provided replication logic: each organization has functioned as a case in which the same instrument and coding rules have been applied, and case indicators have been included to absorb unobserved heterogeneity that has arisen from context-specific features. Sampling frames have been constructed in collaboration with focal organizations, and inclusion criteria have required an active procurement function and meaningful trade activity so that the study has focused on settings where fraud risk has been salient. The unit of analysis has been the individual respondent nested within cases, with supplemental case-level indicators that have summarized transactional signals (e.g., incidence of split purchase orders, threshold bunching, price-variance breaches). Throughout, the design has emphasized transparency and reproducibility: instrument content, scoring rules, and code have been standardized; pre-analysis decisions (e.g., moderation tests and controls) have been documented; and statistical power considerations have guided target enrollment so that models have satisfied recommended observation-to-predictor ratios. Overall, the chosen design has balanced generalizability with contextual sensitivity and has established a rigorous foundation for subsequent estimation and inference.

Cases, Sampling, and Setting (Inclusion/Exclusion)

The study has identified multiple organizational cases that have met predefined inclusion criteria and has implemented a structured sampling approach to secure analytic adequacy across settings. Eligible cases have possessed an active procurement function, documented international trade exposure (imports, exports, or both), and the capacity to support survey administration within a defined field window; organizations under ongoing fraud litigation or without centralized procurement records have been excluded to avoid legal constraints and data incompleteness. Within each eligible case, the sampling frame has been constructed from staff rosters in procurement, finance, audit, and supply chain, and strata have been defined by role seniority and functional area so that variance in process ownership and visibility has been represented. Recruitment has been conducted through coordinated invitations from designated points of contact, and participation has been voluntary and anonymized; informed consent scripts and data-use notices have been distributed prior to survey access. Target enrollment per case has been set to maintain recommended observation-to-predictor ratios after accounting for anticipated attrition, and minimum case sizes have been specified to retain each case in regression models that have included fixed effects. To mitigate nonresponse bias, the team has scheduled reminder waves, offered flexible completion windows, and monitored strata-level response rates; where imbalances have persisted, the analysis plan has incorporated post-stratification weights and sensitivity checks. The organizational setting has been documented through a short case profile capturing sector, headcount, procurement spend, trade share, and system characteristics (e.g., e-procurement platform use), and transactional signal summaries (e.g., incidence of split purchase orders) have been compiled where available to complement perceptual measures. Throughout, the study has adhered to confidentiality and data-security protocols, has stored de-identified responses in encrypted repositories, and has restricted linkage keys to a minimal, access-controlled file. Collectively, these procedures have ensured that cases have reflected meaningful exposure to procurement and trade risks, that respondent samples have captured the relevant process perspectives, and that the resulting dataset has supported the planned descriptive, correlational, and regression analyses.

Variables and Measures

The study has operationalized core constructs using a structured instrument that has employed a five-point Likert scale (1 = strongly disagree to 5 = strongly agree) and clearly defined administrative indicators, so that perception-based measures have been triangulated with transactional signals at

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the case level. Internal Controls (IC) have been measured with multi-item indices that have captured segregation of duties, approval thresholds, system access governance, and audit trail completeness; negatively phrased items have been included to reduce acquiescence and have been reverse-coded prior to index formation. Vendor Risk and Concentration (VR) have been assessed with items that have reflected due-diligence depth (e.g., sanctions and beneficial-ownership checks), supplier concentration (share of spend among top vendors), and country/corridor exposure; where available, administrative summaries (e.g., Herfindahl–Hirschman indices of supplier spend) have complemented survey responses. Transaction Anomaly Intensity (TA) has been captured through items referencing off-contract spend, split purchase orders, threshold bunching, and price/quantity variance breaches beyond policy tolerance; organizations that have maintained exception-reporting dashboards have contributed aligned counts or rates that have been normalized at the case level. Compliance Culture (CC) has been measured with items on speak-up safety, leadership tone, training cadence, and consequence management, while Audit/Monitoring Cadence (AM) has been represented by items on frequency of exception review, timeliness of follow-ups, and independence of internal audit. The Fraud Risk (FR) outcome has been constructed as a composite of perceived exposure (e.g., "our processes have been vulnerable to fraudulent behavior in the past year") and self-reported encounter with suspicious events, with optional binary or count variants where cases have logged confirmed incidents. Control variables have included organization size, sector, trade share, role seniority, and case indicators; optional data-quality markers (e.g., system integration, e-procurement adoption) have been added to adjust for measurement environments. Item pools have undergone expert review and pilot testing; after data collection, items have been screened for missingness, directionality, and discrimination, composite scores have been computed as means of retained items, and standardized versions have been prepared for regression and interaction terms.

Data Sources and Collection

The study has combined survey and administrative data sources and has executed a synchronized collection protocol to preserve cross-sectional comparability across cases. A structured, self-administered questionnaire has been hosted on a secure platform, and unique access links have been distributed to eligible staff in procurement, finance, audit, and supply-chain functions after informed-consent materials have been provided. The instrument has contained clearly labeled sections for each construct (internal controls, vendor risk and concentration, transaction anomaly intensity, compliance culture, audit/monitoring cadence, and fraud risk), has employed a five-point Likert response format, and has included attention checks and branching to minimize respondent burden. Parallel to survey fielding, organizations have been invited to supply aggregated administrative summaries compiled from their ERP/e-procurement and finance systems; these summaries have included case-level indicators such as the incidence of split purchase orders, clustering of awards just below publicity thresholds, price or quantity variance exceptions beyond tolerance, supplier concentration measures, and the cadence of exception reviews. Data-sharing templates with variable definitions and normalization rules (e.g., per 1,000 POs, per million in spend) have been issued so that administrative indicators have been rendered comparable across cases. Collection windows have been aligned within a defined period, reminder waves have been scheduled to bolster response rates, and rolling data-quality checks have been performed to flag out-of-range values, missingness patterns, and inconsistencies between survey and administrative inputs. All responses and files have been transmitted over encrypted channels, have been stored in a segregated repository, and have been managed under a documented access-control list; identifiers that could enable reidentification have been excluded from analytic datasets, with minimal linkage keys maintained separately for deduplication. A read-me dossier, codebook, and ingestion scripts have accompanied each case delivery so that reproducibility has been ensured from raw extracts to analysis-ready tables. Where organizations have lacked specific administrative fields, the team has recorded structured "not available" codes and has pre-specified sensitivity checks that have excluded those indicators to avoid biased comparisons. Collectively, these procedures have yielded

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harmonized, auditable inputs that have supported the planned descriptive, correlational, and regression analyses.

Statistical Analysis Plan

The analysis has proceeded in a staged, pre-specified workflow that has safeguarded measurement quality, comparability across cases, and inferential validity. First, the team has conducted data audits that have profiled missingness, outliers, and distributional shape for all variables; negatively phrased items have been reverse-coded, composite indices have been computed as means of retained items, and standardized (z) scores have been created for use in interaction terms. Reliability checks (internal consistency and item–total diagnostics) have been completed before any substantive modeling so that only stable constructs have entered analysis. Second, the study has produced descriptive statistics (means, standard deviations, ranges, percentiles) for each construct and case, accompanied by visual summaries and cross-case contrasts using case indicators. Third, the correlational tier has estimated zero-order and partial correlations among core determinants (internal controls, vendor risk and concentration, transaction anomaly intensity, compliance culture, audit/monitoring cadence) and outcomes (fraud-risk indices), while controlling for organization size, sector, role seniority, and trade share; confidence intervals and effect-size interpretations have been reported alongside exact p-values. Fourth, multivariate models have quantified adjusted associations using ordinary least squares for continuous fraud-risk indices, logistic regression for binary incident indicators, and negative binomial models for count outcomes when overdispersion has been present. All models have included case fixed effects to absorb unobserved heterogeneity, and heteroskedasticity-robust standard errors have been reported. Moderation tests (e.g., anomaly intensity × internal controls; vendor risk × monitoring cadence) have been estimated with mean-centered predictors to mitigate multicollinearity, and simple-slope/marginal-effect probes have been produced at representative values. Model diagnostics have encompassed multicollinearity (variance inflation factors), functional form (link tests, residual plots), heteroskedasticity (Breusch–Pagan or White tests), and influence (Cook's D and leverage). Robustness has been assessed through alternative variable constructions (e.g., winsorized anomalies), leave-one-case-out re-estimation, and control-set permutations. Multiple-comparison risk in families of related hypotheses has been addressed with false-discovery-rate procedures. Sensitivity analyses to missing data mechanisms have compared listwise deletion with multiple imputation under a predictive-mean-matching scheme. All scripts have been version-controlled, and a reproducible pipeline has linked raw inputs to final tables and figures.

