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NO TRAIL, NO TRUST? DIGITAL RECORD AUDITABILITY AND LENDER DECISION-USE IN SME FINANCE; A FOCUSED CRITICAL REVIEW AND EVIDENCE MAPPING

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Abstract

Small and medium-sized enterprise (SME) lenders increasingly encounter digital financial and transaction records, yet availability neither establishes credibility nor demonstrates organizational use. This focused critical review and structured evidence mapping examined the transition from record creation to auditability, actor-specific verification, lender processing, and financing outcomes. Eight traceable studies were selected retrospectively for contrasts in mechanisms, designs, actors, endpoints, and counterevidence; they were analytical cases, not a representative final sample. Registry comparisons identified reporting discrepancies, auditor-reliability cues influenced hypothetical lender judgments, and third-party certification was associated with observed borrowing terms. Prediction studies demonstrated incremental information but not lender deployment. Electronic invoicing informed tax compliance, while borrower-reported access did not establish observed loan approval. Drawing on SME lending technology and organizational-information theories, the review identified an under-observed conversion from usable verification to authorized credit action. Five falsifiable propositions addressed access and usability, exception resolution, substitute signals, product fit, and decision rights. Qualitative missing-evidence scenarios supported the distinction between record availability and decision-use, while leaving the empirical strength of the verification-to-outcome bridge unresolved. The resulting framework was conditional rather than a universal digitalization effect.

Keywords: *SME Finance, Digital Records, Auditability, Lender Decision-Use, Evidence Mapping*

1. INTRODUCTION

Small and medium-sized enterprise (SME) credit assessment now involves more than financial statements. Payment histories, electronic invoices, account transactions and platform records can be captured at lower cost and incorporated into analytical systems. Yet a transaction stored in digital form may be incomplete, unattributed, inconsistent with liabilities reported elsewhere, or unavailable to the lender making the decision. The central research problem is consequently not a shortage of digital traces alone, but whether those traces can become sufficiently reliable, verifiable and actionable to inform credit decisions.

Treating record availability as evidence of lender trust skips several distinct empirical questions.

This problem matters because SMEs often cannot rely on the disclosure infrastructure available to larger firms. When lenders cannot establish the source, completeness or consistency of reported information, screening and monitoring remain costly. At the same time, producing an audit trail is not free: reconciling external records, acquiring access rights and resolving exceptions demand organizational resources. Evidence from small commercial lending shows that information requirements differ with borrower risk, collateral, relationship length and tax-return availability rather than following a single assurance rule (Minnis & Sutherland, 2017). The value of more data therefore depends both on its credibility and on how lenders process it.

The closest prior literature already shows that verification and assurance can matter in debt contracting. Minnis (2011) reports lower borrowing costs among audited private U.S. firms and greater lender weighting of audited information; Schneider (2018) reviews accounting information and assurance effects on commercial-lending judgments; and Minnis and Sutherland (2017) show that banks' post origination information requests vary with borrower and contract conditions. SME-lending theory likewise defines a lending technology as a joint configuration of information sources, screening and underwriting, contracting, and monitoring rather than as data alone (Berger & Udell, 2006). Stein (2002) explains why information hardness, transmission, organizational structure, and authority can shape information production and capital allocation. These studies rule out novelty based merely on the propositions that credible information matters, that lending technologies are heterogeneous, or that authority affects information use. What remains insufficiently integrated is an actor-specific account of whether digital financial and transaction records have attributable provenance, who verifies them, how discrepancies are resolved and transferred into the lender's workflow, and whether the observed endpoint is judgment, prediction, monitoring, or realized credit allocation.

Record and transaction studies reveal different mechanisms rather than a common verification treatment. Duro, López-Espinosa, Mayordomo, Ormazabal, and Rodríguez-Moreno (2026) compare bank debt disclosed in unaudited accounts with public credit-registry balances and report lower credit quantities for underreporting firms, particularly where banks use registry information; the evidence spans Spanish private firms of different sizes. Registry consultation is selective, and the reported results do not isolate an SME-only digital audit trail effect or the staff level processing that connects a query with credit action. Bellon, Dabla-Norris, and Khalid (2023), by contrast, analyze a staggered Peruvian e-invoicing mandate using firm transaction administrative data. They identify adoption spillovers, changes in tax reporting and the termination of some supplier relationships; neither bank authentication of invoice records nor lending allocation is their empirical

outcome. The comparison identifies an actor-and-endpoint boundary: a tax-reporting network is not evidence that lenders receive, reconcile or use the resulting records.

The gap becomes sharper when predictive and financing evidence are placed beside these studies. Modina, Pietrovito, Gallucci, and Formisano (2023) report that private bank firm variables add to accounting information in one- and two-year SME default prediction. That source grounded claim concerns information content in a prediction model; it does not establish model deployment, calibration in a live underwriting system, or a change in credit allocation. By contrast, the identity-verified accepted manuscript of Andrikopoulos and Khorasgani (2018) documents a hybrid market and accounting model for unlisted SMEs that outperforms the accounting only model in eight of nine annual tests, with a 2011 exception. Gupta, Wilson, Gregoriou, and Healy (2014) provide a counterpoint regarding the incremental value of operating cash flow in their model. These results concern model information content. They do not show that a loan officer consulted the variable, deployed the model, adjusted a limit, or changed a loan price. Likewise, borrower-reported access in Gamage (2013) is not independently observed credit approval.

A related gap concerns the identity of the signal. In an identity-verified accepted manuscript, Wu and Xu (2020) associate venture-capital backing with SME loan balances, borrowing costs, and collateral conditions. Their discretionary-accrual interaction is consistent with certification substituting for weaker accounting information, but venture capital selection is nonrandom despite matching and before–after robustness checks. The venture capitalist is the signal source; the study does not observe a bank authenticating an underlying digital record. More generally, assurance, borrower selection, collateral, relationship information, and proprietary lender data can affect financing conditions through different channels. The literature is therefore heterogeneous not simply in results, but in the object measured, the verifier or signal producer, the information recipient, and the credit-process stage observed.

