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Review Article

# AI-Driven Fraud-Risk Analytics and Risk-Responsive Auditing in U.S. Public Companies Financial Reporting: A Structured Integrative Review and Signal-to-Assurance Framework

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## Abstract

Artificial intelligence (AI) and data analytics can expand auditors' ability to identify unusual transactions and reporting patterns, but their value depends on what happens after a risk signal is generated. This structured integrative review examined how AI-generated fraud and material-misstatement risk signals translate into auditor judgment, risk-responsive procedures, and assurance outcomes in U.S. public company auditing. Literature identification covered peer-reviewed, English-language articles published from January 2010 through August 18, 2026, and combined scholarly and publisher searches, prior reviews, and backward and forward citation chaining. Studies were eligible if they addressed signal validity, auditor interpretation or reliance, conversion of recognized risk into audit procedures, or assurance outcomes. Work confined to internal audit, nonfinancial fraud, or technology unconnected to external financial-statement auditing was excluded, as were preprints and vendor materials. Twenty core empirical studies were retained for the primary synthesis and extracted into a structured evidence matrix; Public Company Accounting Oversight Board standards formed a separate regulatory corpus. Seven core studies addressed signal validity, six auditor interpretations or reliance, six planned or performed audit responses, and one assurance-related outcome. Evidence was concentrated on risk identification and auditor response, with much less evidence on procedural change and assurance outcomes. The only core study examining an assurance-related outcome assessed firm-level AI investment rather than a specific analytical signal. The Four-Gate Signal-to-Assurance Framework was developed to show where those breaks in the evidence occur. The literature does not yet show what happens when a specific AI-generated risk signal is followed through to an audit-quality outcome.

**Categories:** Accounting, Auditing and taxation, Financial reporting

**Keywords:** artificial intelligence, audit data analytics, financial statement fraud, audit quality, professional judgment, machine learning, risk assessment

## Introduction And Background

External auditors of U.S. public companies are required to identify and assess risks of material misstatement and to design procedures that respond to those risks (PCAOB, 2010b, 2010c). Fraud makes this task more difficult because intentional misstatements may be concealed through management override, inappropriate journal entries, biased estimates, or unusual transactions (PCAOB, 2002). At the same time, contemporary financial-reporting systems generate volumes of structured and unstructured information that are difficult to evaluate through conventional sampling and manual review

alone. Artificial intelligence (AI) and advanced data analytics offer a way to examine larger populations of financial information, identify unusual patterns, and direct audit attention toward transactions, accounts, or firms with elevated indicators of fraud or material misstatement (Eilifsen et al., 2020) (PCAOB, 2024).

The risk of material misstatement is broader than fraud risk. Material misstatement may arise from either error or fraud, while fraud risk concerns material misstatement arising from intentional acts, including fraudulent financial reporting or misappropriation of assets. A red flag is a characteristic or condition associated with elevated fraud or misstatement risk. Here, a risk signal means an analytical output that draws attention to a transaction, account, or firm because it appears to carry a higher risk of fraud or material misstatement. In the studies reviewed, those signals came mainly from machine learning (ML) prediction, anomaly detection, and analysis of narrative disclosures. Explainability was not examined as a separate type of signal; where it appeared, it shaped how auditors understood or relied on the output. Audit evidence is the information used by the auditor in reaching conclusions on which the audit opinion is based. A risk signal may prompt further investigation or affect the evaluation of other information, but it does not by itself establish that fraud has occurred or that sufficient appropriate audit evidence has been obtained (PCAOB, 2002, 2010a, 2010b).

Research increasingly supports the ability of analytical methods to generate useful risk information. Machine learning (ML) models have been developed to identify financial-statement fraud and material-misstatement risk using accounting, market, governance, and audit variables (Bao et al., 2020) (Bertomeu et al., 2021). Natural language processing has also been used to extract misreporting indicators from corporate disclosures, while more recent work has moved from retrospective classification toward prospective prediction of material misstatements (Brown et al., 2020) (Zhang Parker et al., 2025). These outputs remain indicators of elevated risk rather than determinations of fraud. Their usefulness depends on data quality, class imbalance, model validation, false-positive burden, and the consequences of missing genuinely high-risk observations (Beneish and Vorst, 2022) (Perols et al., 2017).

The more difficult issue begins after a signal has been generated. An algorithm may identify an unusual transaction or assign a higher probability of misstatement, but the auditor must still decide how much weight the result deserves, how it affects the risk assessment, and what additional work should follow. Behavioral research shows that these judgments can vary with perceived control, professional skepticism, psychological ownership, inspection pressure, and the perceived effort associated with technology-assisted procedures (Cao et al., 2022) (Commerford et al., 2024) (Emett et al., 2023) (Li et al., 2026). Technical performance alone says little about the eventual audit response.

The U.S. regulatory setting reinforces this distinction. Recent Public Company Accounting Oversight Board (PCAOB) amendments to standards governing audit evidence and responses to assessed risks clarify auditor responsibilities when electronic information is analyzed using technology-assisted methods (PCAOB, 2024). An unusual item identified through such analysis does not complete the auditor's responsibility. The auditor must determine its relevance to assessed risk, decide if additional procedures are needed, and evaluate the evidence obtained before reaching an audit conclusion (PCAOB, 2010a, 2010c, 2024).

Gkegkas et al. and Alkhatib et al. reviewed this literature by analytical method and by research topic, respectively (Gkegkas et al., 2025) (Alkhatib et al., 2026). Neither separated studies by the audit stage they measured, so evidence on risk prediction, auditor judgment, audit procedures, and audit outcomes was reviewed together, and the gaps between these stages were not examined.

