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REVIEW ARTICLE

Diagnostic Accuracy of Serial Focused Assessment Sonography in Blunt Abdominal Trauma: A Systematic Review and Meta-Analysis

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Abstract: **Introduction:** A single Focused Assessment with Sonography for Trauma (FAST) examination has limited sensitivity in blunt abdominal trauma (BAT) because free-fluid detection is volume- and time-dependent. This systematic review and meta-analysis aimed to evaluate the pooled diagnostic accuracy of repeated or serial FAST in blunt abdominal trauma. **Methods:** Following the Preferred Reporting Items for Systematic Reviews and Meta-Analyses for diagnostic test accuracy (PRISMA-DTA) and PRISMA 2020 statements, we searched databases for studies reporting the accuracy of serial FAST against a reference standard (abdominal computed tomography (CT) scan, surgery, diagnostic peritoneal aspiration, or clinical follow-up) in BAT patients. Two-by-two data were pooled with bivariate random-effects models (Reitsma; Nyaga generalized linear mixed model (GLMM)); sensitivity and specificity were also pooled separately (DerSimonian–Laird). Risk of bias was assessed using Quality Assessment of Diagnostic Accuracy Studies (QUADAS)-2 and certainty of evidence was evaluated using Grading of Recommendations Assessment, Development and Evaluation (GRADE). **Results:** Eleven studies (>2,800 patients) were included; seven with a complete 2×2 table entered the primary model, two provided sensitivity only, and two were not comparable. Pooled sensitivity was 0.85 (95% confidence interval (CI): 0.77–0.90) and specificity 0.98 (95% CI: 0.92–0.99); positive and negative likelihood ratios were 41.0 (95% CI: 10.7–156.7) and 0.15 (95% CI: 0.10–0.24), and the diagnostic odds ratio 265 (95% CI: 67–1046). Adding the sensitivity-only studies raised sensitivity to 0.87 (95% CI: 0.80–0.93). Specificity was higher in adults than children (0.99 vs 0.90; p<0.001); sensitivity did not differ (p = 0.52). Sensitivity rose with the timing of the repeat scan, from 0.64 at 4 hours to 0.88 at 12 hours. Certainty of evidence was Low. **Conclusions:** Serial FAST has high specificity and moderate, time-dependent sensitivity, with a large positive likelihood ratio. Because the certainty of the evidence is low, these estimates should be interpreted cautiously. A positive result markedly increases the probability of intra-abdominal injury, whereas a negative result does not exclude it and requires structured observation or definitive imaging.

Keywords: Focused Assessment with Sonography for Trauma; Abdominal Injuries; Sensitivity and Specificity; Meta-Analysis.

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1. Introduction

Blunt abdominal trauma (BAT) is a frequent and potentially lethal component of major injury, and the rapid recognition of hemoperitoneum is among the most consequential decisions in early trauma care. In the primary survey, free intraperitoneal fluid in a hemodynamically unstable patient is an indication for immediate hemorrhage control, whereas its apparent absence supports a period of structured observation or definitive cross-sectional imaging (1, 2). Because this judgement is made within minutes of arrival and is revisited repeatedly as the patient's physiology evolves, clinicians need a bedside test that is fast, reproducible, and safe to perform more than once.

The Focused Assessment with Sonography for Trauma (FAST) examination, and its extended form (eFAST), have become embedded in trauma algorithms for exactly these reasons. FAST is rapid, radiation-free, requires no contrast, can be performed during ongoing resuscitation, and can be repeated as often as clinically necessary (1, 2). A positive FAST in an unstable patient is a validated trigger for laparotomy and has largely displaced diagnostic peritoneal lavage in this role (2). Its principal weakness, however, is asymmetric performance: while specificity is high, sensitivity for intraperitoneal injury is limited, so a single negative scan cannot reliably exclude significant abdominal hemorrhage (3, 4).

Three mechanisms explain this limited sensitivity. First, sonographic detection of free fluid is volume-dependent: experimental and clinical work suggests that several hundred milliliters may be required before fluid becomes reliably visible in the dependent peritoneal recesses, so small collections are frequently missed (5). Second, and central to the present question, hemoperitoneum is dynamic, bleeding continues and fluid redistributes over minutes to hours, so an early scan performed before sufficient fluid has pooled may be falsely reassuring (3, 5). Third, operator experience, body habitus, subcutaneous emphysema, and bowel gas all degrade image quality and further reduce sensitivity in real-world practice. Consistent with these mechanisms, systematic reviews of a single FAST or eFAST place pooled sensitivity for intra-abdominal injury in the region of only 0.6–0.7, a level inadequate to rule out injury in isolation (3, 4).

A "serial" or "repeated" FAST is performed after an interval, so that fluid that was previously sub-threshold can be detected on

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a later scan. The physiological rationale is compelling: if free fluid accumulates over time, then a second or later scan should reclassify some initially negative patients as positive, raising sensitivity without materially harming specificity. Individual studies support this expectation across diverse settings, including delayed or secondary scanning in stable adults (6-8), patients with isolated gastrointestinal injury (9), patients with an initially negative admission scan (10), defined-time-point protocols (11), children (12, 13) and hemodynamically unstable adults (14). These reports are nevertheless small, heterogeneous in the timing of the repeat scan and in the reference standard, and have not been synthesized quantitatively for the serial strategy specifically. Existing meta-analyses have addressed the accuracy of a single FAST or eFAST, or the value of ultrasound as a triage tool, rather than the incremental yield of deliberately repeating the examination (3, 4). As a result, clinicians lack a pooled estimate of how well serial FAST performs, how its accuracy depends on the timing of the repeat scan, and whether it behaves differently in children and adults.

Interpreting this literature also demands care, because several superficially similar designs answer different questions and should not be pooled with time-based serial detection. Selective re-scanning performed only in patients who clinically deteriorate (15), secondary ultrasound aimed at detecting solid-organ injury directly rather than free fluid (16), positional (right-roll) repetition, monitoring of an already-diagnosed injury, and follow-up sonography performed after whole-body computed tomography each differ in intent and target condition. Conflating them would bias a synthesis of diagnostic accuracy.

