AI-Driven Food Fraud Detection Systems: A Critical Systematic Review of the Detection–Prevention Gap

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Article

AI-Driven Food Fraud Detection Systems: A Critical Systematic Review of the Detection–Prevention Gap

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Abstract

Food fraud is a persistent global threat estimated to cost the food industry over USD 30 billion annually. The integration of artificial intelligence (AI) with analytical instrumentation has generated significant research activity directed at developing detection systems capable of identifying adulteration, mislabeling, and substitution across diverse food matrices. This systematic review critically examines the extent to which AI-assisted instrumental technologies contribute to food fraud prevention, and identifies the structural limitations that constrain their real-world implementation. A systematic search of peer-reviewed literature published between 2021 and 2026 yielded 72 eligible studies after application of predefined inclusion criteria. Studies were required to report quantitative performance metrics (accuracy, R^2, RMSE, AUC, sensitivity, specificity), describe methodological limitations, and mention laboratory or industrial implementation contexts. Data were extracted into a structured seven-sheet workbook covering study characteristics, instrumental technologies, AI architectures, performance metrics, industrial validation status, implementation evidence, and methodological quality. The corpus reveals a systematic pattern of high reported analytical accuracy—frequently exceeding 95% and in many cases reaching 100%—under controlled laboratory conditions. However, 75% of studies (54/72) conducted no external validation, 100% of studies reported no pilot-scale or routine monitoring application, and no study achieved inter-laboratory validation. The predominant technology was NIR spectroscopy (26/72 studies, 36%), followed by gas chromatography-based systems (14/72, 19%) and electronic noses (8/72, 11%). Classical machine learning—predominantly SVM, Random Forest, and ANN—dominated methodological approaches (43/72, 60%), with deep learning architectures accounting for 26% of studies. Technology Readiness Levels were unreported in 97% of studies. Methodological quality was predominantly moderate (42/72 studies scoring 3/5), with 19 studies scoring 2/5 and only one achieving the maximum score. This review identifies a structural gap between detection and prevention as the central finding: the scientific literature consistently demonstrates high analytical precision in laboratory settings while providing minimal evidence of real-world industrial deployment, regulatory integration, or measurable impact on the prevention of food fraud events. The findings demonstrate that the limitation is not primarily technological but systemic, highlighting the need for a paradigm shift from performance-driven research toward validation-driven, deployment-oriented frameworks.

Keywords: food fraud; adulteration; artificial intelligence; machine learning; deep learning; NIR spectroscopy; electronic nose

1. Introduction

1.1. The Persistent Problem of Food Fraud

Food fraud—defined as the deliberate and intentional substitution, addition, tampering, or misrepresentation of food, food ingredients, or food packaging for economic gain—constitutes one of the most complex challenges confronting contemporary food safety governance. Unlike food safety failures resulting from unintentional contamination, food fraud is characterized by intentionality,

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economic motivation, and systematic evasion of existing detection mechanisms. Its manifestations range from geographic origin misrepresentation [1–3] and species substitution [4–7] to adulteration with chemically similar compounds [8–11], dilution with economically motivated adulterants [12–15], and addition of unauthorized synthetic compounds [16–18].

The economic magnitude of food fraud is substantial. Global estimates consistently place annual costs in excess of USD 30–40 billion, with cascading effects on consumer health, trade relationships, and regulatory credibility. High-value commodities including olive oil, honey, spices, premium teas, and dairy products are disproportionately targeted, as evidenced by the concentration of studies in this review addressing precisely these matrices [9,10,14,15,19–22].

The detection challenge is compounded by the increasing chemical sophistication of fraudulent practices. Geographic origin fraud in soybeans [1] exploits the fact that chemical profiles overlap substantially across origins. The substitution of premium camellia oil with less expensive vegetable oils [11,23,24] requires discriminant methods capable of resolving chemically similar lipid profiles. The adulteration of black tea with synthetic pigments at concentrations as low as 0.1% [16] demands analytical sensitivity at the regulatory margins. These challenges have driven substantial research investment in AI-assisted analytical systems.

1.2. The Rise of AI-Assisted Detection

The period 2021–2026 has witnessed an accelerating convergence of analytical chemistry, instrumental spectroscopy, and machine learning in the domain of food authenticity. The present corpus captures this trend with marked clarity: publication volume grew from 2 studies in 2021 to 33 in 2025, representing a 16-fold increase over five years. The dominant analytical platform has been near-infrared (NIR) spectroscopy [2,9,10,15,17,22,25–28], which offers non-destructive, rapid, and relatively low-cost measurements compatible with high-throughput screening. This has been complemented by electronic nose arrays [1,3,8,12,23,29–33], hyperspectral imaging [9,10,13,16,34–38], and increasingly sophisticated multi-sensor data fusion architectures [4,11,28,30,33,39,40].

The machine learning landscape in this corpus is dominated by classical algorithms—Support Vector Machines, Random Forest, and Artificial Neural Networks—which appear in 43 of 72 studies (60%). Deep learning architectures including Convolutional Neural Networks, Transformers, and hybrid models account for approximately 26% of the corpus [1,6,11,18,24,29,39,41–48]. The reported performance metrics are consistently high: median accuracy across classification studies exceeds 96%, and R^2 values for regression tasks frequently approach or exceed 0.95. These results have generated considerable optimism regarding the potential of AI-driven systems to transform food fraud detection.

1.3. The Central Research Gap

Despite this analytical optimism, a fundamental question remains systematically underaddressed in the literature: to what extent do these detection systems contribute to the prevention of food fraud in real-world supply chains? The distinction between detection and prevention is not merely semantic. Detection in controlled laboratory conditions—with authenticated reference samples, known adulterant concentrations, single geographic origins, and optimized instrument conditions—is a methodologically tractable problem. Prevention requires that detection methods be validated across the full variability of real supply chains, integrated into regulatory enforcement workflows, economically viable for routine deployment, and sufficiently robust to generalize across instruments, laboratories, seasons, and geographic origins.

This systematic review addresses three interrelated questions: (1) What is the actual state of external validation and independent dataset usage in AI-driven food fraud detection research? (2) What is the evidence for industrial implementation and regulatory integration of these systems? (3) What structural factors explain the gap between laboratory performance and real-world prevention impact?