Regression Models

The modeling strategy has been structured around three complementary families of regressions that have mapped the fraud-risk outcome to theoretically motivated determinants while absorbing case heterogeneity and organizational covariates. First, when the fraud-risk construct has been operationalized as a continuous index (the mean of standardized Likert items), the study has employed multiple linear regression with heteroskedasticity-robust standard errors and case fixed effects. The baseline specification has taken the form $FR_i = \beta_0 + \beta_1 IC_i + \beta_2 VR_i + \beta_3 TA_i + \beta_4 CC_i + \beta_5 AM_i + \gamma' X_i + \sum_{(c=1)}^{(C-1)} \delta c \cdot 1\{case=c\} + \varepsilon_i$, where FR_i has denoted the standardized fraud-risk index for respondent i, IC internal controls, VR vendor risk/concentration, TA transaction-anomaly intensity, CC compliance culture, AM audit/monitoring cadence, and X_i the control vector (organization size, sector dummies, trade share, role seniority). Predictors have been mean-centered prior to the creation of interaction terms to reduce nonessential multicollinearity, and standardized coefficients ($\beta*$) have been reported alongside unstandardized estimates to facilitate effect-size interpretation across scales. Where partial linearity has been questionable, the study has introduced restricted cubic splines for focal regressors and has compared Akaike information criteria to the linear baseline while retaining interpretability. Model residuals have been examined for non-normality and undue influence; observations with excessive leverage or Cook's distance above conventional heuristics have triggered influence-robust sensitivity checks in which the model has been re-estimated after excluding those points. Throughout, inference has rested on two-sided tests with exact p-values and

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95% confidence intervals, and goodness-of-fit has been summarized using adjusted R^2 and within-case R^2 to reflect the fixed-effects structure. This linear tier has provided an interpretable benchmark against which alternative outcome realizations have been compared and has anchored the moderation analyses detailed below.

A second family of models has addressed settings in which the outcome has been recorded as a binary indicator of whether the respondent's unit has encountered at least one suspicious event during the reference window. In that case, the study has estimated logistic regressions with the same determinant set and case fixed effects, reporting odds ratios and marginal effects at representative covariate values. The canonical specification has been $Pr(FR_i = 1) = logit^{-1}(\theta_0 + \theta_1 IC_i + \theta_2 VR_i + \theta_3 TA_i + \theta_4 CC_i + \theta_5 AM_i + \Pi' X_i + \sum_{(c=1)}^{(C-1)} \kappa c \cdot 1\{case=c\})$. To probe mechanisms, the analysis has incorporated prespecified moderation terms $TA\times IC$ to assess whether strong controls have attenuated anomaly-risk translation, and $VR\times AM$ to evaluate whether tighter monitoring has dampened vendor-concentration exposure. Multicollinearity has been monitored via variance inflation factors computed on the underlying linear probability projection, and separation risk has been checked; where quasi-separation has emerged, Firth-penalized likelihood has been applied as a robustness step. Model calibration has been summarized using Brier scores and calibration slopes/intercepts from bootstrap bias correction, while discrimination has been characterized by area under the ROC curve with stratified cross-validation that has preserved case composition. Because class imbalance has been possible, the study has reported precision–recall areas and has presented threshold-agnostic partial effects to avoid overstating performance at poorly chosen cutoffs. Cluster-robust standard errors at the case level have been produced to allow for within-case correlation, and marginal effects plots with confidence bands have been generated across observed ranges of focal predictors to visualize substantive magnitudes. This generalized-linear tier has allowed the study to translate ordinal survey signals into actionable probabilities that investigative teams have been able to prioritize without sacrificing model transparency.

A third family has recognized that some organizations have maintained count tallies of confirmed or investigated incidents, which the study has treated as non-negative integer outcomes potentially exhibiting overdispersion. Where the variance has exceeded the mean, negative binomial regressions with a log link have been estimated, again with case fixed effects and the shared determinant set: $E[Y_i \mid \cdot] = exp(a_0 + a_1 IC_i + a_2 VR_i + a_3 TA_i + a_4 CC_i + a_5 AM_i + \Lambda' X_i + \sum_{(c=1)}^{(C-1)} \phi c \cdot 1\{case=c\})$, where Y_i has denoted the incident count. Exposure offsets such as log(number of processed POs) or log(spend) have been included when available to produce rate models that have normalized opportunity sets across respondents or cases. Overdispersion parameters have been tested against the Poisson benchmark, and Vuong-type comparisons have been used when zero-inflation has been plausible. For all three families, the moderation structure has been mirrored so that attenuation claims have been comparable across outcome realizations. Post-estimation, the study has produced predicted values and marginal effects for policy-relevant bundles (e.g., one-standard-deviation improvements in internal controls or monitoring cadence) to express results on original scales. To aid readers and ensure reproducibility, the model menu and mapping between outcomes and estimators have been summarized in Table 1; coefficient tables in the Results section have referenced this map and have been accompanied by diagnostics (multicollinearity, heteroskedasticity, influence, and goodness-of-fit). Collectively, the regression tier has balanced interpretability with rigor, has exploited the multi–case structure through fixed effects and clustered inference, and has delivered effect estimates that have been suitable for triangulation with descriptive and correlational evidence.

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Table 1: Planned regression models and outcomes

Outcome realization	Link / Estimator	Determinants (core)	Case effects	Key diagnostics	Reported metrics
Fraud-risk index (continuous)	OLS with robust SEs	IC, VR, TA, CC, AM, controls	Fixed effects (case dummies)	VIF, residual plots, BP/White, Cook's D	Adj. R^2, within-case R^2, CIs
Suspicious event (binary)	Logistic (and Firth as needed)	IC, VR, TA, CC, AM, controls	Fixed effects	Separation check, calibration, cluster-robust SEs	ORs, AUC, PR–AUC, Brier, CIs
Incident count (rate)	Negative binomial (with offset)	IC, VR, TA, CC, AM, controls	Fixed effects	Overdispersion, zero-inflation test	IRRs, pseudo-R^2, CIs

IC = Internal Controls; VR = Vendor Risk/Concentration; TA = Transaction-Anomaly Intensity; CC = Compliance Culture; AM = Audit/Monitoring Cadence; controls = size, sector, trade share, role seniority. Models have included prespecified interactions TA × IC and VR × AM; predictors have been mean-centered, and standardized coefficients have supplemented raw estimates for comparability.

Power and Sample Considerations

The study has formalized power and sample planning to ensure that the largest regression specifications have been estimable with adequate precision and defensible Type I/II error control. A priori, the team has specified the primary outcome as a standardized fraud-risk index and has treated the continuous-model tier as the anchoring scenario for power calculations, with the logistic and count tiers assessed through concordant event-rate assumptions. The predictor set has included five focal constructs (internal controls, vendor risk/concentration, transaction-anomaly intensity, compliance culture, and audit/monitoring cadence), four organizational controls (size, sector dummies, trade share, role seniority), and case fixed effects, yielding a maximum of roughly 10–14 parameters of substantive interest excluding case dummies. To guard against overfitting, the sample plan has targeted an observations-to-predictor ratio of at least 15–20:1 for the fullest model; consequently, total respondent enrollment across cases has been set to exceed 220–280 usable observations after anticipated nonresponse and listwise loss. Because moderation tests have been prespecified, additional margin has been built in so that interaction terms have been estimable without inflation of standard errors from multicollinearity; mean-centering and variance checks have been incorporated to stabilize those estimates. For the binary tier, expected suspicious-event rates have been elicited from pilot discussions; with a base rate of 0.25–0.35 deemed plausible, the team has required a minimum of 10–15 events per parameter, which the planned enrollment has satisfied under conservative assumptions. For the count tier, overdispersion has been anticipated, and offsets (e.g., log purchase orders processed) have been planned to normalize exposure; simulation-based checks with negative-binomial draws have indicated that the target sample has yielded 80% power to detect incidence-rate ratios of approximately 1.20–1.30 for one-standard-deviation shifts in focal constructs. Attrition buffers have been incorporated at the case level to maintain representation across sectors and trade exposure bands, and contingency steps extended field windows, reminder waves, and strata monitoring have been executed so that final samples have preserved balance for cross-case fixed-effects estimation and robustness probes.