We therefore performed a systematic review and diagnostic test accuracy (DTA) meta-analysis with three objectives: (i) to estimate the pooled diagnostic accuracy of serial FAST for intraperitoneal injury in BAT; (ii) to examine whether accuracy differs between adults and children; and (iii) to explore how accuracy changes with the timing of the repeat scan. Throughout, we distinguished genuine serial-detection studies from the related but distinct designs above, and we made the completeness of each study's data explicit.

2. Methods

2.1. *Study design and setting*

The review was conducted and reported in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses for diagnostic test accuracy (PRISMA-DTA) and the PRISMA 2020 statements (17). A pre-specified PICO protocol defined the population (patients with BAT), index test (a repeated or serial abdominal ultrasound / FAST performed after an initial scan), reference standard (contrast-enhanced computed tomography (CT), operative findings, diagnostic peritoneal aspiration, or structured clinical follow-up), and target condition (clinically significant intraperitoneal injury or free fluid). The primary target condition was clinically important intra-abdominal injury manifest as free intraperitoneal fluid (hemoperitoneum) detectable on FAST.

2.2. *Eligibility*

We included studies of any design that reported, or permitted reconstruction of, the diagnostic accuracy of a repeated/serial abdominal ultrasound against an acceptable reference standard in BAT. There was no age or language restriction. We excluded studies of positional (right-roll) repetition, monitoring of a previously known injury, and follow-up sonography after whole-body CT performed for a different purpose; these were retained only for narrative description. Where a study reported a valid sensitivity but did not permit a valid specificity (for example, when the sonographer was unblinded to CT, or when only injured patients were described), it was flagged as sensitivity-only and excluded from the bivariate model but retained for a pooled-sensitivity analysis.

2.3. *Search and study selection*

Electronic databases (PubMed/MEDLINE, Embase, Scopus, and the Cochrane Library) and the reference lists of relevant reviews were searched from inception to 2025, combining concept blocks for blunt abdominal trauma, FAST/ultrasound, and repetition (serial, repeat, secondary, delayed, sequential, re-FAST).

2.4. *The core concept blocks were combined with AND*

Trauma: ("blunt abdominal trauma" OR "abdominal injury*" OR "blunt trauma")

Index test: (FAST OR "focused assessment with sonography" OR "E-FAST" OR ultrasonograph* OR ultrasound OR sonograph*)

Serial concept: (serial OR repeat* OR "second look" OR secondary OR delayed OR sequential OR "re-FAST" OR reFAST)

No language restriction was applied at screening. Three studies (Lingawi 2000, Poletti 2003, Kendall 2009) were added from a previous systematic-review dataset and citation searching.

2.5. *Study selection and data extraction*

Records were de-duplicated, screened by title and abstract, and then assessed in full text against the eligibility criteria. For each included study we extracted the true-positive (TP), false-positive (FP), false-negative (FN), and true-negative (TN) counts of the serial scan versus the reference standard, together with country, design, sample size, population, the interval to the repeat scan, and the reference standard. Screening, full-text eligibility assessment, data extraction, and Quality Assessment of Diagnostic Accuracy Studies (QUADAS)-2 appraisal were carried out independently by two reviewers, with disagreements resolved by discussion and, where needed, adjudication by a third author against the full text. Where a study's reported accuracy indices and 2×2 counts were internally inconsistent, or differed from a previously published dataset, the counts were re-derived from the reported sensitivity, specificity, and predictive values and cross-checked directly against the source article before being entered into the analysis.

2.6. *Risk of bias*

Methodological quality and applicability were assessed with the QUADAS-2 tool across the patient-selection, index-test, reference-standard, and flow-and-timing domains, using prespecified signaling questions (18).

2.7. *Statistical analysis*

Study-level sensitivity and specificity were calculated from the 2×2 tables with exact (Clopper–Pearson) binomial confidence intervals. The primary synthesis used a bivariate random-effects model that jointly models the logit-transformed sensitivity and specificity and accounts for their correlation across

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studies (19); from this framework we obtained the summary operating point, the positive and negative likelihood ratios (LR+ and LR−), the diagnostic odds ratio (DOR), and the hierarchical summary receiver operating characteristic (HSROC) curve (20). A generalized linear mixed-model implementation of the bivariate model was preferred as the primary estimator because it retains studies with perfect sensitivity or specificity and constrains pooled estimates to the [0,1] interval (21). As a secondary synthesis, sensitivity and specificity were each pooled separately using a random-effects (DerSimonian–Laird) model (22). Between-study heterogeneity was summarized with the I^2 statistic and the estimated between-study correlation. Analyses were performed in Stata 16.1; $p < 0.05$ was considered significant.

The primary model was a bivariate random-effects model. The Nyaga bivariate generalised linear mixed model (Stata command metadta) was the primary estimator because it retains studies with perfect sensitivity or specificity and bounds pooled estimates within [0,1]. The Reitsma bivariate model (metandi) provided the summary operating point, likelihood ratios, diagnostic odds ratio, and HSROC parameters. Sensitivity and specificity were additionally pooled separately with a DerSimonian–Laird random-effects model and exact confidence intervals (metaprop). The embedded-data do-file (serial_fast_meta.do) and the extracted dataset (serial_fast_data.dta / .csv) reproduce all results.

2.8. *Subgroup, sensitivity, and certainty analyses*

Pre-specified analyses comprised an adult-versus-pediatric subgroup comparison; a leave-one-out analysis to assess the influence of individual studies; the addition of studies reporting a valid sensitivity but no calculable specificity to a pooled-sensitivity estimate; and, for a study reporting more than one definition of the repeat scan, a sensitivity analysis using the more conservative definition. Where a study reported accuracy at several time points, sensitivity was examined by the interval to the repeat scan. The certainty of evidence for sensitivity and specificity was rated with the Grading of Recommendations Assessment, Development and Evaluation (GRADE) approach adapted for diagnostic test accuracy, considering risk of bias, inconsistency, indirectness, imprecision, and publication bias (23-25). Trial sequential analysis was not applied, as it is designed for intervention meta-analyses and is not validated for diagnostic test accuracy synthesis.

