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Review

Clinical Applications and Evidence Maturity of Guided Endodontics: A Scoping Review

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Abstract

Background/Objectives: Guided endodontics transfers a virtually planned drilling path into the tooth through printed templates, dynamic navigation and, more recently, robotic and augmented reality systems. This scoping review mapped the reported indications and appraised how mature the evidence is for each one. **Methods:** The review followed PRISMA-ScR. PubMed/MEDLINE was searched from January 2016 to September 2026, with supplementary hand-searching undertaken, and all records were screened in duplicate. Risk of bias was assessed with RoB 2, ROBINS-I, the JBI checklist for case series and criteria adapted from QUIN and PRILE 2021. **Results:** From 341 records and eight studies identified by other means, 234 studies were included across eight indications: six randomised trials, three non-randomised comparative studies, 24 case series, 93 case reports, 11 cadaveric and 97 laboratory studies, so that 108 of the 234 were laboratory or cadaveric and only nine were controlled clinical studies. Controlled clinical evidence exists only for access to obliterated anterior and single-rooted canals and for endodontic microsurgery. In the laboratory, static guides deviated by 1.1 to 2.8 degrees against 9.5 to 21.2 degrees for freehand access; in a randomised trial, static guidance lowered technical failure from 19 to 1.7 per cent. Guided retrieval of separated instruments performed worse than freehand. None of the six trials was at low risk of bias, and none reported how its randomisation sequence was generated. **Conclusions:** Guided endodontics is accurate and conserves tooth structure in laboratory and early clinical work, but the strength of evidence differs sharply between indications. Access to calcified single-rooted canals and guided microsurgery are supported by small trials, none at low risk of bias; the remaining indications rest on laboratory data or case reports, and instrument retrieval is not supported.

Keywords: guided endodontics; dynamic navigation; static navigation; robotic endodontics; pulp canal obliteration; access cavity preparation; endodontic microsurgery; scoping review

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1. Introduction

Pulp canal obliteration, also called calcific metamorphosis, is the progressive apposition of hard tissue along the root canal walls that narrows or radiographically eliminates the pulp space [1]. It follows traumatic dental injury, caries and restorative procedures, vital pulp therapy and revascularisation, and orthodontic loading, and it also accumulates physiologically through lifelong secondary dentine apposition [2,3]. Outside the trauma setting the finding is far from rare. In one cone-beam survey of adult patients, obliteration of the pulp chamber was found in 14.5 per cent of 3078 teeth and correlated with age [4]; a second CBCT archive tied pulp calcification to deep restorations, caries and earlier orthodontic treatment [5]; and new or enlarging denticles have been documented in teeth

after two years of fixed-appliance therapy [6]. Reported frequencies depend on the stimulus: obliteration develops in roughly 4 to 24 per cent of traumatised teeth, mostly anterior, and in 15 to 40 per cent of teeth after luxation injuries specifically [1,2]. Obliteration is in itself a sign of continued pulp vitality and does not mandate intervention. Up to three quarters of affected teeth stay symptom-free and need only radiographic monitoring; root canal treatment becomes indicated in the minority that develop necrosis, symptoms or radiographic signs of apical periodontitis [1].

When intervention is required, these teeth are among the most difficult in clinical endodontics, and the American Association of Endodontists places them in the high-difficulty category of its case difficulty assessment form [7]. Locating a calcified canal demands drilling deeper into a space that cannot be identified visually, which raises the risk of deviation and, downstream, of perforation and of removing so much dentine that the root is weakened [8,9]. Adjunct technology has reduced but not removed the problem. Even with cone-beam computed tomography (CBCT), an operating microscope, ultrasonic tips and long-shank burs, negotiating a severely calcified canal stays technique-sensitive and strongly operator-dependent [10,11]. The conventional protocol is well established and worth stating, because guided endodontics is a response to its specific weaknesses. Preoperative planning relies on a parallel periapical radiograph, often supplemented by a second angulated view or by small-volume CBCT, to estimate the depth and inclination at which the residual canal lumen can be expected. The access cavity is then prepared in stages: the chamber roof is removed; the floor is examined under magnification for the colour change, dentine map and white line that mark the original canal position; and the calcified tissue is removed in small increments with long-shank round burs, Munce or LN burs, or ultrasonic tips, the cavity being dried and re-examined after each increment and the trajectory checked radiographically against the planned path whenever it is in doubt. Dyes, transillumination, sodium hypochlorite bubbling and an electronic apex locator are used to confirm the lumen once it is reached, and the canal is then negotiated with small stainless steel hand files, usually with a chelating lubricant and a watch-winding motion, before being enlarged in the normal way [1,8,10]. Each step of that sequence is a judgement made by the operator at the chairside with the bur already in the tooth, and the published deviation of freehand access in obliterated teeth, 9.5 to 21.2 degrees in the comparative experiments reported in Section 3.3, is the cumulative error of those judgements. When the lumen cannot be found, the alternatives are a surgical approach, with root-end resection and retrograde filling, or extraction, and both carry a worse prognosis for the tooth than orthograde treatment would have [1,9]. Guided endodontics was introduced to move the trajectory decision from the chairside to the planning stage, where it can be made on three-dimensional data and executed mechanically, and the question for this review is for which indications that premise has been tested.

Guided endodontics was introduced to address exactly this. Krastl and co-workers reported the first case in 2016, using a printed template to reach the calcified pulp space of a maxillary central incisor with apical periodontitis [12], and the accuracy of the method was quantified in the same year in an ex vivo series [13]. An expert consensus now distinguishes static guided endodontics, which relies on a printed template built from matched CBCT and surface-scan data, from dynamic guided endodontics, which tracks the handpiece optically against the preoperative plan in real time [14]. The two differ in more than hardware. A static guide is a tooth-supported template, printed in resin from the merged datasets, that carries a metal sleeve or, in sleeveless designs, an open channel or rail, through which a bur of matching diameter is passed along the planned axis; the trajectory is fixed once the guide is printed, the guide must seat fully and reproducibly on the teeth, and it needs interocclusal space for the sleeve and bur assembly, which is why it is easiest to use in anterior teeth

and most constrained in posterior ones. Its advantages are mechanical constraint of the bur, no intraoperative equipment beyond the guide, and a short operator learning curve; its costs are the laboratory step, the delay between scan and treatment, and the impossibility of changing the plan at the chairside. Dynamic navigation uses an infrared or optical stereo camera to track fiducial markers fixed to the patient and to the handpiece, computes their positions by triangulation and displays the bur in real time on the preoperative CBCT; there is no template, the plan can be altered during the procedure, posterior and restricted sites are accessible, and several canals can be planned in one session. Against that, the handpiece is constrained only by the operator following a screen, accuracy depends on registration and on the operator keeping the markers in the camera's line of sight, the equipment cost is higher, and the learning curve is longer: in the only series that has measured it, proficiency took several tens of cases (Section 3.7). Two further modalities have entered the field since: autonomous robotic drilling, which executes the planned trajectory with a robotic arm under the same optical tracking [15,16], and augmented reality overlays that project the plan into the operator's field of view [17,18]. Bibliometric analysis shows publication output on the topic accelerating steeply after 2019 [19].

The clinical footprint has widened well beyond canal location. Guided approaches have been applied to fibre and zirconia post removal [20,21], to access through mineral trioxide aggregate [22], to separated instrument retrieval [23], to developmental anomalies such as dens invaginatus [24,25], to targeted retreatment of the single root carrying periapical pathosis [26], to autotransplantation [27] and to osteotomy and root-end resection in apical microsurgery [28,29]. Whether the supporting evidence has widened with it is a separate question, and the answer differs sharply between these applications.

That gap is what makes the current literature difficult to interpret. Systematic reviews repeatedly conclude that guided endodontics is highly accurate and successful while simultaneously judging the level of evidence to be low [2,30]. The conclusion is defensible as a global statement, but it flattens an evidence base that is in fact stratified. Randomised clinical data now exist for access to obliterated anterior canals [31,32]; for several other indications there is nothing beyond a handful of laboratory experiments, and for at least one of them the laboratory data are unfavourable [33]. Existing reviews have addressed the field in fragments, examining the influence of operator experience [34], guided surgical endodontics in isolation [28], dentine preservation [35] or dynamic navigation alone [36]. None has mapped the full range of indications against the type, volume and methodological maturity of the evidence supporting each one.

A scoping review is the appropriate design for that task, since its stated objectives include charting the breadth of a literature, identifying the types of evidence available, analysing gaps and assessing whether a subsequent systematic review is feasible [37,38]. This review therefore aimed to map the clinical indications for guided endodontics reported in the literature, to summarise the accuracy, tooth-substance, procedural and clinical outcomes reported for each, and to appraise the maturity of the evidence indication by indication, so that applications ready for clinical adoption can be separated from those that still require higher-level investigation.

2. Materials and Methods

2.1. Protocol and Registration

This scoping review was conducted and reported in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses extension for Scoping Reviews (PRISMA-ScR) [39] (completed checklist in Table S1) and with the updated Joanna Briggs Institute methodological guidance [38]. The protocol was not registered prospectively. A scoping design was chosen over a systematic review because the objective was to chart

breadth and characterise evidence type per indication rather than to estimate a pooled effect, and because the anticipated heterogeneity of designs and outcome definitions ruled out meaningful quantitative synthesis.

Two features of the design should be stated explicitly, because they are sometimes taken to belong to a systematic review rather than to a scoping review. The first is critical appraisal. The JBI guidance treats appraisal of included sources as optional in a scoping review and considers it appropriate where the review question concerns the nature or quality of the available evidence [38], and PRISMA-ScR lists critical appraisal of individual sources as an optional reporting item [39]. Since the third objective of this review was to appraise the maturity of the evidence for each indication, appraisal was a necessary part of answering the question, and it was carried out and reported as such. It was not used to exclude studies, to weight the synthesis or to grade a recommendation. The second feature is the evidence-level scale described in Section 2.8, which classifies the type of study available for each indication and not the size or certainty of an effect. The review reports values as charted from individual studies, performs no pooling, no meta-analysis and no GRADE assessment, and draws no conclusion about effectiveness beyond a statement of what type of evidence exists for each indication and how that evidence performed on appraisal. Those are the boundaries that separate it from a systematic review, and they were set before data extraction began. The breadth of the question is deliberate. A narrower question confined to one indication and one outcome would have been answerable by a systematic review, and for two indications that review may now be feasible (Section 4.9), but it would have left unanswered the question this review was designed to settle: how the evidence is distributed across the applications for which the technique is actually being promoted. The clinical message of the review, that a randomised finding about anterior obliterated canals is being used to justify procedures for which the only comparative data are laboratory-based or unfavourable, cannot be reached from a narrow question. Clinical specificity is provided instead by the structure of the synthesis, which is organised indication by indication and states for each one separately what has and has not been shown.

2.2. Review Question and Eligibility Framework

The review question was structured with the Population, Concept and Context framework recommended by the Joanna Briggs Institute. Population: teeth, whether natural, extracted, cadaveric or three-dimensionally printed, and patients undergoing endodontic treatment, retreatment or surgery in which a guided approach was applied, irrespective of tooth type, calcification status or indication. Concept: static guided, dynamic navigation-assisted, robot-assisted or augmented reality-assisted endodontics, covering access cavity preparation, canal location, post and obstruction removal, retreatment, and osteotomy and root-end resection. Context: laboratory, cadaveric and clinical settings in any country or practice environment.

The overarching question was as follows: which clinical indications for guided endodontics have been reported, and what is the nature, volume and methodological maturity of the evidence supporting the outcomes reported for each of them? Three subsidiary questions were specified: for each indication, what accuracy, tooth-substance and procedural outcomes have been reported, and in which settings? for the indications with controlled clinical evidence, what is the quality of that evidence? and which indications have sufficient comparable controlled evidence to justify a subsequent systematic review?

2.3. Search Strategy

PubMed/MEDLINE was searched from 1 January 2016, the year of the first published report of the technique, to 19 September 2026. The search was run on 19 September 2026

without date, species, language or publication-type filters, so that the string alone determines the yield; the full strategy is also given in Table S2. No MeSH heading exists for guided endodontics, dynamic navigation or static navigation in dentistry, and the indexing of these records under broader headings such as Endodontics or Computer-Assisted Surgery is inconsistent, so the search relied on free-text terms restricted to the title and abstract fields: ("guided endodontics"[tiab] OR "guided endodontic"[tiab] OR "guided access cavity"[tiab] OR "endodontic guide"[tiab] OR "endodontic guides"[tiab] OR "dynamic navigation"[tiab] OR "static navigation"[tiab] OR "navigated endodontic"[tiab] OR "guided endodontic microsurgery"[tiab] OR "guided apicoectomy"[tiab]) AND ("root canal"[tiab] OR "pulp canal"[tiab] OR "access cavity"[tiab] OR endodontic*[tiab] OR "apicoectomy"[tiab] OR "root-end"[tiab]).