Theoretically, information asymmetry creates a reason for credible signals to matter, but a signal is useful only if its production or verification conveys information that lenders cannot obtain more cheaply elsewhere. Hard information can travel across institutional boundaries more easily than context-rich soft information (Liberti & Petersen, 2019). Berger and Udell (2006) show that the relevant unit is a lending technology combining information, screening, contracting, and monitoring, while Stein (2002) shows that transmissibility and authority affect whether produced information can influence resource allocation. Organizational incentives can also change how negative information is recorded and acted upon, as lender reporting under loan-officer rotation illustrates (Hertzberg, Liberti, & Paravisini, 2010). The unresolved mechanism is therefore not credibility alone but conversion: attributable evidence must reach the relevant lender in a usable form, exceptions

must be resolved, and a recipient with capacity, incentives, and authority must act. This conditional pathway is a theoretical proposition to be tested, not an assumed causal law.

The analytical pathway is conditional rather than causal by assumption. Digital record availability may support reliability only if records are complete, stable, and attributable. Reliability may become auditability when a knowledgeable party can reproduce and reconcile the record. Verification may affect a credit decision only when the lender receives the evidence, trusts the verification actor, has incentives and authority to act, and lacks a cheaper substitute. Figure 1 depicts these transitions visually; no verified study observes the entire chain.

To examine the boundaries of that proposition, this article asks five linked questions: which record properties matter (RQ1), how they are operationalized and validated (RQ2), what evidence documents verification or use at different stages and which lending outcomes are directly observed (RQ3), which borrower, lender, product, and institutional conditions are empirically associated with use or outcomes and which remain proposed (RQ4), and which null, conflicting, or substitute-information findings challenge the pathway (RQ5). A focused critical review with structured evidence mapping compares studies whose populations, designs, and endpoints differ. It follows the documented historical Scopus trail while separating source-verified observations, theory-informed interpretations, and unresolved evidence.

The contribution is therefore narrower and testable. Existing assurance, SME-lending-technology, and organizational-information research explains why verified reports may matter, how lenders combine information with screening and monitoring, and why transmissibility and authority condition information use. This review adds a construct- and actor-specific conversion model for digital records: record availability; demonstrable reliability and auditability; verification or reconciliation by an identified actor; transfer into a lender workflow; and action by a recipient with decision rights, subject to substitute signals and product fit. The contribution is not a new claim that credible information matters, but a set of observable handoffs and failure points that makes different evidence designs comparable. No claim is made that the full sequence has already been established.

2. RESEARCH METHOD

This study employed a focused critical review with structured evidence mapping to examine where claims concerning digital records were empirically measured and where the available evidence ceased to provide direct observations of lending decisions. The historical Scopus protocol provided search provenance, but neither the subsequent assessment register of 220 records nor the eight illustrative cases constituted an exhaustive final sample. The cases were selected retrospectively following traceable source appraisal or renewed source verification, rather than through a prospectively registered sampling rule for eight studies. Identification and extractability of the information mechanism, relevant actor, research

design, observed outcome, population boundary, and key counterevidence were minimum conditions for meaningful comparison. These conditions were not introduced as additional eligibility criteria under the historical protocol. The selected cases represented distinct analytical contrasts: reporting discrepancies and bank registry consultation (Duro et al., 2026), electronic invoicing within tax networks without a credit outcome (Bellon et al., 2023), hypothetical lender cognition (DeZoort, Holt, & Taylor, 2012), certification and observed SME loan conditions (Wu & Xu, 2020), borrower reported financing access (Gamage, 2013), predictive information gains (Modina et al., 2023; Andrikopoulos & Khorasgani, 2018), and a finding of no incremental predictive improvement (Gupta et al., 2014). These cases were not treated as interchangeable tests of auditability. Their distinct evidential roles, rather than the number of favorable findings, justified their comparison.

Analytical selection proceeded in two stages. First, a recoverable source identity and sufficiently documented information about the actor, mechanism, and outcome were established. Second, cases were compared across verification settings, information sources, research designs, proximity to actual credit outcomes, and favorable, null, or findings unrelated to credit. The study did not assume that all eligible records had an equal probability of entering the comparison or that all eight cases met the primary SME eligibility criteria. In particular, the Peruvian tax study and the Spanish credit registry study involving firms of different sizes provided contextual or boundary evidence rather than separable causal estimates of SME auditability effects. Their distinct scientific classifications were retained in the historical register. Checks for overlapping evidence families were used to reduce duplication, while directly relevant SME financing evidence, contextual mechanisms, and qualitative scenarios concerning missing evidence were interpreted separately. Additional reports, including the eleven prioritized but unavailable sources, could alter the balance of evidence. Source recoverability was therefore recognized as a potential source of selection bias rather than an indicator of superior study quality. No inaccessible record was scientifically excluded solely because its full text could not be obtained.

To examine the sensitivity of the analytical selection, Appendix A14 documents the provenance, analytical purpose, and plausible alternatives for each comparison. A preserved audit also challenged the selection against the other 58 appraisal dossier entries, although the standalone final matrix was unavailable for independent reinspection during the final review. This audit identified alternative evidence involving external audit and borrowing costs, banker interviews, borrower perceptions of screening, another hypothetical banker judgment, prediction models labelled as relevant to decisions, other predictive applications, and technical reporting architectures. These alternatives were not added to the displayed cases when they duplicated an existing comparison of actors, designs, and outcomes, belonged to the same evidence family, lacked sufficient source material to support a consequential claim, or provided no additional observation of the transition from verification

to lending action. Their documented positive, nonsignificant, and contextual findings nevertheless informed the assessment of potential omissions. Because most of these alternatives remained historical or provisional dossier records rather than complete texts independently reassessed during the final review, this examination informed selection sensitivity rather than new eligibility decisions or effect estimation (Appendix A18.3).

The historical literature search was conducted in Scopus on 16 October 2026 using seven locked search streams based on TITLE-ABS-KEY. All streams applied the recorded publication year filter, PUBYEAR < 2027. The searches produced 1,065 occurrences, which were reduced to 991 unique Scopus EIDs through exact EID deduplication. The publication year filter did not independently establish first public availability by the research cutoff of 16 October 2026. Publication date eligibility was therefore assessed separately for individual records. The complete search expressions are reproduced verbatim in the Reproducibility Appendix to preserve the original search protocol.

Study eligibility was distinguished from source accessibility, methodological quality, evidence proximity, and analytical role. Reports were not excluded merely because their full texts were inaccessible, and methodologically weak but relevant studies were not deemed ineligible solely on quality grounds. Conversely, an accessible prediction study or technical framework was not automatically interpreted as direct evidence of lender decision making. Under the locked codebook, exclusion code X2 applied to generic adoption, FinTech, or prediction research that lacked material record evidence or SME lending relevance. Code X5 was reserved strictly for confirmed duplicate records or publication versions that were not independent studies. Related reports and studies based on overlapping samples retained their individual report identities but were not interpreted as independent replications.