3. Results

3.1. *Study selection and characteristics*

Eleven studies met the inclusion criteria (Figure 1; Table 1), comprising more than 2,800 patients. Seven were prospective and four were retrospective; nine enrolled adults and two enrolled children. Reference standards were abdominal CT scan in most studies, with operative findings, diagnostic peritoneal aspiration, or structured clinical follow-up in others, and the interval to the repeat scan ranged from 30 minutes to 24 hours. Seven studies provided a complete 2×2 table and formed the primary pooled set (6-8, 11-14). Poletti 2003 (10) and Mohammadi 2012 (9) provided valid sensitivity only; Lingawi 2000 (15) (repeat scan performed only in clinically deteriorating patients) and Kendall 2009 (16) (secondary scan targeting solid-organ injury directly) were not comparable and were described narratively.

3.2. *Data verification*

Re-checking the two counts flagged during extraction confirmed the true-negative count for Blackbourne 2004 as 485, which reproduced the reported sensitivity (72.1%), specificity (99.8%), negative predictive value (96.6%), and accuracy (96.7%). For Kanafi 2014, the interloop-inclusive secondary scan (reported sensitivity 92.7%, specificity 99.3%) corresponded to a 2×2 of TP 38, FP 2, FN 3, and TN 268 from 311 patients (positive predictive value 95.0%, negative predictive value 98.9%); the previously recorded true-negative value was corrected accordingly, and this correction is reflected in all analyses reported below.

3.3. *Risk of bias*

On QUADAS-2 assessment (Figure 2), the reference-standard domain was at low or unclear risk for all studies. The retrospective studies (Quinn 2024, Bahrami-Motlagh 2020, Nti 2024, Mohammadi 2012) raised concerns in the patient-selection and flow-and-timing domains, reflecting selective enrolment and variable intervals to the repeat scan, whereas two prospective studies (Yazıcı 2023, Kakran 2025) were at low risk across all four domains. Poletti 2003 and Mohammadi 2012 were at high risk in the index-test domain because the reader was not blinded to subsequent imaging.

3.4. *Primary diagnostic accuracy*

Across the seven primary studies, the pooled sensitivity of serial FAST was 0.85 (95% confidence interval (CI): 0.77–0.90) and the pooled specificity was 0.98 (95% CI: 0.92–0.99) (Figures 3–4; Tables 2–3). The study-level and pooled sensitivity and specificity are shown as forest plots in Figure 4 (A, sensitivity; B, specificity). The summary positive likelihood ratio was 41.0 (95% CI: 10.7–156.7), the negative likelihood ratio 0.15 (95% CI: 0.10–0.24), and the diagnostic odds ratio 265 (95% CI: 67–1046). The Nyaga bivariate generalized linear mixed model (GLMM) gave concordant estimates (sensitivity 0.85, specificity 0.98). Separate DerSimonian–Laird pooling yielded sensitivity 0.84 (95% CI: 0.76–0.92) and specificity 0.98 (95% CI: 0.97–1.00).

3.5. *Heterogeneity and the summary ROC*

On the SROC plot the studies clustered in the upper-left quadrant, close to the ideal operating point (Figure 3). The estimated between-study correlation between logit-sensitivity and logit-specificity was −0.43, so the HSROC curve was well behaved rather than degenerate. Heterogeneity was moderate to high, with I^2 of 63% for sensitivity and 70% for specificity in the separate-pooling analysis, and lower values (17% and 35%, respectively) under the bivariate GLMM; the 95% prediction region was correspondingly wide, reflecting genuine clinical diversity in the timing of the repeat scan and the reference standard.

3.6. *Secondary and sensitivity analyses*

Adding the two sensitivity-only studies (Poletti 2003, Mohammadi 2012) (9, 10) to a pooled-sensitivity analysis increased sensitivity to 0.87 (95% CI: 0.80–0.93; nine studies; $I^2 = 75\%$) (Figure 5A), consistent with the primary estimate.

A leave-one-out analysis excluding Kanafi 2014 left the estimates essentially unchanged (sensitivity 0.82, 95% CI: 0.75–0.88; specificity 0.97, 95% CI: 0.88–0.99), indicating that no single study drove the result. Substituting the routine (5-space) variant of Kanafi 2014 (7), which ignores the interloop

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space, lowered pooled sensitivity to 0.80 (95% CI: 0.72–0.86) while specificity was unchanged at 0.98 (95% CI: 0.92–0.99), as expected because the two variants differ only in how sensitivity is defined.

3.7. *Subgroup analyses*

Sensitivity did not differ between adults and children (0.85, 95% CI: 0.75–0.95 vs 0.80, 95% CI: 0.70–0.91; test for subgroup difference p = 0.52). Specificity, in contrast, was markedly higher in adults than in children (0.99, 95% CI: 0.99–1.00 vs 0.90, 95% CI: 0.85–0.96; p < 0.001); the adult estimate corresponds to 863/869 true negatives, with six false positives across the adult studies, so specificity was very high but not literally perfect. This difference should be interpreted cautiously given only two pediatric studies.

3.8. *Timing of the repeat scan*

In the one study reporting timed repeat scans (Kakran 2025) (11), sensitivity increased monotonically with the interval to the repeat scan, from 0.64 at 4 hours to 0.76 at 8 hours and 0.88 at 12 hours (Figure 5B). Because these three estimates are derived from the same, overlapping cohort scanned serially rather than from independent studies, they are presented as a within-study, descriptive dose–response relationship and not as a pooled meta-analytic estimate; accordingly, no between-study heterogeneity (I^2) statistic is reported for them. They nonetheless provide direct within-study support for the time-dependent rationale of serial FAST.

3.9. *Certainty of evidence*

Applying GRADE, the overall certainty of evidence was Low for both sensitivity and specificity, downgraded once for risk of bias (retrospective designs and flow-and-timing concerns) and once for inconsistency (I^2 up to 70%); indirectness and imprecision were not judged serious, and publication bias could not be formally assessed with so few studies (Table 4).