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This gap is not only empirical but also conceptual, requiring a reframing of how performance and utility are defined in AI-based detection systems.

From Analytical Accuracy to Decision-Theoretic Utility

A critical but systematically overlooked dimension in AI-assisted food fraud detection research is the distinction between analytical accuracy and decision-theoretic utility. While metrics such as accuracy, AUC, or R^2 quantify predictive performance under controlled laboratory conditions, they do not directly translate into actionable decision-making within regulatory or industrial environments.

In real-world contexts, detection systems operate within cost-sensitive frameworks characterized by asymmetric risks: false negatives may allow fraudulent products to enter the supply chain, whereas false positives may generate unnecessary economic losses, regulatory burden, and trade disruption.

From a decision-theoretic perspective, the practical utility of a detection system depends not only on sensitivity and specificity, but also on the prior probability of fraud occurrence, the cost structure associated with classification errors, and the specific deployment context within the supply chain. None of these parameters are explicitly modeled in the reviewed studies, resulting in a systematic overestimation of real-world applicability.

This discrepancy indicates that current research paradigms are primarily optimizing surrogate analytical metrics rather than operational performance. Bridging this gap requires the integration of decision theory, cost-sensitive learning, and probabilistic risk assessment into the design and evaluation of AI-based detection systems, thereby aligning analytical outputs with real-world prevention objectives.

1.4. Objectives

The primary objective of this systematic review is to critically assess the contribution of AI-assisted instrumental technologies to food fraud prevention, and to characterize the structural limitations that constrain their translation from laboratory proof-of-concept to operational deployment. Secondary objectives include: characterizing the distribution of analytical technologies and AI architectures in the recent literature; quantifying the frequency and rigor of external validation strategies; mapping the landscape of industrial and regulatory implementation evidence; and evaluating the methodological quality of included studies using a structured risk-of-bias assessment framework.

2. Methods

2.1. Protocol and Registration

This review was conducted in accordance with the PRISMA 2020 (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) guidelines and follows best practices for reproducibility in data-driven reviews. The review protocol was defined a priori, specifying the research question, inclusion and exclusion criteria, data extraction framework, and methodological quality assessment instrument before screening commenced.

2.2. Eligibility Criteria

Studies were eligible for inclusion if they met all of the following criteria: (1) reported at least one quantitative performance metric (accuracy, precision, recall, F1-score, AUC, R^2, RMSE, or equivalent); (2) described the application of one or more AI or machine learning methods to a food authentication, adulteration detection, or fraud-related classification or regression problem; (3) described the instrumental technology used for data acquisition; (4) were published in peer-reviewed journals between January 2021 and April 2026; and (5) were available in English or contained

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sufficient data in tabular or graphical form to enable extraction. Review articles synthesizing primary research were included when they provided meta-level analytical insights directly relevant to the review question.

Studies were excluded if they: addressed food safety problems unrelated to fraud (e.g., pathogen detection unrelated to adulteration), reported only in vitro cell or animal studies, were conference abstracts without sufficient methodological detail, or exclusively addressed post-market surveillance without an AI modelling component.

2.3. Search Strategy and Study Selection

The systematic literature search was conducted across ScienceDirect, PubMed, and MDPI databases using a combination of terms including: 'food fraud detection', 'food adulteration', 'food authentication', 'machine learning food', 'deep learning food fraud', 'NIR spectroscopy adulteration', 'electronic nose food', 'hyperspectral imaging food fraud', 'AI food quality', and 'chemometrics adulteration'. The final corpus comprised 72 eligible studies published between 2021 and 2026, distributed across 18 peer-reviewed journals.

2.4. Data Extraction

Data were extracted independently using a structured seven-domain framework operationalized as a seven-sheet Excel workbook: (Sheet 1) Study Information—authors, year, journal, country, food matrix, fraud type, sample size, and funding; (Sheet 2) Instrumental Technology—sensor type, specific instrument, target compounds, sample preparation, preprocessing, and measurement conditions; (Sheet 3) AI Models—algorithm category, specific model, input/output variables, training strategy, cross-validation, external validation, and software; (Sheet 4) Performance Metrics—accuracy, R^2, RMSE, AUC, F1, sensitivity, specificity, and overfitting discussion; (Sheet 5) Industrial Validation—pilot-scale testing, industrial environment testing, inter-laboratory validation, real supply chain samples, TRL, industrial partners, cost analysis, and scalability; (Sheet 6) Implementation Evidence—routine monitoring application, regulatory integration, adoption barriers, economic constraints, technical expertise required, time-to-result, standard method comparison, and author conclusions; (Sheet 7) Methodological Quality—sample size adequacy, dataset diversity, preprocessing clarity, validation strategy robustness, risk of bias, and overall quality score (1–5), Table 1.

2.5. Methodological Quality Assessment

Methodological quality was assessed using a five-point structured rubric incorporating six domains: sample size adequacy (adequate if $n \geq 150$ or justified by study design), dataset diversity (geographic, seasonal, and source diversity), preprocessing description clarity, validation strategy robustness (external validation given highest weight), risk of bias categorization (Low / Low-Moderate / Moderate / High), and an overall composite score from 1 (critically limited) to 5 (exemplary). Quality scores were assigned by the primary reviewer with independent verification for scores of 4–5 or 1–2, where disagreements were resolved by structured consensus.

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Table 1. Risk of bias classification framework.