The evidence mapping framework distinguished six categories of research evidence. E1 represented direct lender judgment or experimental evidence; E2 represented observed lending or administrative credit outcomes; E3 covered borrower surveys or field evidence; E4 concerned transaction data and prediction studies; E5 included accounting systems and technical verification research; and E6 comprised conceptual or review evidence. Level 1 encompassed the eligible research universe of mechanisms and relevant contexts, while Level 2 was reserved for evidence supported by full text that materially connected verification relevant information to lender evaluation or credit outcomes. Researcher approved classifications retained Wu and Xu (2020) as bounded E2/Level 2 evidence because the study reported observed financing conditions without directly observing lender verification of an audit trail. Gamage (2013) remained E3/Level 1 contextual evidence concerning borrower reported access to financing. Earlier provisional classifications and subsequent researcher decisions remained available in the supplementary decision history.

Methodological quality was assessed using criteria appropriate to each research design. Observational credit studies were evaluated for selection, confounding, endogeneity, measurement, missing data, and external validity. Experimental studies were assessed for

assignment procedures, manipulation and measurement validity, reliance on hypothetical outcomes, and generalizability. Prediction studies were examined for data provenance, information leakage, validation, calibration, discrimination, and evidence of actual decision use. Surveys were evaluated for sampling, self-reporting construct validity, while technical frameworks were assessed for implementation, independent testing, and deployment in real settings. Historical appraisal records carried forward without recoverable evidence for individual quality domains were classified as insufficiently documented rather than automatically assigned a low risk of bias. For the sixteen detailed appraisals, record specific methodological concerns are documented in Supplementary Appendix A7c without imposing an overall ordinal risk score where the underlying evidence was incomplete. The remaining records were not assigned overall quality ratings when sufficiently detailed information was unavailable.

The data extraction framework connected bibliographic identity and original Scopus provenance with SME population characteristics, financial or transaction record attributes, verification actors, lender involvement, observed financing outcomes, research designs, and reported limitations. Appendix Table S4 specifies the fields used for structured evidence mapping. The distinction between record coverage and substantive evidence availability was maintained throughout extraction. Bibliographic metadata could be recovered for more records than could be assessed through substantive source examination. Detailed contemporary appraisals were available for sixteen records, while fifty historically carried forward appraisal dossiers could not be treated as fifty complete texts independently extracted during the final review. Missing report level information remained unassessed rather than being reconstructed through inference.

Screening decisions and subsequent adjudications were preserved in the original register according to their respective stages and in the dated decision history. Researcher decisions governed the exclusion of the retracted report, the bounded classification of the lending outcomes reported by Wu and Xu (2020), and the contextual interpretation of Gamage (2013). Potential duplicate publications, related versions, and dependent samples were tracked separately to avoid interpreting overlapping evidence as independent confirmation. The available documentation did not establish that two independent human reviewers had screened the complete corpus or provide a valid denominator for paired screening decisions. Consequently, no formal interrater agreement statistic was reported. AI assisted appraisals, researcher adjudications, and independent source verification were treated as distinct activities. The STEP02C procedure had been internally locked before subsequent screening and full text appraisal, but no prospective public protocol registration or registration identifier could be verified. The study therefore did not claim prospective preregistration or completion of the original Critical SLR and final PRISMA flow. The operational evidence availability diagram was not presented as a substitute for final PRISMA

accounting. Further decision histories, provenance details, and quality evidence are documented in Supplementary Appendix A7c to A8b.

Consequential claims were assessed using original study appraisals where recoverable, publisher records, and the register linking claims to their supporting sources. Bibliographic identity, depth of source access, and substantive claim support were recorded separately. The official Banco de España record and publisher HTML material for Duro et al. (2026) confirmed the study's five author identity and supported a bounded interpretation of discrepancies between reported debt and registry information, conditional credit quantities, and selective registry consultation. However, lawful access to the exact full text was not recovered again during the final review. An earlier appraisal of the exact PDF remained part of the historical project provenance and was not represented as a new verification of the complete methodology. Publisher metadata and abstract material for Modina et al. (2023) supported the reported incremental prediction findings based on accounting and bank and firm information, but detailed validation procedures, implementation, and lender decisions were not independently reverified from full text. Bellon et al. (2023) provided evidence concerning administrative electronic invoicing adoption, tax compliance, and supplier network outcomes, rather than verification by banks or observed credit decisions. For the remaining comparative studies, Appendix A17 distinguishes the evidence supported by exact or accepted manuscripts, accessible articles, historical appraisals, and abstracts. Supplementary theoretical literature was considered separately and was not added to the denominators of the locked Scopus search.

An additional provenance constraint concerned the transition from historical screening to the operational assessment register. Initial screening of the 991 unique EIDs retained 454 records and excluded 537. The subsequent register contained 220 records, including 217 carried forward from the historical retained set and three documented restorations. Thus, 237 originally retained records were absent from the later register. The available evidence indicated different subsequent dispositions for some of these records, but later decisions could not be recovered for many others. Accordingly, the register of 220 records was interpreted as a bounded operational assessment set rather than a final count of eligible studies. The unresolved transition was retained as a methodological limitation rather than reconciled through unsupported assumptions. Detailed accounting and exceptions are provided in the Reproducibility Appendix.

Interpretive synthesis and framework development followed a traceable sequence rather than a separately preregistered coding procedure. First, the eight cases were compared according to their underlying records or signals, verification actors, study populations, research designs, and observed outcomes. Second, direct observations were distinguished from indirect or contextual mechanisms and transitions that remained unobserved. Conflicting findings and results showing no incremental improvement were retained rather than combined into an assumed overall effect. Third, these comparisons were interpreted in

relation to assurance theory, SME lending technologies, distinctions between hard and soft information, and organizational authority over credit decisions. Finally, the identified gaps between observed evidence and proposed mechanisms, together with plausible rival explanations, informed the development of five testable propositions (P1–P5). This approach maintained a distinction between observations supported by the reviewed evidence, theoretical interpretations, and newly formulated propositions. No independent reviewer consensus process, intercoder agreement statistic, grounded theory procedure, or prospective framework generation protocol was claimed.