The search returned 341 records, and the entire retrieved set was carried forward to screening rather than sampled. Reference lists of all included studies and of every review identified on the topic were hand searched, and forward citation chasing was performed on the seminal reports [12,13]. Journals publishing guided endodontic research that are not indexed in MEDLINE were also searched directly by browsing their tables of contents for 2016 to 2026. The complementary searches were completed on 19 September 2026, the same day as the electronic search. These complementary methods identified 8 eligible studies that the electronic search had not retrieved. Restricting the electronic search to a single database is a deliberate trade-off. It was made so that the entire yield could be screened in duplicate and the process reproduced exactly by a reader from the string above, and because the field is small and journal-concentrated: of the 226 studies retrieved electronically, 62 per cent appeared in five MEDLINE-indexed endodontic and dental titles, and every included randomised trial, non-randomised comparative study and case series was indexed in MEDLINE. The complementary searching was designed to recover the engineering, robotics and non-indexed dental literature that MEDLINE under-represents, and it is there that the 8 additional studies were found. The approach is nonetheless less sensitive than a multi-database search, and its consequences for the completeness of the evidence map are considered in Section 4.8.

2.4. Eligibility Criteria

Studies were eligible if they were primary research of any design, that is, in vitro, ex vivo, cadaveric, case report, case series, cohort, controlled clinical trial or randomised controlled trial; evaluated static guided, dynamic, robotic or augmented reality guided endodontics in any indication; reported at least one outcome of interest, namely angular, coronal or apical deviation, tooth-substance or dentine loss, procedural time, canal location or clinical success rate, or complications; and were available in full text in English.

Systematic reviews, meta-analyses, narrative reviews and prior scoping reviews were not charted as data sources, because doing so would double-count the primary studies they contain. They were retained for two other purposes: identifying primary studies through their reference lists, and framing the introduction and discussion, where they are cited as such. At the title and abstract stage, five categories were prespecified: reviews, meta-analyses and bibliometric analyses; records mentioning guided endodontics without reporting any outcome of interest; navigation outside endodontics; editorials, letters and errata; and records not published in English. Five further categories were prespecified for the full-text stage: a sample overlapping a more complete report, in which case the more complete report was retained; outcomes lying outside the scope defined above; guidance used only as an incidental adjunct in a case whose primary subject was something else; a non-English full text; and a full text that could not be obtained. Counts for all ten categories, including zeros, are given in the flow diagram in Section 3.1.

2.5. Study Selection

Records were imported into Mendeley Reference Manager (version 2; Elsevier, Amsterdam, The Netherlands) and checked for duplicates by digital object identifier, title and first author; the single-database search returned no duplicates, and the 8 records identified by complementary searching were checked against the electronic yield in the same way before being added. Before independent screening the two reviewers calibrated on a random sample of 40 records, screened them jointly against the eligibility criteria and discussed every point of difference until the criteria were being interpreted in the same way; the same exercise was repeated on 10 full texts before the full-text stage. Two reviewers then independently screened all 341 titles and abstracts, the calibration sample included, and assessed the full record of every potentially eligible study. Disagreement at either stage was resolved by discussion, with a third reviewer available where consensus could not be reached; in the event, every disagreement was settled by discussion and the third reviewer was not required. Exclusions at the title and abstract stage were assigned to one of five prespecified categories and exclusions at the full-text stage to one of five further categories; both sets are reported with counts in the flow diagram in Section 3.1.

2.6. Data Charting

Two reviewers charted data independently using a standardised form piloted on ten randomly selected studies; the pilot led to the addition of fields for the degree and method of simulated obliteration and for the number of additional radiographs, and to the merging of two indication categories. Charted entries were compared item by item. Discrepancies arose most often in three fields—the classification of operator experience, the identification of the comparator where a study had several arms, and the assignment of studies spanning two indications—and were resolved in every case by returning to the source paper together and agreeing the entry; the senior author (Z.K.) was designated to arbitrate where consensus could not be reached, and was not required. Charted items were author and year; country; study design; sample type and size; tooth type; clinical indication; guidance modality and system; comparator; operator experience; outcome measures reported; follow-up duration where applicable; and principal findings. Indications were assigned to eight categories defined a priori and refined during the pilot charting of ten studies, where two provisional categories were merged and the boundary between retreatment and post removal was fixed; the categories were frozen before full charting began and were not altered afterwards: access to obliterated canals in anterior and single-rooted teeth; access in posterior and multirooted teeth; intraradicular post removal; retreatment, including access through obturation and filling materials such as mineral trioxide aggregate and hydraulic calcium silicate cements; separated instrument retrieval; developmental anomalies; autotransplantation and intentional replantation; and endodontic microsurgery.

2.7. Critical Appraisal

Because the anticipated designs were heterogeneous, no single appraisal instrument could be applied across the whole dataset. Laboratory studies were appraised against six criteria adapted from the PRILE 2021 guideline for reporting laboratory studies in endodontology [40] and the QUIN tool for in vitro dental studies [41]. Neither instrument could be applied unchanged: QUIN scores twelve items, several of which (for example, the detail of the specimen preparation protocol) do not discriminate between guided endodontic studies, and PRILE is a reporting guideline rather than an appraisal tool. The six domains retained were those that bear directly on the validity of an accuracy comparison and were reported variably across the pilot sample: a priori sample size justification, randomised allocation of specimens or operators, a defined comparison group, reporting of operator

number and experience, blinding of the outcome assessor, and appropriateness of the statistical analysis. Each was scored as met, not met or not reported; clinical case series were appraised with the Joanna Briggs Institute checklist for case series, a case series being defined for this review as three or more cases reported together; randomised trials were appraised with the Cochrane RoB 2 tool [42] and non-randomised comparative clinical studies with ROBINS-I [43]; a summary of the appraisal across the design strata is given in Table S3. The rationale for including appraisal in a scoping review is given in Section 2.1. Before appraisal, the two reviewers piloted each instrument on two studies of the corresponding design, compared their domain judgements and agreed written decision rules for the domains that proved contentious: in RoB 2, allocation described only as random was rated as some concerns rather than high risk unless there was additional evidence of baseline imbalance, and in the laboratory criteria an unreported domain was scored as not reported rather than as not met. Each reviewer then rated every domain of every study independently using the categories of the relevant instrument. Initial disagreement was confined almost entirely to the boundary between some concerns and high risk in RoB 2 and between moderate and serious in ROBINS-I, and was resolved by discussion with reference to the written rules; no study required a third reviewer. Individual appraisal with per-study documentation by both reviewers was applied to the 6 trials, the 3 non-randomised comparative studies, the 24 clinical case series and the 56 laboratory and cadaveric studies that contribute charted outcome data to the outcome tables in Section 3, amounting to 89 studies overall. The remaining 52 laboratory and cadaveric studies, which report a single guided arm without comparator or which replicate a tabulated comparison, were scored on the same six domains by one reviewer and checked by the second, and they enter the summary appraisal of laboratory and cadaveric studies in Section 3.8 only in the summary row across the stratum; the 93 single-case clinical reports were charted for the indication map but not appraised individually, since no instrument discriminates usefully between them. Consistent with scoping review methodology, appraisal was used to characterise the evidence base and was not a ground for exclusion, nor was it used to weight any synthesis.

2.8. Synthesis

Data were synthesised narratively and descriptively and organised primarily by indication rather than by outcome, because the central question concerned the distribution of evidence across indications. Accuracy, tooth-substance and procedural data were tabulated separately for the non-surgical and surgical indications. Evidence maturity per indication was classified on a four-level scale defined a priori: level A, at least one randomised controlled clinical trial; level B, controlled or prospective clinical studies without randomisation; level C, laboratory or cadaveric comparative studies only; and level D, case reports and case series only. Because the presence of a trial says nothing about its quality, level A was subdivided after appraisal: A1, at least one randomised trial judged at low risk of bias and A2, randomised trials available but all judged at some concerns or at high risk. Each indication was assigned the highest level for which evidence existed, so an indication at level A may also hold non-randomised clinical and laboratory evidence, which is counted in the evidence map in Section 3.1 but does not change the level; a level is not a stage that every indication passes through, and an indication reaches level B only if non-randomised comparative clinical studies are the best available. The scale therefore describes the type of evidence available and, at its top level, the quality of that evidence; it is not a validated instrument and is not a measure of effect size or certainty. Outcomes were charted in five tiers that were kept separate throughout the synthesis: technical accuracy (angular and linear deviation); procedural performance (canal location, tooth-substance loss, time, intraoperative complications); clinical success (healing, resolution of symptoms); patient-

reported outcomes; and long-term tooth survival. A finding in one tier was not taken as evidence in another. The outcome tables in Section 3 list representative rather than all quantitative studies. A study was listed if it met any of three criteria set before charting: it was a clinical study of any design that reported a quantitative outcome; it was a laboratory or cadaveric study with a freehand or alternative-modality comparator; or it was the first report of a modality, imaging approach or guide design. Laboratory studies reporting a single guided arm without comparator, and studies replicating a comparison already listed with a concordant result, were charted and are summarised in the text and in the counts, but were not tabulated. Indication-specific meta-analysis was considered for the two indications with randomised data and rejected: the two trials in anterior access report different primary outcomes (deviation in one, canal location and substance loss on radiographs in the other), and of the four microsurgery trials only two share a conventional comparator and those two report different healing endpoints at different times. No meta-analysis was performed.

3. Results

3.1. Selection and Characteristics of the Evidence Base

The search retrieved 341 records. At the title and abstract stage 112 were excluded: 63 reviews, meta-analyses or bibliometric analyses, 26 records in which guided endodontics was mentioned without any outcome of interest, 11 records on navigation outside endodontics, eight editorials, letters or errata, and four records not published in English. Hand searching of reference lists and complementary searching of journals not indexed in MEDLINE identified a further eight eligible studies, three of them in a single engineering-indexed title. The 229 records retained after title and abstract screening (341 minus 112) and the eight studies from complementary searching, 237 in all, were assessed in full text against the five prespecified exclusion categories of Section 2.4, and three were excluded: one for reporting a sample already covered by a more complete publication from the same group, and two for reporting only outcomes outside the scope of the review (guide fit assessed without any endodontic procedure being performed). None was excluded for incidental use of guidance, for language at this stage, or for an unobtainable full text; the counts for all five categories, including the three zeros, are shown in Figure 1. The remaining 234 studies (237 minus 3) were included (Figure 1). Of these, 104, comprising 92 single-case reports and 12 small series, establish that an indication has been attempted but supply no measurable outcome, and they contribute to the indication map (Table 1) and to the complications narrative only; the remaining 130 report at least one quantitative outcome and are the source of the values in Tables 2–5, which present representative studies selected by the criteria in Section 2.8 rather than all 130.

How that evidence is distributed across indications is the principal finding of this review, and the distribution is extremely uneven (Table 1). Controlled clinical evidence exists for exactly two of the eight indications. Access to obliterated canals in anterior and single-rooted teeth accounts for 94 of the 234 studies and carries two randomised trials [31,32] and three non-randomised comparative clinical studies [44–46]. Endodontic microsurgery accounts for 59 and carries four randomised trials [47–50], eight clinical series and, unusually for this literature, nine of the 11 cadaveric studies. The other six indications, 81 studies between them, contain no controlled clinical study of any design. Access in posterior and multirooted teeth, with 32 studies, and intraradicular post removal, with 23 studies, rest on laboratory work with a scattering of case reports; retreatment with seven studies, separated instrument retrieval with seven and autotransplantation with four rest on a handful of laboratory studies each; and developmental anomalies, with eight studies, have no laboratory or cadaveric evidence at all, only one small series and seven case reports. The clinical component of the anterior access literature is dominated by small uncontrolled

series with follow-up, where it is reported at all, rarely exceeding twelve months [51–59], and the posterior component of individual case reports in molars and in teeth complicated by furcation perforation [60–62].