4. Discussion

In this diagnostic test accuracy meta-analysis of serial FAST for blunt abdominal trauma, repeating the ultrasound examination achieved a pooled sensitivity of 0.85 and a pooled specificity of 0.98 across seven studies with complete data, with concordant estimates from two independent modelling frameworks. To our knowledge this is the first quantitative synthesis focused specifically on the serial-detection strategy, as distinct from a single FAST or from the various follow-up and monitoring designs with which it is often conflated. The high specificity and large positive likelihood ratio are the most robust findings; sensitivity, while moderately high and clearly time-dependent, remains the limiting property of the test.

The clinical value of a test is captured less by sensitivity and specificity in isolation than by the likelihood ratios, which translate directly into a change in the probability of injury. The summary positive likelihood ratio of approximately 41 is very large: for a patient with a pre-test probability of intraperitoneal injury of 20%, a positive serial FAST raises the post-test probability to roughly 91%, which in the appropriate clinical context can justify proceeding directly to definitive management. The negative likelihood ratio of 0.15, however, is only moderately reassuring; formally, values below 0.1 are usually required before a negative result can be considered to rule out disease. For the same 20% pre-test probability, a negative serial FAST lowers the post-test probability only to about 4%. A negative serial FAST therefore reduces, but does not eliminate, the probability of injury and must be interpreted together with the mechanism of injury, hemodynamic status, and either continued observation or CT (3, 4). This asymmetry — a test far stronger at ruling in than at ruling out — is the central practical message of our analysis.

The monotonic rise in sensitivity with later repeat scanning, from 0.64 at four hours to 0.88 at twelve hours in the single study with timed data (11), is biologically coherent and reinforces the volume- and time-dependent nature of free-fluid detection: as bleeding continues and fluid pools in the dependent recesses, collections that were sub-threshold on an early scan cross the threshold of visibility later (5). The clinical implication is that, when a patient can be observed safely, the yield of a repeat examination increases with the interval, and a deliberately timed second scan may be more informative than an immediate one. This provides an evidence base for the common but previously unquantified practice of "scanning again later" in a patient whose first FAST is negative but whose clinical picture remains concerning, and it suggests that protocols specifying the timing of the repeat scan warrant prospective evaluation.

Specificity was significantly higher in adults than in children (0.99 vs 0.90; p < 0.001). Several factors may contribute: children more often sustain solid-organ injury without appreciable free fluid, a small volume of physiological pelvic fluid can be misread as pathological, body habitus and probe positioning differ, and the pediatric evidence rests on only two small studies (12, 13). This is consistent with trials questioning the incremental value of routine FAST in pediatric blunt torso trauma (26). The subgroup difference should therefore be regarded as hypothesis-generating rather than definitive, and it underlines the need for dedicated pediatric studies before serial-FAST thresholds are applied to children.

Our estimates are consistent with, and extend, the primary literature. Individual studies have repeatedly shown that a second or later scan increases the sensitivity of FAST relative to a single early examination (6-11), and reviews of single or extended FAST report sensitivities well below those we observed for the serial strategy (3, 4). By pooling only genuine serial-detection studies and explicitly separating them from selective re-scanning (15), solid-organ-targeted secondary ultrasound (16), positional repetition, injury monitoring, and post-CT follow-up sonography, we provide an estimate specific to the question clinicians actually face: does repeating the FAST improve the detection of intraperitoneal injury? Serial FAST does not replace CT, which remains the reference standard for stable patients, but it may reduce the number of early negative scans that are falsely reassuring and help triage which patients need earlier definitive imaging.

A strength of this work is its analytic rigor. With few studies and cells containing zero counts, standard bivariate estimation can fail to converge; we therefore used the Nyaga generalized linear mixed model as the primary estimator, which retains studies with perfect sensitivity or specificity and constrains pooled estimates to plausible values, and we transparently classified every study as contributing a complete 2×2, sensitivity-only, or non-comparable data (19, 21). We also re-verified the two most influential counts against the source papers, correcting the true-negative count for Kanafi 2014 (7); this correction improved model behavior (the between-study correlation moved off its boundary) and increased the preci-

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sion of the specificity estimate. These steps prevented the common error of forcing studies without a valid specificity into a bivariate model and made the limitations of the evidence explicit rather than hidden.

In practice, a positive serial FAST is a strong rule-in signal that can accelerate definitive management, whereas a negative serial FAST should never be used alone to exclude injury; it should be integrated with the mechanism of injury, serial clinical assessment, and CT where indicated. For research, adequately powered prospective studies with standardized repeat-scan timing, complete 2×2 reporting, and pre-specified reference standards are needed to refine these estimates, to define the optimal interval to the repeat scan, and to establish performance in children. Cost-effectiveness and implementation studies would help determine where a serial-FAST protocol adds most value within existing trauma pathways.

4.1. *Limitations*

Several limitations temper our conclusions. First, only seven studies provided a complete 2×2 table, so the primary model rests on a modest evidence base. Second, heterogeneity in the timing of the repeat scan, the reference standard, and the definition of a positive result was moderate to high (I^2 up to 70%), reflecting genuine clinical diversity; the pooled operating point should therefore be read alongside the wide prediction region. Third, several studies were retrospective and two provided sensitivity only, raising the possibility of partial verification and spectrum effects. Fourth, the timing analysis rests on a single study, so the dose–response relationship between interval and sensitivity, although biologically plausible, requires confirmation. Fifth, few pediatric studies limit the strength of the subgroup comparison, and publication bias could not be reliably assessed with so few studies. These considerations are reflected in the low certainty of evidence according to GRADE for both outcomes.

5. Conclusions

Serial (repeated) FAST offers high specificity and moderately high, time-dependent sensitivity for intraperitoneal injury in blunt abdominal trauma, with a large positive likelihood ratio. Although the certainty of the evidence is low, a positive serial FAST markedly increases the probability of intra-abdominal injury and may accelerate management, whereas a negative result does not exclude injury and should be combined with structured observation or definitive imaging. Larger prospective studies with standardized repeat-scan timing and complete 2×2 reporting are needed to refine these estimates, particularly in children.