3. RESULTS AND DISCUSSION

The later evidence-assessment register contains 220 unique study records. Historical documentation identifies 66 appraisal records, 151 reports that could not be assessed because of access, version, or identity barriers, two linked records without independent appraisal, and one report later excluded after formal retraction. That later decision does not retroactively change the historical register. Of the 66 appraisal records, 16 contain detailed recent assessments; the other 50 are earlier carried-forward records with uneven domain-level documentation. Appendix Figure S1 therefore distinguishes the 66-record bibliographic denominator from the 16-record design denominator.

Evidence-source categories could be recovered for 33 of the 66 recorded appraisal records; the remaining 33 are not assessable for E1–E6 distribution. Detailed design and level coding is available for only 16 records. Appendix Figure S2 therefore separates documentation coverage from the endpoints of the eight maximum-contrast cases in Table 1. Neither panel estimates the prevalence of mechanisms in the 991-EID search universe or the 220-record assessment register.

Table 1 compares eight analytically selected cases because each isolates a different inferential problem rather than because the set represents a final systematic-review corpus. Duro et al. (2026) connects a debt–registry discrepancy with lending quantities but spans firm sizes; Bellon et al. (2023) identifies a transaction-reporting mechanism and tax outcomes outside bank credit; DeZoort et al. (2012) observes lender judgment in a hypothetical decision; and Wu and Xu (2020) relates a third-party certification signal to realized SME borrowing conditions without observing digital-record authentication. Gamage (2013) supplies borrower-reported access, Modina et al. (2023) and Andrikopoulos and Khorasgani (2018) show predictive information content, and Gupta et al. (2014) preserves counterevidence to automatic incremental gains. Together, they contrast actors (tax administrator, bank, auditor, venture capitalist and borrower), endpoints (compliance, judgment, realized lending, reported access and prediction), and research designs. Positive effects were not a necessary selection rule; a non-credit outcome, an observational association, and a null prediction result are integral to the comparison. The corresponding

information is not pooled, interpreted as representative frequencies, or elevated to causal verification-to-lender-use evidence. Selection limitations and omitted alternatives are documented in Appendix A14–A15.

Table 1 Maximum-contrast studies: mechanisms, outcomes, and inferential limits.

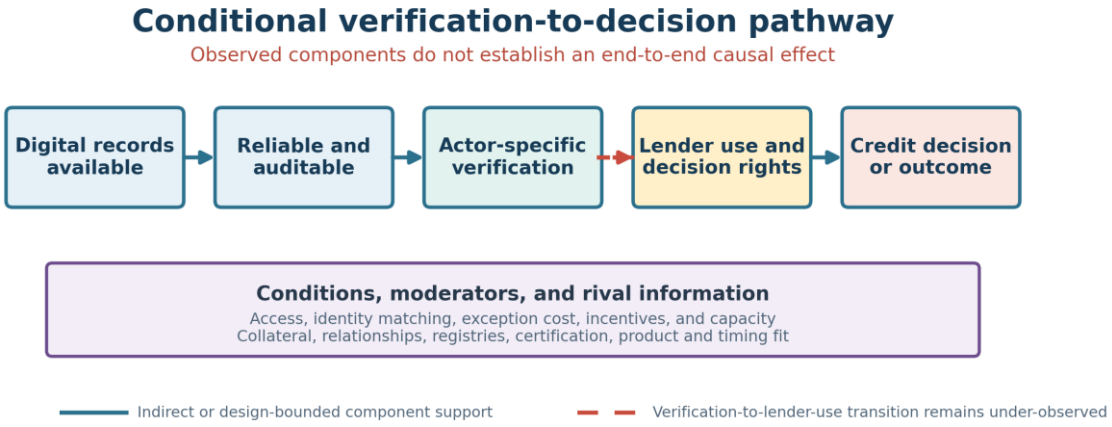
Study and design	Information / actor	Endpoint actually observed	Principal inferential boundary
Duro et al. (2026) Spain; administrative records, mixed-size private firms	Debt-disclosure versus credit-registry balances; bank query indicator	Conditional lending quantity	Selective registry queries; no separable SME-only causal digital-audit-trail effect.
Bellon et al. (2023) Peru; staggered administrative reform	Mandated electronic invoices; firm-to-firm transaction and VAT reporting	E-invoicing adoption, VAT outcomes and supplier links	Tax-system findings; no bank verification or credit outcome; SME-only lending eligibility not established.
DeZoort et al. (2012) 168 commercial lenders; experiment	Financial-statement and auditor-reliability cues	Hypothetical default-risk and reporting-reliability judgments	Not realized loan approval, lending quantity or pricing.
Wu and Xu (2020) 10,468 Chinese SMEs; observational panel	Venture-capital certification and accounting-quality proxy	Loan balances, borrowing cost and collateral conditions	Non-random certification; no observed digital-record authentication at the bank.
Gamage (2013) Sri Lanka; borrower survey	Borrower-reported audited-statement status	Self-reported bank-financing access	Lender verification and loan approval unobserved; sample reduction requires caution.
Modina et al. (2023) Italy; default-prediction model	Accounting plus internal bank–firm indicators	One- and two-year default prediction	Default-prediction evidence only; live lender deployment and allocation were not observed.
Andrikopoulos and Khorasgani (2018) UK; SME prediction	Accounting and market-derived proxies	Hybrid-model prediction improvement (8 of 9 annual tests)	Model performance does not document lender deployment; 2011 exception.
Gupta et al. (2014) UK; SME risk model	Operating cash-flow variables	Null incremental prediction benefit	A model result, not a finding of no lender decision effect.

Source: *Proceed Data, 2026*

The eight cases do not exhaust the available research or confer a uniform primary-evidence status. The Spanish debt-registry study remains restricted as primary SME evidence because firm-size-specific estimates cannot be separated; the Peruvian e-invoicing case serves as an external mechanism contrast and has no verified SME-lending endpoint. Other appraised records, including overlapping prediction, survey and technical studies, remain in the supporting registers; their absence from this table is not a scientific exclusion. Minnis and Sutherland (2017) and Minnis, Sutherland, and Vetter (2024) provide reidentified historical contextual comparators, whereas Minnis (2011) and Schneider (2018) provide separately sourced theoretical context. The eleven high priority inaccessible reports could change the apparent balance between lender side verification, prediction and technical

mechanisms, and therefore cannot be assumed to corroborate this selection. The appendix identifies each case's historical role and the limitations of the source material available for its interpretation. A structured challenge against the other dossier records identified additional audit/cost of debt, bank-information-practice, borrower-screening, hypothetical-judgment, prediction and technical-architecture roles. None of the source-recoverable alternatives added a jointly observed SME record-verification event, lender workflow and realized credit endpoint. One historical borrower-screening dossier records a non-significant audited-report association, while other records preserve contextual or prediction-only signals. These records broaden possible counterevidence but, because most were not independently re-read, they are not used to infer effect direction or prevalence (Appendix A18.3).