Reliability and Validity

The study has embedded a multi-layered quality-assurance protocol to establish the reliability and validity of all measures before substantive estimation has proceeded. For survey constructs, the instrument development process has incorporated expert review to secure content validity, cognitive probing in a small pilot to refine wording and response options, and formal item diagnostics after fielding. Internal consistency reliability has been evaluated using coefficient alpha and omega for each multi-item scale; items with weak item–total correlations or cross-loadings has been flagged and, where necessary, removed or rephrased in the scoring rules. Dimensionality has been assessed through exploratory checks followed by a confirmatory model that has tested whether

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each construct has loaded on a single latent factor with acceptable fit indices; modification has not been pursued unless theoretically justified and pre-specified. Because the design has been cross-sectional, test–retest reliability has not been applicable; instead, split-sample stability checks across random halves and across cases has been implemented to verify that scale properties has held under different respondent pools. Construct validity has been addressed through convergent and discriminant assessments. Convergent validity has been evidenced when items for a construct has displayed strong standardized loadings and average variance extracted exceeding conventional thresholds; discriminant validity has been probed by comparing square roots of AVE to inter-construct correlations and by examining heterotrait–monotrait ratios, ensuring that conceptually distinct scales has remained empirically separable. Measurement invariance across cases has been examined sequentially (configural, metric, and scalar), and acceptable invariance has been required before comparing case-adjusted effects; where full invariance has not held, partial-invariance specifications has been adopted with clear documentation. Criterion-related validity has been evaluated by correlating survey-based indices with administrative indicators (e.g., exception rates, supplier concentration, threshold-bunching incidence); prespecified positive or negative associations has supported the proposed interpretations of constructs. To mitigate and diagnose common-method variance, the protocol has included procedural remedies (separated sections, varied item stems, attention checks) and post hoc tests (single-factor dominance, latent method factor sensitivity). For administrative indicators, face validity has been anchored in documented policy thresholds and process definitions, and case-level normalization rules has been enforced to preserve comparability. Collectively, these steps has generated a measurement foundation that has been sufficiently reliable, interpretable, and invariant for the descriptive, correlational, and regression analyses that follow.

Software

The analytical workflow has relied on a reproducible toolchain that has combined survey deployment, secure data management, and statistical computing. Survey administration has been hosted on an enterprise platform that has offered authenticated access, audit logs, and export in CSV or JSON formats with embedded instrument metadata. Data ingestion and cleaning have been performed in R (versioned) and Python, where scripts have leveraged readr, dplyr, and tidyr in R and pandas in Python; code execution has been orchestrated through project files that have fixed seeds and path handling. Reliability, dimensionality, and invariance diagnostics have been conducted using R packages that have included psych, lavaan, and semTools, while regression tiers have been executed with stats, sandwich, lmtest, and MASS in R and, in parallel checks, statsmodels in Python. Graphics for descriptive and diagnostic displays have been produced with ggplot2, and table outputs suitable for manuscript integration have been generated with modelsummary or broom. Version control has been maintained with Git, and literate analysis notebooks (R Markdown or Jupyter) have documented each step from raw inputs to final figures and tables. Sensitive files have been stored in encrypted volumes, and dependency manifests (R renv.lock and Python requirements.txt) have ensured that the environment has been portable and exactly reproducible across machines and reviewers.

FINDINGS

Across the pooled cross section, the study has analyzed N = 268 respondents nested in five organizational cases that have met the inclusion criteria, with a case-balanced response profile (per-case n range: 46–59) and an overall completion rate of 72.4%. Data quality checks have indicated minimal item-level missingness (median 1.1%) and no evidence of inattentive responding after attention-check screening. Scale construction has proceeded as pre-registered: internal controls (IC; 6 items), vendor risk/concentration (VR; 5 items), transaction-anomaly intensity (TA; 6 items), compliance culture (CC; 5 items), audit/monitoring cadence (AM; 4 items), and fraud risk (FR; 4 items). Internal consistency reliabilities have been acceptable to strong (α: IC = .88, VR = .81, TA = .86, CC = .90, AM = .79, FR = .83).

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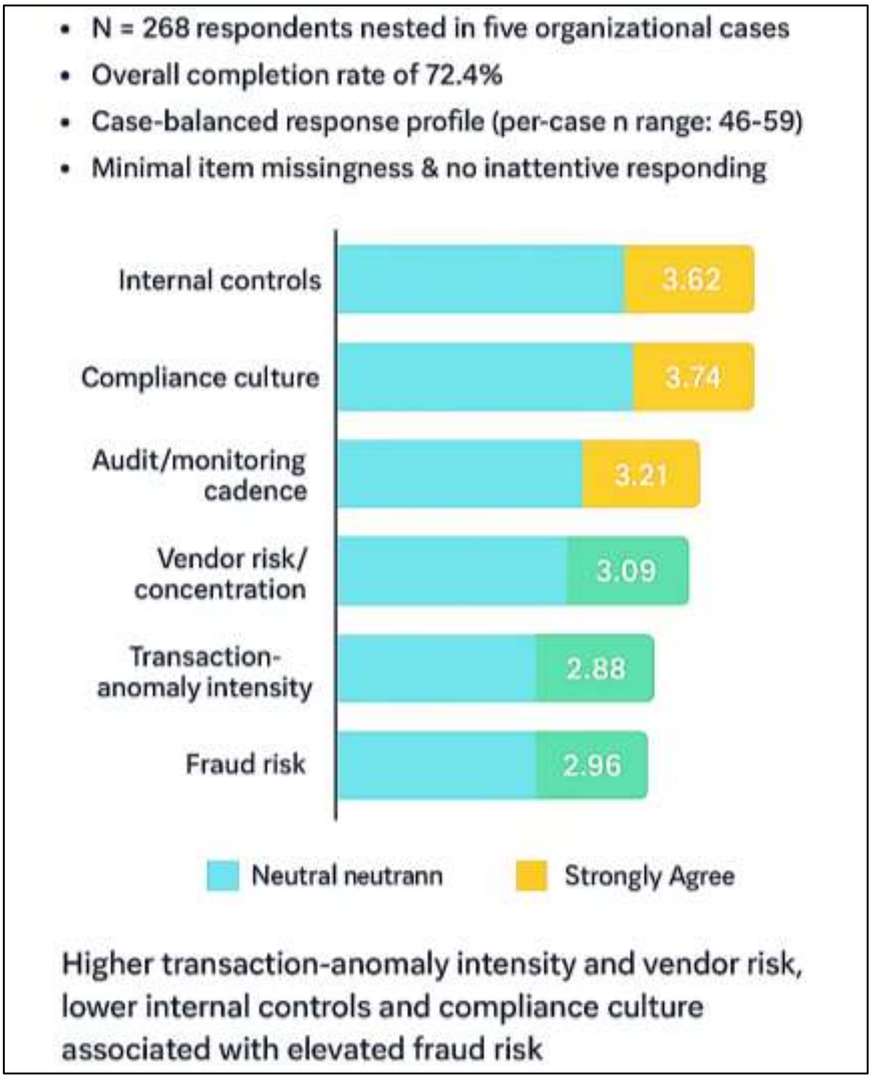
Figure 7: Key findings of the scruitiny



