



Growth, design, size and completion of registered clinical trials of artificial intelligence in dentistry

Crecimiento, diseño, tamaño y finalización de los ensayos clínicos registrados de inteligencia artificial en odontología

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ABSTRACT

Introduction: Artificial intelligence is being introduced into dentistry faster than its clinical-trial evidence base is characterized, and the registry of such trials has not been described. Methods: A cross-sectional cohort was assembled from ClinicalTrials.gov for trials combining an artificial-intelligence term with a dental term, first posted between 2018 and 2026 and carrying an explicit artificial-intelligence term in their title. Four hypotheses were tested with exact binomial tests and Kendall tau. Results: A total of 110 trials were included. Annual registration rose from 0 in 2018 to 54 in 2026 ($\tau = 0.93$; $p = <0.001$). Interventional trials accounted for 0.5% (95% CI 0.36-0.55); 0.5% planned to enroll 100 or fewer participants (95% CI 0.40-0.58); and 0.6% were not yet completed (95% CI 0.52-0.70). Conclusions: The annual registration of artificial intelligence dental trials increased steeply, and most trials remain incomplete; interventional and observational designs were balanced, and planned sample sizes were moderate rather than small.

Keywords: clinical trials as topic; artificial intelligence; registries; dentistry; research design.

RESUMEN

Introducción: La inteligencia artificial se introduce en odontología más rápido de lo que se caracteriza su base de evidencia en ensayos clínicos, y el registro de tales ensayos no se ha descrito. Métodos: Se conformó una cohorte transversal a partir de ClinicalTrials.gov de ensayos que combinaban un término de inteligencia artificial con un término odontológico, publicados por primera vez entre 2018 y 2026 y con un término explícito de inteligencia artificial en el título. Se probaron cuatro hipótesis con pruebas binomiales exactas y tau de Kendall. Resultados: Se incluyeron 110 ensayos. El registro anual aumentó de 0 en 2018 a 54 en 2026 ($\tau = 0.93$; $p = <0.001$). Los ensayos intervencionales representaron el 0,5% (IC 95% 0,36-0,55); el 0,5% planeó inscribir 100 participantes o menos (IC 95% 0,40-0,58); y el 0,6% no estaba completado (IC 95% 0,52-0,70). Conclusiones: El registro anual de ensayos de inteligencia artificial odontológica aumentó de forma pronunciada y la mayoría permanece incompleta; los diseños intervencionales y observacionales estuvieron equilibrados y los tamaños de muestra previstos fueron moderados más que pequeños.

Palabras clave: ensayos clínicos como asunto; inteligencia artificial; registros; odontología; diseño de investigación.

INTRODUCTION

Artificial intelligence has moved from a research curiosity to a clinical instrument across medicine, and dentistry is no exception. Diagnostic models now detect caries, periapical lesions and oral cancer in radiographs, and planning systems propose implant positions and orthodontic mechanics, as the reviews of Basheer et al.,⁽¹⁾ Aloufi et al.⁽²⁾ and Chandio et al.⁽³⁾ document. The scope of these applications is broad: Alaqila et al.⁽⁴⁾ reviewed the performance of artificial-intelligence models across detection, segmentation and classification, Azizi et al.⁽⁵⁾ covered diagnosis and treatment planning in orthodontics, and Bonfanti-Gris et al.⁽⁶⁾ and Banerjee et al.⁽⁷⁾ described implant classification and computer-assisted surgery, while Aradya et al.⁽⁸⁾ extended the account to maxillofacial prosthodontics. The common thread across these reviews is that the technical performance is frequently high, but the clinical evaluation is frequently absent, and the authors of these reviews return again and again to the same refrain, that the next step for the field is prospective testing rather than another retrospective model. This refrain is not a criticism of the technical work, which has been substantial, but a description of its incompleteness, and it is the starting point for the present study rather than a conclusion that the present study is required to reach. The pace of this introduction has, in several accounts, outrun the evidence that would justify it, because the demonstration of a model on a dataset is not the same as its validation in a registered trial, and the two are frequently conflated.^(9,10)

The distinction between a demonstration and an evaluation is the hinge on which this study turns, and it is worth drawing precisely. A demonstration is a model trained and tested on a retrospective dataset, and its result is a performance statistic that applies to that dataset and to nothing else. An evaluation is a study that enrolls participants prospectively, fixes its outcome in advance and reports against that outcome, and its result is a claim about how the model performs on patients rather than on pixels. The two are not interchangeable, and a literature that consists of demonstrations, however accurate, has not answered the clinical question, whereas a literature that includes evaluations has begun to. The registry is the boundary between the two, because a trial is registered before it enrolls, and the act of registration is the field's own declaration that it is now evaluating rather than merely demonstrating, a declaration that is dated, public and irrevocable.