Existing studies leave an important part of the audit process unclear: how risk information produced by analytics affects auditor judgment, changes the work performed, and relates to assurance outcomes. The review is organized around four distinct stages encompassing signal validity, auditor interpretation and reliance, risk-responsive procedures, and assurance outcomes. Findings are interpreted only at the stage they directly measure; predictive performance, for example, is not taken as evidence that audit procedures changed or that assurance improved. This evidentiary separation defines the Four-Gate Signal-to-Assurance Framework.

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## Review

### Methodology

The review covers peer-reviewed, English-language research published from January 1, 2010 through August 18, 2026. The literature spans predictive modeling, auditor experiments, archival studies, qualitative research, and methodological work, making a structured integrative review appropriate for the question being examined. PCAOB standards were treated separately from the research literature and used to establish the regulatory context for U.S. public company auditing.

Literature identification combined scholarly and publisher searches, prior reviews, and backward and forward citation chaining. Searches were run in Web of Science, Scopus, EBSCO Business Source Complete and ABI/INFORM, and on the AAA Digital Library, Wiley Online Library, ScienceDirect and SpringerLink. Google Scholar was also searched. Search terms covered AI, ML, audit data analytics, anomaly detection, process mining, financial-statement fraud, material misstatement, financial misreporting, auditor judgment, professional skepticism, reliance, audit evidence, audit procedures, journal-entry testing, restatements, and audit quality. The process was iterative and was not designed as an exhaustive systematic database search; formal PRISMA-style identification and screening counts were not prospectively recorded. Titles and abstracts were first screened against the scope of the review. Articles that passed were read in full and assessed against the eligibility criteria described below, and the same steps were applied to studies found through citation chaining. Twenty core empirical studies were retained for the primary synthesis. Additional sources were used for contextual, methodological, or mechanism evidence.

Eligibility depended on the part of the audit process examined. Research on analytical performance had to measure financial-statement fraud, misreporting, material misstatement, or another audit-relevant risk target. Evidence on auditor interpretation required an auditor judgment, reliance, skepticism, risk assessment, or a closely related decision measure. Studies used to examine risk-responsive auditing had to report a change in planned or performed audit work, including effort allocation, transaction selection, journal-entry testing, evidence collection, or another identifiable audit procedure. Evidence placed at the assurance-outcome stage had to measure a subsequent reporting or audit-quality outcome rather than model performance or auditor intention alone. Studies limited to internal audit or unrelated fraud domains were excluded, as were working papers, preprints, vendor materials, and professional commentary. International studies were retained when they examined an audit mechanism relevant to the review. A small number of non-AI studies were retained where they directly examined a link needed for the framework.

Studies were assigned to a gate according to the outcome they measured, not the technology they examined. A study that measured more than one stage was placed at the latest stage it measured directly. Planned audit effort was treated as a procedural response. Audit fees and auditor turnover were also placed at the procedural-response stage, but as indirect evidence, since they show a response to estimated risk without identifying the work performed.

Each study was recorded in an evidence matrix covering its design, sample, analytical method, target, measured outcome, principal findings, human involvement, limitations, and supported inference. Predictive performance, risk identification, auditor judgment, procedural response, and assurance outcomes were coded separately. Restatements, material restatements, regulatory investigations, adjustments, and comparable audit-quality measures were treated as assurance-related outcomes. Audit fees and auditor turnover were not treated as assurance outcomes on their own. One author coded each study and assigned it to a gate, and a second author checked the entries against the original articles. Differences were resolved by discussion.

Quality appraisal was conducted using design-specific criteria rather than a common numerical score because the empirical corpus included predictive modeling studies, auditor experiments, archival analyses, and qualitative research. Predictive studies were examined for label validity, class imbalance, train-test separation, temporal validation, potential data leakage, benchmark comparison, performance measures, and external validity. Experimental studies were assessed with attention to participant characteristics, study design, outcome measurement, and task realism; archival studies were examined for measurement validity, temporal ordering, confounding, identification strategy, and robustness; and

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qualitative studies were considered in relation to participant selection, data collection, analytical transparency, and transferability. Appraisal findings were used to constrain the inferences drawn from individual studies rather than to exclude studies through a cross-design numerical threshold. Table *1* reports the design-specific appraisal findings for the 20 core empirical studies. For each study, the appraisal recorded which of these criteria were addressed and which were not, and Table *1* reports the main strengths and the limitation that most restricts the inference the study can support. A study with a weakness on a criterion central to its gate, such as label validity for a predictive model or task realism for an experiment, was used only for the narrower inference its design allowed.

| Study | Design | Appraisal strengths | Main limitation for inference |
|---|---|---|---|
| (Perols, 2011) | Predictive model comparison | Compared several statistical and machine-learning approaches under severe class imbalance and different misclassification assumptions | Performance remained sensitive to fraud prevalence and error costs; prediction does not establish audit response |
| (Perols et al., 2017) | Rare-event machine learning | Directly addressed extreme class imbalance and compared a large set of predictive specifications | Improvement depended on sampling and modeling choices; evidence remains limited to prediction |
| (Bao et al., 2020) | Machine-learning prediction | Used out-of-sample testing and multiple benchmarks; corrected results were used in this review | Relative performance changed across measures and periods; no evidence of auditor response or audit quality |
| (Brown et al., 2020) | NLP-based prediction | Added narrative information to conventional financial and textual variables and tested out-of-sample performance | Predicts misreporting rather than auditor action; sample reflects an earlier reporting period |
| (Bertomeu et al., 2021) | Large-sample machine learning | Large firm-year sample and comparison across models and inspection thresholds | Results varied with the operating threshold; predictive performance alone does not establish audit usefulness |
| (Beneish and Vorst, 2022) | Fraud-prediction model comparison | Evaluated models using the economic consequences of classification errors rather than accuracy alone | Findings depend on assumed investigation costs and error consequences; no audit-response outcome |
| (Zhang Parker et al., 2025) | Prospective machine-learning prediction | Moves beyond retrospective classification toward prospective material-misstatement prediction | Evidence stops at prediction and does not observe auditor response |
| (Commerford et al., 2022) | Auditor experiment | Controlled comparison of auditor reliance on AI and human-specialist evidence | Measures reliance judgment in an experimental setting rather than completed audit work |