6. Declarations

6.1. *Acknowledgments*

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6.2. *Conflict of interests*

The authors declare no conflict of interest.

6.3. *Funding and supports*

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6.4. *Authors' contributions*

Apirut Hongsuwan, Amarit Tansawet, and Suphakarn Techapongsatorn contributed equally to all aspects of this study, including conceptualization, methodology, literature search and study selection, data extraction, risk-of-bias assessment, formal analysis, interpretation, and writing (original draft, review, and editing). Suphakarn Techapongsatorn is the corresponding author. All authors meet the ICMJE authorship criteria, read and approved the final manuscript, and are accountable for all aspects of the work; they declare that it was written originally with no data fabrication, falsification, or plagiarism.

6.5. *Ethical statement*

This study was reviewed by the Research Ethics Committee, Faculty of Medicine Vajira Hospital, Navamindradhiraj University, which granted a certificate of exemption (COE 036/2026 X; date of acknowledgement: 4 August 2026). As a systematic review and meta-analysis of previously published, aggregate data, it did not involve new human participants, human tissue, or identifiable personal information.

6.6. *Informed consent*

Informed consent was waived because this study analyzed only previously published, aggregate data and did not involve human participants.

6.7. *Availability of supporting data*

The authors guarantee that the data of the study, the extracted 2×2 dataset and the Stata analysis code, are available and will be provided upon reasonable request.

6.8. *Using artificial intelligence chatbots statement*

An AI-based language tool was used solely to assist with English-language editing (grammar and readability). It was not used to generate, analyze, or interpret any data, results, or scientific content. All authors reviewed and verified the final text and take full responsibility for its accuracy and integrity.

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Table 1: Characteristics of included studies

Study (year)	Country	Design	N	Population	Serial index test & timing	Reference
Lingawi (2000)	Canada	Prospective	1090	Adult	Repeat US after negative initial US (deterioration)	CT / surgery
Poletti (2003)*	Switzerland	Prospective	217	Adult	Repeat US ≤6 h if admission US negative	CT / surgery
Blackbourne (2004)	USA	Prospective	547	Adult	Secondary US ≤24 h	CT / operative / observation
Kendall (2009)	USA	Prospective cohort	152	Adult	Secondary US for solid-organ injury	CT
Mohammadi (2012)*	Iran	Retrospective	88	Adult	Repeated US 12–24 h	Intra-operative
Kanafi (2014)	Iran	Prospective	311	Adult	Secondary US ≤12 h (interloop-inclusive)	CECT / laparotomy
Bahrami-Motlagh (2020)	Iran	Retrospective	129	Pediatric	FAST + reFAST	CT
Yazıcı (2023)	Turkey	Prospective	84	Adult	Repeated eFAST 0/3/6 h	CT
Nti (2024)	USA	Retrospective	38	Pediatric	Serial FAST 30 min–24 h	CT / operation / 30-day follow-up
Quinn (2024)	Australia	Retrospective	29	Adult (unstable)	Serial FAST (≥2 scans)	Laparotomy / DPA
Kakran (2025)	India	Prospective	132	Adult	Serial FAST 4/8/12 h	CECT
Studies assessed but not pooled						
Lingawi (2000): repeat ultrasound only in clinically deteriorating patients, selective repeat, not a clean serial-detection 2×2.						
Kendall (2009): secondary ultrasound targeting solid-organ injury directly, different target from free-fluid detection.						
Geyer (2013), Schneck (2017): follow-up sonography after whole-body CT, different clinical question.						
Maurer (2012): cost/impact of follow-up US after CT.						
Cibulas-Shumway (2023): positional (right-roll) repeat scan, not time-based serial.						
Rozycki (2005): surgeon-performed monitoring of a known injury (BOAST).						
Karamercan (2008): retrospective review of diagnostic options, no discrete repeat-US index test — excluded.						

CECT: contrast-enhanced computed tomography; CT: computed tomography; DPA: diagnostic peritoneal aspiration; eFAST: extended Focused Assessment with Sonography for Trauma; FAST: Focused Assessment with Sonography for Trauma; h: hours; N: number of patients; reFAST: repeat FAST; US: ultrasonography. *Sensitivity-only (no full 2×2). Lingawi, Poletti, and Kendall were identified from a previous review dataset/citation searching and were not pooled in the bivariate model.

Table 2: Pooled diagnostic accuracy of serial FAST

Analysis	Studies (n)	Sensitivity	Specificity	I^2 (Se / Sp)
Reitsma bivariate (metandi), cross-check	7	0.85 (0.77–0.90)	0.98 (0.92–0.99)	— / — *
Primary: bivariate GLMM (metadta)	7	0.85 (0.77–0.90)	0.98 (0.92–0.99)	17% / 35%
Separate pooling (metaprop, DL)	7	0.84 (0.76–0.92)	0.98 (0.97–1.00)	63% / 70%
Including sensitivity-only studies#	9	0.87 (0.80–0.93)	—	75% / —
Adults	5	0.85 (0.75–0.95)	0.99 (0.99–1.00)	— / —
Children	2	0.80 (0.70–0.91)	0.90 (0.85–0.96)	— / —
Leave-one-out (excl. Kanafi)	6	0.82 (0.75–0.88)	0.97 (0.88–0.99)	— / —
Kanafi routine (5-space) variant	7	0.80 (0.72–0.86)	0.98 (0.92–0.99)	— / —

Including sensitivity-only studies; * I^2 not estimated; metandi (Reitsma bivariate model) does not report I^2 statistics; CI: confidence interval; DL: DerSimonian–Laird; GLMM: generalized linear mixed model; I^2: I-squared heterogeneity statistic; Se: sensitivity; Sp: specificity. In the separate (DerSimonian–Laird) pooling, studies with 100% sensitivity (n = 2) or 100% specificity (n = 1) are displayed but do not contribute weight. metadta, metandi and metaprop are the Stata commands implementing the bivariate GLMM, the Reitsma bivariate model, and separate random-effects pooling, respectively. Summary likelihood ratios (primary model): LR+: 41.0 (95% CI: 10.7–156.7); LR−: 0.15 (0.10–0.24); DOR: 265 (67–1046). Between-subgroup difference: specificity: $p < 0.001$; sensitivity: $p = 0.52$.