Figure 1. Conditional verification-to-decision pathway



Source: *Proceed Data, 2026*

RQ1: The literature identifies several properties needed to assess the evidential value of business records. Provenance establishes source and attribution; integrity concerns undetected changes; coverage and timeliness determine whether a record reflects the relevant activity and period. Reconciliation compares a reported amount with another information source, while attestation makes a third party accountable for specified assertions. Access and traceability determine whether a lender can obtain, reconstruct, and challenge the information. These properties are analytically distinct, not a single indicator of digital quality. Importantly, this is an evidence-informed construct taxonomy: only some properties are directly operationalized in the selected empirical studies. Identity matching, completeness controls, exception-resolution logs, and lender access rights are not jointly validated as a multi-item auditability instrument by the compared evidence. Bellon et al. (2023) observe electronic invoicing and tax-compliance responses; their design does not establish bank-side record verification. Duro et al. (2026) document account-versus-registry

discrepancies, selective registry consultation, and conditional credit differences, but the mixed-size sample and endogenous checking do not identify an SME-specific causal digital-auditability effect.

RQ2: Operationalizations range from direct record comparisons to distant proxies. Reconciliation studies compare reported liabilities with registry balances. Assurance studies use audit or attestation status. Other studies proxy accounting information quality through accrual measures, survey whether statements are audited, or infer usefulness from prediction gains. These indicators are not interchangeable: some describe a measured discrepancy or external assurance status, whereas others are proxies for credibility, information content, or self-reported practices. A source being digital is not itself a validated measure of provenance or integrity. A discretionary-accrual measure can reflect reporting quality without identifying data provenance. An audit indicator signals third-party involvement but not which transaction assertions were verified. Predictive accuracy can show discrimination but not the record's authenticity or the lender's reliance on it.

Design validity is strongest for discrete observed components. Bellon et al. (2023) use administrative transaction data and a staggered reform design to examine e-invoice adoption and tax-compliance spillovers, but not credit outcomes. Wu and Xu (2020) observe loan balances, costs, and collateral conditions in a large SME panel, yet venture-capital certification is not randomly assigned and no bank-side record-authentication act is measured. Gamage (2013) starts from a 610-enterprise survey frame, removes 109 large enterprises, and reports a logistic model with N=452; the accessible article does not explain the remaining reduction. Its audit indicator and access outcome are borrower reported, not bank-side verification. DeZoort et al. (2012) identify a lender-judgment response in a hypothetical case. The accepted manuscript of Andrikopoulos and Khorasgani (2018) validates predictive discrimination without validating organizational use. Modina et al. (2023) supports a bounded incremental-prediction claim, but not live deployment or credit allocation. The evidence therefore supports heterogeneous, design-specific measures of particular constructs, not a shared, independently validated auditability scale. In particular, observed registry mismatches should not be presented as direct estimates of all technical properties, and prediction accuracy should not serve as a surrogate for verification or lender adoption.

RQ3: The evidence documents different points in credit assessment, but rarely joins them. DeZoort et al. (2012) directly observe commercial lenders' hypothetical judgments; Minnis and Sutherland (2017) observe post-origination information demand; prediction studies observe model performance rather than organizational use. Wu and Xu (2020) observe realized SME loan terms associated with venture-capital certification, while Duro et al. (2026) report conditional credit quantity in a selected mixed-size setting. Gamage (2013) records firms' survey-classified access, not an independently observed bank approval. Accordingly, RQ3 can be answered as a map of observed stages and endpoints:

the literature contains lender judgment, information demand, modelled risk, realized terms, and borrower reports, but the verified cases do not observe the complete sequence from record verification through lender processing to financing action.

RQ4 is answered by separating tested conditions from proposed ones. Empirically examined heterogeneity includes borrower and contract conditions in Minnis and Sutherland (2017). Wu and Xu (2020) examine ownership, high-technology status, regional banking development, firm size, collateral, and an accounting-information-quality interaction; the size-based hypothesis was not supported uniformly, which cautions against a single certification effect. Contextual associations include technological alternatives and lender competition linked to reduced statement collection in Minnis et al. (2024), plus market-specific nonbank substitution. By contrast, access rights, exception-resolution cost, workflow compatibility, processing capacity, decision authority, and product fit remain propositions in this review rather than estimated moderators. The evidence-mapping question is answerable even though many causal contingencies remain untested.

RQ5: Counterevidence changes the interpretation of the pathway rather than merely weakening it. Gupta et al. (2014) report no incremental improvement in SME distress prediction from adding operating cash-flow information to the tested model. Bellon et al. (2023) document e-invoice adoption, tax-compliance responses, and changes in supplier relationships, but do not estimate bank-credit effects or a general financing transition cost. Minnis et al. (2024) associate reduced collection of attested statements with settings in which alternative information and lender competition are more prominent. Wu and Xu (2020) report that venture-capital certification is more strongly associated with bank borrowing where accounting information is weaker, which is consistent with substitution but not a randomized test. Taken together, the comparative evidence does not establish a uniform benefit from digital records across SME lending contexts.

Scenario A restricts interpretation to directly relevant SME financing evidence. Wu and Xu (2020) then supplies bounded observational evidence on realized loan terms, while Gamage (2013) remains borrower-side contextual evidence; the other cases contribute hypothetical judgment, mixed-size lending, prediction, or non-credit mechanisms. Under this restriction, no case jointly observes SME digital-record provenance, verification, lender processing, and a credit decision. The construct distinctions survive, but empirical support for the full bridge is very limited.