| | | | |
|---|---|---|---|
| (Cao et al., 2022) | Auditor experiment | Tests how inspection risk and auditor mindset affect reliance on analytical evidence | Behavioral response is context dependent; no procedure-level or assurance outcome |
| (Koreff, 2022) | Auditor experiment | Examines how analytic type and data type affect planned audit effort | Planned effort may not correspond to work ultimately performed |
| (Emett et al., 2023) | Experienced-reviewer experiments | Held assurance constant while isolating perceptions of analytics-based procedures and effort | Measures reviewer judgment rather than engagement-level audit outcomes |
| (Commerford et al., 2024) | Auditor experiment with interviews | Combines experimental evidence with interviews to examine perceived control and reliance | Establishes a reliance mechanism but not subsequent audit procedures or outcomes |
| (Li et al., 2026) | Auditor experiment | Examines psychological ownership and skeptical action when analytics are inherited rather than developed | Skeptical action is an intermediate behavioral outcome, not an assurance result |
| (Peecher et al., 2026) | Auditor experiments with replication | Uses experienced auditors and replicates the central professional-identity effect | Addresses reliance on AI-supported advice rather than completed audit response or quality |
| (Rose et al., 2017) | Audit-planning experiment | Uses experienced auditors and directly measures changes in concern and planned audit hours | Planned hours were not observed as work actually performed |
| (Bauer et al., 2020) | Fraud-planning experiment | Shows that fraud-risk recognition may fail to produce a corresponding plan revision | Does not involve AI and therefore provides mechanism evidence rather than direct AI evidence |
| (Eilifsen et al., 2020) | Engagement survey/field evidence | Documents analytics use across 109 audits and several specific audit procedures | Reported practice does not establish that analytics improved audit quality; evidence comes from a non-U.S. setting |
| (Hunt et al., 2022) | Archival study | Connects estimated misstatement risk with observable auditor responses such as fees and turnover | Does not identify the specific procedures changed, and observed associations do not establish procedure-level causation |

| | | | |
|---|---|---|---|
| (Abu Al Rob et al., 2026) | Qualitative interviews | Provides detailed evidence on how analytics enter journal-entry selection and testing | Small sample of nine senior auditors limits transferability and does not establish frequency or comparative effectiveness |
| (Fedyk et al., 2022) | Archival study | Large U.S. sample linking audit-firm AI investment with several restatement-related outcomes | AI investment is measured at the firm level; the design does not trace a specific signal through auditor response to an outcome |

**TABLE 1: Design-Specific Appraisal of the Core Empirical Studies.**

Appraisal was conducted using criteria appropriate to each study design. No cross-design numerical score was assigned because predictive models, experiments, archival studies, and qualitative studies present different sources of validity and bias. AI, Artificial Intelligence.

### U.S. public company audit context: From fraud risk to audit response

Under PCAOB standards, fraud-risk analytics operate within a broader risk-based audit process. AS 2110 requires auditors to identify and assess risks of material misstatement, whether caused by error or fraud, at both the financial-statement and assertion levels. That assessment provides the basis for determining how the audit should respond to the identified risks (PCAOB, 2010b). Fraud risks receive particular attention because they are treated as significant risks and may require greater professional skepticism and more persuasive evidence (PCAOB, 2002, 2010b).

Risk identification does not complete the auditor's responsibility. AS 2301 requires assessed risks to be addressed through appropriate overall responses and through changes in the nature, timing, or extent of audit procedures. Depending on the risk, these responses may involve different staffing or supervision, greater unpredictability, tests of controls, substantive procedures, or additional evidence directed at specific assertions (PCAOB, 2010c). For significant risks, including fraud risks, substantive procedures must be specifically responsive to the assessed risk. Identifying a transaction, account, or pattern as unusual has value only if the information is appropriately incorporated into the auditor's assessment and, where warranted, changes the audit work performed.

The resulting audit work must satisfy the evidence requirements of AS 1105. Audit evidence must be sufficient and appropriate, with appropriateness determined by relevance and reliability. The standard also requires auditors to evaluate the accuracy, completeness, and level of detail of company-produced information when it is used as audit evidence (PCAOB, 2010a). AS 2810 requires auditors to evaluate if the evidence obtained supports the audit opinion and if findings from the audit require risk assessments or procedures to be reconsidered (PCAOB, 2010d). Unusual or unexpected transactions may lead to additional investigation or reassessment of risks identified earlier in the audit.

Technology-assisted analysis also has a documentation consequence. AS 1215 requires the audit file to record the procedures performed, evidence obtained, and conclusions reached and to show the link between identified risks and the auditor's responses to those risks. Applied to an analytical risk signal, the documentation should make it possible to follow what was identified, how the auditor evaluated its significance, any resulting change in risk assessment or audit procedures, and the conclusion reached from the additional work. This record is also relevant to inspection because PCAOB inspection teams may review audit documentation when assessing audit quality and compliance with applicable requirements (PCAOB, 2004).