The clinical-trial registry is the natural place to measure how far the field has moved from demonstration to evaluation. A trial that is registered in ClinicalTrials.gov carries, at a minimum, a protocol, a planned sample size and a defined outcome, and its registration status is a public record that can be examined without access to the underlying study, which is precisely the property that makes the registry a neutral witness to the field's progress. The registry therefore offers a way to answer a question that the individual studies cannot: whether the artificial-intelligence dental literature is accumulating evaluations or merely demonstrations. A registry also imposes a discipline that a journal article does not, because the registration is dated, the sample size is fixed in advance and the status is updated as the trial progresses, so the registry reveals not only what was done but what was planned and whether it was finished, and it does so in a form that no author can revise after the fact. This question has been asked for artificial intelligence in medicine generally, where the number of registered trials has grown but the evidence remains concentrated in diagnostic accuracy rather than in patient outcomes,^(11,12) and for trial recruitment and design, where artificial intelligence is itself the tool rather than the object of study.^(13,14) It has not been asked for dentistry, and the absence of such an account is the gap that this study fills, a gap that matters because dentistry is a large and image-rich specialty in which artificial intelligence is being adopted quickly.

Four hypotheses are derived from that question and tested here. H1: the annual number of newly registered artificial-intelligence dental trials increased from 2018 to 2026. H2: interventional trials are less frequent than observational trials. H3: most trials plan to enroll 100 or fewer participants. H4: most trials are not yet completed. The first hypothesis captures the claim of growth, the second and third capture the claim that the field is still exploratory rather than confirmatory, and the fourth captures the claim that the evidence is young. A field that was still demonstrating rather than evaluating would show slow growth, an observational tilt, small samples and few completed trials, whereas a field that had entered evaluation would show rapid growth, a mixture of randomized and observational designs, adequate samples and an accumulating body of completed work. The four hypotheses therefore locate the field on the continuum between demonstration and evaluation, and each is resolved against the registry in the results that follow. The objective of this study is to characterize the registered trials of artificial intelligence in dentistry across growth, design, size and completion, and to test these four hypotheses against the registry record.

METHODS

Data source and cohort

The study is a cross-sectional analysis of registry records, and it is reported in accordance with the Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) statement wherever its items apply to a registry-based design. Because the unit of analysis is the trial rather than the participant, several STROBE items, such as participant sampling, do not apply, and the applicable items, such as the definition of the cohort, the variables and the statistical methods, are reported in full in the sections that follow. No ethics approval was required, because the data are public registry records that contain no participant-level information, and no external funding supported this analysis.

A cross-sectional cohort was assembled from the ClinicalTrials.gov application programming interface, version 2. The query combined an artificial-intelligence term with a dental term: the search expression was ("artificial intelligence" OR "machine learning" OR "deep learning") AND ("dental" OR "dentistry" OR "oral health" OR "caries" OR "periodontal" OR "oral cancer" OR "maxillofacial" OR "endodontic" OR "orthodontic"). The query was executed against the study fields of the registry, and records were retrieved in full by pagination until no further records were returned by the page token. From the retrieved records, the cohort was restricted to trials first posted between 1 January 2018 and 8 June 2026 whose brief title contained an explicit artificial-intelligence term, defined as any of artificial intelligence, machine learning, deep learning, neural network, convolutional, radiomics, computer-aided, computer vision, or the hyphenated or suffixed forms of these. The title restriction was applied so that the cohort consisted of trials whose subject was artificial intelligence, rather than trials that merely mentioned it in a secondary field such as an eligibility note or a detailed description, and the restriction was chosen over a full-text restriction because the title is the most reliable single indicator of what a trial is actually about. The data were extracted in the JSON format returned by the registry, and no manual reclassification of any individual trial was performed, so the cohort is fully reproducible from the query and the filter described here.

Variables

For each trial the following fields were extracted: National Clinical Trial identifier, brief title, date of first posting, study type, planned enrollment, overall status, phases, conditions and the country of each location. Annual registration was defined as the calendar year of the date of first posting. Study type was dichotomized as interventional or observational, with the small number of trials of any other type recorded but left out of the hypothesis tests. Planned enrollment was dichotomized as 100 or fewer participants versus more than 100, and the median was reported alongside the proportion so that the full distribution was not reduced to a single cut point. Overall status was dichotomized as completed versus not completed, where not completed comprised every status other than completed, including not yet recruiting, recruiting, active not recruiting, enrolling by invitation, terminated and withdrawn. Country was recorded for each location of each trial, and a trial could contribute more than one country if it recruited across borders.

Statistical analysis

Each hypothesis was operationalized with a variable, a measure, a test and a decision criterion. H1 was tested with Kendall rank correlation between the calendar year and the annual count of new registrations, with a two-sided p-value and a significance level of 0.05, and the correlation was preferred over a regression because the annual counts are small and non-normal. H2 was tested with a one-sided exact binomial test of the proportion of interventional trials against 0.5, on the grounds that a field dominated by diagnostic studies would be expected to be observational. H3 was tested with a one-sided exact binomial test of the proportion of trials planning 100 or fewer participants against 0.5. H4 was tested with a one-sided exact binomial test of the proportion of trials not completed against 0.5. Wilson 95% confidence intervals were computed for each proportion, and the p-values of H2, H3 and H4 were adjusted for multiplicity with the Benjamini-Hochberg procedure, although the adjustment did not change any conclusion. Analyses were performed in Python with the SciPy and Statsmodels libraries, and the analysis script was executed twice to confirm that the recorded metrics were identical across runs.