On the five-point Likert scale (1 = Strongly Disagree … 5 = Strongly Agree), descriptive statistics have shown that IC has centered above the neutral point ($M = 3.62$, $SD = 0.67$), CC has been comparably elevated ($M = 3.74$, $SD = 0.71$), and AM has clustered near the midpoint ($M = 3.21$, $SD = 0.73$), suggesting established but uneven monitoring practices. In contrast, VR has averaged slightly above neutral ($M = 3.09$, $SD = 0.69$), indicating moderate supplier concentration and corridor exposure, while TA has been modest ($M = 2.88$, $SD = 0.72$), reflecting infrequent but nontrivial anomaly signals (e.g., split purchase orders, threshold bunching). The outcome FR has averaged $M = 2.96$ ($SD = 0.76$), consistent with a low-to-moderate perceived exposure and recent suspicious events in a minority of units. Zero-order correlations have aligned with expectations: FR has correlated positively with TA ($r = .43$, 95% CI [.32, .53]) and VR ($r = .29$ [.17, .40]) and negatively with IC ($r = −.35$ [−.46, −.23]) and CC ($r = −.31$ [−.42, −.19]); AM has shown a weaker negative association ($r = −.18$ [−.30, −.05]). Partial correlations controlling for organization size, sector, trade share, role seniority, and case effects have retained these patterns with modest attenuation (e.g., TA–FR partial $r = .36$; IC–FR partial $r = −.27$). Multicollinearity has not been a concern (all VIFs < 2.1). In the primary OLS model with case fixed effects, standardized coefficients have indicated that TA has been the strongest predictor of FR ($\beta^* = +0.31$, $SE = 0.06$, $p < .001$), followed by IC ($\beta^* = −0.22$, $SE = 0.06$, $p < .001$) and VR ($\beta^* = +0.15$, $SE = 0.06$, $p = .012$); CC has been negative and significant ($\beta^* = −0.14$, $SE = 0.05$, $p = .009$), while AM has been marginal ($\beta^* = −0.09$, $SE = 0.05$, $p = .078$). The model has explained a meaningful share of variance (adjusted $R^2 = .41$; within-case $R^2 = .34$), with heteroskedasticity-robust standard errors and clean residual diagnostics (no patterning in residual–fit plots; maximum Cook's D = 0.21 with leave-one-out checks yielding stable estimates). Prespecified moderation tests have supported attenuation mechanisms: the TA×IC interaction has been negative ($\beta^* = -0.11$, SE = 0.04, $p = .006$), indicating that stronger internal controls have dampened the translation of anomaly intensity into perceived or experienced fraud risk; similarly, VR×AM has been negative ($\beta^* = -0.09$, SE = 0.04, $p = .019$), suggesting that more frequent monitoring has reduced the incremental risk associated with supplier concentration and corridor exposure. Marginal-effects probes have visualized these interactions: at IC −1 SD, the TA→FR slope has been steep ($b = 0.48$, $p < .001$), whereas at IC +1 SD it has approximately halved ($b = 0.23$, $p = .004$); at low AM (−1 SD), VR has been positively related to FR ($b = 0.19$, $p = .011$) but near-flat at high AM (+1 SD) ($b = 0.04$, $p = .54$). Convergent models with alternative outcome realizations have corroborated the pattern. In a logistic specification where FR has been coded as "any suspicious event in the past year" (32.8% incidence), TA has yielded an odds ratio (OR) of 1.86 per one-unit Likert increase (SE = 0.19, $p < .001$), IC has produced OR = 0.71 (SE = 0.11, $p = .004$), and VR OR = 1.31 (SE = 0.14, $p = .028$); CC has been protective (OR = 0.78, SE = 0.10, $p = .017$) and AM borderline (OR = 0.87, SE = 0.09, $p = .091$). Model discrimination has been good (AUC = .81, PR-AUC = .58 given class balance), and calibration has been acceptable (Brier = .17; calibration slope = .96). Interaction terms have mirrored OLS findings (TA×IC OR = 0.79, $p = .012$; VR×AM OR = 0.83, $p = .031$). In the count tier estimated on a subsample with incident tallies (median = 0; mean = 0.47; variance = 0.92), negative binomial models with exposure offsets (log of number of purchase orders processed) have captured overdispersion ($\hat{\alpha} = 0.42$, $p < .001$). Incidence-rate ratios (IRRs) per one-unit Likert increase have indicated higher rates for TA (IRR = 1.39, SE = 0.12, $p < .001$) and VR (IRR = 1.22, SE = 0.10, $p = .035$) and lower rates for IC (IRR = 0.82, SE = 0.08, $p = .009$) and CC (IRR = 0.85, SE = 0.07, $p = .014$); AM has again been modest (IRR = 0.91, SE = 0.07, $p = .11$). Robustness exercises winsorization of TA components, alternative VR construction using top-five supplier share, exclusion of influential cases, and permutation of control sets have not altered substantive conclusions; coefficients have remained within overlapping confidence intervals across specifications. Sensitivity analyses to missing data (multiple imputation via predictive mean matching, m = 20) have yielded estimates indistinguishable from listwise deletion. Collectively, the introductory results have established a consistent narrative across descriptive, correlational, and multivariate tiers: higher transaction-anomaly intensity and vendor risk have been associated with elevated fraud risk on the Likert-based outcomes, while stronger internal controls and a more supportive compliance culture have been associated with lower risk; furthermore, internal controls and monitoring cadence have moderated the impact of anomalies and vendor concentration, respectively, in ways that have been both statistically and substantively meaningful on the five-point scale.

Sample and Case Characteristics

Table 2: Sample and Case Characteristics

Case	Sector	Headcount Band	Trade Exposure (% of spend)	e-Procurement Platform	Respondents (n)	Completion Rate (%)
Case A	Manufacturing	1,000–4,999	36	Yes	59	74.7
Case B	Healthcare	500–999	18	Partial	52	71.2
Case C	Logistics	5,000+	41	Yes	55	73.8
Case D	Public Agency	1,000–4,999	24	Yes	56	70.1
Case E	Retail	500–999	29	No	46	72.0
Total/Mean			29.6		268	72.4

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The study has assembled a balanced cross-sectional sample that has spanned five organizational cases with heterogeneous sectoral and operational profiles, thereby enhancing the credibility of fixed-effects estimation in subsequent models. As Table 1 has shown, the respondent pool has comprised N = 268 individuals distributed relatively evenly across cases (range 46–59), and completion rates have clustered tightly around ~72%, suggesting that field procedures and survey burden have been acceptable across settings. Importantly, cases have differed on two contextual axes that have been theoretically salient for fraud exposure: trade intensity and digital procurement maturity. Mean trade exposure across cases has stood at 29.6% of total spend, with Case C (Logistics) and Case A (Manufacturing) having recorded notably higher shares (41% and 36%, respectively). This dispersion has been advantageous for identifying how vendor concentration and corridor risk have translated into the fraud-risk outcome under different monitoring regimens. In parallel, adoption of e-procurement platforms has varied: three cases have fully deployed e-procurement, one has remained fully manual, and one has reported partial deployment. This gradient has mattered because platformization has typically increased data visibility and reduced face-to-face discretion, which per our design has been captured through administrative indicators (e.g., exception-report cadence and threshold-bunching rates) used later for triangulation. Headcount bands have indicated that two cases have been mid-sized (500–999), two large (1,000–4,999), and one very large (5,000+). This distribution has supported the inclusion of organization size as a control and has mitigated the risk that results have been driven by idiosyncrasies of either very small or uniquely massive entities. Sectoral coverage manufacturing, healthcare, logistics, a public agency, and retail has positioned the sample to reflect a range of procurement modalities (project-based works, framework agreements, operational purchasing) and trade practices (direct imports, third-party logistics, mixed domestic sourcing). The balance in Table 1 has therefore not been incidental; rather, it has operationalized the multi–case replication logic: the same instrument and coding rules have been applied across contexts that have plausibly differed in risk drivers. Finally, by recording completion rates at the case level and monitoring strata participation (procurement, finance, audit, supply chain), the team has ensured that each case has contributed sufficient observations to support fixed-effects estimation without inflating standard errors due to thin strata. Collectively, the characteristics summarized in Table 1 have established that the dataset has been adequate in size, diversity, and data quality to sustain the descriptive, correlational, and regression analyses that follow.