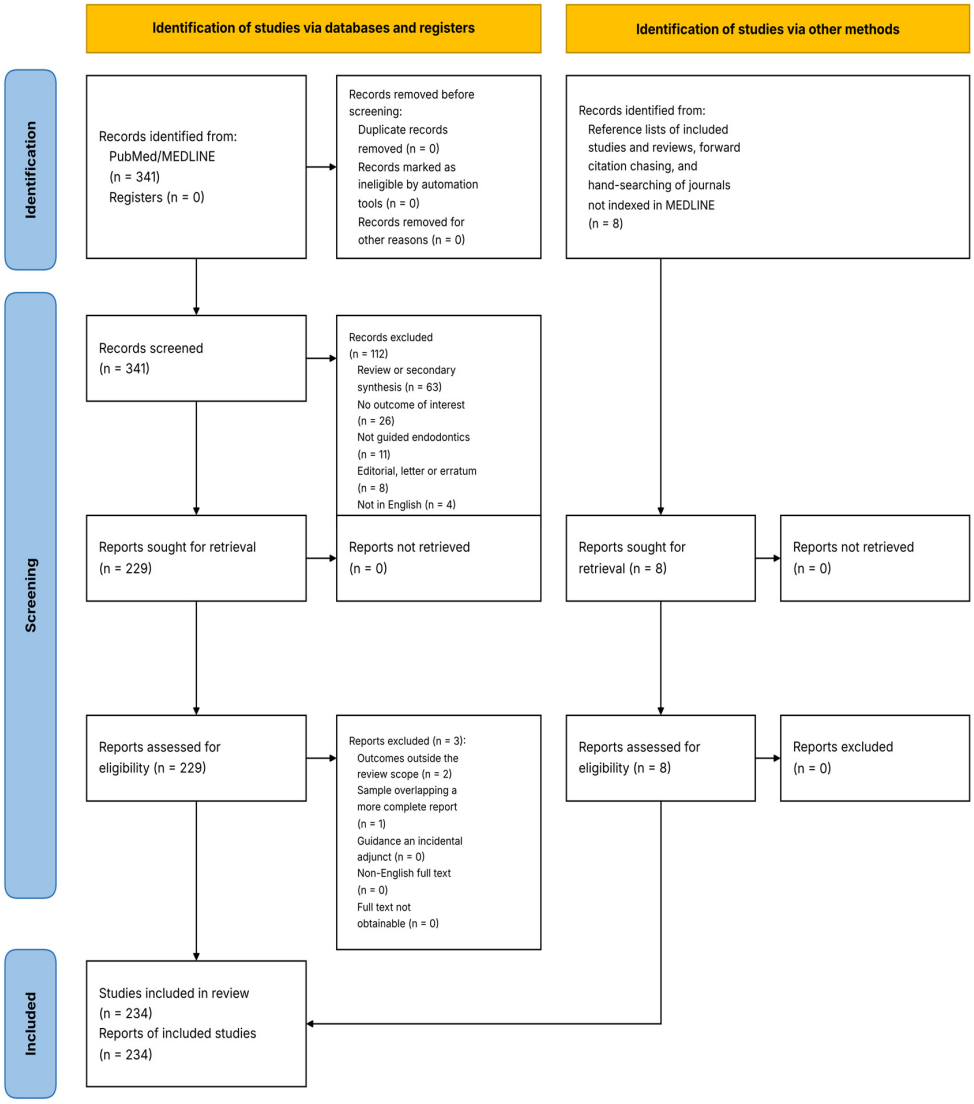


Figure 1. PRISMA-ScR flow diagram of the study selection process. The electronic search of PubMed/MEDLINE returned 341 records covering 1 January 2016 to 19 September 2026; the search string is given in full in Section 2.3 and the yield is reproducible from it. Hand-searching of reference lists and complementary searching of journals not indexed in MEDLINE identified 8 further eligible studies, 3 of them in a single engineering-indexed title. All 341 retrieved records were screened in duplicate by title and abstract, with 112 excluded in five prespecified categories: review or secondary synthesis, 63; no outcome of interest reported, 26; not guided endodontics, 11; editorial, letter or erratum, 8; not published in English, 4. The full record of the remaining 229, together with the 8 studies from other sources, was assessed against the eligibility criteria, and 3 were excluded in two of the five prespecified full-text categories, the other three categories returning zero. Of the 234 included studies, 104 are narrative clinical reports contributing to the indication map in Table 1 and to the complications narrative only, while 130 report at least one quantitative outcome and are the source of the values in Tables 2–5, which present representative studies rather than all 130.

Within the 130 studies that contribute quantitative data, output was sparse between 2016 and 2018, with five studies across those three years, expanded steadily from 2019, and accelerated sharply thereafter: 21 in 2023, 22 in 2024, 27 in 2025 and 17 in the first nine months of 2026. Composition shifted with volume. All six randomised trials appeared

in 2025 or 2026, and four of them concern microsurgery rather than access, which is the opposite of what the earlier literature would have led one to expect. Publication is also concentrated: within the 124 of these 130 studies retrieved by the systematic screen, the Journal of Endodontics carries 43, the International Endodontic Journal 11, BMC Oral Health 10, the Journal of Dentistry seven and Clinical Oral Investigations six, so five titles account for 62 per cent of that subset. That figure is partly an artefact of the search, since a MEDLINE-only strategy will over-represent MEDLINE-indexed journals, and it should be read as describing the retrieved literature rather than the field. A comparable concentration applies to research groups, principally in Basel and Würzburg, Leuven, Maryland, Wuhan and Guangzhou, which bears on the independence of the reported effects and is taken up in the Discussion.

Table 1. Evidence map of guided endodontics by clinical indication (n = 234 included studies: 226 from the PubMed/MEDLINE screen and 8 from complementary searching). Cells give the number of studies of each design. Controlled clinical evidence: RCT, at least one randomised controlled clinical trial; NRS, non-randomised comparative clinical study only; None, no controlled clinical study of any kind. Evidence level: A1, randomised controlled clinical trial at low risk of bias available; A2, randomised controlled clinical trials available but none at low risk of bias; B, non-randomised comparative clinical study available; C, comparative laboratory or cadaveric evidence only; D, uncontrolled clinical series and case reports only. The level is the highest for which evidence exists: anterior access holds 3 non-randomised comparative studies as well as 2 trials and is therefore at level A2, not B. No indication in this review falls at level A1 or level B. Abbreviations: RCT, randomised controlled clinical trial; NRS, non-randomised comparative clinical study; Series, prospective or retrospective clinical case series; Reports, clinical case reports; Cad, cadaveric; Lab, in vitro or ex vivo; MTA, mineral trioxide aggregate.

Indication	RCT	NRS	Series	Reports	Cad	Lab	Total	Controlled Clinical Evidence	Level
Access to obliterated canals, anterior and single-rooted teeth	2	3	11	44	1	33	94	RCT	A2
Access in posterior and multirooted teeth	0	0	1	8	0	23	32	None	C
Intraradicular post removal	0	0	1	7	1	14	23	None	C
Retreatment, including access through MTA and filling materials	0	0	1	3	0	3	7	None	C
Separated instrument retrieval	0	0	1	4	0	2	7	None	C
Developmental anomalies (dens invaginatus and related)	0	0	1	7	0	0	8	None	D
Autotransplantation and intentional replantation	0	0	0	3	0	1	4	None	C
Endodontic microsurgery (osteotomy and root-end resection)	4	0	8	17	9	21	59	RCT	A2
Total	6	3	24	93	11	97	234		

3.2. Technical and Procedural Parameters

The technical workflow reported across studies was broadly consistent, with variation concentrated in a few parameters (Table 2). CBCT acquisition relied mainly on small-volume high-resolution scanners, among them the Morita Accuitomo 80 and 170, NewTom VGi and VGi evo, Planmeca ProMax and Romexis, Galileos Comfort Plus, KaVo OP300 Maxio and Hyperion X, with voxel sizes usually between 75 and 250 µm. That variation is not cosmetic. A dedicated in vitro evaluation showed that reducing CBCT resolution from 80 to 120 µm increased median angular deviation from 2.00 to 3.10 degrees and total linear deviation from 1.06 to 1.41 mm, and that printing resolution independently affected vertical linear deviation [63]. coDiagnostiX was the most frequently reported planning software, followed by Blue Sky Plan, SICAT Endo, 3Shape Implant Studio, RealGuide and Exoplan; a

comparison of two planning workflows found SICAT Endo more accurate at the bur tip, 0.68 against 1.57 degrees, but roughly twice as slow to plan and three times as demanding in operator input [64].

Drill diameters clustered between 0.75 and 1.5 mm, most often in a two-bur sequence, and simplified single-visit workflows intended to reduce laboratory dependence have been described and tested [65–67]. Guide design parameters have been the subject of recent, more granular work. Sleeve height mattered more than sleeve material: increasing height from 3 to 7 mm progressively improved directional control, whereas the effect of sleeve material was inconsistent across heights [68]. A separate factorial analysis found that resin templates without a metal sleeve produced greater angular and apical deviation than templates incorporating one, that apical deviation increased with drilling distance, and that drill diameter, rotational speed and specimen type had no measurable effect [69]. Fixation pins and metal sleeves altered accuracy by roughly 1.3 degrees depending on configuration [70]. Guide fit is a further error source, quantified in two bench studies that fall outside the charted set because no endodontic procedure was performed: an intraoral scanner detected fit discrepancies corresponding to a mean angular error of 5.91 degrees, and expert visual scoring performed no better than instrumented assessment at detecting them [71,72].

Canal location with guided access was high but not invariably superior to freehand. In a typodont comparison, guided access located 28 of 30 canals against 30 of 30 with conventional preparation, with two perforations in the guided group [8], and an MRI-based proof-of-principle study located 91 of 100 canals with two perforations [73]. A dynamic navigation laboratory study missed 12 of 168 canals, seven of which were perforated [74]. Complications recorded across the charted set were dominated by perforation and by failure to negotiate; their true frequency cannot be estimated from this literature, for reasons set out in Section 3.8.

Table 2. Technical and procedural determinants of guided endodontic accuracy. Only studies that varied a technical parameter as an experimental factor are listed; descriptive reporting of equipment used is summarised in the text. Abbreviations: CBCT, cone-beam computed tomography; CoCr, cobalt–chromium; IOS, intraoral scanner; RPM, revolutions per minute; deg, degrees.

Study	Parameter Varied	Comparison	Effect on Accuracy	Practical Implication
[63]	CBCT voxel size; 3D print resolution; drilling depth	80 µm vs. 120 µm voxel; standard vs. low print resolution	Angular deviation 2.00 deg (0.78–2.60) vs. 3.10 deg (2.30–5.00); total linear 1.06 mm vs. 1.41 mm; print resolution affected vertical linear deviation only	High-resolution acquisition is a prerequisite, not an optional refinement
[64]	Planning software and template manufacture	coDiagnostiX with printed template vs. SICAT Endo with subtractive template	Bur-tip angular deviation 1.57 ± 0.76 deg vs. 0.68 ± 0.41 deg; 3D vector 0.74 mm vs. 0.35 mm; canal located 15/16 vs. 16/16	Higher accuracy costs planning time: 10 min 50 s and 107 clicks vs. 20 min 28 s and 341 clicks
[69]	Template material, sleeve presence, sleeve height, RPM, drill diameter, drilling distance	Resin without sleeve vs. resin with metal sleeve vs. full metal template; sleeve 4, 6, 8 mm	Resin without sleeve had highest angular and apical deviation; 6 and 8 mm sleeves outperformed 4 mm; apical deviation increased with drilling distance; drill diameter, RPM and specimen type had no effect	Resin templates without a metal sleeve should not be used; sleeve height of at least 6 mm is advisable

Table 2. *Cont.*

Study	Parameter Varied	Comparison	Effect on Accuracy	Practical Implication
[68]	Sleeve material and height	Titanium, CoCr and zirconia at 3, 5 and 7 mm vs. freehand	All guided groups outperformed freehand ($p < 0.001$); 7 mm sleeves gave lowest deviation (coronal 1.83 ± 1.84 deg, sagittal 2.48 ± 2.43 deg vs. freehand 30.08 ± 5.93 deg); material effect inconsistent	Sleeve height is the dominant design variable; material choice can follow cost
[70]	Fixation pins and metal sleeves	Four fixation and sleeve configurations	Angular deviation 1.37–2.64 deg across configurations; no perforation in any group	Configuration accounts for roughly 1.3 deg of achievable accuracy
[75]	3D printer cost tier	Desktop low-cost printer vs. laboratory workflow	Successful guided access achieved with cost-effective printers	Entry cost of static guidance can be lowered without losing function
[76–78]	Sleeveless and non-restrictive guide designs	Guiding rails, open-frame titanium, non-restrictive guides vs. sleeved templates	Angular deviation 1.5 deg (operator 1, 1.47 deg; operator 2, 1.61 deg); accuracy independent of operator; deviations equal to or lower than sleeved comparators	Sleeveless designs address limited interocclusal space, irrigation and visibility without an accuracy penalty
[73]	Imaging modality	MRI-based planning instead of CBCT	Angular deviation 1.82 deg (median 1.50, max 7.60); 91/100 canals located; 2 perforations	Radiation-free planning is technically feasible and within the CBCT accuracy range

3.3. Accuracy

Accuracy is the most consistently reported and most internally coherent body of evidence in the field (Table 3). The ranges given in this section and in Table 3 are descriptive: they report the span of values charted from individual studies and are not summary estimates. The studies differ in specimen type, obliteration model, measurement reference and operator, so a value from one study should not be compared with a value from another, and no ordering of modalities below rests on such a comparison; where modalities are ranked, the ranking comes from studies that compared them within the same experiment. Static guides achieved mean angular deviations between roughly 1.1 and 2.8 degrees across independent laboratories [13,79–82]. Dynamic navigation clustered slightly higher in most direct comparisons, at roughly 1.7 to 2.8 degrees [74,82,83], although one recent randomised in vitro study of three dynamic systems in molars reported angular deviations below 0.7 degrees and a 99.5 per cent location rate, substantially better than any earlier dynamic dataset [84]. Freehand access cavity preparation, where reported as a comparator in the same experiments, deviated by 9.5 to 21.2 degrees [82,84,85]. Linear deviations followed the same ordering: static guides remained in the sub-millimetre to low-millimetre range at both base and tip, whereas freehand and less restrictive comparators reached 0.88 mm and above [13,80,85].