The PCAOB's recent technology-assisted analysis amendments make this sequence especially relevant. Effective for audits of financial statements for fiscal years beginning on or after December 15, 2025, the amendments to AS 1105 and AS 2301 address the use of technology-based tools to analyze electronic information. They clarify the auditor's responsibilities when items identified through a test of details require further investigation and when the same procedure is used for more than one audit purpose (PCAOB, 2024). The amendments do not create a separate standard for AI auditing. They apply existing responsibilities for evidence and risk-responsive procedures to technology-assisted analysis.

Table *2* summarizes the 20 core empirical studies and the evidentiary boundary of each study.

| Gate | Study | Design/Sample | Main finding | Evidence boundary |
|---|---|---|---|---|
| 1 | (Perols, 2011) | Model comparison; 51 fraud firms | Simpler methods remained competitive | Prediction, not audit response |
| 1 | (Perols et al., 2017) | Rare-event ML; 109 variables | Undersampling improved prediction | Prediction only |
| 1 | (Bao et al., 2020) | U.S. public firms; ML | Higher AUC in reported test periods; relative performance varied across other measures and settings | Prediction, not audit quality |
| 1 | (Brown et al., 2020) | 10-K NLP; 1994-2012 | Topics improved out-of-sample detection | Misreporting prediction only |
| 1 | (Bertomeu et al., 2021) | 54,354 firm-years | ML improved detection at a 10% inspection rate | Performance depends on operating point |
| 1 | (Beneish and Vorst, 2022) | Seven fraud-prediction models | False positives made models costly | Economic usefulness, not audit outcome |
| 1 | (Zhang Parker et al., 2025) | ML material-misstatement prediction | Prospective prediction of material-misstatement risk | Prediction only; no auditor response |
| 2 | (Commerford et al., 2022) | Auditor experiment | Less reliance on AI than on a human specialist | Reliance judgment only |
| 2 | (Cao et al., 2022) | Auditor experiment | Inspection risk and mindset changed reliance | Judgment conditions |
| 2 | (Emett et al., 2023) | Experienced reviewer experiments | Lower perceived quality reflected an effort heuristic | Reviewer judgment only |

| | | | | |
|---|---|---|---|---|
| 2 | (Commerford et al., 2024) | Experienced auditors + interviews | Limited input increased reliance for some auditors | Reliance mechanism |
| 2 | (Li et al., 2026) | Auditor experiment | Inherited analytics reduced skeptical action | Skeptical action, not assurance outcome |
| 2 | (Peecher et al., 2026) | 127 experienced auditors + replication | Skill disaffirmation reduced AI-supported reliance | Professional-identity mechanism |
| 3 | (Rose et al., 2017) | 127 Big Four senior auditors | Timing increased concern and planned hours | Planned effort, not executed work |
| 3 | (Bauer et al., 2020) | Fraud-planning experiment | Risk recognition did not ensure plan revision | Mechanism evidence; non-AI |
| 3 | (Eilifsen et al., 2020) | 109 audits; 216 partners/managers | Analytics used in journal entries, sampling, and controls | Reported use, not quality |
| 3 | (Hunt et al., 2022) | Archival ML risk estimates | Higher risk associated with fees and turnover | No procedure-level causation |
| 3 | (Abu Al Rob et al., 2026) | 9 Big Four managers+ | BDA informed journal-entry selection and testing | Small qualitative sample |
| 3 | (Koreff, 2022) | Auditor experiment | Planned effort differed by analytic type and data type | Planned effort, not assurance outcome |
| 4 | (Fedyk et al., 2022) | 36 U.S. audit firms; 2010-2019 | AI investment associated with fewer restatements | Firm-level association, not signal causation |

**TABLE 2: Principal Empirical Evidence Across the Four Gates.**

The 20 core studies do not cover the four stages equally. Seven examine signal validity, six focus on auditor interpretation or reliance, six examine planned or performed audit responses, and one examines assurance-related outcomes. The later stages are represented by fewer studies, especially the link between a specific analytical signal, the audit work performed in response, and the resulting assurance outcome. ML, Machine Learning; AI, Artificial intelligence.
Source: Authors' synthesis of the studies listed in the table.

### Gate 1: Decision-valid AI risk signals

AI-assisted fraud-risk assessment first depends on the analytical output providing information useful to the audit decision. Early comparative evidence cautioned against assuming that greater algorithmic complexity necessarily produces better fraud identification. Perols evaluated logistic regression, neural networks, bagging, decision trees, stacking, and support vector machines using 51 fraud firms against 15,934 nonfraud firm-years, a fraud prevalence of

approximately 0.3%. Logistic regression and support vector machines remained competitive with more complex approaches under different assumptions about fraud prevalence and misclassification costs (Perols, 2011). Perols et al. later used the same highly imbalanced setting, with 109 explanatory variables and more than 10,000 prediction models, to test methods designed for rare-event prediction (Perols et al., 2017). One observation-undersampling implementation improved prediction performance by approximately 10.8% relative to benchmark approaches (Perols et al., 2017).

Later studies broadened both the methods and information used to generate risk signals. Bao et al. developed a RUSBoost ensemble model for publicly traded U.S. firms using theory-motivated raw accounting numbers rather than conventional financial ratios (Bao et al., 2020). The article was subsequently corrected, and the corrected results are used here (Erratum, 2022). For the 2003-2005 test period, RUSBoost produced an area under the curve (AUC) of 0.743, compared with 0.674 for logistic regression using the same raw financial variables, 0.648 for a ratio-based logistic model, and 0.623 for the support-vector-machine benchmark. RUSBoost identified nine true fraud observations under the study's ranking criterion, compared with two, two, and four for the respective alternatives. Over the longer 2003-2008 test period, its AUC remained higher at 0.723, although relative performance varied across other evaluation measures. The corrected results do not support a general claim of uniform model superiority (Bao et al., 2020) (Erratum, 2022).