Scenario B adds clearly separated contextual and mechanism studies. Duro et al. (2026), Bellon et al. (2023), DeZoort et al. (2012), and the prediction studies then illuminate reconciliation, actor differences, cognition, and information content without becoming estimates of an SME digital-auditability effect. Scenario C further recognizes the 151 constrained reports: stronger direct findings could narrow the apparent bridge gap; null or harmful results would make the pathway more conditional; and design-dependent access

could change the apparent evidence mix. Across all scenarios, the inference that availability, credibility, verification, organizational use, and financing outcomes are non-equivalent remains defensible. The prevalence and strength of direct effects do not. Substituting the register's role-equivalent alternatives leaves the core endpoint distinctions intact: audit and lender-practice records remain observational or contextual, additional models remain prediction studies, and technical architectures lack deployment outcomes. The exercise therefore reduces the risk that the eight cases were chosen only for favorable findings, but it cannot eliminate availability-driven selection because the underlying evidence was not uniformly recoverable.

The evidence is best interpreted as partially connected mechanisms rather than a single digitalization effect. Record creation can lower storage or transmission costs, but auditability requires attribution, coverage, controls, and reproducible reconciliation. Verification affects lending only if an appropriate actor communicates the result, the lender can resolve exceptions, and a decision maker or system has authority and incentives to act. Collateral, relationships, tax returns, registries, proprietary data, and third-party certification may complement or displace that evidence. Figure 1 and the case-by-case comparisons in Table 1 make the resulting gap explicit: component observations exist, yet none of the eight comparative cases documents the entire sequence from digital record to lending outcome.

Duro et al. (2026) and Bellon et al. (2023) examine information mechanisms that differ in both actor and economic endpoint. Duro et al. compare unaudited debt disclosures with credit-registry balances and report less credit for underreporting firms, particularly where banks consult the registry. Yet registry consultation is selective, the analyses remain observational, and the article expressly stops short of establishing that verification caused firms to report more truthfully. The private-firm universe also does not yield a separable SME effect. Bellon et al. study a Peruvian electronic-invoicing rollout and document adoption, VAT-compliance responses, and changes in supplier relationships. Their observed actor is the tax-administration system, not a lender, and the study does not estimate credit allocation.

These cases separate the availability of a cross-check from the consequences of using it. A tax authority can obtain transaction information that influences compliance without the resulting evidence entering a credit file. A bank can compare debt disclosures with registry data without thereby validating the full provenance and integrity of an enterprise’s transaction ledger. A credible SME-finance pathway would require jointly observed identifiers, reconciliation events, lender access, exception resolution and resulting loan decisions. Neither study tests that entire sequence in a separable SME population.

Evidence about credibility can be compelling while still measuring different dependent variables. In an experiment involving commercial lenders, DeZoort et al. (2012) examine how perceived auditor reliability affects financial-report credibility and default-risk judgments in a hypothetical loan case. Wu and Xu (2020), in contrast, use observed SME

bank-borrowing conditions and associate venture-capital backing with loan balances, costs, and collateral conditions, especially in several information-constrained settings. Gamage (2013) relates a borrower-reported audited-statement indicator to survey-reported bank access. The first observes lender cognition, the second realized financing conditions, and the third borrower reports; none directly observes a bank authenticating a digital audit trail.

The common lesson concerns competing channels for reducing uncertainty. Auditor reliability is an assurance cue in DeZoort et al. (2012), venture-capital backing is a third-party certification signal in Wu and Xu (2020), and audited-statement status is borrower reported in Gamage (2013). These mechanisms are not verification acts performed by the same bank. Wu and Xu use matching and first-investment comparisons, yet acknowledge non-random venture-capital selection; Gamage does not observe lender verification; and DeZoort does not observe a loan being made. Their differences explain why an association between information quality and credit cannot be translated into a general causal effect of digital audit trails.

Studies of default prediction address a different question from lender information use. Modina et al. (2023) report that internal bank–firm indicators add predictive information to accounting variables for one- and two-year default probabilities; this review uses only that bounded result and does not infer calibration in a live system, deployment, or lending action. The accepted manuscript of Andrikopoulos and Khorasgani (2018) combines accounting data for unlisted UK SMEs with market-derived information from listed comparators; its hybrid model exceeds the accounting-only model in eight of nine annual tests, but not in 2011. Gupta et al. (2014) provide a counterpoint in which operating cash flow offers no clear incremental predictive improvement. None of these endpoints records whether a loan officer used the score or changed an approval, price, or limit. Prediction performance therefore locates information content and transport risks; it does not establish operational decision-use.

A test of decision-use must follow the information into the lending workflow: whether an officer or system receives the variable, its timing relative to application review, the incumbent screening rule, authority for overrides and the subsequent credit outcome. Without this additional observation, prediction studies are useful for locating information content and validation problems, but cannot be treated as evidence of realized allocation effects.

The value of one information source is contingent on the availability of others, but the empirical status of substitution differs across studies. Minnis and Sutherland (2017) test conditional bank demand for statements and tax returns across borrower and contract conditions. Wu and Xu (2020) report an interaction consistent with venture-capital certification being more valuable where an accrual-based accounting-quality proxy is weaker; they also report no collateral-synergy interaction and heterogeneous size results,

while non-random certification remains a threat. Minnis et al. (2024) provide contextual evidence that statement collection declined alongside technological alternatives and nonbank competition. By contrast, the proposition that digital audit trails complement statements through transaction information remains conceptually plausible but untested in the selected evidence. The synthesis therefore does not label every alternative source as an empirically established substitute.

Hard information is portable, but portability can strip context, while soft information depends on relationships and organizational authority (Liberti & Petersen, 2019). Lender incentives also shape what becomes reportable and actionable (Hertzberg et al., 2010). Thus, an audit trail may complement statements when it cheaply confirms transactions, but it may be dominated by collateral, registry data, relationship knowledge, or third-party certification when those signals are more enforceable or easier to process. FinTech and finance-company substitution in small-business markets (Gopal & Schnabl, 2022) and the program-specific reach of PPP FinTech lenders (Erel & Liebersohn, 2022) reinforce the point that distribution technology and record assurance are distinct mechanisms.

Technical infrastructures can retain invoices, transaction identifiers and reporting histories, yet these properties have no automatic interpretation as independently established lending benefits. Bellon et al. (2023) document a real e-invoicing rollout, administrative transaction evidence, adoption spillovers and tax-compliance or supplier-network responses, but do not measure bank verification or credit allocation. Likewise, a scoring model can reveal information content without recording its adoption by a loan officer or automated approval system. The cross-study gap is therefore an institutional one: when, by whom and at what cost can record-specific information be verified, communicated, reconciled and acted upon within a particular lender's decision rights?