Three findings qualify this picture. First, the head-to-head ranking is not fixed: the three-way comparison by Huth and colleagues placed static ahead of dynamic ahead of freehand for angular accuracy, yet found dynamic navigation the most conservative of the three in tooth substance and found no significant difference between methods for bur base and tip localisation [82]. Second, robotic drilling now matches static guidance. In three-dimensionally printed mandibular models with obliterated canals, a robotic system located all twelve canals, as did the static guide, while dynamic navigation located ten of twelve and perforated two; robotic apical and angular deviations were significantly lower than dynamic and statistically indistinguishable from static [15]. Third, and most importantly, laboratory accuracy transfers to patients. A randomised controlled trial in 80 patients with obliterated canals reported 0.14 ± 0.08 mm coronal, 0.35 ± 0.16 mm apical and 1.75 ± 0.30 degrees of angular deviation under static guidance, all significantly lower than freehand,

with no difference in intraoperative pain or in one-year clinical outcome [31]. A clinical series measured by postoperative intraoral scanning gave concordant figures [86].

Precision at this level is not a guarantee of safety, because angular error compounds over root length. An angular deviation of 2.8 degrees translates into a millimetric displacement over a typical access depth (the lateral displacement equals the drilling distance multiplied by the tangent of the angle, so 2.8 degrees over 15 mm gives 0.73 mm and over 20 mm gives 0.98 mm), which is sufficient to explain why perforations continue to be reported despite favourable mean accuracy [8,73,74]. A safety margin of at least 1 mm around the planned trajectory has been proposed on the basis of measured clinical error [86].

Table 3. Angular and linear deviation of guided endodontic access, by guidance modality. Where a study reported more than one subgroup, the overall or representative value is given. Values are means unless stated otherwise. The design column gives the setting of each study; for clinical studies it also gives the overall risk-of-bias judgement from Tables 6–8, and for laboratory and cadaveric studies the group-level appraisal is given in Table 9. Abbreviations: FH, freehand; SG, static guide; DN, dynamic navigation; AR, augmented reality; RCT, randomised controlled trial; PCO, pulp canal obliteration; n.s., not specified.

Study (Year)	Design and Sample	Modality	Angular Deviation (deg)	Linear Deviation (mm)	Complications
[13]	Ex vivo, 60 single-rooted teeth	SG	1.81 (median 1.4; range 0–5.6)	Base 0.16–0.21; tip 0.17–0.47	None
[79]	Ex vivo, 60 mandibular incisors and canines	SG	1.59	Base 0.12–0.13; tip 0.12–0.34	None
[80]	Ex vivo, 84 teeth, 117 cavities	SG	2.8 ± 2.6 (molars higher)	Coronal 0.13 ± 0.21; apical 0.46 ± 0.4	23/117 initially inaccessible; none perforated
[81]	Ex vivo, 67 posterior teeth, 135 canals	SG	1.39 (median 1.10; max 7.8)	Base 0.26–0.29; tip 0.24–0.31	None; 100% located
[82]	In vitro, 144 cavities, 4 tooth types, 4 operators	SG/DN/FH	1.12 ± 0.85/2.82 ± 1.8/9.53 ± 6.36	No significant difference between methods at bur base and tip	n.s.
[76]	In vitro, 88 cavities, sleeveless guides	SG	1.5 (1.47 and 1.61 by operator)	Coronal 0.5; apical 0.7	None
[87]	In vitro, 40 premolars	SG	3.62	Tip 0.28–0.30; base 0.25–0.28	n.s.
[83]	In vitro, 84 teeth, 138 canals	DN	1.7 (higher in molars)	2D horizontal 0.9; 3D 1.3 from orifice	n.s.
[74]	In vitro, 132 teeth, 168 cavities	DN	2.81 ± 1.53	Entry 0.67; apical 0.63; total 1.60	12 canals missed; 7 perforated
[85]	In vitro, ultraconservative cavities	DN/FH	4.8 / 21.2	0.34 / 0.88 (max distance from ideal)	n.s.
[84]	Randomised in vitro, 96 molars, 3 DN systems	DN/FH	0.59–0.64/ 11.03 ± 4.70	Entry 0.57–0.70/1.64; target 0.59–0.80/1.26	Success 99.5% vs. 93.1%
[15]	In vitro, printed mandibular models with PCO	Robot/SG/DN	Robot significantly lower than DN; equal to SG	Robot apical deviation lower than DN ($p < 0.05$)	DN 10/12 located, 2 perforations; robot and SG 12/12
[17]	In vitro, 90 cavities, 2 operators	AR	8.5 (anterior and premolar)	Coronal 0.51; apical 0.77	n.s.
[73]	In vitro, 100 teeth, MRI planning	SG	1.82 (median 1.50; max 7.60)	Base 0.21–0.31; tip 0.28–0.44	2 perforations; 7 failures without perforation
[86]	Ex vivo (40 teeth) plus clinical (33 patients)	SG	Clinical 1.91 (0.36–5.19)	Clinical coronal 0.2; apical 0.45	None

Table 3. *Cont.*

Study (Year)	Design and Sample	Modality	Angular Deviation (deg)	Linear Deviation (mm)	Complications
[31]	RCT, 80 patients with PCO; RoB 2 some concerns	SG vs. FH	1.75 ± 0.30 ($p <$ 0.0001 vs. FH)	Coronal 0.14 ± 0.08; apical 0.35 ± 0.16	No perforation; no difference in pain or 1-year outcome
[44]	CCT, 133 teeth (60 guided, 73 freehand); ROBINS-I serious	SG vs. FH	Not measured (categorical outcome)	Not measured	Guided 1 canal not found, 0 perforations; freehand 7 not found, 7 perforated
[32]	RCT, 30 patients, traumatised single-rooted teeth; RoB 2 high	SG vs. FH	Not measured	Not measured	Guided 0 failures; freehand 13% canals not located; 3× more tooth structure loss freehand

3.4. Tooth Substance and Procedural Time

Guided access removed less hard tissue than freehand preparation in the large majority of comparative studies, with reductions between 26 and 80 per cent depending on indication, tooth type and comparator (Table 4). In a typodont model guided access removed 34 to 48 per cent less dentine across incisors, premolars and molars [8]. A simulation of pulp canal calcification in printed teeth produced the largest reported effect, 9.8 against 49.9 mm^3, close to an 80 per cent reduction; in the same experiment the canal was located in 22 of 24 guided attempts against 10 of 24 conventional attempts, and treatment took 11.3 against 21.8 min, so in that model the guided approach was simultaneously more conservative, more successful and faster, and none of the three effects depended on operator experience [9]. Real-time dynamic navigation gave 10.5 against 29.7 mm^3 [88], and a randomised in vitro study in molars reported 9.79 to 11.03 mm^3 for three dynamic systems against 14.98 mm^3 freehand, a 26 to 35 per cent reduction [84]. In posterior printed replicas the absolute savings were larger still, 29.6 against 82.6 mm^3 in molars [89].

One study runs against this trend and deserves more weight than it has been given. Hildebrand and co-workers matched 108 extracted teeth with pulp canal calcification in pairs and compared conventional access performed by a specialist endodontist with guided access performed by a general dentist. All canals were located by both methods, and the confidence intervals for substance loss overlapped completely, 15.9 to 29.6 mm^3 conventional against 17.6 to 27.5 mm^3 guided; those for procedural time overlapped only marginally, 163 to 249 s against 232 to 327 s, so the guided arm was if anything the slower of the two. The guided arm, however, required 31 additional intraoperative radiographs against none in the conventional arm [90]. Two conclusions follow. The tissue-conservation advantage of guided access is measured against a freehand operator of average skill, not against a specialist working under magnification; and the radiographic burden of the guided workflow has been almost entirely unreported elsewhere.

A comparable qualification applies at the level of individual canals. An ex vivo study of mandibular incisors and maxillary molars found the difference in removed volume statistically significant in molars but not in incisors [91]. Procedural time was reported inconsistently and showed no uniform direction: guided access was faster in post removal [20], slower than freehand access cavity preparation in several comparisons [82,90], faster in others [9,88], and equivalent in a third group [84].

Table 4. Tooth-substance loss and procedural time, guided versus conventional access. Volumetric values are in mm^3 unless otherwise stated. Where a study stratified results by tooth type or operator, representative values are given. Abbreviations: SG, static guide; DN, dynamic navigation; FH, freehand; PCC, pulp canal calcification; CI, confidence interval; n.s., not specified.

Study (Year)	Design and Sample	Conventional	Guided	Reduction	Procedural Time
[8]	In vitro, typodont incisors, premolars, molars (10 each)	16.1 ± 3.7 incisor; 44.2 ± 8.9 premolar; 99.3 ± 3.1 molar	10.3 incisor; 51.8 molar (pre-enlargement)	34–48%	n.s.
[9]	In vitro, printed teeth with simulated PCC, 3 operators	49.9 (95% CI 42.2–57.6); canal located 10/24 (41.7%)	9.8 (95% CI 6.8–12.9); canal located 22/24 (91.7%)	≈80%	21.8 min (15.9–27.7) vs. 11.3 min (6.7–15.9)
[90]	Ex vivo, 108 matched teeth with PCC; specialist freehand vs. general dentist guided	15.9–29.6 (95% CI)	17.6–27.5 (95% CI)	None (CIs overlap)	163–249 s vs. 232–327 s; 31 extra radiographs in guided arm
[88]	In vitro, 72 printed teeth with PCC, real-time DN	29.7 (operator 1, 19.9; operator 2, 39.4)	10.5	≈65%	Shorter with DN
[89]	In vitro, 192 printed posterior replicas, DN, operator experience	Molars 82.6; premolars 25.3	Molars 29.6; premolars 11.8	53–64%	n.s.; 2 perforations in guided arm
[84]	Randomised in vitro, 96 molars, 3 DN systems	14.98 ± 3.09	9.79–11.03	26–35%	176–183 s, no difference between groups
[91]	Ex vivo, 20 mandibular incisors and 20 maxillary molars	Incisors 31.68; molars 62.53	Incisors 26.52 (p = 0.960); molars 45.68 (p = 0.004)	Molars only	n.s.
[92]	In vitro, calcified canals	38.85 ± 19.07; angular 13.16 ± 2.34 deg	17.19 ± 6.11; angular 4.82 ± 1.66 deg	56%	n.s.
[93]	In vitro, printed resin incisors with simulated PCC, 3 operator grades	66.87 ± 12.54 (incisal access)	Single-tooth template-guided access significantly lower	Significant	28.1 ± 4.7 min conventional
[82]	In vitro, 144 cavities, 4 tooth types, 4 operators	Freehand greatest substance loss (p < 0.001)	DN least (p < 0.0001); SG intermediate	DN > SG	FH shortest, then SG, then DN
[26]	In vitro, 22 two-rooted maxillary premolars, targeted retreatment	27.64 ± 11.34	11.73 ± 1.85	58%	n.s.
[20]	In vitro, 90 maxillary central incisors, fibre post removal	General dentist 7.29 ± 2.44; specialist 7.24 ± 4.16	Significantly lower than both (p < 0.05)	Significant	Shortest in guided group
[94]	In vitro, 30 mandibular premolars, fibreglass post removal	53.15 (92.40% of post space)	13.75 (23.90%)	74%	4.75 min conventional
[22]	In vitro, immature premolars, access through MTA	Excessive removal of sound structure; fracture strength 472.3 N	Structure and fracture strength preserved	Significant	n.s.