Useful signals can also come from information that conventional financial-ratio models do not capture. Brown et al. analyzed 10-K narratives from 1994 through 2012 using Bayesian topic modeling (Brown et al., 2020). Adding thematic information to models containing conventional financial and textual-style variables improved out-of-sample detection of financial misreporting by as much as 59%, with particularly strong results for serious revenue recognition and core-expense errors. Bertomeu et al. examined 54,354 firm-years, including 3,599 misstatement firm-years, using accounting, market, governance, and audit variables (Bertomeu et al., 2021). Their test sample contained 11,323 firm-years and 428 restatement observations. At a 10% inspection rate, GBRT, random forest, and RUSBoost identified 33.4%, 35.3%, and 36.4% of misstatements, compared with 27.8% for backward logistic regression. At lower inspection rates, the differences were smaller. The practical advantage of ML depends partly on the operating point at which the model is used (Bertomeu et al., 2021).

Financial-reporting fraud is rare, so a classifier can perform well on aggregate measures while producing many false alarms among the observations auditors would need to investigate. Beneish and Vorst compared seven fraud-prediction models using the economic consequences of prediction errors rather than classification performance alone (Beneish and Vorst, 2022). Even the best-performing models generated more than 100 false positives for every true positive, and the authors concluded that all seven were too costly for auditors under their estimated cost structure. False-positive burden is not a minor classification issue when each flagged observation creates an investigative cost.

Supervised models commonly rely on later-observed Accounting and Auditing Enforcement Releases, restatements, or other documented reporting failures as labels (Bao et al., 2020) (Bertomeu et al., 2021) (Perols et al., 2017). Those labels capture observed reporting failures rather than every instance of fraud that may have occurred.

For this review, decision validity sets a higher threshold than predictive accuracy. A useful fraud-risk signal must perform under credible out-of-sample conditions, account for rare-event prevalence and imperfect labels, and produce an acceptable balance between missed risks and investigative burden (Perols et al., 2017) (Beneish and Vorst, 2022) (Bertomeu et al., 2021). A signal is decision-valid when its output is sufficiently reliable and relevant to justify attention in the audit process.

**Gate 2: Auditor interpretation and calibrated reliance**

A technically credible risk signal can still receive the wrong weight in the audit. Auditors must decide how credible the output is, how much it should influence judgment, and if it warrants further attention. Experimental research shows that those decisions can change even when the underlying evidence does not.

Commerford et al. examined auditors evaluating contradictory evidence about a complex accounting estimate (Commerford et al., 2022). Auditors relied less on evidence produced by an AI system than on otherwise comparable evidence from a human specialist, with the aversion particularly evident under conditions involving more objective

inputs. Commerford et al. later tested if giving auditors some input into the process could reduce that reluctance (Commerford et al., 2024). Highly experienced auditors again showed greater hesitation toward AI-based evidence, but a small amount of input increased reliance among auditors with a more external locus of control. Auditors with a stronger internal locus of control remained comparatively reluctant. Interviews conducted as part of the study were consistent with the experiment: auditors described a greater sense of control when working with human specialists than with AI-based systems.

Inspection pressure can alter the same judgment. Cao et al. found that auditors facing inspection risk did not respond uniformly to data-analytic evidence (Cao et al., 2022). Reliance declined among auditors induced to hold a fixed mindset, while auditors with a growth mindset responded differently under the same inspection conditions. Inspection pressure changed reliance differently across mindset conditions (Cao et al., 2022).

Peecher et al. located part of the resistance in auditors' professional self-perceptions (Peecher et al., 2026). Their first experiment included 127 highly experienced auditors from two Big Four firms. Among auditors whose digital skills had been disaffirmed, mean weight placed on specialist advice fell from 0.71 when specialist reliance on AI was lower to 0.40 when it was higher. Under high AI reliance, auditors who had first affirmed professional skills in which they felt competent assigned a mean weight of 0.67, compared with 0.40 among disaffirmed auditors. The authors reproduced the central effect in a second experiment and found that disaffirmed auditors were also more likely to question the competence and advice quality of specialists who relied more heavily on AI.

Li et al. compared auditors who used a data-analytic test they had helped develop with auditors who inherited the test from others (Li et al., 2026). The test identified the same fraud red flag, yet auditors who inherited it were less likely to take skeptical actions. Mediation analysis linked the difference to reduced psychological ownership rather than lower perceived reliability of the analysis. Providing inheriting auditors with information about the test-development process mitigated part of the effect.

Emett et al. held the level of assurance constant while experienced external reviewers evaluated either data-and-analytics procedures or traditional audit procedures (Emett et al., 2023). Reviewers initially rated the analytics-based work as lower quality because they perceived it as requiring less effort. In a second experiment, an intervention designed to reduce reliance on that effort heuristic largely removed the difference in perceived quality.

The reviewed Gate 2 evidence is much clearer on underreliance and distorted reliance than on systematic overreliance. Explainability is rarely tested as a separate intervention; it appears indirectly through perceived control, knowledge of how an analytic was developed, and the auditor's ability to assess the basis of the output. In this review, calibrated reliance means giving the signal weight that matches its relevance and reliability.