RISK OF BIAS CLASSIFICATION FRAMEWORK—METHODOLOGICAL_QUALITY (Table supplementary Sheet 7)			
PART A—Field-level Y/N Criteria			
Field (Sheet 7 column)	**Criterion for Y (assign Y when…)**	**Criterion for N (assign N when…)**	**Notes / Edge cases**
Sample Size Adequate	n ≥ 150 total samples (train + test combined) Intermediate (50–149): Y only if class-balanced AND dimensionality reduction is applied	n < 50 → automatic N n = 50–149 with high-dimensional spectral data and NO dimensionality reduction → N	*Sum train + test splits before applying threshold. State the exact n used.*
Dataset Diversity Adequate	Samples include ≥ 2 of: • Multiple geographic origins • Multiple producers / suppliers • Multiple harvest seasons or lots • Commercially sourced market samples	All samples from a single producer, lot, or geographic origin OR dataset is 100% lab-spiked with no real commercial samples	*Lab-spiked only = N regardless of n. Note in 'Notes' column of Sheet 7.*
Clear Preprocessing Description	Paper explicitly states: • Which preprocessing steps were applied (e.g., SNV, MSC, derivative) • Order of application • Parameters used (window size, polynomial order, etc.)	Preprocessing mentioned only generically (e.g., 'data were preprocessed') OR omitted entirely OR impossible to reproduce from description	*Reproducibility test: could another lab replicate the preprocessing exactly?*
Validation Strategy Robust	Y ONLY if an independent dataset is used—collected at a different time, different instrument, OR different laboratory. Must be explicitly declared in the paper.	Validation relies solely on: • k-fold cross-validation (any k) • Leave-one-out CV • Random train/test split from the same dataset → All of these = N regardless of accuracy reported	*PIVOTAL CRITERION: Its absence prevents Low risk and elevates classification by ≥1 level.*
PART B—Risk of Bias: Decision Matrix			
Risk Level	**Conditions (ALL must be met unless OR stated)**		**Score range**
LOW	Sample Size Y AND Dataset Diversity Y AND Clear Preprocessing Y AND Validation Robust Y → All four fields must be Y. Any single N disqualifies Low risk.		**4–5**
MODERATE	Validation Robust Y AND at least 2 of remaining 3 fields are Y — OR — Validation Robust N BUT all other 3 fields Y AND n ≥ 150 (Robust validation is the pivotal criterion—its absence always elevates risk ≥1 level)		**3**
HIGH	Validation Robust N AND ≥ 2 of remaining fields are N — OR— n < 50 regardless of other fields — OR— Dataset is entirely lab-spiked AND Validation Robust N		**1–2**
PART C—Overall Quality Score (1–5)			
Score	**Description**	**Implied Risk Level**	**Typical profile**
5	Exemplary rigor	**Low**	*External validation + diverse dataset + full preprocessing disclosure*

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4	Good design; minor limitations	**Low–Moderate**	*Internal CV + large n + commercial samples + clear preprocessing*
3	Acceptable with notable gaps	**Moderate**	*Cross-validation only, adequate n, one major gap*
2	Major limitations; limited generalizability	**High**	*Small n, spiked only, no external validation*
1	Critical flaws; results unreliable	**High**	*n < 50, no preprocessing description, no validation strategy*

PART D—Modifier Flags (elevate risk level when present)

Flag	Condition	Effect on classification	Action required
Artificial dataset	Adulterants added in controlled proportions in lab; no market samples included	**Elevate 1 level (e.g., Moderate → High)**	*Note 'artificial dataset' in Sheet 7 Notes column*
Overfitting not discussed	Accuracy > 95% reported with NO mention of regularization, generalization risk, or overfitting	**Elevate 1 level**	*Mark Overfitting_Discussed = N in Sheet 4*
Very small classes	Any class in the classification task has < 10 samples	**Elevate 1 level**	*Record actual class sizes in Sheet 3 Notes*
Industry-funded, no disclosure	Private company funding with commercial interest AND no conflict-of-interest statement	**Flag only—do NOT auto-elevate**	*Note funding source in Sheet 1 Funding column; flag for reviewer attention*

PART E—Decision Flowchart (Step-by-Step Classifier)

Apply the steps below in order for each study. Stop at the first condition that matches.

▼ STEP 1 | CHECK SAMPLE SIZE	
❓ **QUESTION**	Is n < 50?
✅ **YES path**	YES → HIGH risk (Score 1–2). Stop here.
❌ **NO path**	NO → Proceed to Step 2
▼ STEP 2 | CHECK DATASET DIVERSITY	
❓ **QUESTION**	Is the dataset 100% lab-spiked with no real market samples?
✅ **YES path**	YES → Flag 'artificial dataset'. Proceed but apply modifier (+1 level at end).
❌ **NO path**	NO → Proceed to Step 3
▼ STEP 3 | CHECK VALIDATION STRATEGY	
❓ **QUESTION**	Is there a truly independent external dataset (different time / instrument / lab)?
✅ **YES path**	NO → Cannot be LOW risk. Proceed to Step 4 for Moderate vs High.
❌ **NO path**	YES → Proceed to Step 4
▼ STEP 4 | COUNT Y FIELDS	
❓ **QUESTION**	How many of the 4 Y/N fields are Y? (Sample Size, Diversity, Preprocessing, Validation)
✅ **YES path**	All 4 = Y → LOW risk (Score 4–5)
❌ **NO path**	3 Y (incl. Validation) → MODERATE (Score 3) 2 Y or fewer → HIGH (Score 1–2)
▼ STEP 5 | APPLY MODIFIER FLAGS	
❓ **QUESTION**	Are any modifier flags present? (artificial dataset / overfitting undisclosed / tiny classes)
✅ **YES path**	YES → Elevate final risk level by 1 (Low→Moderate, Moderate→High). Cannot exceed High.
❌ **NO path**	NO → Final classification stands
⬇ FINAL OUTPUT → Enter Risk of Bias (Low / Moderate / High) and Quality Score (1–5) in Table supplementary Sheet 7	

Generalization Gap Formalization

To explicitly characterize the discrepancy between laboratory performance and real-world applicability, we define the generalization gap (ΔG) as the difference between model performance under internal validation (P_internal) and performance under external deployment conditions (P_external):

ΔG = P_internal − P_external, where P represents a generic performance metric (e.g., accuracy, AUC, or R^2 depending on the task)

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Although P_external is rarely reported in the literature, indirect evidence from studies employing partial external validation suggests that ΔG can be substantial, particularly in high-dimensional spectral datasets subject to instrument-specific variance and sample heterogeneity.

In the absence of true external validation, ΔG remains unobservable, leading to systematic optimism bias in reported performance metrics. This conceptualization provides a quantitative framework for interpreting the reliability of reported accuracies and highlights the necessity of independent validation datasets to approximate real-world predictive behavior.