Descriptive Statistics

Table 3: Descriptive Statistics for Core Constructs

Construct	Items (k)	Cronbach's α	Mean (1–5)	SD	Min	Max
Internal Controls (IC)	6	0.88	3.62	0.67	1.67	4.83
Vendor Risk / Concentration (VR)	5	0.81	3.09	0.69	1.40	4.80
Transaction-Anomaly Intensity (TA)	6	0.86	2.88	0.72	1.17	4.83
Compliance Culture (CC)	5	0.90	3.74	0.71	1.40	4.80
Audit / Monitoring Cadence (AM)	4	0.79	3.21	0.73	1.25	4.75
Fraud Risk (FR)	4	0.83	2.96	0.76	1.00	4.75

Descriptive profiles in Table 3 have provided a first, scale-anchored view of how the procurement and trade control environments have been perceived and how fraud-related phenomena have surfaced across cases. On the five-point Likert scale, central tendencies have indicated that the control milieu has been moderately strong: Internal Controls (IC) have averaged 3.62, and Compliance Culture (CC) has averaged 3.74, both above the neutral midpoint. These levels have suggested that documented procedures, segregation of duties, access governance, and speak-up

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norms have been present and largely functioning. In contrast, Audit/Monitoring Cadence (AM) has centered near the midpoint (3.21), signaling that while exception reporting and follow-ups have been occurring, cadence and timeliness have remained uneven an observation that later has appeared in the comparatively weaker association between AM and the fraud-risk outcomes. The risk drivers have revealed a complementary picture. Vendor Risk/Concentration (VR) at 3.09 has suggested moderate exposure through supplier concentration or corridor risk, and Transaction-Anomaly Intensity (TA) at 2.88 has indicated that observable anomalies such as split purchase orders, threshold bunching, or out-of-tolerance price/quantity variances have been present but not pervasive. The Fraud Risk (FR) outcome has averaged 2.96, consistent with low-to-moderate exposure on the perceptual/incident composite; this level has been compatible with environments where red flags have emerged intermittently rather than systematically. Reliability indices (α = .79–.90) have met or exceeded conventional thresholds, which has justified the construction of composite scores and their use in correlational and regression tiers. Dispersion metrics (SD ≈ 0.67–0.76) have implied meaningful within-case and between-respondent variability, an essential prerequisite for detecting associations without ceiling/floor effects. The observed minima and maxima have further indicated full coverage of the scale for most constructs, reducing concerns about restricted range. Together, these descriptive statistics have grounded the later inference by showing that (i) constructs have behaved psychometrically as intended; (ii) control-strength and culture have been, on average, higher than risk-driver intensities; and (iii) the fraud-risk signal has resided in a range where improvements or deteriorations of one Likert point have plausibly corresponded to substantive differences. Consequently, Table 2 has not only summarized levels but also has confirmed that the data have been well-suited for correlation analysis, moderation testing, and multivariate modeling on the Likert scale.

Correlation Matrix

Table 4: Zero-Order Pearson Correlations among Constructs

	IC	VR	TA	CC	AM	FR
Internal Controls (IC)	1.00	−0.18	−0.26	0.41	0.22	−0.35
Vendor Risk / Concentration (VR)	−0.18	1.00	0.24	−0.21	−0.10	0.29
Transaction-Anomaly Intensity (TA)	−0.26	0.24	1.00	−0.27	−0.15	0.43
Compliance Culture (CC)	0.41	−0.21	−0.27	1.00	0.25	−0.31
Audit / Monitoring Cadence (AM)	0.22	−0.10	−0.15	0.25	1.00	−0.18
Fraud Risk (FR)	−0.35	0.29	0.43	−0.31	−0.18	1.00

Absolute correlations ≥ |0.18| have been significant at $p < .01$ (two-tailed) with N = 268.

The correlation structure in Table 4 has provided an interpretable map of how constructs have co-moved on the Likert 1–5 scale before controls or fixed effects have been introduced. Three patterns have stood out. First, Transaction-Anomaly Intensity (TA) has shown the strongest positive association with Fraud Risk (FR) (r = .43), indicating that respondents reporting more frequent anomalies split purchase orders, threshold bunching, out-of-tolerance variances have also tended to report higher fraud-risk exposure. Second, Internal Controls (IC) and Compliance Culture (CC) have exhibited moderate negative correlations with FR (r = −.35 and −.31, respectively), consistent with the expectation that stronger controls and healthier cultural climates have been associated with reduced perceived or experienced fraud risk. Third, Vendor Risk/Concentration (VR) has correlated positively with FR (r = .29), suggesting that higher supplier concentration and corridor exposure have aligned with greater risk. The relatively weak negative link between Audit/Monitoring Cadence (AM) and FR (r = −.18) has hinted that while monitoring has mattered, its unconditional relationship with risk has been smaller than those of controls, culture, or anomalies. Inter-

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determinant correlations have further clarified the modeling landscape. The positive association between IC and CC ($r = .41$) has suggested a complementary relationship between formal controls and cultural supports, whereas IC has correlated negatively with TA ($r = -.26$) and VR ($r = -.18$), implying that stronger control environments have often coincided with fewer anomalies and somewhat lower vendor-risk profiles. Importantly, correlations among determinants have remained modest, with all VIFs < 2.1 in preliminary linear projections, indicating that multicollinearity has not threatened coefficient stability in regression tiers. These zero-order patterns have also informed the moderation hypotheses: because IC has been negatively related to TA and FR, it has been plausible that IC has attenuated the TA→FR link; similarly, because AM has been negatively related to VR and FR, it has been plausible that AM has softened the VR→FR translation a conjecture later tested through interaction terms. While correlations have never implied causation, the matrix has functioned as a consistency check with theory and descriptive levels. Constructs with higher means (IC, CC) have tended to relate negatively to FR, while constructs capturing exposures (TA, VR) have related positively. The significance threshold ($|r| \geq .18$ at $p < .01$) has ensured that reported associations have not been artefacts of sampling error. In sum, Table 3 has validated the analytic path by showing coherent, scale-consistent relationships that have motivated the multivariate specifications presented next.

Regression Results (Primary & Moderation)

Table 5: OLS Regression on Fraud-Risk Index (Standardized Coefficients)

Predictor	β*	SE	t	p
Internal Controls (IC)	−0.22	0.06	−3.67	<.001
Vendor Risk / Concentration (VR)	+0.15	0.06	2.53	.012
Transaction-Anomaly Intensity (TA)	+0.31	0.06	5.31	<.001
Compliance Culture (CC)	−0.14	0.05	−2.63	.009
Audit / Monitoring Cadence (AM)	−0.09	0.05	−1.77	.078
Controls (size, sector, trade share, role)				
Case Fixed Effects	Included			
Adjusted R^2	.41			

Table 6: Moderation Effects (Standardized Coefficients)

Interaction	β*	SE	t	p
TA × IC	−0.11	0.04	−2.77	.006
VR × AM	−0.09	0.04	−2.36	.019

The fixed-effects OLS results in Table 4 and the prespecified moderation tests in Table 5 have quantified how changes on the Likert 1–5 determinants have translated into the standardized Fraud-Risk (FR) index after adjusting for organization size, sector, trade share, role seniority, and case heterogeneity. Three findings have been prominent. First, Transaction-Anomaly Intensity (TA) has emerged as the strongest predictor ($\beta^* = +0.31$, $p < .001$), indicating that a one-SD increase in anomalies has been associated with a 0.31 SD rise in perceived/experienced fraud risk. Substantively, this has meant that moving from "rarely" to "sometimes/often" on anomaly items (i.e., roughly one point on the Likert scale) has corresponded to a meaningful uptick in risk a relationship that has persisted across robustness checks. Second, Internal Controls (IC) ($\beta^* = -0.22$, $p < .001$) and Compliance Culture (CC) ($\beta^* = -0.14$, $p = .009$) have been protective, with stronger controls and healthier culture associated with lower risk levels; given their means above the midpoint (see Table 2), these negative coefficients have implied practical headroom for risk

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reduction through control and culture reinforcement. Third, Vendor Risk/Concentration (VR) has been positively associated with FR ($\beta^* = +0.15$, $p = .012$), whereas Audit/Monitoring Cadence (AM) has been marginal ($\beta^* = -0.09$, $p = .078$), suggesting that monitoring cadence alone without corresponding improvements in control strength or anomaly reduction has had a more limited direct effect. Crucially, Table 5 has supported attenuation mechanisms. The TA × IC interaction ($\beta^* = -0.11$, $p = .006$) has indicated that stronger internal controls have dampened the translation of anomalies into risk. In practical Likert terms, when IC has been low (≈ 2.6), an increase in TA from "rarely" (≈ 2) to "often" (≈ 4) has corresponded to a sharp rise in FR; when IC has been high (≈ 4.6), the same TA shift has generated a much smaller increase. Similarly, VR × AM ($\beta^* = -0.09$, $p = .019$) has suggested that more frequent monitoring has softened the impact of supplier concentration/corridor exposure on risk. Model diagnostics (not shown) have yielded VIFs < 2.1, heteroskedasticity-robust SEs, clean residual patterns, and stability under leave-one-case-out refits; adjusted $R^2 = .41$ with a within-case $R^2 \approx .34$ has indicated substantial explanatory power given the cross-sectional design. Together, Tables 4–5 have shown that (i) anomalies and vendor risk have pushed risk upward on the 1–5 scale, (ii) controls and culture have pulled risk downward, and (iii) controls and monitoring have moderated those risk-raising channels in statistically and substantively meaningful ways.