**Gate 3: Risk-to-procedure conversion**

Once an analytical signal is considered relevant, the audit should show what changed because of it. That may be a different journal-entry selection, broader or more targeted testing, a change in timing, or additional evidence. The analytical output does not choose the procedure. The auditor does, and the documentation should make that link visible.

Rose et al. provide an early example of analytics affecting planned work rather than risk judgment alone (Rose et al., 2017). Their experiment involved 127 senior auditors from two Big Four firms who reviewed traditional audit evidence and Big Data visualizations containing information inconsistent with management's explanation of gross margin. Timing mattered. Auditors who encountered the contradictory visualizations after reviewing the traditional evidence expressed greater concern about potential misstatement and allocated more audit hours to the sales account than auditors who saw the visualizations earlier. The study does not show that the additional hours were ultimately spent or that they produced better evidence.

Koreff also examined planned effort (Koreff, 2022). Predictive models and anomaly-detection models did not affect planned effort in the same way across financial and nonfinancial data, so the response differed with both the analytical method and the information used.

Evidence from actual engagements shows analytics entering specific audit procedures. Eilifsen et al. interviewed the heads of professional practice at five international accounting firms in Norway and collected engagement-level information from 216 partners and managers covering 109 audits (Eilifsen et al., 2020). Analytics were used for journal-entry testing on 74 engagements, calculating sample sizes on 55, selecting random samples on 51, summarizing ledgers on 41, visually identifying deviations or outliers on 37, and two- or three-way matching on 36. Thirty-seven engagements also used analytics in evaluating or testing internal controls. These are reported uses within audit work, not evidence that the procedures improved audit quality.

The weakness in this conversion process is not unique to AI. Bauer et al. examined auditors responding to seeded fraud cues in a realistic audit-planning case and found that auditors acting as decision makers often failed to revise the audit plan in a way that tracked their own fraud-risk assessments (Bauer et al., 2020). Auditors placed in an advisory role produced plans that were more closely aligned with their own risk assessments and with expert recommendations. The study provides mechanism evidence for Gate 3 even though it did not involve AI.

Hunt et al. examined machine-learning estimates of misstatement risk (Hunt et al., 2022). Higher estimated risk was associated with higher audit fees and greater auditor turnover, but the study does not identify which engagement-level procedures changed. The authors report that the fee association appears primarily consistent with a risk premium, so the result should not be read as proof of additional substantive testing.

Abu Al Rob et al. interviewed nine manager-level or more senior auditors from Big Four firms about their use of Big Data Analytics in selecting and testing journal entries (Abu Al Rob et al., 2026). Participants described analytics being used to refine selection criteria, identify entries with red flags, automate parts of testing, and direct attention toward higher-risk accounts, balances, and transactions. The qualitative design documents how analytics can enter the selection and testing process but does not establish how often these changes occur or if they outperform traditional procedures.

These studies capture different forms of audit response. Rose et al. and Koreff measure planned effort (Rose et al., 2017) (Koreff, 2022); Eilifsen et al. report analytics used in specific engagement tasks (Eilifsen et al., 2020); Hunt et al. observe fee and auditor-switching associations (Hunt et al., 2022); and Abu Al Rob et al. describes changes in journal-entry selection and testing (Abu Al Rob et al., 2026). None observes the same analytical signal from identification through a documented change in procedure and the evidence obtained from that procedure. Bauer et al. add a separate caution: recognition of fraud risk does not necessarily produce a matching revision to the audit plan (Bauer et al., 2020).

Gate 3 requires more than recognition of risk or greater planned effort. The audit response must be identifiable and linked to the risk that prompted it.

**Gate 4: Assurance realization**

The last stage concerns assurance-related outcomes. Evidence at this stage is much less developed than evidence on prediction, auditor judgment, or planned procedures.

Fedyk et al. provide the main large-sample evidence at this stage (Fedyk et al., 2022). The study examined the 36 largest U.S. public accounting firms, more than 310,000 employee profiles, and the period from 2010 to 2019. AI investment was measured from the share of employees whose skills and work histories indicated AI expertise rather than from the use of a particular analytical tool or signal. A one-standard-deviation increase in an audit firm's recent AI investment was associated with a 5.0% reduction in the likelihood of a restatement, a 1.4% reduction in material restatements, a 1.9% reduction in restatements involving accruals or revenue recognition, and a 0.3% reduction in restatement-related SEC investigations. The restatement results remained when the authors controlled for investment in software engineering and general data-analysis skills, and the estimated relationship was linked to auditors' AI investment rather than clients' AI investment. Firm-level AI investment also had a stronger relationship with the outcomes than office-level investment (Fedyk et al., 2022).

The same study found efficiency associations. A one-standard-deviation increase in recent AI investment was associated with a 0.9% decline in audit fees after one year, with larger reductions over longer horizons. Accounting employment fell 3.6% after three years and 7.1% after four years, with reductions concentrated among junior employees (Fedyk et al.,

2022). These findings do not establish that every reduction in labor or fees is quality-neutral.

Fedyk et al. link firm-level AI investment with several assurance-related proxies, but their study does not identify a particular effect of an individual analytical signal (Fedyk et al., 2022). Restatements and SEC investigations can also be influenced by management behavior, internal controls, client characteristics, and other features of the reporting environment. The study supports an association between AI investment and better outcome proxies, not signal-level causation.

Gate 4 remains the least directly observed part of the framework because the reviewed evidence does not follow a specific signal through the auditor's response to a measured assurance outcome.