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Table 3: Two-by-two data and per-study diagnostic accuracy of serial FAST

Study (year)	TP	FP	FN	TN	Sensitivity	Specificity	Pooling
Blackbourne (2004)	44	1	17	485	0.72 (0.59–0.83)	1.00 (0.99–1.00)	Primary
Kanafi (2014)	38	2	3	268	0.93 (0.80–0.98)	0.99 (0.97–1.00)	Primary
Yazıcı (2023)	3	0	0	81	1.00 (0.29–1.00)	1.00 (0.96–1.00)	Primary
Quinn (2024)	11	1	0	17	1.00 (0.72–1.00)	0.94 (0.73–1.00)	Primary
Kakran (2025)	104	2	14	12	0.88 (0.81–0.93)	0.86 (0.57–0.98)	Primary
Bahrami-Motlagh (2020)	40	10	9	70	0.82 (0.68–0.91)	0.88 (0.78–0.94)	Primary
Nti (2024)	4	2	2	30	0.67 (0.22–0.96)	0.94 (0.79–0.99)	Primary
Poletti (2003)	80	—	3	—	0.96 (0.90–0.99)	Not calculable	Sens-only
Mohammadi (2012)	29	—	5	—	0.85 (0.69–0.95)	Not calculable	Sens-only
Lingawi (2000)	—	—	—	—	0.94 (reported)	0.98 (reported)	Not pooled
Kendall (2009)	—	—	—	—	Target: solid-organ injury	—	Not pooled

TP true positive; FP false positive; FN false negative; TN true negative. Exact (Clopper–Pearson) confidence intervals. Kanafi 2014 uses the interloop-inclusive secondary scan (Se 92.7%, Sp 99.3%); the routine 5-space variant (Se 65.9%) is analysed as a sensitivity analysis. Poletti and Mohammadi reported a valid sensitivity but no calculable specificity; Lingawi and Kendall were not comparable and were not pooled.

Table 4: GRADE summary of findings (certainty of evidence)

Outcome	N studies (patients)	Pooled estimate	Reasons for downgrading	Certainty
Sensitivity	7 (≈1,270)	0.85 (0.77–0.90)	Risk of bias (−1); inconsistency, I^2=63% (−1)	⊕⊕◯◯ Low
Specificity	7 (≈1,270)	0.98 (0.92–0.99)	Risk of bias (−1); inconsistency, I^2=70% (−1)	⊕⊕◯◯ Low

Pooled estimates are presented with 95% confidence interval. Certainty rated with the Grading of Recommendations Assessment, Development and Evaluation (GRADE) approach for diagnostic test accuracy (starts High; downgraded for risk of bias and inconsistency). Indirectness and imprecision were not serious; publication bias could not be assessed with few studies. The inconsistency downgrade reflects the substantial statistical heterogeneity observed in the separate DerSimonian–Laird pooling (I^2=63% for sensitivity, 70% for specificity), together with clinical diversity in repeat-scan timing and reference standards; the bivariate model's own I^2 was lower (17% and 35%, respectively).

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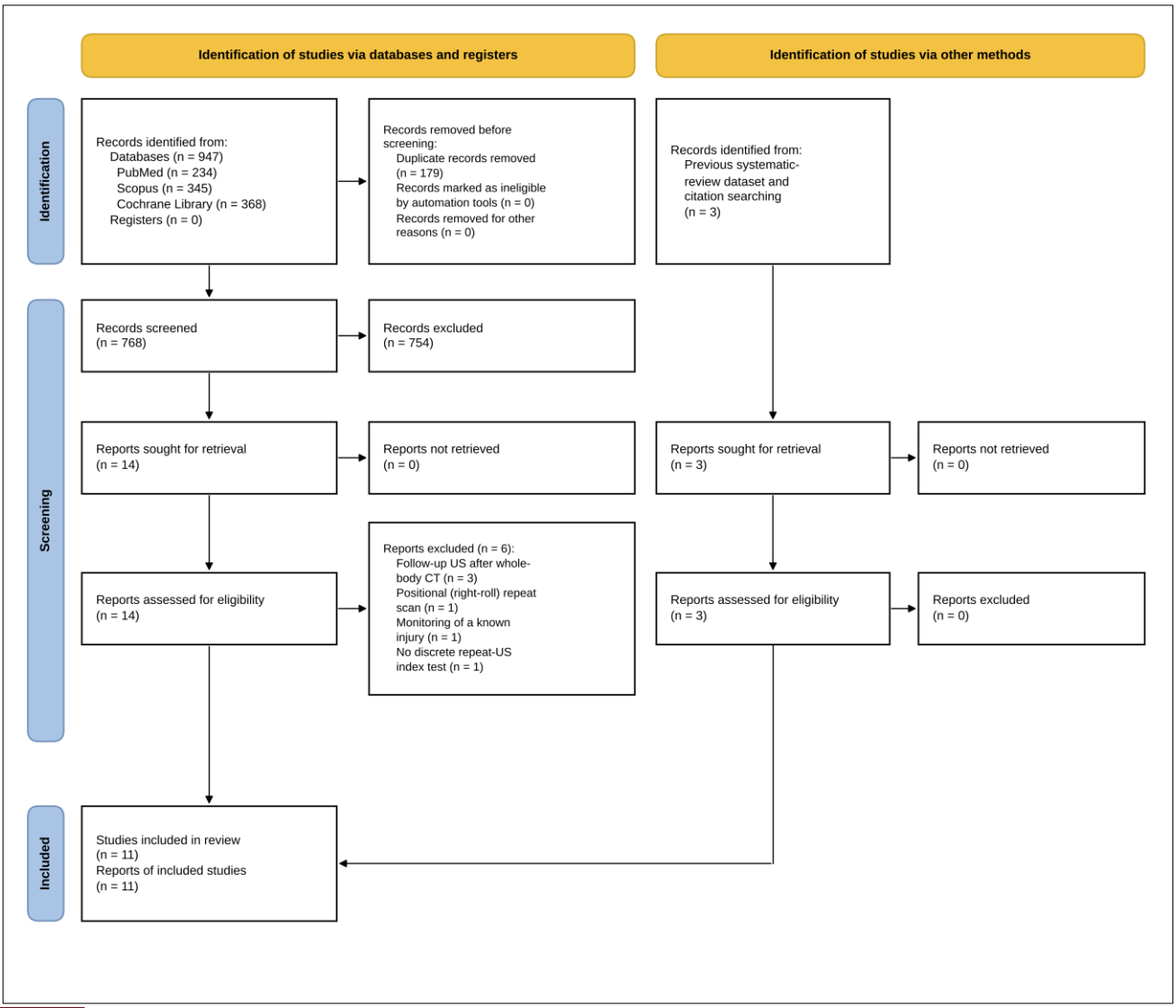