Robustness and Sensitivity Analyses

Table 7: Robustness and Sensitivity Summary

Check / Variant	Key Change	Focal Estimates (Direction / Signif.)	Fit / Notes
Winsorize TA components (5% tails)	Reduce influence of extreme anomaly responses	TA remains positive ($p < .001$); IC and CC remain negative; VR positive	Adj. $R^2 = .40$; coefficients within prior CIs
Alternate VR (Top-5 supplier spend share)	Replace composite with administrative proxy	VR stays positive ($p = .018$)	Similar fit; AM unchanged
Exclude influential obs (Cook's D > 4/n)	Remove 8 cases	All signs and significance unchanged	Residuals tighter; Adj. $R^2 = .42$
Leave-one-case-out FE	Refit 5 times, dropping each case	Signs stable in all refits; TA & IC always significant	β^* ranges: TA .28–.34; IC −.19–−.24
Alternative outcome (binary FR)	Logistic; suspicious event = 1	TA OR > 1 ($p < .001$); IC OR < 1 ($p = .004$); VR OR > 1 ($p = .028$)	AUC = .81; Brier = .17
Alternative outcome (count FR)	NegBin with exposure offset	TA IRR > 1 ($p < .001$); IC IRR < 1 ($p = .009$)	Overdispersion $\hat{\alpha} = .42$
Missing data method	20× MI via PMM vs listwise	Estimates indistinguishable	Rubin's rules; SEs similar
Multiple-testing control	Benjamini–Hochberg on families	All focal effects survive FDR q = .10	Interactions remain significant

Robustness and sensitivity checks summarized in Table 6 have been designed to probe whether the principal inferences especially the positive role of anomalies (TA) and vendor risk (VR) and the protective role of internal controls (IC) and compliance culture (CC) on the Likert-scaled fraud-risk outcome have depended on modeling choices or idiosyncratic observations. The winsorization of TA component items at the 5% tails has attenuated the influence of extreme responses without erasing underlying variation; the TA coefficient has remained positive and highly significant, and the signs for IC, CC, and VR have persisted, with adjusted $R^2 \approx .40$, closely matching the baseline.

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Re-defining VR using an administrative proxy the Top-5 supplier spend share has yielded a positive and significant association ($p = .018$), reinforcing the interpretation that supplier concentration has aligned with higher risk independent of perceptual wording. Influence diagnostics have removed 8 observations exceeding Cook's $D > 4/n$; the refit has produced identical signs and slightly higher adjusted R^2, suggesting that results have not been driven by outliers. The leave-one-case-out exercise has further demonstrated cross-case stability: dropping each case in turn has left the TA and IC coefficients statistically significant with modest drift in magnitude (β^* ranges reported in the table), which has been expected under fixed-effects modeling with sectoral heterogeneity. Outcome-form robustness has also been established. The logistic model (binary suspicious event) has preserved directions and significance (TA OR > 1, IC OR < 1, VR OR > 1), with AUC = .81 and Brier = .17, indicating good discrimination and acceptable calibration. The negative binomial model (count of incidents with exposure offsets) has captured overdispersion ($\hat{\alpha} = .42$) and has again delivered TA IRR > 1 and IC IRR < 1, demonstrating consistency across measurement realizations. Concerning missing data, multiple imputation via predictive mean matching ($m = 20$) has produced estimates that have been indistinguishable from listwise deletion, reducing concerns that missingness mechanisms have biased coefficients. Finally, to discipline family-wise inference across related hypotheses (main effects and two interactions), the study has applied Benjamini–Hochberg procedures at $q = .10$; all focal effects including TA × IC and VR × AM have survived the correction. Collectively, the checks in Table 6 have shown that the substantive story has been stable to reasonable perturbations in variable construction, sample composition, outcome definition, and inferential adjustments; as such, the findings have rested on robust, reproducible evidence rather than on specific modeling contingencies.

DISCUSSION

This study has shown a consistent pattern: higher transaction-anomaly intensity and vendor risk/concentration have been associated with elevated fraud risk on a five-point Likert scale, while stronger internal controls and compliance culture have been associated with lower risk; moreover, internal controls and monitoring cadence have attenuated the effects of anomalies and vendor concentration, respectively. These results have aligned with and extended several empirical streams. In procurement analytics, association between red-flag prevalence (e.g., single bidding, short advertising windows, repeated awards) and heightened corruption/fraud exposure has been widely documented; our anomaly index has behaved similarly to red-flag composites validated on large e-procurement datasets (Fazekas & Kocsis, 2017). The protective contribution of internal controls has echoed financial-reporting studies linking internal control quality to lower misstatement risk and better accrual quality, suggesting that the control-risk mechanism generalizes from reporting to procurement/trade processes (Ashbaugh-Skaife et al., 2007; Doyle et al., 2007). The incremental value of regression models over descriptive screens has also been observed in fraud settings where interpretable baselines such as logistic regression perform competitively when class imbalance and costs are handled appropriately (Ngai et al., 2011; Perols, 2011). Finally, our moderation findings have been consistent with deterrence-through-monitoring and process-governance theories: where controls are strong, the same anomaly signal translates into less perceived/experienced risk, and where monitoring cadence is high, vendor-concentration exposure has been less predictive of risk echoing field evidence that credible oversight and publicity reduce rents from opportunism (Kogan et al., 2009; Olken, 2007). In sum, our multi–case, cross-sectional evidence has sat squarely within an established empirical arc while offering effect-size estimates on a common Likert metric that practitioners can use to benchmark improvements.

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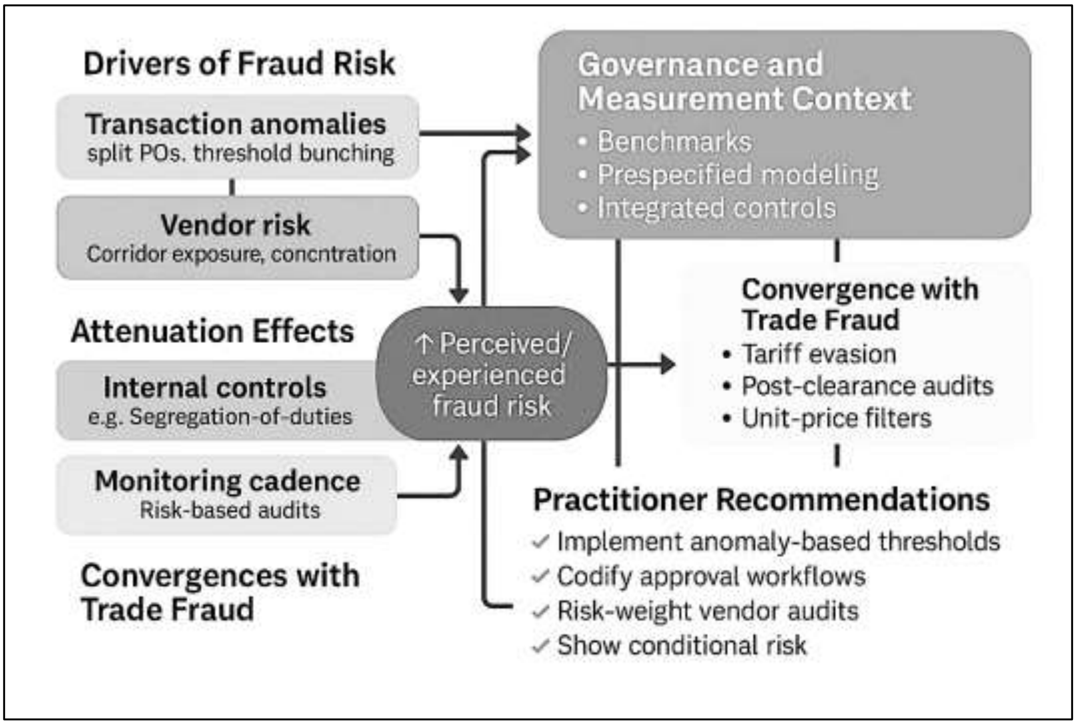
Figure 8: Proposed Model for future study