### Four-gate signal-to-assurance framework: Integrative discussion

AI-assisted auditing is often evaluated as though better analytical performance should lead naturally to better assurance. The studies reviewed here do not support that assumption. A model may identify elevated fraud or misstatement risk accurately and still have little effect on the audit if its output is discounted, misunderstood, or never connected to a change in audit work. An analytically weak or poorly understood signal may also receive more weight than it deserves. Figure *1* presents the four stages and the main points at which the connection can fail.

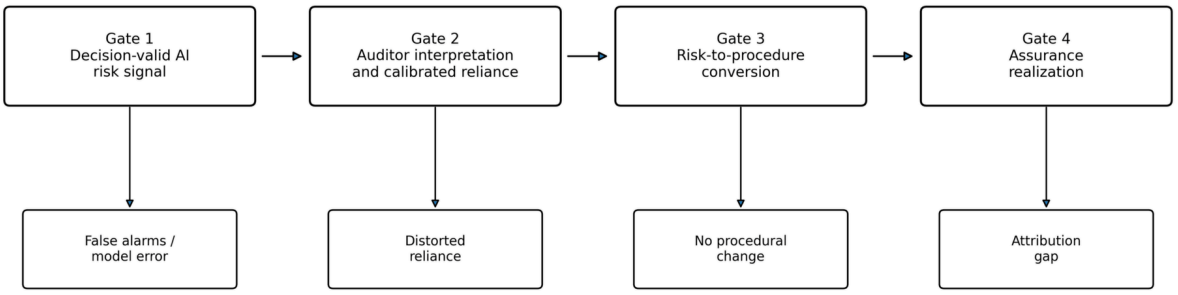



**FIGURE 1: Study-Selection Process For the Structured Integrative Review.**

Counts not prospectively recorded are labeled as unavailable. PCAOB standards form a separate regulatory corpus. Source: Authors' synthesis of the review methodology.

Table *3* states how each gate can be operationalized in empirical work.

| Gate | Construct | Operational definition | Examples of empirical measures |
|---|---|---|---|
| 1 | Signal validity | The extent to which an analytical output identifies audit-relevant fraud or misstatement risk with sufficient reliability for use in an audit decision. | AUC; precision; recall; false-positive and false-negative rates; calibration; operating-threshold performance; temporal or external validation |

| | | | |
|---|---|---|---|
| 2 | Auditor interpretation and reliance | The weight an auditor gives to the signal relative to its relevance and reliability. | Change in assessed risk; reliance weights; skeptical actions; investigation decisions; confidence judgments; response to contradictory evidence |
| 3 | Risk-to-procedure conversion | A documented change in audit work linked to the recognized risk. | Changes in nature, timing, or extent of procedures; journal-entry selection; sample size; testing scope; additional evidence obtained; audit hours directed to the risk |
| 4 | Assurance realization | An assurance-related outcome associated with the audit response that followed the identified risk. | Detected or corrected misstatements; audit adjustments; restatements; material restatements; regulatory findings; other validated audit-quality outcomes |

**TABLE 3: Operationalization of the Four-Gate Signal-to-Assurance Framework.**

Source: Authors' operationalization based on the review synthesis. AUC, Area Under the Curve.

Problems can arise before the auditor acts on the signal. Fraud and material misstatement are rare, available research labels capture only observed reporting failures, and model performance can change substantially depending on the metric or operating threshold used (Bao et al., 2020) (Beneish and Vorst, 2022) (Bertomeu et al., 2021) (Perols et al., 2017). A classifier may rank risky observations effectively while still producing too many false positives to be useful in an engagement. Beneish and Vorst show how the economic consequences of false alarms and missed fraud can alter the interpretation of apparently strong predictive performance (Beneish and Vorst, 2022).

A credible signal can also lose value during auditor interpretation. Reliance has changed with the source of the evidence, perceived control over the technology, professional self-perceptions, inspection conditions, and psychological ownership of the analysis (Cao et al., 2022) (Commerford et al., 2022, 2024) (Li et al., 2026) (Peecher et al., 2026). Emett et al. also found that reviewers could judge analytics-based procedures less favorably when they appeared to require less effort despite equivalent assurance (Emett et al., 2023). These studies show several ways in which a technically useful signal can receive less weight than its evidentiary value warrants. The reviewed evidence is much clearer on underreliance and distorted reliance than on systematic overreliance.

Recognition does not guarantee a corresponding change in audit work. Rose et al. showed that analytical information can affect planned audit hours, and Eilifsen et al. documented analytics being used in journal-entry testing, sample selection, control testing, and identification of unusual items (Rose et al., 2017) (Eilifsen et al., 2020). Bauer et al. show that recognition of fraud risk can fail to produce a corresponding revision to the audit plan (Bauer et al., 2020). The analytics studies provide only partial views of the later procedural response.

Fedyk et al. link firm-level AI investment with several restatement-related outcomes, but it does not identify a particular analytical signal or the engagement-level response that produced the association (Fedyk et al., 2022). Restatements and regulatory investigations are also influenced by management behavior, internal controls, client characteristics, and other

aspects of the reporting environment.

Organizational design affects more than one point in the sequence. Fedyk et al. report stronger associations for firm-level than office-level AI investment, while Li et al. show that auditors who inherit an analytic can experience lower psychological ownership (Fedyk et al., 2022) (Li et al., 2026). The studies examine different questions, so they do not establish that centralized development weakens audit quality.

*Explainability and Data Reliability*

Fraud prediction models are trained on detected cases, such as enforcement releases and restatements, and most of their inputs come from company-produced data (Bao et al., 2020) (Bertomeu et al., 2021) (Perols et al., 2017). AS 1105, as amended in 2024, requires auditors to evaluate the accuracy and completeness of such information before using it as evidence, including when it is analyzed with technology-assisted tools (PCAOB, 2010a, 2024). This applies however well a model performed in testing. Koreff found that auditors planned different levels of effort depending on whether an analytic used financial or nonfinancial data (Koreff, 2022).