3. Results

3.1. Study Selection and General Characteristics

The final corpus comprised 72 studies published between 2021 and 2026, Table 2, drawn from 18 peer-reviewed journals. The temporal distribution showed marked acceleration: 2 studies in 2021, 2 in 2022, 5 in 2023, 17 in 2024, 33 in 2025, and 13 in the first months of 2026, confirming the rapid growth of the field. The leading publication venues were Food Chemistry (29 studies, 40%), followed by Journal of Food Composition and Analysis (10 studies, 14%), Food Control (8 studies, 11%), and LWT – Food Science and Technology (6 studies, 8%). This concentration in established food science journals reflects the interdisciplinary positioning of the field at the intersection of analytical chemistry and computational sciences, Figure 1.

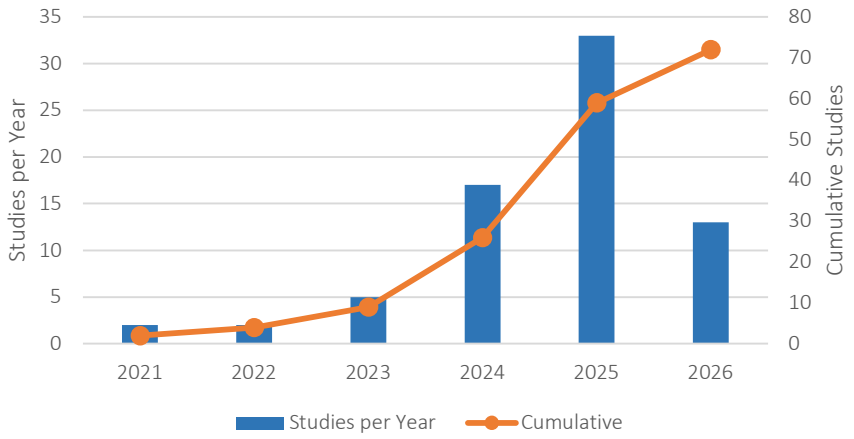

Figure 1. Publications by Year & Cumulative Growth.

Geographically, China dominated the corpus with 37 studies (51%), followed by Iran (6), India (4), Spain (3), Belgium/South Korea (2), South Korea (2), and a diverse set of single-study contributions from Italy, Brazil, Germany, Indonesia, Pakistan, Bangladesh, Taiwan, Saudi Arabia, Finland, the USA, Russia, Australia, and Mexico. This geographic concentration is consistent with the broader landscape of global food science research output but raises important questions about the generalizability of findings, particularly since food fraud patterns, adulterant availability, and regulatory contexts differ substantially across regions, Figure 2.

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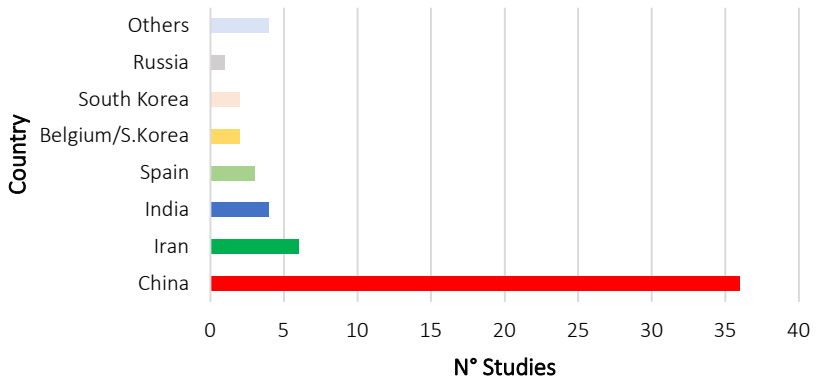

Figure 2. Number of studies per country.

Food matrices were remarkably diverse, encompassing oils and fats (olive oil [10,49]; camellia oil [11,23,24]; sesame oil [8,50]; peanut oil [51]; avocado oil [21]; vegetable oils [36,52]; cactus seed oil [47], dairy products (milk [12,13,15,42,53]; dairy speciation [54]; tallow [55]; powdered milk [38]), beverages and teas (black tea [16,19,20,39]; herbal teas [40]; spirits [56]; wine [57]), meat products (pork [5,58]; beef [6,59]; salted goose [4], spices (turmeric [2,17]; black pepper [35]; cinnamon [27,60]; saffron [61]), and numerous other matrices [28,45,46,48,62].

Table 2. Summary of key indicators.

Indicator	Value	Implication
Total studies included	72	Comprehensive corpus 2021–2026
Studies with NO external validation	75% (54/72)	Critical validation deficit
Studies with inter-laboratory validation	0% (0/72)	No regulatory readiness evidence
Pilot-scale or industrial testing	0% pilot; 1 industrial	Laboratory-only evidence base
Routine monitoring application documented	0% (0/72)	No operational deployment
TRL reported	3% (2/72)	Development stage opaque
Studies NOT discussing overfitting	61% (44/72)	Accuracy claims unqualified
Methodological quality score ≥ 4/5	15% (11/72)	Most studies moderately rigorous
Studies with high risk of bias	25% (18/72)	Significant proportion unreliable
Typical reported accuracy (median)	> 95%	High in-lab performance

3.2. Instrumental Technologies

NIR spectroscopy—including both benchtop and portable instruments—was the dominant analytical platform, featuring in 26 studies (36%). Its prevalence reflects well-established advantages: non-destructive analysis, minimal sample preparation, rapid acquisition (typically 1–5 minutes), compatibility with solid and liquid matrices, and the availability of portable instruments suitable for field deployment [15,17]. NIR instruments ranged from high-end research-grade spectrometers

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(Antaris II FT-NIR; Thermo Scientific) used in studies such as [2] and [25], to low cost portable devices such as the NeoSpectra Scanner employed by [17], which achieved 98% classification accuracy for Metanil Yellow adulteration in turmeric; similarly, the multispectral sensor (based on Vis-NIR LEDs) to discriminate sugar cane vs coconut sugar [28], Figure 3.