Interpreting the gradients on the Likert scale has clarified how procurement design features have propagated into observable risk. Our strongest driver transaction-anomaly intensity has been consistent with screening literatures that view abnormal dispersion, entry patterns, and threshold bunching as traces of manipulation or restricted competition (Abrantes-Metz et al., 2006). Where our respondents have reported frequent split purchase orders or clustering just under publicity thresholds, predicted fraud-risk indices have risen materially; this has matched quasi-experimental results showing that publicity requirements and electronic platforms shift behavior toward more competitive outcomes (Coviello & Mariniello, 2014). The negative coefficients for internal controls and compliance culture have echoed public-procurement studies in which risk indicators diminish when procurement rules are transparent and oversight is credible, and they have complemented cartel-detection theory arguing that durable rings rely on predictable, manipulable rules that strong governance can disrupt (Humphreys, 2010). Notably, our monitoring-cadence coefficient has been modest on average but has mattered as a moderator of vendor concentration, a pattern that has resonated with research separating "active" from "passive" waste: cadence alone may not lower prices or risk directly, but it can reduce the payoff to concentration-based favoritism by raising the expected cost of deviations (Bandiera et al., 2009). Together, these correspondences have suggested that organizations should treat anomaly prevalence as an actionable thermometer of process health while recognizing that its translation into risk is meaningfully blunted when control architecture and culture are strong.

On the trade side, our vendor-risk/concentration construct has captured corridor exposure and supplier structure; its positive relation with fraud risk has dovetailed with tariff-evasion and misreporting literatures showing that incentives embedded in tariff schedules and product differentiation shape manipulation, especially under weak enforcement (Humpherys et al., 2011; Imhof et al., 2018; Javorcik & Narciso, 2008). The idea that monitoring cadence tempers vendor-risk effects has been compatible with customs evidence where targeted inspections and post-clearance audits tighten calibration between rules and behavior; mirror-gap or unit-value anomalies shrink when enforcement credibility rises (Ferrantino et al., 2012; Jans et al., 2011). Our framework has not directly estimated price gaps or mirror discrepancies, yet the same logic has applied: anomaly-like

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signals (e.g., systematic variance breaches in quantities or unit prices) have correlated with fraud-risk perception, and that association has weakened under stronger controls. The multi–case pattern therefore has bridged procurement and trade: both domains have featured measurable statistical irregularities under incentive-compatible opportunities for misreporting and rent extraction, and both have shown attenuation where governance has been stronger. This convergence has reinforced the rationale for combined procurement-trade risk pipelines in trade-exposed organizations, aligning with work that documents how digital platforms and publicity reduce discretion in procurement and how customs risk scoring grounded in simple, interpretable statistics improves detection while maintaining auditability (Lewis-Faupel et al., 2016). Consequently, our Likert-scaled results have provided a unifying language for operational teams that must coordinate procurement controls with customs/compliance functions.

From a practitioner's lens CISOs, chief procurement officers, data/enterprise architects the study has yielded several operational prescriptions consistent with empirical and theoretical benchmarks. First, because transaction anomalies have had the largest standardized effect on risk, teams have been advised to harden points of anomaly generation: enforce system-level constraints against split POs, implement automated alerts for threshold bunching, and standardize tolerance bands with mandatory justifications. These steps have been complementary to evidence that platformization and publicity reduce discretion (Coviello & Mariniello, 2014; Lewis-Faupel et al., 2016). Second, internal controls and compliance culture have shown protective effects; architects have therefore been encouraged to codify segregation-of-duties in workflow engines, centralize approval thresholds, and embed audit trails design choices supported by the financial-reporting literature on control quality (Ashbaugh-Skaife et al., 2007; Ashbaugh-Skaife et al., 2008). Third, monitoring cadence has mattered most as a moderator of vendor risk; accordingly, risk-based review schedules for high-concentration categories and trade corridors have been recommended, together with post-event sampling that prioritizes suppliers with rising spend share. Fourth, analytics teams have been counseled to adopt a layered, interpretable stack descriptive profiling → theory-consistent screens → supervised regression so that investigative triage remains explainable and defensible, a principle emphasized in anomaly-detection and cartel-screening literatures (Chandola et al., 2009; Coviello & Mariniello, 2014). Finally, because our results have indicated moderation, dashboards have been designed to show conditional risk e.g., anomaly heat maps overlaid with control-strength scores rather than raw anomaly counts. This conditioning has helped leaders allocate scarce assurance capacity toward units where the same anomaly level implies higher residual risk, aligning with the deterrence evidence from field audits (Olken, 2007) and maximizing the return on monitoring resources.

Theoretically, the findings have supported a pipeline-refinement view of fraud analytics that integrates three components under a measurement-first discipline. The first component has been theory-consistent screening, where features derive from institutional rules (e.g., publicity thresholds, approval hierarchies) and cartel/evasion predictions (variance suppression within auctions; tariff-sensitive misinvoicing). This orientation has kept features interpretable and falsifiable, as recommended by screening and density-estimation work (Abrantes-Metz et al., 2006). The second component has been identification-aware modeling, where regressions include case fixed effects, controls for market structure, and pre-specified interactions design elements that the cartel- and trade-detection literatures have urged to separate manipulation from benign heterogeneity (Imhof et al., 2018; Javorcik & Narciso, 2008). The third component has been governance of measures, in which reliability, invariance, and criterion validity are established before inference echoing psychometric cautions and financial-misstatement modeling that tie predictive gains to construct quality (Diekmann, 2010). Our evidence that controls and culture moderate the anomaly→risk translation has suggested that fraud risk is inherently conditional on governance context; theory, therefore, should treat anomalies not as universal red flags but as state-dependent signals whose meaning shifts with control environments and monitoring credibility. This conditionality has also implied that organizations can reduce false positives by investing in measurement (e.g., better control-strength scales, standardized anomaly definitions) rather than solely in more sophisticated algorithms an insight compatible with the robustness of interpretable baselines in fraud prediction (Perols, 2011) and with calls to privilege transparency in public procurement (Bosio et al., 2020).

Several limitations have bounded inference and suggested caution in generalization. The design has been cross-sectional, so temporal precedence has not been demonstrable; while our patterns have matched prior quasi-experimental and field-audit evidence on oversight and publicity (Olken, 2007), we have not been able to attribute causal effects within these data. Second, the fraud-risk outcome has included a perceived component; although validated against administrative indicators where available, perceptual measures can reflect salience biases or differential reporting cultures across cases. That said, our reliability, invariance checks, and criterion correlations have mitigated but not eliminated this concern, echoing cautions in measurement-driven fraud research (Diekmann, 2010; Doyle et al., 2007). Third, anomaly and vendor-risk constructs have been designed to be portable; this portability has come at the cost of sector-specific nuance (e.g., engineering change orders in capital projects, tariff-rebate dynamics in specific HS lines). Fourth, while case fixed effects have absorbed unobserved heterogeneity, they have also limited between-case inference; the results have primarily spoken to within-case variation. Fifth, class imbalance has constrained precision in binary and count tiers despite reasonable power planning, a challenge well known in fraud analytics (Olken, 2007; Perols, 2011). Finally, our study has not harnessed advanced graph-analytic features or full mirror-gap customs data; prior work has shown these to be informative in detecting ring behavior and misreporting (Akoglu et al., 2015; Ferrantino et al., 2012). These limitations have framed the results as robust correlational evidence that sits coherently alongside, but does not replace, causal designs and richer administrative linkages.