Li et al. offer the closest evidence on explainability (Li et al., 2026). Auditors who inherited an analytic took fewer skeptical actions than those who had helped develop it, though telling them how the test was developed reduced part of the gap. In Commerford et al., giving auditors limited input into an AI system increased reliance for some of them (Commerford et al., 2024). These studies concern what auditors knew about how a tool was built rather than explanations of a particular output, and they do not show whether that knowledge made reliance more appropriate. Explainability may also matter at Gate 3. AS 2301 and AS 1215 require the audit response to be linked to the assessed risk and documented, and an auditor who does not know why an item was flagged will have difficulty showing that link.

A material analytical finding should be traceable through the audit file. The record should show the signal, how the auditor evaluated it, any change in the risk assessment, the work performed in response, and the evidence obtained (PCAOB, 2004).

A signal with unstable validation or a high false-positive burden should receive less weight than one that remains reliable under conditions close to intended use. If a technically credible signal is discounted because of its source or limited auditor involvement, the problem lies in reliance rather than model performance. A recognized signal does not automatically require more audit work. The response should reflect the significance and reliability of the risk identified, and the file should show why procedures were changed or why no change was warranted. Claims about assurance outcomes require the most restraint because no reviewed study follows a specific AI-generated signal through the engagement to a demonstrated audit-quality result.

Testing the full framework requires preserving the link between a defined analytical signal, the auditor's response, the resulting change in audit work, and an outcome that can reasonably be connected to that response. Studies observing only one or two of these points can test individual gates, but they cannot establish the full signal-to-assurance path.

*Limitations*

The studies do not use fully interchangeable constructs. "Fraud-risk analytics" covers models aimed at fraud, material misstatement, misreporting, or unusual observations, and these targets differ in meaning and prevalence. Measures used later in the audit process range from planned hours and testing decisions to restatements and regulatory investigations. Restatements, in particular, are influenced by factors beyond the auditor and are only proxies for audit quality. The gate structure keeps these outcomes separate, but it cannot remove the underlying differences in how the original studies define and measure them.

The search was iterative, and screening counts were not recorded at each step, so some relevant studies may have been missed. Coding was also done by one author and checked by another rather than carried out independently by two coders.

*Research and Practice Implications*

Research needs to connect stages of the AI-audit relationship that are usually studied separately. Model development, auditor judgment, and technology adoption are already well represented. Much less is known about a specific analytical signal changing the audit in a way that can be followed to an assurance outcome. Designs that observe the signal, auditor evaluation, risk reassessment, procedural change, evidence obtained, and engagement outcome would make that connection easier to test.

Technical research should also place greater emphasis on conditions that resemble actual audit use. Rare-event prevalence, false-positive burden, temporal validation, model drift, and the economic consequences of classification errors can matter more to an audit decision than headline accuracy alone. External validation across firms, industries, and reporting periods would help establish how well a model travels beyond its development sample.

The studies reviewed here do not test the documentation, training, governance, or regulatory measures discussed below. Those points are recommendations drawn from the framework and existing PCAOB responsibilities, not empirical findings of the review.

For audit firms, implementation should be governed around the path from signal to response rather than access to an AI tool. Engagement documentation should make it possible to determine what produced a material analytical signal, how its reliability was considered, who evaluated it, if it affected the risk assessment, and what audit work followed. An AI-generated flag should not stand as an audit conclusion on its own; the engagement team remains responsible for evaluating the signal and deciding what evidence or further work it warrants.

Centralized analytics teams can provide technical scale and consistency, but engagement auditors still need enough understanding of the analytics to question assumptions and limitations. Training should address uncertainty, verification, and the circumstances in which further evidence is required.

Existing PCAOB requirements already place responsibility on the auditor for risk assessment, responsive procedures, sufficient appropriate evidence, documentation, and evaluation of audit results (PCAOB, 2010a, 2010b, 2010c, 2010d) (PCAOB, 2004). For regulators and standard setters, the relevant issue is how those responsibilities are demonstrated when technology-assisted analysis materially affects an audit, rather than the mere presence of AI.

## Conclusions

AI-enabled analytics have demonstrated value in identifying fraud and material-misstatement risk, but the same level of support does not extend across the rest of the audit. Studies show that analytical signals can affect auditor judgment and, in some settings, planned effort, journal-entry testing, transaction selection, and other audit work. None of the reviewed studies follows a specific AI-generated risk signal through a completed audit response to a demonstrated assurance result. Fedyk et al. provide firm-level evidence linking greater AI investment with lower restatement-related outcomes, but their study does not establish signal-level causation.

The Four-Gate Signal-to-Assurance Framework keeps these claims separate. This review supports AI-enabled analytics as a useful source of fraud and misstatement risk information and shows that auditor judgment affects how that information is used. It does not support a general claim that AI improves audit quality. Establishing that claim requires evidence connecting a credible signal to a responsive audit procedure and then to an assurance outcome.

## Additional Information

### Author Contributions

All authors have reviewed the final version to be published and agreed to be accountable for all aspects of the work.

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## Disclosures

**Conflicts of interest:** In compliance with the ICMJE uniform disclosure form, all authors declare the following:

- **Payment/services info:** All authors have declared that no financial support was received from any organization for the submitted work.
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### Data Availability Statements

No datasets were generated during the current study. The findings are based exclusively on information reported in previously published studies, which are cited within the manuscript.

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