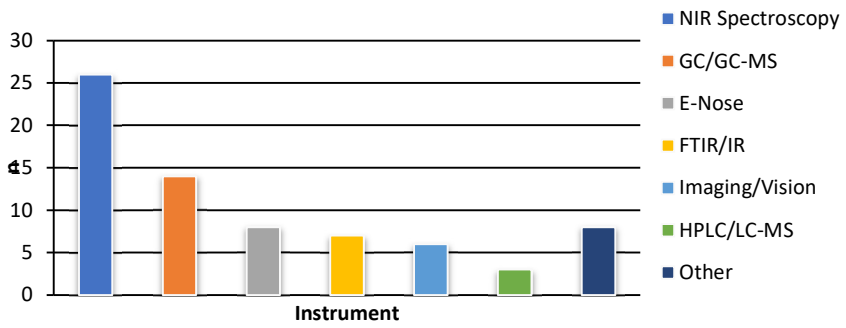

Figure 3. Instrumental Technologies Used.

Gas chromatography and GC-MS-based systems were the second most frequent technology (14 studies, 19%), typically serving as the gold-standard reference method or as the primary analytical platform for volatile compound fingerprinting. Electronic nose arrays, most commonly based on metal oxide semiconductor (MOS) sensor arrays such as the commercial PEN3 system, appeared in 8 studies (11%), often in combination with other sensors in data fusion architectures [4,20,30,31,33,39,46,48,62]. Hyperspectral imaging (HSI) was employed in 6 studies and represents an emerging and methodologically sophisticated approach offering both spectral and spatial discrimination capabilities. Additional technologies included FTIR spectroscopy [44,53,56,57,61,63], NMR [55,61], Raman spectroscopy [44,54], computer vision [18,43,59,64,65], mass spectrometry [7,11], UV-Vis spectroscopy [14,49], and dielectric spectroscopy [50].

A notable methodological trend is the increasing adoption of multi-sensor data fusion strategies, wherein two or more analytical platforms are combined to exploit complementary chemical information. Studies [4,11,20,30,31,33,39–41,65,66], and [44] all employed data fusion architectures, and several demonstrated that fusion consistently outperformed single-sensor approaches. The study by Ren et al. [39] on Yunnan black tea combined NIR, electronic eye, electronic tongue, and electronic nose data in a CNN framework achieving 99.14% classification accuracy on the prediction set—substantially exceeding any single-sensor approach. Similarly, Song et al. [11] demonstrated that mid-level data fusion of LA-REIMS lipidomic fingerprints with GC fatty acid data achieved 99.56% classification accuracy for camellia oil adulteration, outperforming either platform alone.

3.3. AI Models and Algorithms

Classical machine learning dominated the methodological landscape, appearing in 43 of 72 studies (60%). Within this category, Support Vector Machines (SVM) were the most frequently applied algorithm, appearing in 38 studies either as a primary classifier or as part of a comparative analysis. Random Forest appeared in 31 studies, and Artificial Neural Networks (ANN, MLP, BP-ANN) in 28 studies. These algorithms are well-established in chemometrics, benefit from decades of optimization in food science applications, and function effectively with relatively small datasets—a critical practical advantage given the sample size constraints typical of authenticated reference sample collections.

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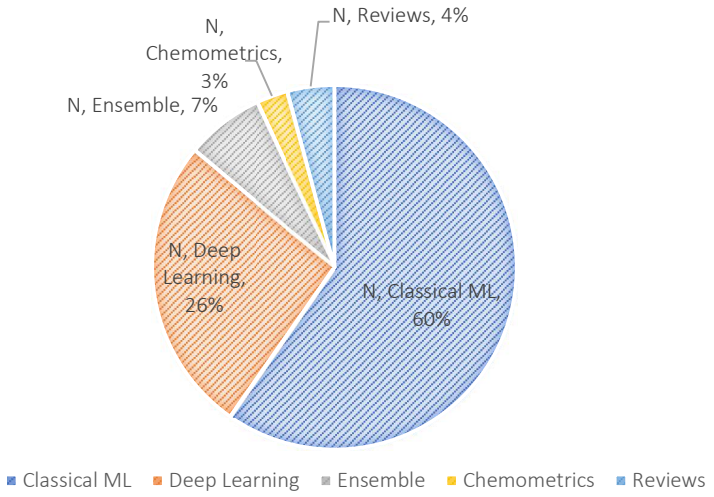

Figure 4. AI Algorithm Types.

Deep learning architectures appeared in 19 studies (26%), encompassing Convolutional Neural Networks [1,6,11,18,29,39,42,43,62], hybrid Transformer-SVM systems [24], ResNet architectures [4,18,42,43,65], and custom novel architectures such as the AKCA-Net proposed by Sun et al. [1] with Adaptive Convolutional Kernel Channel Attention, and the Cross-Channel Sensory Transformer (CCST) developed by Shen et al. [41]. Ensemble methods appeared in 5 studies, including the stacking ensemble of LightGBM, CatBoost, XGBoost, and Ridge regression applied by Noman et al. [14] to honey adulteration detection, which achieved $R^2 = 0.9991$ for adulteration quantification.

Training and validation strategies varied substantially across the corpus. The most common approach was a single random train-test split, typically 70/30 or 80/20, applied in 41 studies. K-fold cross-validation (typically 5-fold or 10-fold) was used in 28 studies, frequently in combination with a fixed test set. Only 15 studies employed any form of external validation using a dataset genuinely independent from the training data—and among these, the rigor varied considerably. The most methodologically rigorous external validation was found in [9] (Malavi et al., NIR-HSI for coffee adulteration), which employed a cultivar-disjoint design ensuring that no Arabica cultivar appeared in both calibration and external test sets, constituting the highest quality validation design in the corpus.

3.4. Performance Metrics: High Precision, Low External Evidence

The performance metrics reported across the 72 studies are remarkable in their consistency: classification accuracies almost universally exceed 90%, and many report values at or approaching 100%. Among the most striking results: Sun et al. [1] reported 98.21% ± 0.71% accuracy for soybean geographic origin classification using a custom CNN with e-nose data; Lu et al. [19] achieved 100% F1-score for green tea quality classification using SVM; Tian et al. [2] reported 100% classification accuracy for turmeric origin identification after SNV preprocessing; Guo et al. [12] achieved 100% classification accuracy for vegetable oil adulteration in raw milk using a Flash GC e-nose with Random Forest; and Firouz et al. [50] reported 100% classification accuracy and $R^2 \approx 1.000$ for sesame oil adulteration using dielectric spectroscopy and ANN.