3.5. Indication-Specific Findings Beyond Canal Location

3.5.1. Post Removal

Twenty-three studies addressed intraradicular post removal, 14 of them reporting quantitative outcomes. It is one of the better-supported non-surgical indications in volume of laboratory work, but it has no clinical comparative data at all. In 90 human central

incisors, guided removal by a general dentist produced significantly less radicular dentine loss than conventional removal by either a general dentist or a specialist, the shortest working time of the three arms, and no perforation in any group [20]. Microguided access through a bonded fibre post achieved mean deviations of 0.39 mm coronally and 0.40 mm apically despite composite-related CBCT artefacts [95]. Guided access to fibreglass posts in mandibular premolars reduced removed volume from 53.15 to 13.75 mm^3 [94], and single- and double-sleeve guide designs performed equivalently, which matters practically because the double-sleeve design allows bilateral cases to be treated with one guide [96]. Operator experience influenced the result in posterior teeth even under guidance, with mean angular deviation of 2.54 degrees for an inexperienced operator against 1.55 degrees for an experienced one [97]. Dynamic navigation has also been applied to post removal, with significantly lower volumetric loss and shorter working time than freehand in a matched in vitro comparison [98]. The remainder of this indication consists of technique descriptions and case reports covering fibre-reinforced composite and glass-fibre posts [99–102]. Extension to zirconia posts rests on a single three-case series [21].

3.5.2. Access Through Filling Materials, Retreatment and Separated Instruments

Guided access through mineral trioxide aggregate preserved more tooth structure and fracture strength than freehand access in immature premolars [22]. Targeted retreatment restricted to the root carrying periapical pathosis reduced substance loss from 27.64 ± 11.34 to 11.73 ± 1.85 mm^3 [26]. A systematic review of seven in vitro studies of guided non-surgical retreatment found fewer iatrogenic errors in guided groups irrespective of operator experience for post removal and for access through mineral trioxide aggregate, but identified separated instrument retrieval as the exception: guided retrieval produced more iatrogenic errors and took longer than the freehand approach [33]. A meta-analysis of dynamic navigation in retreatment confirmed a consistent reduction in procedural time and angular deviation against freehand, with high heterogeneity and moderate overall risk of bias [103]. Guided retreatment of an anterior tooth whose canal had already been deviated during a previous attempt has been described, with the guide used to re-establish the original trajectory rather than to create a new one [104]. Access to calcified anterior teeth with apical periodontitis, the classical indication, is documented in early case work that established the clinical protocol [105]. Separated instrument retrieval in the charted set is otherwise represented only by single case reports [23].

3.5.3. Developmental Anomalies and Autotransplantation

Guided access to dens invaginatus has been reported in one small series and one two-case report with favourable short-term outcomes, achieved by planning a debridement route that preserves the main pulp tissue where it is still vital [24,25]. A narrative review of anomalous teeth identified nine eligible studies, all of them descriptive, although its scope spans dentinogenesis imperfecta, dentinal dysplasia, dens evaginatus and fusion as well and is therefore broader than the developmental anomaly category used here [106]. Guided socket preparation in autotransplantation has been reported with five-year follow-up in a single case, where the reduction in extra-alveolar time was sufficient to allow the donor tooth to be observed rather than prophylactically root-treated [27]. These indications are conceptually attractive and evidentially thin.

3.5.4. Endodontic Microsurgery

Guided microsurgery has developed along a different trajectory from guided access, and it is now the most rapidly moving part of the field (Table 5). A systematic review and meta-analysis of 33 studies found that static guidance and dynamic navigation both

significantly reduced three-dimensional global deviation at the bur tip compared with freehand, that drilled root length error did not differ between the two guided modalities, that dynamic navigation produced significantly greater resection angle deviation than static guidance, and that static guidance required the least operating time [29]. A separate systematic review of 14 dynamic navigation studies covering 240 roots reported mean platform and apical deviations of 1.17 and 1.21 mm and angular deflection of 2.29 degrees, with favourable healing and infrequent complications [107].

Clinical evidence in this indication is no longer emerging but present, and it stands beside access to obliterated canals as one of the two applications with randomised data, although the trials are small and, as Section 3.8 shows, none is at low risk of bias. Four randomised trials have been published. Two compare guidance against conventional microsurgery and both report the same effect on operating time: 18.2 against 26.1 min in 28 patients, with complete lesion resolution at one year in 92.9 against 71.4 per cent, a difference that did not reach significance in a trial of that size [50]; and 26 to 27.5 against 39 to 47 min in 24 patients, with no difference in six-month lesion volume between guided and non-guided arms or between two retrograde filling materials [48]. The other two compare the two guidance modalities against each other. In 18 patients, static and dynamic navigation were comparably accurate overall, with dynamic navigation significantly better at the surgical endpoint [49]; in 60 patients, quality of life and analgesic use over the first postoperative week were equivalent, while static navigation was significantly faster [47]. Taken together the two trials with a conventional comparator establish that guidance shortens surgery substantially and does not compromise healing, while the two modality comparisons indicate that the choice between static and dynamic navigation is a matter of anatomy and workflow rather than of outcome. Only the two trials with a conventional arm report a healing outcome, and the two modality comparisons establish equivalence between modalities rather than benefit over no guidance [47,49]. Uncontrolled clinical data are consistent with the trials: a prospective case series reported osteotomy platform, apical and angular deviations of 1.05 mm, 1.2 mm and 6.24 degrees, with success in 9 of 10 teeth among the eight patients reviewed at a minimum of one year [108], and a larger prospective cohort of 46 teeth reported radiographic success in 38 of 40 teeth on two-dimensional and 33 of 35 on three-dimensional criteria at a minimum of one year, with no prognostic factor reaching significance [109]. A retrospective comparison of the two modalities through a single vertical incision found them equally accurate overall, dynamic navigation less accurate with lesions above 9 mm and static navigation more accurate posteriorly [110], and an in-house printed-guide protocol with trephine burs achieved complete healing in all six patients by twelve months [111]. A retrospective series of twelve teeth treated with a straight-incision template protocol reported median resection length deviation of 0.79 mm, angular deviation of 19 degrees and clinical success in all cases at one year [112].

Guidance also confers an anatomical safety benefit that freehand technique cannot replicate: in printed mandibular models the guided osteotomy remained significantly further from the mental foramen than the freehand osteotomy across all three foramen positions tested, with smaller osteotomies and shorter procedures [113]. Cadaveric work in 119 roots confirms that guidance narrows the performance gap between experienced and inexperienced operators and that every complication in the freehand arm occurred among non-experts [114]; a parallel dynamic-navigation study reached the same conclusion, finding that novices improved with guidance without reaching expert accuracy [115]. A separate cadaver study of a tooth-bone-supported removable guide reported stable seating in 14 of 15 placements and resection closer to the planned 3 mm than unguided surgery achieved [116].

Table 5. Guided endodontic microsurgery: accuracy of osteotomy and root-end resection. The design column gives the setting of each study and, for clinical studies, the overall risk-of-bias judgement from Tables 6–8; group-level appraisal of laboratory and cadaveric studies is given in Table 9. Abbreviations: SN, static navigation; DN, dynamic navigation; RA, robot-assisted; FH, freehand; RCT, randomised controlled trial; n.s., not specified.

Study (Year)	Design and Sample	Modality	Osteotomy Deviation	Root-End Resection	Outcome
[108]	Prospective clinical case series, 9 patients, 12 roots; JBI low	DN	Platform 1.05 mm; apex 1.2 mm; angular 6.24 deg	Length 0.46 mm; angle 4.9 deg	90% success at ≥1 year
[49]	RCT, 18 patients, 19 teeth, 20 roots; RoB 2 high	SN vs. DN	No difference in start point, angular; DN better at end point ($p = 0.02$)	No difference in length or angle	Both clinically acceptable
[112]	Retrospective series, 12 teeth, straight-incision template; JBI moderate	SN	n.s.	Median length 0.79 mm (95% CI 0.49–1.16); angle 19 deg (8.03–25.47)	100% success at 1 year
[117]	In vitro, 72 teeth, 3 standardised jaw models	RA vs. DN vs. SN	RA significantly better in platform and angular deviation ($p < 0.05$)	RA better than DN in length deviation	SN shortest operating time
[118]	In vitro, 4 printed mandibular models	RA vs. DN	Platform, apex and angular deviation all lower with RA ($p < 0.05$)	n.s.	RA longer registration, shorter drilling
[16]	Clinical report, 1 patient, mandibular first molar	RA	3 mm osteotomy executed autonomously	Autonomous root-end resection	First reported clinical case
[116]	Cadaver, 59 roots, tooth-bone-supported removable guide	SN	14/15 guides stable without mobility	Resected length 3.2 mm vs. 4.0 mm unguided	Improved accuracy vs. unguided
[114]	Cadaver, 119 roots, 2 DN systems vs. FH, expert and non-expert	DN vs. FH	Both DN systems comparable; better efficiency than FH	n.s.	All FH complications occurred among non-experts
[113]	In vitro, 48 printed models, variable mental foramen position	SN vs. FH	Guided osteotomy significantly further from mental foramen in all 3 positions; smaller osteotomy area	n.s.	Shorter procedure time with guidance in all three foramen positions
[119]	In vitro, 38 mandibular molars, ultrasonic device integrated into DN	DN vs. FH	Lower 3D deviation for bone window ($p < 0.0001$)	Resected length ≈ 3 mm; lower resection angle with DN	Fully guided workflow feasible

3.6. Emerging Modalities

Robotic systems are the most consequential recent development. In microsurgery, robot-assisted osteotomy and root-end resection were significantly more accurate than both dynamic and static navigation for platform, angular and resection angle deviation, and more accurate than dynamic navigation for resection length, although static navigation remained the fastest of the three [117]; a direct robot-versus-dynamic comparison found smaller platform, apical and angular deviations with the robot at the cost of longer registration [118]; and the first clinical report of autonomous robotic apicoectomy has been published [16]. For non-surgical access the robot matched the static guide [15]. Augmented reality has produced mixed results: in vitro angular deviation of 8.5 degrees in anterior and premolar teeth [17]; a cadaveric feasibility study of a head-mounted display in 12 apicoectomies,

which reported a mean preparation time of 162 s, an operating time of 9 min and a system usability score of 80.4 [120]; and a systematic review of three studies reporting improved spatial accuracy and operator confidence but inconsistent efficiency and, in one study, greater residual lesion volume [18]. Magnetic resonance imaging as a radiation-free planning substrate reached 1.82 degrees of angular deviation and located 91 of 100 canals, which is within the range of CBCT-planned static guidance [73]. Computer-controlled CO2 laser ablation has been reported as a bur-free alternative at pilot stage [121].

Artificial intelligence does not yet appear in any charted guided endodontic study as an intervention, but it addresses the workflow's most labour-intensive step. Convolutional neural network segmentation of root canals on CBCT achieved Dice coefficients of 0.89 to 0.93 in single-rooted teeth while reducing segmentation time from roughly 2260 s to 42 s, a 54-fold saving [122], and comparable performance has been reported for the pulp cavity system of mandibular molars [123]. Since planning time is one of the structural objections to guided endodontics, this is the most plausible near-term route to making the technique practical in general practice, and no charted study evaluated a fully automated planning-to-guide pipeline.

3.7. Failures and Complications

Failure reports are informative precisely because they are rare. Dynamic navigation has failed due to tracking instability and handpiece adaptation problems, requiring rescue by a more experienced operator working conventionally [124]. Static guides have failed due to inaccurate mesh registration during planning and imprecise fit between sleeve and bur, in one instance producing a perforation that required endodontic microsurgery to resolve [125]. The common factor in both modalities is that the error was introduced before the bur entered the tooth. That is consistent with the quantitative work on guide fit [71] and on CBCT and printing resolution [63], and it identifies the planning and fabrication stages, rather than the drilling stage, as the decisive determinant of safety. Operator learning is a second, separable determinant that the accuracy literature has largely concealed by reporting pooled means. A risk-adjusted cumulative sum analysis of one endodontist with no prior experience of dynamic navigation, working through 45 consecutive calcified maxillary anterior teeth, identified inflection points at the fifth and eighteenth cases for angular deviation and at the thirteenth and twenty-eighth for linear deviation, and concluded that technical proficiency required roughly 18 to 28 cases [126]. The overall canal location rate in that series was 93.3 per cent, below the figures reported by established groups, which is what a learning curve looks like when it is measured rather than averaged away.