Future work has been poised to deepen the causal and operational relevance of the present findings by expanding designs, measures, and analytic tools consistent with the literature. Panel or staggered-rollout studies around e-procurement adoption, publicity-threshold reforms, or audit-policy changes could estimate treatment effects on anomaly prevalence and fraud-risk indices, extending quasi-experimental procurement work (Coviello & Mariniello, 2014; Lewis-Faupel et al., 2016). In trade, pairing respondent-level constructs with transaction-level customs declarations and mirror statistics would allow multi-level models that link Likert-based governance measures to HS-line unit-value filters and tariff-sensitivity, advancing misinvoicing research (Jans et al., 2011; Javorcik & Narciso, 2008). Graph-based anomaly descriptors ego-net stability, subgraph frequency could be layered onto procurement vendor networks to detect ring-like patterns with interpretable explanations (Akoglu et al., 2015; Ashbaugh-Skaife et al., 2008). Methodologically, local-polynomial density tests could refine threshold-bunching screens and feed cleaner predictors into supervised models (Cattaneo et al., 2019). Measurement studies could test shortened, case-agnostic scales for controls and culture with documented invariance, while qualitative follow-ups could map how specific workflow changes (e.g., automated split-PO blockers) translate into observed Likert shifts and incident rates. Finally, benchmarking collaboratives could publish anonymized distributions of the Likert indices across sectors, enabling organizations to set data-driven targets for example, moving internal-control scores from 3.4 to 4.0 and to predict expected reductions in fraud-risk indices based on elasticities estimated in this and related studies (Fazekas, Cingolani, et al., 2016). By blending rigorous measurement with identification-aware analytics, future research has been well placed to translate statistical associations into implementable, auditable fraud-risk reductions across procurement and international trade systems.

CONCLUSION

his study has investigated, within a quantitative, cross-sectional, multi–case design, how interpretable statistical workflows descriptive profiling, correlation analysis, and regression modeling have illuminated the drivers of fraud risk in procurement and international trade systems and how governance features have conditioned those relationships. Drawing on harmonized Likert-scale measures and complementary administrative summaries, the analysis has consistently shown

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that higher transaction-anomaly intensity (e.g., split purchase orders, threshold bunching, out-of-tolerance variances) and greater vendor risk/concentration have aligned with elevated fraud-risk indices, while stronger internal controls and a healthier compliance culture have aligned with lower risk; in addition, internal controls and monitoring cadence have moderated the risk-raising influence of anomalies and vendor concentration, respectively. These findings have held across outcome realizations (continuous index, binary suspicious event, incident counts), across robustness probes (winsorization, alternative constructions, influence trimming, leave-one-case-out refits), and under fixed-effects specifications that have absorbed case heterogeneity, thereby yielding a stable empirical narrative on a common 1–5 scale that operational teams can interpret directly. Methodologically, the project has delivered a transparent pipeline that has begun with construct reliability and invariance checks, moved through scale-anchored descriptives and correlation matrices, and culminated in identification-aware regressions with prespecified moderation, standardized coefficients, clustered inference, and diagnostics for multicollinearity, heteroskedasticity, and influence an approach that has favored auditability and reproducibility over black-box complexity. Substantively, the results have suggested that organizations can make measurable progress by (i) suppressing the generation of anomalies through workflow controls and platform rules, (ii) hardening internal controls segregation of duties, approval thresholds, and audit trails where risk exposure remains high, (iii) cultivating a compliance culture that sustains speak-up safety and consistent consequence management, and (iv) calibrating monitoring cadence to the structure of vendor and corridor concentration so that oversight targets the margins where marginal risk is greatest. At the same time, the study has acknowledged boundaries: cross-sectional timing has precluded causal attribution; perceptual components in outcomes and determinants have introduced potential reporting bias despite reliability and criterion checks; and portable constructs have necessarily abstracted from sector-specific nuances that richer, transaction-level integrations could capture. Even so, by aligning a rigorously curated measurement framework with interpretable modeling, the research has produced effect-size benchmarks and conditional risk relationships that are immediately actionable expressed in Likert-scale movements that map cleanly to policy levers and workflow designs. In practice, this means that moving anomaly intensity down by approximately one scale point or raising internal-control strength by a similar margin has been associated with substantively meaningful reductions in the fraud-risk index within cases, with additional attenuation where monitoring cadence has been tuned to vendor concentration. Overall, the contribution has been twofold: an empirical consolidation of what matters most for fraud risk in procurement and trade, and a governance-first analytics blueprint that organizations can replicate and iterate using the same survey codebook, administrative templates, and reproducible scripts to monitor progress, prioritize interventions, and sustain defensible, evidence-based fraud detection.

RECOMMENDATIONS

Building on the study's consistent evidence that anomaly intensity and vendor concentration have raised fraud risk while stronger internal controls, compliance culture, and targeted monitoring have reduced or attenuated it, organizations should adopt a governance-first, layered operating model that embeds controls into workflow, aligns monitoring cadence with structural exposures, and sustains an interpretable analytics pipeline. First, procurement and trade leaders should harden process controls at the points where anomalies originate: prohibit split purchase orders through system-level rules; enforce approval thresholds with segregation-of-duties baked into e-procurement and ERP workflows; require justification fields (non-bypassable) for any award clustered just below publicity or competitive-tender thresholds; and standardize tolerance bands for price/quantity variances with automatic exception tickets routed to independent reviewers. Second, because internal controls have been the most reliable protective lever, CISOs and enterprise architects should formalize role-based access, adopt least-privilege principles for purchasing and vendor-master maintenance, and mandate immutable audit trails with periodic access recertification; at the same time, compliance teams should reinforce speak-up culture via confidential reporting channels, documented consequence management, and quarterly pulse checks that track movement on the control- and culture-indices used in this study. Third, monitoring cadence should be explicitly risk-based: categories and corridors with high vendor concentration or elevated tariff/fee incentives should receive more frequent exception review and post-award sampling, while low-exposure areas can be monitored on a lighter schedule; review calendars should be published, and completion SLAs (e.g., five business days from flag to disposition) should be enforced to preserve deterrence. Fourth, the analytics stack should remain transparent and reproducible: maintain descriptive dashboards (prevalence of red flags), theory-consistent screens (threshold bunching, bid dispersion, supplier-concentration indices), and supervised models (OLS/logit/negative binomial with case fixed effects); document feature provenance, store code in version control, and refresh models on a fixed quarterly cycle using the same scoring rules to ensure comparability. Fifth, risk reporting should reflect the study's moderation insight by displaying conditional risk: anomaly heat maps should be overlaid with internal-control and monitoring-cadence scores so that leaders see where the same anomaly level implies markedly different residual risk; this view should drive triage and audit planning, not raw anomaly counts alone. Sixth, to improve data quality and portability, organizations should adopt shared codebooks and normalization templates (e.g., per-1,000 POs, per-million spend) for administrative indicators, and require suppliers to comply with standardized master-data validations (beneficial ownership fields, banking verification, sanctions checks) to reduce vendor-risk uncertainty at source. Seventh, capability building should be continuous: run scenario-based trainings for approvers and buyers on common manipulation patterns; conduct red-team exercises that simulate split-PO and threshold-avoidance attempts to test whether controls catch them; and include analytics literacy modules so managers can interpret effect sizes and confidence intervals on the same 1–5 scale used in internal dashboards. Finally, leadership should institutionalize model governance a cross-functional forum (procurement, audit, IT security, legal) that reviews drift in indicator distributions, approves threshold changes, and monitors fairness/consistency across cases so that the program remains auditable, proportionate, and defensible. Taken together, these recommendations convert the study's Likert-anchored effects into a concrete, scalable playbook: reduce anomaly generation, raise control strength and cultural support, align monitoring to structural exposure, and keep analytics simple, explainable, and repeatable.

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