For regression tasks quantifying adulteration levels, R^2 values in the training set frequently approach unity. The critical question—systematically under addressed in this corpus—is whether these metrics reflect genuine predictive capacity or are artefacts of insufficient validation design. The concurrent analysis of methodological quality data provides a sobering answer: 44 of 72 studies (61%) do not discuss the risk of overfitting, despite reporting accuracies that in many cases approach the

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practical ceiling of the measurement system. Where overfitting is discussed (28 studies), authors typically address it through cross-validation, regularization, or permutation testing—but almost never through independent external validation on truly novel samples from different laboratories or supply chain positions.

Several studies provide instructive examples of the training-to-test performance gap that emerges even within the same study when appropriate validation is applied. Su et al. [51] reported training $R^2 = 0.9153$ for peanut oil adulteration regression but test $R^2 = 0.7254$—a gap of 0.19 that was not discussed by the authors. Zhang et al. [32] explicitly acknowledged differences between training and prediction R^2 as potentially indicative of overfitting in E-nose-based ham grade classification. The LA-REIMS study by Song et al. [11]—one of the methodologically strongest in the corpus—achieved 99.56% classification accuracy on the internal test set but 97.80% on a 500-sample blind LiveID™ real-time validation test, a more modest but still commercially relevant performance level. This study is exceptional in providing genuine real-time external validation.

Performance Inflation and Validation Dependency

A cross-sectional analysis of the dataset reveals a strong dependency between reported performance metrics and validation strategy. Studies relying exclusively on internal validation (random splits or cross-validation) systematically report higher accuracy values compared to those incorporating any form of external validation.

This pattern is consistent with known statistical effects in machine learning, where model evaluation on data drawn from the same distribution as the training set leads to optimistically biased estimates of predictive performance. In high-dimensional spectroscopic data, where feature redundancy and collinearity are prevalent, this effect is further amplified by implicit data leakage through preprocessing pipelines.

The absence of standardized validation protocols across studies prevents direct quantitative meta-analysis; however, the qualitative consistency of this pattern across the corpus strongly supports the hypothesis that a substantial proportion of reported accuracies above 95% are not indicative of real-world predictive capability but rather reflect evaluation under constrained experimental conditions.

This observation suggests that reported performance should be interpreted as an upper-bound estimate rather than a realistic indicator of deployment performance.

3.5. External Validation: A Systemic Absence

The most consequential finding of this review concerns the systematic absence of external validation across the corpus. Seventy-five percent of studies (54/72) conducted no external validation of any kind. Among the 18 studies that did report external validation, the rigor varied enormously: several used held-out concentration ranges from the same laboratory batch [22], supplementary experimental conditions from the same study design [46], or real-time blind tests on samples prepared by the same research team [11]. Only a small number of studies—[6,9,10,12] and [7]—employed what can be genuinely characterized as independent external validation using samples not involved in any aspect of model development.

The absence of inter-laboratory validation was absolute: no study in the corpus conducted validation across multiple analytical laboratories using independently calibrated instruments. This is a critical omission given that food fraud detection must function reliably not just within a single research laboratory but across the heterogeneous instrumental environment of regulatory testing laboratories, industry quality control facilities, and customs inspection points. The spectral variability introduced by different instruments of the same model, different operators, different environmental conditions, and different sample presentation methods is well-documented in the chemometrics literature and routinely leads to substantial performance degradation when models trained in one laboratory are applied in another.

None of the 72 studies included in this review reported interlaboratory validation, regardless of year of publication, funding level, or type of technology used (0/72, 0%). This finding is empirical and

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does not depend on interpretation. Measured against the analytical method validation criteria established by AOAC and ISO 17025, none of the reviewed systems would meet the standard required for accredited routine implementation. The fact that this pattern remains consistent across six years of literature and across studies with documented industry funding suggests that the lack of interlaboratory validation reflects more than a circumstantial resource constraint—although the data in this review do not allow for definitive causality to be established between the academic incentive structure and this observed pattern.

3.6. Industrial Validation and Implementation Evidence

The gap between laboratory detection and real-world prevention is most starkly illustrated by the industrial validation and implementation evidence data. Across all 72 studies: zero studies conducted pilot-scale testing; one study ([67], Kan et al.) tested in a genuine industrial environment (soy sauce fermenters at Guangdong Meiweixian Flavoring Foods Co., Ltd., with samples collected from three 100-tonne industrial fermenters at 9 time points across a 120-day fermentation cycle); zero studies reported inter-laboratory validation; and zero studies documented routine monitoring application in any operational context.

The sole industrial environment study [67] merits detailed attention precisely because it is exceptional. The study used PTR-TOF-MS combined with machine learning to classify fermentation stage in soy sauce production, achieving 100% classification accuracy with QDA and 99.83% with Extra Trees. However, this study is a process monitoring application—not a food fraud detection system—and its inclusion in the corpus reflects the broadened scope through which quality monitoring and adulteration prevention are being conflated in the literature. No study demonstrated deployment of a fraud detection system within an operational food supply chain.

Technology Readiness Levels (TRL) were unreported in 97% of studies. Among the three studies that did address TRL-related information, none explicitly assigned a TRL score but acknowledged engineering challenges associated with industrial deployment. Song et al. [11] stated that LA-REIMS installation in a factory setting is 'challenging yet feasible,' requiring dedicated computing infrastructure, robotic arms, and automated data pipelines. This honest assessment stands in contrast to the uncaveated optimism of many conclusions sections, which routinely describe methods as having 'strong potential' for 'on-site real-time detection' without providing cost analyses, hardware specifications, or validated deployment timelines.

Regulatory integration was mentioned in 15 studies (21%), but in most cases this consisted of referencing existing regulatory frameworks (Codex Alimentarius, EU directives, Chinese national standards) as motivation for the research rather than as evidence of actual integration. The study by Malavi et al. [10] referenced IOC standards and Codex Alimentarius STAN 33-1981 for EVOO purity; Mayorga-Martínez et al. [21] explicitly framed their avocado oil authentication against Codex CXS-210-1999; and Tian et al. [12] achieved the most practically meaningful regulatory connection by validating their model against ISO 17678:2019, confirming one positive adulterated commercial milk sample among 300 real market samples. No study reported that its developed method had been formally adopted, tested, or integrated into a regulatory enforcement workflow.