Additional data can also impose burdens. Incomplete digital coverage may misclassify informal businesses; inconsistent records can propagate errors; and access rules may concentrate informational power in platforms or lenders. These are plausible governance risks rather than measured effects across the reviewed studies. A consequential evaluation would compare the benefits of reconciliation with implementation costs, error-correction procedures, contestability, privacy and the use of substitutive lender information.

Information-asymmetry and signaling theories explain why costly, credible information can separate borrowers, while hard-versus-soft information research explains portability and relational context (Liberti & Petersen, 2019). Berger and Udell (2006) already conceptualize SME lending technologies as combinations of information, screening, contracting, and monitoring; Stein (2002) already links information hardness and transmissibility to organizational structure and authority. Minnis (2011) connects audited statements to debt pricing, Schneider (2018) synthesizes assurance effects on lending judgments, and Minnis and Sutherland (2017) demonstrates conditional information demand. The present framework therefore does not claim novelty for credible signaling,

lending-technology heterogeneity, information transfer, or authority in isolation. Its incremental contribution is an explicit measurement and handoff model for the conversion of digitally generated evidence across separate institutional actors: which record property is asserted, who can verify it, how an exception is reconciled, which lender recipient obtains a usable result, whether that recipient can act, and which observed endpoint would reveal use.

The framework's incremental contribution is to separate two organizational failures that established lending-technology theory leaves empirically bundled: a usable verification result may fail to arrive at its intended recipient (P1), or may arrive but fail to change a decision because processing capacity, incentives or authority are inadequate (P5). Berger and Udell (2006) already treat SME lending as a configuration of information, underwriting, contracts and monitoring; Stein (2002) already predicts that information hardness and organizational authority shape allocation. Neither result alone identifies the source identity, matched borrower and document identifiers, unresolved exceptions, recipient access, workflow timing, or logged action required to test an external verification handoff. Accordingly, P1 asks whether comparable SME applications with and without authenticated, timely, interpretable verification output received by the correct lender unit show a different probability of documented information use. P5 is conditional on receipt of an equivalent usable output and asks whether variation in decision rights, incentives or capacity explains actual approval, limit, price or monitoring changes. An experiment or staggered system rollout could randomize or instrument access for P1 and compare organizational authority or processing constraints for P5, while recording overrides and final outcomes. Equal use despite missing access would challenge P1; equal action across measured processing or authority differences, conditional on receipt and case mix, would challenge P5. Alternative information, product suitability and exception costs supply explicit rival predictions rather than after-the-fact reasons to preserve the model.

The propositions below separate empirical anchors from extensions that remain untested. Their purpose is to expose rival predictions and required observations, not to convert the evidence map into a causal model.

Table 2 Falsifiable propositions for the verification to decision pathway.

Proposition and theoretical basis	Observable prediction and falsification	Current evidential status
P1 — Access-and usability alignment Hard-information transmissibility and lending-technology fit	Unit and mechanism: a borrower-record verification output, linked to a correct borrower identifier, reaches the intended loan officer or decision engine before review. Record receipt, permissions, interpretability and timing. Test: compare application-level logged use under staged/randomized output access, holding case mix and baseline lender rules constant. Falsifier: access restrictions or failure of recipient delivery do not change verified information use. This tests receipt/usability, not loan approval directly.	Actor contrasts are observed; the record-level access-by-use interaction is not jointly tested.
P2 — Exception resolution capacity	Mechanism: lender operations or an independent verifier resolves discrepancies through matched identifiers and reproducible	Reconciliation components are observed;

Verification-cost and control reasoning	evidence. Moderator: cost and decision timing. Prediction: resolved exceptions produce more consistent responses than unresolved flags. Falsifier: no difference despite adequate resolution capacity and authority.	workflow and cost remain untested moderators.
P3 — Substitute-signal attenuation or complementarity Information asymmetry and signaling	Mechanism: the lender combines verification with collateral, relationships, tax returns, registries, proprietary data, or certification. Prediction: marginal value falls with strong substitutes but rises with independent corroboration. Falsifier: invariant effects across substitute strength and corroboration.	Conditional demand and certification interactions are observed; digital-record complementarity is not directly tested.
P4 — Product-and timing fit Lending-technology configuration	Mechanism: a product-specific unit matches record horizon and update frequency to repayment risk. Prediction: transaction verification matters more for short-horizon cash-flow or receivables products unless longer-horizon coverage is assured. Falsifier: no verification-by-product or timing interaction.	Theory-informed extension; not estimated in the selected cases.
P5 — Decision-rights and processing capacity Organizational information processing and authority	Unit and mechanism: a lender team receives the same usable verification output but differs in processing capacity, incentives or formal authority. Test: compare recorded overrides, approval, limits, pricing or monitoring decisions across organizational conditions conditional on receipt, credit-product fit and borrower risk; exploit organizational change when possible. Falsifier: no outcome or action differences across measured authority/capacity despite usable receipt and sufficient statistical precision. This tests conversion into action, not initial information access.	Judgment and outcomes are observed in different studies; organizational conversion is not jointly observed.

Source: *Proceed Data, 2026*

The five research questions converge on the distinction between a record, its credibility, and its organizational use. RQ1 identifies provenance, integrity, coverage, timeliness, reconciliation, attestation, and controlled access as separate properties. RQ2 finds that these properties are measured through non-equivalent direct comparisons, assurance indicators, reporting quality proxies, surveys, experiments, and prediction metrics; no common validated auditability measure emerges. RQ3 maps lender judgment, post-origination information demand, model-based risk classification, realized terms, and borrower-reported access, but none of the eight comparative cases documents the complete sequence from record verification and lender processing to credit decisions.

RQ4 distinguishes empirically examined borrower and contract conditions, contextual institutional associations, and proposed workflow moderators that still require direct tests. RQ5 identifies a null incremental prediction result, conditional demand for financial statements, non-credit e-invoicing effects, and alternative signals that may substitute for accounting information. The direct-SME-only sensitivity test leaves evidence for the complete bridge very limited; adding contextual mechanisms improves explanation without converting them into direct effects. Taken together, the comparative evidence does not establish a uniform benefit from digital records across SME lending contexts. It supports a narrower proposition: auditability may become decision-useful when the relevant lender receives usable verification, exceptions can be resolved, decision makers have capacity and authority to act, the information fits the product and timing, and substitute signals do not dominate.