Because the charted studies report adverse events inconsistently, they are summarised here by category rather than pooled, and technical failures are kept separate from harms. Laboratory perforation was reported in 2 of 30 guided typodont cavities [8], 2 of 100 MRI-planned cavities [73], 7 of 168 dynamic navigation cavities [74], 2 of 12 dynamic navigation attempts in printed models with obliteration against none with a static guide or robot [15], and 2 of 192 printed posterior replicas [89]; in the matched guide-design experiments no perforation occurred in any configuration [70]. Clinical perforation was reported in none of 80 patients in the static-guidance arm of the randomised trial [31], in none of 60 guided against 7 of 73 freehand teeth in the controlled study [44], and in one case in the static-guide failure analysis, where it required microsurgical repair [125]. Failure to locate the canal, which is a technical failure rather than a harm, occurred in 2 of 30 [8], 9 of 100 [73] and 12 of 168 [74] laboratory cavities, in 1 of 60 guided against 7 of 73 freehand teeth clinically [44], in none of the guided against 13 per cent of the freehand canals in the trial of traumatised teeth [32], and in 3 of 45 teeth in the dynamic navigation learning-curve series [126]. Excess removal of dentine relative to a specialist comparator was recorded

in one matched study [90]. Additional radiation exposure attributable to the workflow, 31 intraoperative radiographs across 54 teeth, was recorded in the same study and nowhere else [90]. Procedural complications specific to the modality were tracking instability and handpiece adaptation failure with dynamic navigation [124] and registration error and poor sleeve-to-bur fit with static guides [125]. No study reported a postoperative complication attributable to guidance, and no study followed patients long enough to detect root fracture or loss of a tooth treated with guided access.

3.8. Risk of Bias and Critical Appraisal

Appraisal was carried out within each design stratum with the instrument appropriate to it, and the results are reported stratum by stratum rather than pooled, because a single aggregated figure across incommensurable instruments would convey nothing. The overall picture differs sharply from the impression given by narrative summaries of this literature: the evidence is not at uniformly low risk of bias, and the randomised evidence in particular is weaker than its existence suggests.

None of the six randomised controlled trials was judged to be at low risk of bias overall (Table 6, Figure 2A,B). Three were at high risk and three raised some concerns. The randomisation domain accounts for most of this and is almost entirely a reporting failure: every one of the six describes allocation only as participants being randomly assigned, divided or allocated, and not one reports how the sequence was generated or whether allocation was concealed. Three trials were downgraded further for specific reasons. One measured tooth-structure loss on two-dimensional postoperative radiographs rather than volumetrically, which cannot capture the outcome the trial is reporting [32]; another randomised 18 patients into groups of ten and eight without explanation and was not powered for the comparison it presents [49]. A third combined a two-by-two factorial design with six patients per cell and no stated adjustment for multiplicity [48]. Prospective registration was present in only two of the six [32,50], retrospective in one [31], and absent or unstated in three, so prespecification of outcomes cannot be verified for half the randomised evidence in the field.

Table 6. Risk of bias in the six randomised controlled clinical trials, assessed with the Cochrane RoB 2 tool. D1, randomisation process; D2, deviations from intended interventions; D3, missing outcome data; D4, measurement of the outcome; D5, selection of the reported result. L, low risk; S, some concerns; H, high risk. Abbreviations: EMS, endodontic microsurgery; PCO, pulp canal obliteration; SN, static navigation; DN, dynamic navigation; FH, freehand.

Trial	Indication and Comparison	n	D1	D2	D3	D4	D5	Overall	Basis of the Judgement
[31]	PCO, static guided vs. FH access	80	S	L	L	S	S	Some concerns	Allocation described only as "randomly divided"; sequence generation and concealment not reported; registered retrospectively, so prespecification of outcomes cannot be verified; outcome assessors not reported as blinded
[32]	PCC in traumatised teeth, static guided vs. FH access	30	S	L	L	H	L	High	Tooth-structure loss quantified on two-dimensional postoperative radiographs with image-analysis software rather than volumetrically, which cannot capture the outcome the trial reports; randomisation method not described; prospectively registered (CTRI)
[47]	EMS, SN vs. DN (no FH arm)	60	S	L	S	S	S	Some concerns	Randomisation method not described; primary outcome is patient-reported quality of life with no blinding and no reported diary-completion rate; no trial registration stated

Table 6. *Cont.*

Trial	Indication and Comparison	n	D1	D2	D3	D4	D5	Overall	Basis of the Judgement
[48]	EMS, guided vs. non-guided, two retrograde materials	24	S	S	S	S	H	High	Two-by-two design with six patients per cell; multiple comparisons without a stated adjustment or power calculation; no registration; six-month attrition not reported
[49]	EMS, SN vs. DN (no FH arm)	18	H	L	L	L	S	High	Randomised allocation produced unequal groups of ten and eight in a trial of eighteen patients and the method is not described; single-blind design and CBCT superimposition support the outcome domain; no registration stated; not powered for the comparison reported
[50]	EMS in critical anatomical regions, guided vs. conventional	28	S	L	L	S	L	Some concerns	Prospectively registered (NCT05283252) with twelve-month follow-up on all participants; randomisation method not described; CBCT lesion measurement with no reported assessor blinding

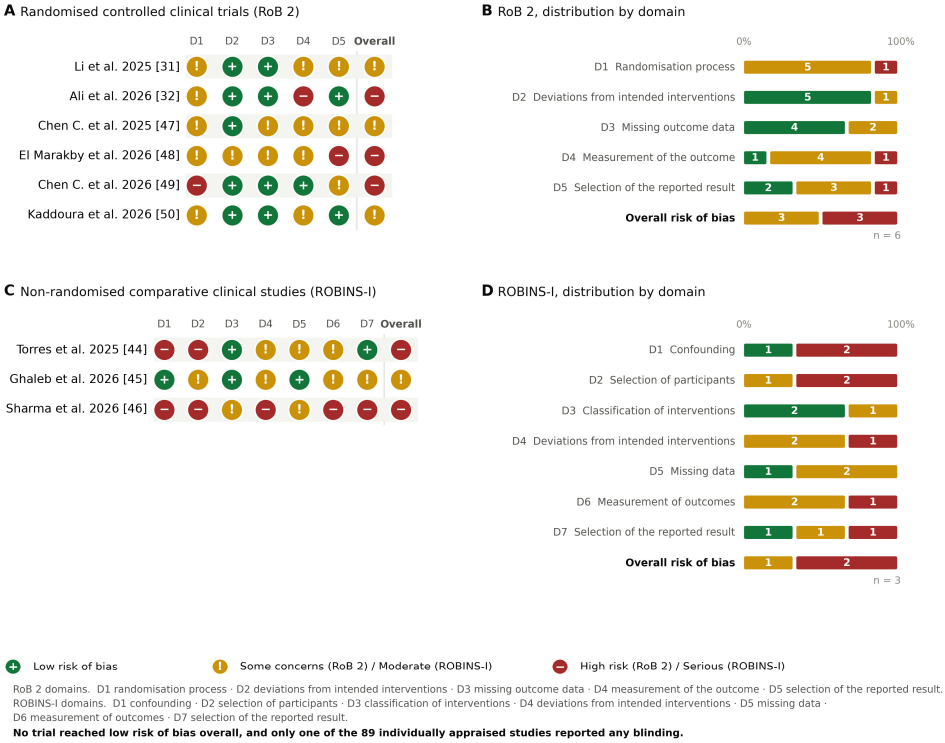


Figure 2. Risk of bias in the comparative clinical evidence. (**A**) Domain-level and overall judgements for the six randomised controlled clinical trials, assessed with the Cochrane RoB 2 tool. (**B**) Distribution of those judgements by domain. (**C**) Domain-level and overall judgements for the three non-randomised comparative clinical studies, assessed with ROBINS-I. (**D**) Distribution of those judgements by domain. Each mark carries a symbol as well as a colour, so that no judgement is conveyed by colour alone. Numbers inside the bars in panels B and D are study counts, not percentages; the bars are scaled to the denominator of each panel (n = 6 and n = 3 respectively). Each row is labelled with the first author, year and reference number; the two trials with the first author Chen C. are distinct studies, a patient-reported outcome trial [47] and an accuracy trial [49]. The per-study judgements and the reasoning behind each are given in Tables 6 and 7. Two patterns dominate. No trial reached low risk of bias overall, and the randomisation domain is the single worst-performing domain in the figure because not one of the six trials reports how its allocation sequence was generated. The laboratory, cadaveric and case-series strata are not shown here because their instruments are not domain-commensurable with RoB 2 and ROBINS-I; they are reported in Tables 8 and 9.

The three non-randomised comparative clinical studies span moderate to serious risk of bias, and none reaches low (Table 7, Figure 2C,D). The largest and most influential of them draws its freehand comparator from historical clinical records treated by different operators in an earlier period, which places it at serious risk of confounding; the authors state explicitly that randomisation was considered unethical, so this is a structural feature of the question rather than a remediable design flaw [44]. A split-mouth pilot in seven patients is at moderate risk, the within-patient design removing most confounding at the cost of sample size and of an indirect method of measuring substance loss [45]. The third reports too little methodological detail for any domain to be verified independently and was judged at serious risk across most domains; it is counted in Table 1 but contributes no outcome data to the synthesis [46].

Table 7. Risk of bias in the three non-randomised comparative clinical studies, assessed with ROBINS-I. D1, confounding; D2, selection of participants; D3, classification of interventions; D4, deviations from intended interventions; D5, missing data; D6, measurement of outcomes; D7, selection of the reported result. L, low; M, moderate; S, serious; C, critical.

Study	Design and Comparison	n	D1	D2	D3	D4	D5	D6	D7	Overall	Dominant source of bias
[44]	Prospective single-arm with external control drawn from clinical records	133 teeth/128 patients	S	S	L	M	M	M	L	Serious	The freehand comparator is historical, treated by different operators in a different period; the authors state that randomisation was considered unethical, which makes the confounding structural rather than remediable
[45]	Split-mouth in vivo pilot, contralateral incisor as control	7 patients/14 teeth	L	M	L	M	L	M	M	Moderate	Within-patient design removes most confounding, but the sample is a seven-patient pilot, allocation of the two incisors is not described, and substance loss is inferred indirectly from post-core volume
[46]	Prospective clinical study, guided vs. freehand	60 teeth/48 patients	S	S	M	S	M	S	S	Serious	Allocation, blinding, operator and statistical methods are not reported and patient-reported outcomes are collected unblinded; the report is too brief for any domain to be verified independently, so it is counted in Table 1 but contributes no outcome data

The 24 clinical case series were appraised with the JBI checklist (Table 8). Performance was better than the design's reputation would suggest in the larger series and poor in the smaller ones. Five of the twelve series with quantitative data met nine or ten of the ten items, among them the only cohort in the review with independent calibrated outcome assessors [109] and a fifty-patient consecutive referral series with explicit inclusion criteria [127]. The recurring failures are items four, five, eight and ten: consecutive inclusion is rarely stated, denominators are often absent, follow-up is under twelve months or not reported at all, and analysis is frequently descriptive where comparison was possible. Follow-up is the most consequential of these. Only a minority of the 24 series report twelve months or more, and among those contributing quantitative data none sets a follow-

up longer than a minimum of twelve months, so no included clinical study speaks to the durability of a guided-access restoration.

Table 8. Critical appraisal of the 24 clinical case series with the JBI checklist for case series. Items: 1, clear inclusion criteria; 2, standard and reliable measurement of the condition; 3, valid identification methods; 4, consecutive inclusion; 5, complete inclusion; 6, demographic reporting; 7, clinical information; 8, outcome and follow-up reporting; 9, setting reported; 10, appropriate statistical analysis. Y, yes; N, no; U, unclear.