3.7. Methodological Quality Assessment

The structured methodological quality assessment revealed a corpus predominantly of moderate quality, with a distribution skewed toward the lower end of the quality scale. Forty-two studies (58%) scored 3/5, 19 studies (26%) scored 2/5, and only 11 studies (15%) scored 4/5 or higher. A single study—Malavi et al. 2026 [9], a NIR-HSI study of Robusta coffee adulteration in Arabica with cultivar-disjoint external validation—achieved the maximum score of 5/5.

Studies scoring 2/5 were characterized by combinations of: very small sample sizes ($n < 50$ original samples; [4,31,38,40,41,55,56,67–69]; absence of cross-validation; high-performance metrics reported without mechanistic justification; and single-laboratory, single-batch, or single-origin designs that preclude generalization. Studies scoring 4/5 typically featured: large sample sizes

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exceeding 300 [6,9–11,42]; genuinely independent external test sets; systematic preprocessing comparison; multiple algorithm benchmarking; and explicit acknowledgment of remaining limitations [6,7,9–12,25,30,42,64].

Risk of bias was categorized as Moderate in 41 studies (57%), High in 18 studies (25%), Low or Low-to-Moderate in 10 studies (14%), and Not Applicable (review articles) in 3 studies (4%). High-risk studies were concentrated among those using very small datasets ($n < 50$), synthetic data augmentation without independent validation [4], or applying classification models to the full dataset without holdout splits [8,68]. The concentration of Moderate risk in the majority of studies reflects the structural feature of this field: most researchers apply adequate but not exceptional validation designs, achieving acceptable internal consistency while lacking the external validation that would confirm real-world generalizability.

4. Discussion

4.1. The Detection-Prevention Structural Gap

The central argument emerging from this systematic review is that the food fraud detection literature has achieved something analytically impressive but strategically incomplete: it has demonstrated with high confidence that AI-assisted instrumental systems can classify food samples with exceptional accuracy under controlled conditions, while providing negligible evidence that these systems prevent food fraud events. This distinction—between detection performance and prevention impact—is the structural gap that defines the current state of the field.

Prevention requires more than detection: it requires that detection capacity be embedded in supply chain workflows, accessible to regulatory authorities, economically viable for routine deployment, robust across the full variability of commercial samples, and capable of deterring fraudulent actors who adapt their practices in response to known detection methods. None of these requirements are addressed by the standard laboratory study design that dominates this corpus. A system that achieves 99% accuracy on 200 authenticated laboratory samples from a single origin, tested by a single instrument in a single laboratory, with a holdout set from the same experimental batch, cannot make a credible claim to preventing food fraud in real supply chains.

This structural gap is not unique to the 2021–2026 literature reviewed here—it has been noted by scholars of food safety governance for decades. What is new is the specificity and scale at which AI is now being applied, and the degree to which the optimism of reported performance metrics obscures the distance between proof-of-concept and operational deployment. Review articles within this corpus [70–72] themselves note generalizability challenges, overfitting risks, and the need for collaborative data pooling, though they typically frame these as future research directions rather than as current disqualifying limitations.

This structural gap can be more precisely interpreted through the lenses introduced in this review. The absence of external validation renders the generalization gap (ΔG) effectively unquantified, while the reliance on internal performance metrics reflects an optimization of analytical accuracy rather than decision-theoretic utility. As a result, the literature systematically overestimates the operational value of detection systems by failing to account for deployment-specific risk structures and economic constraints.

4.2. The Validation Deficit and Its Implications

The 75% rate of absent external validation in this corpus is not merely a methodological shortcoming—it is a fundamental epistemological problem. Without external validation, reported accuracy rates cannot be reliably extrapolated to the conditions under which food fraud detection systems would actually need to operate. The literature systematically confounds classification performance on the training distribution with predictive capacity on the deployment distribution—a distinction that becomes critical when the deployment environment includes seasonal variation in

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chemical composition, different geographic origins, different processing methods, different instrument operators, and different instrument calibration states.

The most methodologically sophisticated studies in this corpus—[6,7,9,10,12]—all share the feature of investing substantially in validation design. Malavi et al. [9] achieved cultivar-disjoint external validation ensuring no sample contamination between training and test sets. Jo et al. [6] validated counterfeit beef detection across three anatomical cuts and two countries, providing genuine generalizability evidence. Tian et al. [12] validated their milk adulteration model against 300 real commercial samples from multiple dairy plants, confirming detection of one genuine positive. Venegas et al. [7] tested walnut adulteration detection using 8 commercially purchased samples entirely independent from the 27-sample reference collection. These studies demonstrate that rigorous external validation is achievable—but they also demonstrate that it is the exception rather than the rule.

The near-complete absence of inter-laboratory validation represents a particularly significant gap. Inter-laboratory validation is a prerequisite for regulatory adoption of any analytical method: it demonstrates that the method produces consistent results across the heterogeneous instrumental environment of real enforcement practice. The complete absence of this validation modality from 72 consecutive publications suggests that the research community is not yet oriented toward the methodological standards required for regulatory integration, despite frequently claiming regulatory relevance as a motivation.

4.3. Technology Concentration and Diversity Considerations

The geographic concentration of research in China (51% of studies) creates a particular form of generalizability concern. Chinese regulatory contexts, available food matrices, common adulterant types, and commercial supply chain structures may differ substantially from those in Europe, North America, or other major food trade regions. A model trained on Yunnan black tea grades [39], Jinhua ham quality [32], or Shanxi vinegar fermentation stages [66] addresses authentication challenges specific to Chinese regulatory and commercial frameworks. While the analytical methods developed in these studies may be transferable, the specific models, reference databases, and threshold calibrations are not directly applicable to other regional contexts without substantial revalidation.

Similarly, the dominance of NIR spectroscopy and electronic nose technologies, while understandable given their practical accessibility, means that certain important fraud modalities—particularly species substitution in meat products detectable only through DNA or proteomic methods—are underrepresented. The proteomics study by Venegas et al. [7] stands as the sole mass spectrometry-based proteomics approach in the corpus, providing allergen-relevant walnut adulteration detection at 1% w/w with 100% accuracy on independent commercial samples. This performance profile, combined with the non-targeted analytical approach that allows post-hoc reprocessing for new adulterant species, represents a methodological direction largely absent from the broader literature.