Integrity controls affect the scope of this synthesis. A formally retracted report is not used as substantive evidence, and unresolved historical identity or quality issues remain in the source register rather than being silently resolved. The accessible publisher HTML abstract and introduction support bounded statements about Duro et al. (2026) concerning registry reconciliation, selective consultation, and conditional credit quantity; the historical project register also records a separate full-text appraisal, which was not repeated here; it does not supply an SME-only causal estimate or a complete staff workflow, so it is not treated as primary SME evidence. Wu and Xu (2020) and Andrikopoulos and Khorasgani (2018) have identity-matched accepted manuscripts supporting their bounded claims. Modina et al. (2023) is used only for the prediction result stated in authoritative source records; detailed validation and deployment are not independently reappraised here. Wu and Xu provide observational evidence on realized credit conditions, whereas Gamage (2013) provides borrower-side contextual evidence. Earlier appraisal records without adequate documentation remain NOT ASSESSABLE for domain-level quality ratings.

Missing evidence can distort both the apparent balance of the map and the eight-case comparison. Among 151 access-, version-, or identity-constrained reports, eleven were marked high priority because their titles and metadata potentially concern lender assurance, internal audit, supply-chain finance, digital footprints, prediction, or invoice-financing architectures. Their outcomes were not imputed. The distinction between record availability, independent verification, model performance, and observed credit decisions remains conceptually necessary under each defined scenario; this is an inferential stress test, not evidence that the unseen reports would corroborate the model. By contrast, the apparent scarcity of direct E1/E2 evidence, the relative importance of assurance versus substitute signals, and the strength of the verification-to-use bridge remain contingent on inaccessible reports and retrospective source availability.

The scenarios are directional stress tests, not inferred findings. Under a positive-evidence scenario, stronger direct lender studies would weaken the claim that E1/E2 evidence is scarce but would still require verification of population, actor, workflow, and endpoint. Under a null-or-negative scenario, any benefit claim would weaken and implementation costs or counterevidence would become more central. Under selection-dependent missingness, proprietary lender-side studies may be systematically harder to access than prediction or technical studies, so design and endpoint frequencies cannot be interpreted as literature prevalence. The case-set sensitivity produces the same boundary: restricting the synthesis to direct SME financing evidence reduces empirical coverage sharply; adding separated contextual mechanisms improves explanation, not effect identification. The construct boundaries therefore remain analytically defensible across the specified thought experiments, whereas the distribution of evidence, effect strength, and potential exceptions are unresolved empirical questions.

Future research should measure an auditability bundle rather than digital adoption alone: source identity, tamper evidence, coverage, reconciliation success, exception rates, verifier independence, lender access, and actionability. Designs should observe the decision process and outcomes together, preferably through staged rollouts, policy discontinuities, matched lender–borrower panels, or field experiments. Prediction studies should report calibration, leakage safeguards, deployment context, overrides, and decision impact. Evidence-family controls are necessary where multiple publications share a platform or administrative dataset.

Lenders should distinguish data ingestion from evidential governance and tailor verification to the credit product. Conventional term lending may require statements, assurance, registry checks, collateral, and relationship information; working-capital and invoice finance place greater weight on invoice provenance, debtor identity, duplicate detection, settlement matching, and exception handling; cash-flow-based assessment requires coverage, timing, account ownership, and reconciliation of transaction feeds; digitally mediated screening additionally requires model governance, monitoring, override rules, and appeal channels. These are design implications, not proven credit-supply effects. Across products, a useful system documents origin, transformations, access rights, reconciliations, exceptions, model inputs, and human actions.

Policy should avoid mandating digital records as if format guaranteed reliability. Interoperable identifiers, proportionate assurance, data portability, privacy, and correction mechanisms matter more than digitization alone. For SMEs, accounting-system investment is most likely to become finance-relevant when records are consistent, attributable, reconcilable, and exportable in a form that the intended lender can lawfully process. For lenders, the governance responsibility extends beyond collection to exception resolution and accountable decision authority. These implications are hypotheses for product-specific testing, not established improvements in approval or pricing.

This focused synthesis is not an exhaustive systematic review. The original search is reproducible, but the transition from 454 title–abstract retains to a later 220-record register is incomplete: 237 retained records did not carry forward, including 221 without a recovered later disposition. Within the later assessment register, 151 reports were inaccessible or affected by identity/version constraints, and 50 historical appraisal records lack uniform domain-level documentation. These gaps create availability and design-selection bias, especially if proprietary lender studies are less accessible than prediction papers. The eight contrast cases were selected retrospectively for analytical variation and source recoverability, not representativeness. Population differences, observational selection, and the absence of jointly observed verification events, lender processing, and credit outcomes prevent a universal causal conclusion. The qualitative sensitivity analysis bounds these risks but cannot estimate their magnitude. The structured alternative-case challenge reduces, but does not remove, availability-driven selection: it can show whether recorded role-equivalent

evidence changes the construct map, but it cannot replace unavailable full texts or recover undocumented dispositions.

Supplementary theoretical and counterevidence literature clarifies organizational incentives and substitute information, but is distinguished from the historically retrieved set. External work cannot substitute for unavailable primary reports, and publication prestige or quartile labels were not used as evidence-quality ratings. The implications and proposed links remain conditional on the bounded source base.

4. CONCLUSION

Digital business records can become useful in SME lending, but usefulness depends on more than existence, apparent quality, or predictive performance. Prior research already explains assurance effects, heterogeneous lending technologies, hard and soft information, and organizational authority. This review's incremental contribution is to expose the actor specific conversion problem: who verifies which record property, whether discrepancies can be resolved, whether the relevant lender receives an interpretable output, whether decision rights permit action, and whether substitute signals or product requirements dominate. Registry comparisons, assurance cues, certification, borrower reports, and risk models illuminate different components, not a completed causal chain. The resulting propositions can be tested through matched source-verification logs, lender access and action records, and financing outcomes. The present synthesis supports a defensible analytical distinction among availability, credibility, verification, organizational use, and financing outcomes; the missing-evidence scenarios do not constitute an empirical robustness test of causal effects. The magnitude and prevalence of direct effects remain uncertain because missing reports could strengthen or weaken the evidence balance and the original Critical SLR remains unclosed.

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