Figure 1: Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 flow diagram of study identification, screening, and inclusion.

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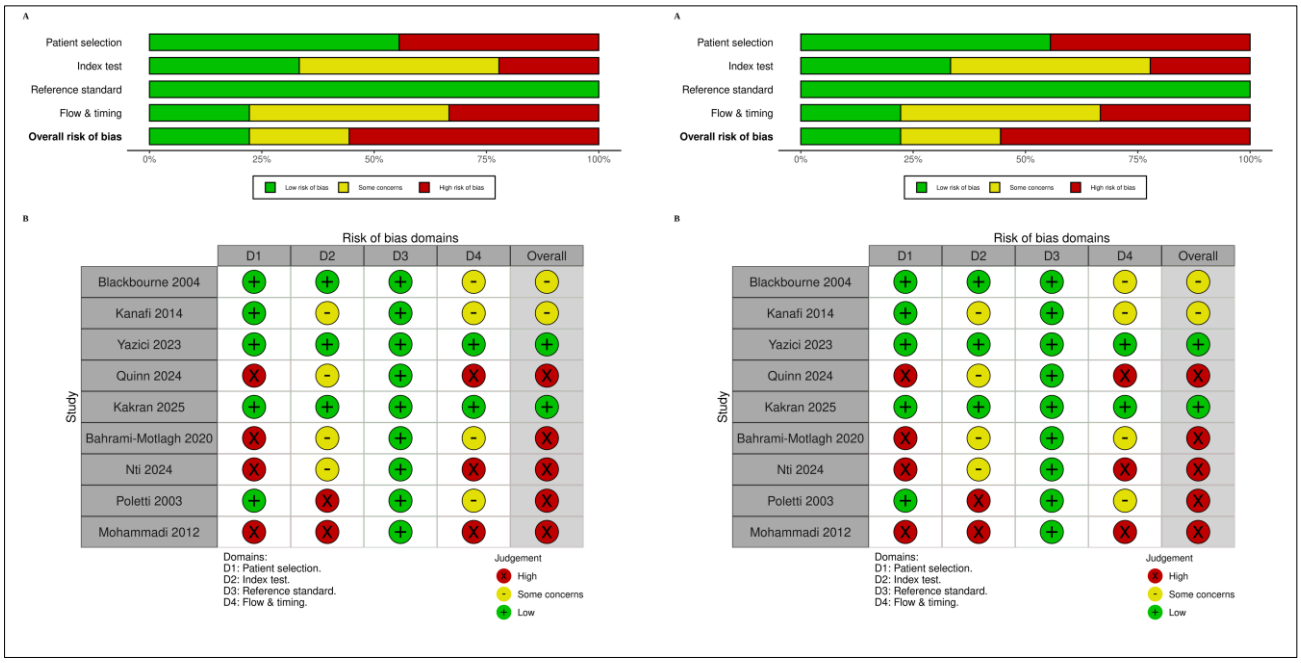


Figure 2: Quality Assessment of Diagnostic Accuracy Studies (QUADAS)-2 risk-of-bias summary (traffic-light plot) across the four domains for the nine studies contributing quantitative data. Each domain was rated as low, high, or unclear risk; "some concerns" in the figure denotes unclear risk. The main concerns were in the flow-and-timing and patient-selection domains, reflecting retrospective designs and variable intervals to the repeat scan.

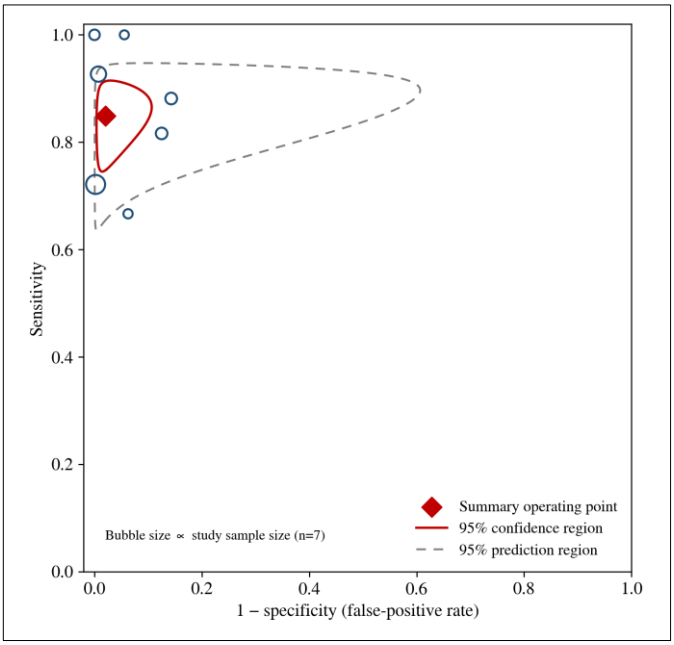


Figure 3: Summary receiver operating characteristic (SROC) plot of serial Focused Assessment with Sonography for Trauma (FAST) in the seven primary studies. Open circles are individual studies (size proportional to sample size); the red diamond is the summary operating point (sensitivity 0.85, specificity 0.98); the solid and dashed contours are the 95% confidence and 95% prediction regions.