4.4. The Role of Reporting Conventions in Obscuring Limitations

A systematic analysis of the claims made in conclusions sections reveals a recurrent rhetorical pattern: authors describe their methods as ‘promising,’ ‘efficient,’ or ‘feasible’ for ‘real-time on-site detection’ or ‘industrial application,’ while the evidence section documents exclusively laboratory-scale results with internal validation only. This disconnect between the scope of methodological claims and the scope of supporting evidence is not unique to food science but is particularly consequential in a field where the practical objective—preventing food fraud—requires precisely the translation from laboratory to operational context that is systematically lacking.

The 39 studies (54%) that do not discuss overfitting while reporting accuracies at or above 95% on small-to-moderate datasets represent a particularly clear illustration of this problem. A Random Forest model achieving 100% accuracy on 72 samples from three Chinese provinces [2], or a dielectric spectroscopy ANN achieving 100% accuracy and $R^2 = 1.000$ on 135 oil samples from one Iranian

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laboratory [50], cannot credibly be characterized as generalized solutions for food fraud detection without substantially more validation evidence. The reporting conventions of the field—which routinely accept holdout set performance as sufficient evidence of practical utility—are structurally ill-suited to the prevention objective.

4.5. What Would Genuine Translation Look Like?

Bridging the structural gap between detection and prevention would require a fundamental reorientation of research design and reporting conventions. Based on the evidence from this review, several conditions appear necessary: (1) External validation using samples from genuinely independent sources—different laboratories, different instrument operators, different geographic origins, and different harvest seasons. (2) Inter-laboratory validation demonstrating that developed models produce consistent results across different instrumental environments, comparable to the ring-trial standards required for regulatory method adoption. (3) Real supply chain sample inclusion, ensuring that authenticated reference collections include commercially obtained products rather than exclusively laboratory-prepared blends. (4) Cost and scalability analysis, providing realistic assessments of the economic viability of routine deployment, including instrument costs, consumable costs, operator training requirements, and throughput rates. (5) Technology Readiness Level assignment, positioning each study within the development pathway from concept (TRL 1) to operational deployment (TRL 9) and identifying the specific gaps that separate current status from implementation readiness.

The study by Song et al. [11] on LA-REIMS camellia oil authentication comes closest to addressing these requirements, with its 500-sample real-time LiveID™ blind test and explicit discussion of the engineering requirements for factory installation. The authors acknowledge that the system is 'challenging yet feasible' for industrial deployment—an honest qualification that most comparable studies omit. This combination of high analytical performance (99.56% classification accuracy) with genuine real-time external validation (97.80% on 500 blind samples) and explicit deployment pathway discussion represents the kind of translational evidence that the broader literature urgently needs.

Economic and Operational Constraints

In the context of translating detection into prevention, a critical omission in the reviewed literature is the absence of techno-economic analysis evaluating the feasibility of routine deployment. The implementation of AI-assisted detection systems entails not only capital expenditure (instrument acquisition, computational infrastructure) but also operational costs, including calibration maintenance, personnel training, data management, and model updating.

For instance, while portable NIR devices are frequently described as low-cost solutions, their effective deployment at scale requires robust calibration transfer models, standardized sampling protocols, and continuous model validation—all of which introduce hidden costs rarely acknowledged in experimental studies.

Without explicit cost-benefit analysis, claims regarding industrial applicability remain speculative. The integration of economic modeling, including cost-per-sample analysis and return-on-investment metrics, is therefore essential to transition from proof-of-concept to scalable implementation.

5. Limitations of This Review

This review is subject to several limitations. The corpus of 72 studies, while comprehensive for the 2021–2026 period, may not fully capture the grey literature, conference proceedings, or studies published in languages other than English. Publication bias may have caused preferential inclusion of studies reporting positive results, potentially overstating the typical performance of AI detection systems. The methodological quality assessment, while structured and criterion-based, involves

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judgment at several scoring boundaries that a different assessor might resolve differently. Finally, the rapid pace of publication in this field means that several important studies published in early 2026 may not be fully represented in databases at the time of search completion.

6. Conclusions

This systematic review of 72 peer-reviewed studies published between 2021 and 2026 provides a comprehensive and sobering assessment of the current state of AI-assisted food fraud detection research. The empirical findings support three principal conclusions.

First, analytical precision under controlled conditions is consistently high. AI-assisted instrumental systems—particularly those combining NIR spectroscopy, electronic nose arrays, and hyperspectral imaging with machine learning algorithms—routinely achieve classification accuracies exceeding 95% and regression R^2 values above 0.90 in laboratory settings. This represents genuine scientific progress and demonstrates that the underlying analytical chemistry and machine learning infrastructure for food fraud detection is methodologically mature.

Second, external validation is systematically absent. Seventy-five percent of studies conducted no external validation. No study achieved inter-laboratory validation. No study documented routine monitoring application. Technology Readiness Levels were unreported in 97% of studies. The methodological quality of the corpus was predominantly moderate, with over a quarter of studies rated as having high risk of bias due to very small sample sizes, absent cross-validation, or single-laboratory designs.

Third, and most consequentially, there is a structural gap between detection and prevention. The literature has succeeded in demonstrating that AI systems can detect adulteration under controlled conditions but has not demonstrated that they prevent food fraud in real supply chains. This gap is not primarily technological—it is systemic. It reflects the design of research incentive structures that reward novel methods and high-performance metrics over translation, validation rigor, and deployment evidence.

Closing this gap will require a fundamental reorientation of research design priorities: toward inter-laboratory validation, real commercial sample inclusion, cost-benefit analysis, regulatory pathway development, and Technology Readiness Level transparency. Until these conditions are met, the impressive analytical performance documented in this corpus will remain a demonstration of what is technically possible rather than evidence of what is actually preventing food fraud.

Ultimately, the challenge facing the field is not the development of more accurate or complex models, but the redefinition of success criteria: from maximizing classification performance under laboratory conditions to demonstrating measurable impact within real-world food systems. Until this transition occurs, AI-assisted food fraud detection will remain a technologically advanced yet operationally unrealized promise.

Bridging this divide will require not incremental methodological improvements, but a paradigm shift toward validation-driven, system-oriented, and economically grounded research frameworks.

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