Series	Indication	n	Follow-Up	Items Met	Items Not Met or Unclear	Overall
[127]	PCO, anterior and single-rooted	50 patients	Not stated	9/10	Item 8: radiographic drill-path precision is scored but no healing or survival follow-up is reported	Low
[109]	Endodontic microsurgery	46 teeth/32 patients	≥12 months	10/10	None; two calibrated endodontists assessed outcomes against defined radiographic criteria	Low
[128]	PCO, mixed anterior and posterior	100 teeth	Not stated	7/10	Items 4, 8, 10: consecutiveness not stated, no clinical follow-up, success defined only as reaching the canal lumen	Moderate
[126]	PCO, maxillary anterior, single operator	45 teeth	None (immediate CBCT)	9/10	Item 8: no clinical follow-up; the design addresses operator learning rather than treatment outcome	Low
[108]	Endodontic microsurgery	11 teeth, 12 roots/9 patients	≥12 months	9/10	Item 5: eight of nine patients available at follow-up; loss not analysed	Low
[110]	Endodontic microsurgery, SN vs. DN	60 teeth	None (postoperative CBCT)	8/10	Items 4, 8: retrospective allocation to technique, no healing follow-up	Moderate
[111]	Endodontic microsurgery	9 roots/6 patients	6 and 12 months	9/10	Item 10: descriptive statistics only, appropriate to a proof-of-concept design	Low
[51]	PCO, anterior and single-rooted	18 teeth/ 17 patients	Not stated	8/10	Items 8, 10: precision scored radiographically without follow-up or inferential analysis	Moderate
[53]	PCO, four tooth types	11 cases	Not stated	7/10	Items 4, 8, 10: single-operator convenience sample, no follow-up, no analysis	Moderate
[59]	PCO, anterior	3 patients	Not stated	6/10	Items 1, 4, 8, 10: three-case technique description; below the threshold at which series-level appraisal is informative	Moderate
[124]	PCO, dynamic navigation failures	7 teeth/4 patients	12 months	8/10	Items 4, 5: a deliberately selected failure series, which is its purpose but limits generalisability	Moderate
[112]	Endodontic microsurgery, straight-incision template	12 teeth/ 9 patients	12 months	8/10	Items 4, 5: retrospective, consecutiveness of inclusion and the denominator of eligible cases not stated	Moderate

Table 8. *Cont.*

Series	Indication	n	Follow-Up	Items Met	Items Not Met or Unclear	Overall
Twelve narrative series (pooled)	All indications; 47 of the 104 narrative reports concern anterior access	3–8 cases each	0–12 months	4–7/10	Items 4, 5, 8 and 10 were not met in the majority: inclusion was not consecutive, denominators were absent, follow-up was under twelve months or unstated, and no analysis was performed. These series contribute to the indication map only and to no outcome estimate	Moderate to high

The laboratory and cadaveric stratum, 108 studies, was appraised against criteria adapted from QUIN and PRILE 2021 (Table 9). Two domains are met almost universally: a defined comparison group and an appropriate statistical analysis. Two are met almost never. Among the 56 laboratory and cadaveric studies examined in detail, not one reports an a priori sample size justification and not one reports blinding of the outcome assessor. The second of these is often defended on the grounds that CBCT superimposition is objective, but superimposition involves operator-dependent registration and threshold selection, and the assessor in most of these studies is also an author with a stake in the comparison. Randomised allocation of specimens is reported in roughly half, more consistently in the substance-loss and modality-comparison studies than in the guide-design work. The cadaveric subgroup is methodologically the strongest in the review after the randomised trials, because nine of the eleven use a split-mouth or contralateral control that removes between-specimen variation [98,114,115,129–134].

Table 9. Critical appraisal of the laboratory and cadaveric evidence with criteria adapted from the QUIN tool and PRILE 2021. Domains scored: SS, a priori sample size justification; RA, randomised allocation of specimens or operators; CG, defined comparison group; OD, operator number and experience reported; AB, outcome assessor blinding; SA, appropriate statistical analysis. Y, criterion met; N, not met; U, not reported. The denominator for the per-study rows is the 56 laboratory and cadaveric studies that contribute charted outcome data to Tables 2–5; the summary row gives domain-level performance across all 108 laboratory and cadaveric studies.

Study Group	Studies	SS	RA	CG	OD	AB	SA	Prevailing Deficiency
Seminal static-guide accuracy series [13,79–81]	4	U	U	Y	Y	U	Y	Assessor blinding and sample size justification unreported throughout; CBCT superimposition is standardised and well described
Three-way modality comparisons [15,82,84,117,118]	5	U	Y	Y	Y	U	Y	Specimen randomisation is reported and effect sizes given; assessor blinding still unreported
Substance-loss comparisons [8,9,26,88–93]	9	U	Y	Y	Y	U	Y	Matched-pair or matched-model allocation is common and operator grade is reported; only [90] prespecified the comparison of a specialist against a general dentist

Table 9. *Cont.*

Study Group	Studies	SS	RA	CG	OD	AB	SA	Prevailing Deficiency
Guide-design and workflow determinants [63,64,68–72,75,77,78,135]	11	U	U	Y	Y	U	Y	Single-factor designs with adequate statistics; no study justified its sample size and none blinded the assessor
Post removal and retreatment [20,22,94–97]	6	U	Y	Y	Y	U	Y	Operator grade is an explicit factor in most; outcome measurement by micro-CT or CBCT is well described
Alternative imaging and modalities [17,65–67,73,74,76, 83,85,87,121]	11	U	U	Y	U	U	Y	Proof-of-principle designs; operator number and experience frequently unreported
Cadaveric studies [98,114–116,120,129–134]	11	U	Y	Y	Y	U	Y	Nine of the eleven use a split-mouth or contralateral control, which is the strongest available design in this stratum; assessor blinding unreported in all eleven
Tendency across the stratum as a whole	108	U	U	Y	Y	U	Y	A defined comparison group and appropriate statistics are near-universal. A priori sample size justification and outcome assessor blinding were reported by none of the 56 studies examined in detail, and these two domains are the reason this stratum cannot be described as being at uniformly low risk of bias

Two cross-cutting observations follow. First, of the 89 studies appraised individually, exactly one reports any blinding: a single-blind randomised trial in 18 patients that does not identify which party was blinded [49]. No laboratory, cadaveric or observational study in the review reports blinding of the outcome assessor. A field that measures effects of one to two degrees cannot continue to leave that domain unaddressed. Second, publication bias is a structural concern in the case-report and case-series strata that no instrument captures: unsuccessful guided cases are unlikely to be written up or accepted, and the small number of published failure analyses [124,125] is more plausibly a reflection of publication practice than of true complication frequency. The two together mean that the evidence base is best described as methodologically adequate within each stratum, concentrated in strata that cannot answer clinical effectiveness questions, and produced by a small number of groups with a declared interest in the technique.

4. Discussion

4.1. Principal Findings

This review set out to establish not whether guided endodontics can be accurate, which the laboratory evidence settles, but where the clinical evidence for it actually sits. Mapping 234 studies against eight indications gives a blunt answer: controlled clinical evidence exists for two of them and for none of the other six. Access to obliterated canals in anterior and single-rooted teeth and endodontic microsurgery are at level A2: randomised trials exist for both, but none of them is at low risk of bias, so neither indication reaches level A1. Access in posterior and multirooted teeth, post removal, retreatment, separated instrument retrieval and autotransplantation are at level C, supported by comparative laboratory work alone. Developmental anomalies are at level D, with no laboratory or cadaveric evidence whatsoever. No indication falls at level A1 or at level B. The second

of these is itself informative. Since each indication carries the highest level available, an indication would sit at level B only if non-randomised comparative studies were the best clinical evidence for it; the three such studies in the review all concern anterior access, which also holds two trials and is therefore at level A2. For the six indications without trials, no non-randomised comparative clinical study exists either, so where clinical comparison has been attempted at all, it has gone straight to randomisation. Treating all of this as one technique has produced a literature in which a randomised clinical finding about anterior obliterated canals is routinely used to justify a procedure on a separated instrument for which the only comparative data are unfavourable. Narrative reviews published in parallel with this work reach compatible descriptive conclusions but do not stratify by indication, which is precisely the step that changes the clinical message [11,136].

A second finding cuts across the first. The randomised evidence that does exist is not strong. None of the six trials is at low risk of bias, three are at high risk, and the reason in every case is at least partly a reporting deficiency rather than a design one: not a single trial states how its randomisation sequence was generated. Four of the six are unregistered or registered retrospectively. The field has reached the stage of producing randomised trials without yet adopting the reporting conventions that make them interpretable, and the distance between those two things is the main obstacle to any future meta-analysis. The direction of the findings is also worth noting: four of the six trials concern microsurgery, not access, so the indication that the field has discussed most has been tested least.

A third point governs how the first two should be read. The outcomes charted in this review fall into five tiers, and the evidence thins at each step. Technical accuracy, measured as angular and linear deviation, is documented in more than a hundred laboratory studies and two clinical ones. Procedural performance, meaning canal location, substance loss, time and intraoperative complications, is documented in the laboratory and in a handful of clinical comparisons. Clinical success, as healing or symptom resolution, is reported in the microsurgery trials and in a few case series with twelve-month follow-up. Patient-reported outcomes appear in two trials. Long-term tooth survival appears in none. The laboratory findings on accuracy and dentine conservation are therefore evidence that the technique can be executed precisely, and nothing more; they are not evidence of clinical benefit, and in this review they have not been treated as such. Where the Discussion draws a clinical implication from a laboratory result, it is stated as an inference to be tested rather than as a finding.

4.2. Accuracy and Its Determinants

The accuracy data are the most reproducible element in the field, recurring across independent groups rather than originating in a single laboratory [13,79–82]. That reproducibility is a statement about direction and order of magnitude, not about the values themselves: the laboratory methodologies are not standardised, and a difference of one degree between two in vitro studies is as likely to reflect the specimen, the obliteration model or the measurement reference as the technique. Comparisons in this section are therefore drawn within studies, and the between-study ranges are reported to show the spread rather than to rank. What the earlier literature underestimated is how much of that accuracy is contingent on workflow quality rather than intrinsic to the concept. Scan resolution, print resolution, planning software, sleeve height, sleeve presence, guide fit and drilling depth each shift the result by an amount comparable to the difference between guidance modalities [63,64,68–71]. Reported mean deviations are therefore best read as the performance of a well-executed digital chain, not of a technique. That has a direct clinical implication: the transfer of published accuracy to a practice using a lower-resolution scanner, a consumer printer and a resin template without a sleeve should not be assumed.

The hierarchy between modalities is also less settled than usually presented. Static guidance retains an advantage in most direct comparisons of angular accuracy and in operating time [29,82], but the most recent dynamic data in molars are better than anything reported for static guides in posterior teeth [84], and robotic systems have equalled static guidance in access and surpassed both in microsurgery [15,117]. Systematic reviews of dynamic navigation reach the same unsettled conclusion from the opposite direction, reporting a consistent advantage over freehand but too few comparable studies to rank the modalities against each other [137,138]. The practical question is shifting from which modality is more accurate to which is more accurate in which anatomical situation, and the available comparisons are not yet stratified finely enough to answer it.

4.3. Tissue Conservation and the Discordant Evidence

Tissue conservation is the outcome with the clearest clinical rationale, since removed pericervical dentine is the principal determinant of the long-term survival of a root-treated tooth, and it is the outcome most consistently reported in favour of the guided approach [8,9,35,84,88]. Two qualifications should be stated more prominently than the literature currently states them. The first is the comparator. Nearly all the reported savings are measured against freehand access by an operator of ordinary skill; when a specialist working under magnification is the comparator, the difference disappears [90]. If the clinical claim is that guided access conserves tissue relative to what a specialist would remove, that claim is currently unsupported. If the claim is that it allows a non-specialist to approach specialist-level conservation, it is well supported, and that is the more useful and more defensible formulation. The point generalises beyond substance loss. Operator experience differs between the arms of many of the charted comparisons, is unreported in a substantial minority of them, and is in effect a confounder of every guided-versus-freehand contrast in which the two arms were not performed by the same operators: the guided arm measures the system, the freehand arm measures the operator, and the difference between them is a mixture of the two. The comparisons that control for this, by matching operators across arms [9,82,84] or by crossing operator grade with technique [20,89,93,114,115], consistently show that guidance reduces the dependence of the result on experience, which is a more specific and more useful claim than that guidance is superior to freehand.

The second qualification is that no study has demonstrated that the conserved dentine translates into improved tooth survival. Population-level morphometric data on the distance from the incisal edge and the cingulum to the pulp chamber in maxillary anterior teeth, which set the limit on how conservative any planned trajectory can be, have only recently begun to be reported outside Western cohorts, and they differ significantly by tooth and by sex [139]. The chain from reduced volume of removed dentine to increased fracture resistance to longer survival is plausible and partially evidenced in the general access cavity literature, but it has not been tested for guided access specifically, and the general literature on minimally invasive access cavities is itself equivocal about whether reduced cavity extent confers a mechanical benefit once marginal ridges are lost. Long-term clinical follow-up of guided cases is the missing link, and it has been called for repeatedly [11,35] without yet being delivered.

4.4. Indication-Specific Interpretation

Reading the evidence by indication produces recommendations that a global reading cannot. For obliterated anterior and single-rooted canals the case is the strongest in the field, although it rests on two trials of 30 and 80 patients: the two randomised trials and two of the three non-randomised comparative studies agree, the third being too sparsely reported to interpret, and the controlled trial's reduction of technical failure from 19 to 1.7 per cent is the

single most clinically meaningful number in the field [31,32,44]. A split-mouth pilot adds the most direct comparison available, treating one calcified lower incisor conventionally and its contralateral partner with a guide in the same patient, and reporting 7.8 against 5.3 mm^3 of intracanal volume and 69 against 14 min to reach the orifice [45]. For posterior teeth the same claim is not yet supportable at the same level; posterior access is complicated by limited interocclusal space for the guide and bur assembly, by metal artefact from adjacent restorations, and by the curvature that constrains any straight-trajectory technique [11,89]. For microsurgery the case rests on a different outcome and on trials of 24 and 28 patients: the two randomised trials that include a conventional comparator agree that guidance shortens the procedure substantially, by roughly a third, and that healing is at least as good as without it [48,50], while the two modality comparisons show static and dynamic navigation to be interchangeable on patient-centred and accuracy outcomes [47,49]. That is a different claim from the accuracy claim made for access, and it is arguably more useful, since operating time in apical surgery is a direct determinant of morbidity. For post removal the laboratory evidence is strong and internally consistent, and the operator-independence finding is clinically attractive, but no clinical comparative study exists [20,94].