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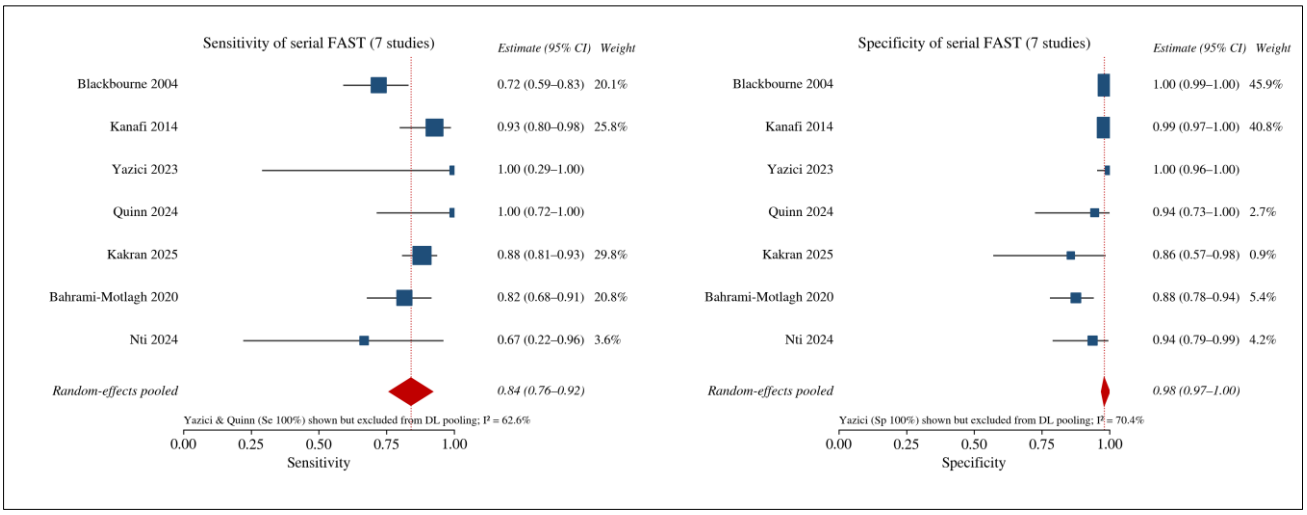


Figure 4: A) Forest plot of the sensitivity of serial Focused Assessment with Sonography for Trauma (FAST) (seven primary studies) with the random-effects pooled estimate (0.84, 95% confidence interval (CI): 0.76–0.92). B) Forest plot of the specificity of serial FAST (seven primary studies) with the random-effects pooled estimate (0.98, 95% CI: 0.97–1.00). Studies with 100% sensitivity (Yazıcı 2023, Quinn 2024) or 100% specificity (Yazıcı 2023) are displayed but did not contribute weight to the DerSimonian–Laird pooled estimates.

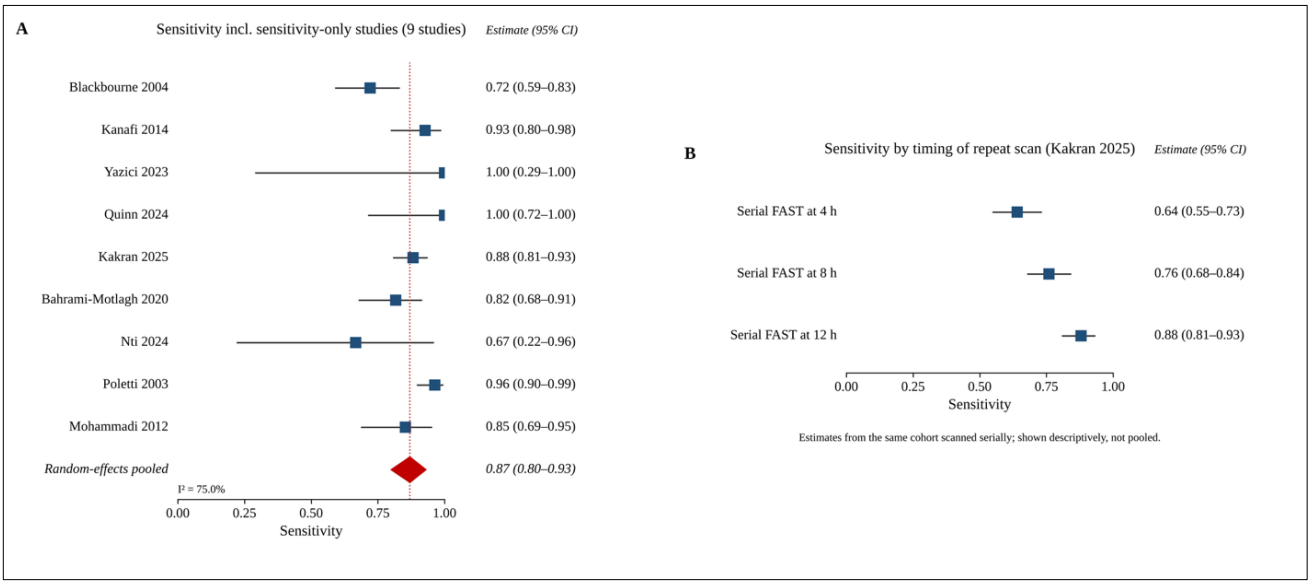


Figure 5: A) Forest plot of pooled sensitivity including the two sensitivity-only studies (nine studies); random-effects pooled sensitivity 0.87 (95% CI 0.80–0.93). B) Sensitivity of serial FAST by timing of the repeat scan in Kakran 2025 (4, 8, and 12 hours); the three estimates come from the same cohort and are shown descriptively, without pooling.

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