Separated instrument retrieval requires an explicit negative statement, because the field has tended to absorb it into the general enthusiasm. The only comparison available is indirect: a systematic review of seven in vitro studies, itself not a charted study under this review's criteria, reports more iatrogenic errors and longer procedures with the guided approach than with the freehand approach for this indication [33]. The two laboratory studies charted here under that heading do not by themselves settle it, and that is the point: an indication is being promoted on the strength of a synthesis that its own primary literature does not yet support. The plausible explanation is geometric. Retrieval requires working around a fragment whose position is known but whose surrounding dentine is minimal, so the rigidity that makes a template safe for approaching a canal becomes a liability when the trajectory must be adjusted tactilely. Until that is addressed by design rather than by extrapolation, guided retrieval should be regarded as experimental.

4.5. Heterogeneity and Its Clinical Implications

The ranges reported in Tables 2–5 are wide, and the width is not noise. The charted studies differ on at least nine dimensions, and each of them moves the result. Tooth type and position are the first two: posterior teeth give larger deviations than anterior teeth in every dataset that stratifies by site [80,81,83], because the guide is seated further from the target, the interocclusal space constrains the bur and sleeve assembly, and the planned trajectory is more often oblique to the long axis. The degree and the nature of obliteration are the third: most laboratory work either uses teeth without obliteration [8,13,79] or simulates it in printed resin [9,15,88,89,93], and only a minority uses extracted teeth with naturally calcified canals [90,92]; a printed replica removes the tactile feedback and the dentine heterogeneity that make the clinical procedure difficult, which is one reason laboratory location rates run higher than clinical ones. Operator experience is the fourth, and it acts in both directions: guidance narrows the gap between novices and experts [9,20,114,115], yet the gap does not close in posterior teeth [97] and proficiency with dynamic navigation took 18 to 28 cases in the one series that measured it [126]. The guidance system, the sleeve design, the imaging protocol and the printing workflow are the next four, and Section 3.2 shows that each of them alone shifts angular deviation by an amount of the same order as the difference between static and dynamic guidance [63,64,68–70]. The ninth is the outcome itself: angular deviation is reported at the bur base, at the tip or as a three-dimensional vector; linear deviation at the entry point, at the apex or as a total; and clinical success as

anything from reaching the canal lumen to radiographic healing at twelve months, so two studies that both report deviation may not be reporting the same quantity.

Three consequences follow for the reader. First, values should not be compared across studies, even within one table, unless the studies match on these dimensions; a static guide tested in printed anterior models at 80 µm voxel size and a dynamic system tested in extracted molars at 200 µm are not measuring the same thing, and the ordering of modalities in Section 3.3 should be read with that in mind. Second, this heterogeneity is the reason the review did not pool. The apparent precision of a pooled mean would have been false, and the published meta-analyses that do pool report high heterogeneity statistics for exactly this reason [29,103]. Third, and clinically most important, a published accuracy figure describes one combination of tooth, model, operator, system and workflow, and a practice that departs from that combination on any dimension should expect a different result. The failure analyses in Section 3.7 are in effect a record of what happens when one link in the chain, usually imaging or guide fit, falls outside the conditions under which the accuracy data were generated [124,125].

The implication for research is that reporting, rather than further accuracy experiments, is the bottleneck. Until tooth type, obliteration status and how it was produced, operator experience, guidance system, sleeve dimensions, voxel size, printing parameters and the exact definition of each outcome are reported routinely, comparisons between studies will remain indirect and any future systematic review will face the obstacle this one did. The reporting items listed in Section 4.9 are drawn directly from the dimensions identified here.

4.6. Structural Constraints

Several limitations are structural and will not be removed by further studies. Guided access depends on a straight or nearly straight trajectory and cannot follow a curved root [3,11]. It mandates CBCT, and the dose is not negligible: effective doses for mandibular incisor protocols range from 53 to 175 µSv at standard tube currents and from 8.6 to 18 µSv at reduced currents, the lowest setting corresponding to roughly six periapical radiographs [140]. Since the technique is disproportionately applied to traumatised teeth in young patients, dose optimisation is not a peripheral consideration, and the demonstration that reduced-current protocols retain diagnostic adequacy is directly relevant. Two qualifications follow. CBCT is not a routine prerequisite of a guided procedure to be ordered because the technique is planned; it is a diagnostic examination that must be justified for the individual patient under the ALARA and ALADA principles, on the basis that conventional imaging has failed to answer the clinical question, and in most reported cases of obliteration the scan was already indicated for diagnosis before guidance was considered. Where a scan is justified, the smallest field of view and the lowest dose that retain adequate resolution for planning should be used, and the additional intraoperative exposures of the guided workflow should be counted as part of the dose attributable to the procedure. The radiographic burden of the guided workflow itself, 31 additional intraoperative exposures across 54 teeth in the one study that recorded it, should be added to that account and should be reported routinely [90]. Cost and access form the second structural constraint and are taken up separately in Section 4.7.

4.7. Economic and Practical Considerations

No charted study reported treatment cost, and this review found no cost-effectiveness analysis of guided endodontics in any indication. The search was not designed to retrieve economic evaluations, so one may exist, but if it does it is not indexed under the terms the field uses for itself. What can be said about cost therefore has to be assembled from the technical reports. Static guidance requires a CBCT scan of adequate resolution, a surface

scan, planning software, a printer with biocompatible resin and, in most designs, a metal sleeve; the fixed investment falls on the practice, and the per-case cost consists of the scan, the planning time and the guide. Dynamic navigation replaces the printed guide with a tracking system and a registration step; the fixed cost is higher, the per-case consumable cost is lower, and planning can be done at the chair [36,74]. Robotic systems raise the fixed cost again and are at present confined to a small number of centres [15,16,117]. None of this has been reported in a form that allows a cost per case to be derived for any modality, and that absence is itself a finding of the review.

Time is the better-documented part of the burden, and it is distributed unevenly across the workflow. Planning took roughly 11 min in one software workflow and 20 in another, and the more accurate of the two was the slower and needed three times as many operator inputs [64]. Chairside time with a guide was shorter than freehand in some comparisons [9,20,88], longer in others [82,90] and equivalent in a third group [84], so time saved at the chair cannot be assumed to offset time spent at the screen. The guided workflow also generated 31 additional intraoperative radiographs across 54 teeth in the one study that counted them [90], a cost in dose and in time that no other study has recorded. The learning curve is a further expense: an experienced endodontist needed 18 to 28 cases to reach proficiency with dynamic navigation [126], and no equivalent measurement exists for static guidance or for the planning step, which is where a general practitioner without prior digital experience is most likely to lose time.

Several reported developments lower the threshold, and they deserve more systematic evaluation than they have received, because they determine whether the technique diffuses beyond specialist centres. Desktop printers produced functional guides [75]; sleeveless and open-frame designs remove the metal sleeve, and with it a manufacturing step, without an accuracy penalty [76–78]; simplified single-visit workflows reduce dependence on a laboratory [65–67]; and automated canal segmentation cut the most labour-intensive planning step from roughly 38 min to under one [122]. Against these stand the requirements that will not change: a CBCT unit and a surface scanner, or access to both, and a trajectory that must be planned by someone who understands the anatomy and the software. In practical terms the present evidence supports a referral model, in which the indications at level A2 are treated with guidance in centres that hold the equipment and the case volume to stay on the learning curve, and it does not yet support an expectation that the technique will be available in routine general practice. Whether that expectation is reasonable will depend on cost and time data the field has not produced, and collecting them should be a condition of future clinical trials.

4.8. Limitations of This Review

Seven limitations apply. The electronic search covered PubMed/MEDLINE only. This was a deliberate choice, taken so that the whole yield could be screened in duplicate and the entire process reproduced from the published string, but it will have missed studies indexed only in Scopus, Web of Science or Embase, and it under-represents journals outside MEDLINE; complementary searching added eight eligible studies that the string had not retrieved, which marks the limit of its sensitivity. Second, restriction to English-language publications excluded four otherwise eligible primary studies published in Chinese and Hungarian. This is a source of language bias, and it is material because the review itself identifies a geographic concentration of research groups: the Chinese-language literature in particular is likely to contain further laboratory and clinical work on dynamic navigation and robotic systems, and the evidence map should be read as a map of the English-language literature rather than of the field as a whole. Third, scoping methodology does not permit the pooling of effect estimates, so the accuracy and substance-loss ranges reported here are

descriptive and should not be read as summary estimates; where meta-analytic estimates exist they have been cited as such. Fourth, the exclusion of reviews as data sources, although methodologically necessary, means that any primary study captured only inside a review and not independently retrievable in full text will have been missed. Fifth, and most consequentially, the evidence-maturity classification applied here is a pragmatic instrument constructed for this review rather than a validated one, and the boundary between level C and level D in particular required judgement in studies combining a laboratory arm with an illustrative clinical case. Sixth, the review includes critical appraisal and an evidence-level classification, which some readers may regard as extending beyond the mapping purpose of a scoping review; the reasons for including them and the boundaries that were kept are set out in Section 2.1, and the indication map in Table 1 and the charted values in Tables 2–5 can be read independently of the appraisal by readers who prefer to do so. Seventh, publication bias is likely to affect the case-report and case-series strata, which make up half of the included studies: unsuccessful guided cases are less likely to be written up or accepted, and no instrument used here can detect that. The direction of the bias is towards overstating success in the indications at levels C and D, which is a further reason for the caution with which those indications are described.

4.9. Research Priorities

The priorities that follow from this map are specific rather than general. Randomised comparison of guided against specialist-performed freehand access in posterior teeth would resolve the single largest gap, since posterior anatomy is where the technique is both most needed and least validated. Clinical comparative studies of guided post removal would convert a strong laboratory case into a usable recommendation. Long-term survival data, at five years or more, on teeth treated with guided access would test the tissue-conservation rationale directly rather than by inference. A purpose-designed guide geometry for separated instrument retrieval, or an explicit statement that the indication is unsuitable, would close the one clearly negative finding. Adequately powered randomised trials in microsurgery are needed to replace the four small trials of 18 to 60 patients now carrying that indication, and two of those four lack a conventional comparator altogether. On the third subsidiary question of Section 2.2, the two indications at level A2 are the only ones for which a systematic review with quantitative synthesis is now conceivable, and even there it would require the trials to report a common outcome at a common time point, which at present they do not; for the other six indications a systematic review would have nothing to synthesise. Two reporting reforms would cost nothing and would change the interpretability of everything that follows. Trials should report how the randomisation sequence was generated and whether allocation was concealed, and should register prospectively; at present neither is standard, and that alone prevents any of the six existing trials from being rated at low risk. Laboratory and clinical studies alike should blind the outcome assessor to the allocation and say that they have done so, since not one study in this review reports it. Beyond that, voxel size, printing parameters, sleeve dimensions, drill diameter, planning software, additional radiographic exposures and operator experience should all become mandatory reporting items, since this review found each of them to be an independent determinant of accuracy and each of them to be unreported in a substantial minority of studies.

5. Conclusions

The evidence for guided endodontics is stratified by indication, and it should be read that way. Laboratory work establishes that the technique can be executed with a precision that freehand access does not reach and with less loss of dentine, but that is a statement about execution, not about benefit. Randomised clinical evidence exists for two indications

only, access to obliterated anterior and single-rooted canals and endodontic microsurgery, and in both it comes from small trials with reporting deficiencies that keep every one of them short of low risk of bias; those two indications are clinically supported, not proven. The remaining six rest on laboratory data, case series and case reports, and, for one of them, retrieval of separated instruments, the only comparative evidence points the other way. What the field needs is not another demonstration of accuracy but trials that report their randomisation and blind their assessors, follow-up long enough to test whether conserved dentine translates into tooth survival, cost and time data that would allow the technique to be placed in practice, and reporting of the technical and operator variables that this review found to govern the result. Until then, statements about guided endodontics should name the indication they refer to.

Supplementary Materials: The following supporting information can be downloaded at https://www.mdpi.com/article/10.3390/dj14110721/s1, Table S1: PRISMA-ScR checklist; Table S2: Full search strategy; Table S3: Summary of risk of bias and critical appraisal across design strata.

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