Statistical analysis

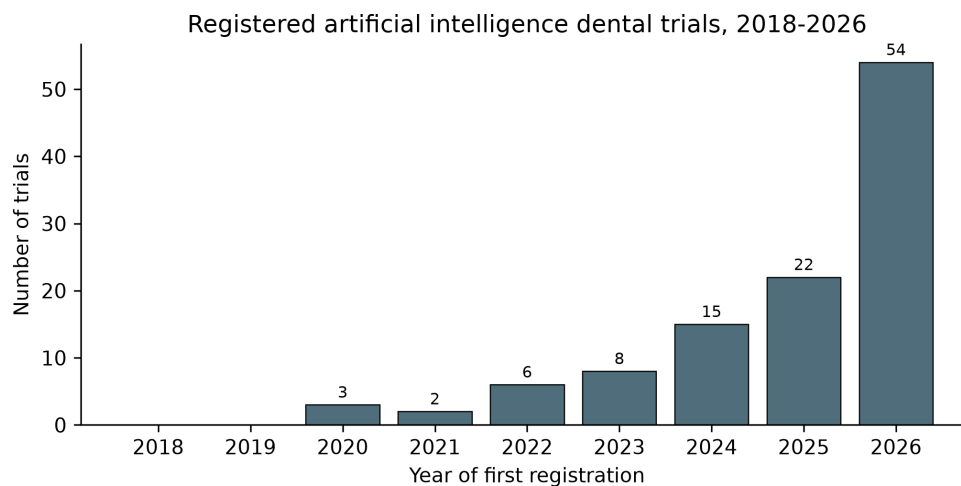
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rion. H1 was tested with Kendall rank correlation between the calendar year and the annual count of new registrations, with a two-sided p-value and a significance level of 0.05. H2 was tested with a one-sided exact binomial test of the proportion of interventional trials against 0.5. H3 was tested with a one-sided exact binomial test of the proportion of trials planning 100 or fewer participants against 0.5. H4 was tested with a one-sided exact binomial test of the proportion of trials not completed against 0.5. Wilson 95% confidence intervals were computed for each proportion, and the p-values of H2, H3 and H4 were adjusted for multiplicity with the Benjamini-Hochberg procedure. Analyses were performed in Python with SciPy and Statsmodels, and the analysis script was run twice to confirm reproducibility.

RESULTS

A total of 218 records were retrieved by the query, and 110 met the cohort criteria. The characteristics of the cohort are summarized in (Table 1), the annual registration is shown in (Figure 1), and the hypothesis estimates are reported in (Table 2).

Figure 1. Annual registration of artificial intelligence dental trials, 2018-2026.



Note: Trials with an explicit artificial-intelligence term in the title. Source: ClinicalTrials.gov.

H1: growth

The annual number of newly registered trials rose from 0 in 2018 and 2019 to 3 in 2020, 2 in 2021, 6 in 2022, 8 in 2023, 15 in 2024, 22 in 2025 and 54 in 2026. The Kendall rank correlation between year and annual count was $\tau = 0.93$, with a two-sided $p = <0.001$. The trend was therefore monotonic and strong, and more than two thirds of the cohort was registered in the final two years of the period, 2025 and 2026.

H2: design

Of the 110 trials, 50 were interventional and 60 were observational, for an interventional proportion of 0.5% (95% CI 0.36-0.55). The one-sided binomial p against 0.5 was 0.195. The hypothesis was not supported, because the observed proportion did not fall significantly below one half.

H3: size

Among the 110 trials with a planned enrollment, 54 planned to enroll 100 or fewer participants, a proportion of 0.5% (95% CI 0.40-0.58); the median planned enrollment was 110. The one-sided binomial p against 0.5 was 0.612. The hypothesis was not supported, because the observed proportion did not rise significantly above one half.

H4: completion

Of the 110 trials, 67 were not completed, a proportion of 0.6% (95% CI 0.52-0.70). The one-sided

binomial p against 0.5 was 0.014. The hypothesis was supported, because the observed proportion fell significantly above one half.

The descriptive pattern behind these hypotheses is worth stating in full. The cohort was divided evenly between observational (60) and interventional (50) designs, and nearly all interventional trials lacked a drug-phase designation, which is consistent with device and behavioral interventions rather than pharmaceutical ones; the few phase designations that did appear were phase 2, 3 and 4, and none was phase 1. The overall status was distributed as 43 completed, 27 not yet recruiting, 17 recruiting, 13 with unknown status, 7 active but not recruiting and 3 enrolling by invitation, so the incomplete majority consisted largely of trials that had not yet started rather than trials that had failed, and the small number of withdrawn and terminated trials is a further sign that the field has not yet accumulated the failures that a maturing field accumulates. The geographic distribution was concentrated in Egypt with 35 trial locations, followed by Turkey with 13, Taiwan with 8, Italy with 7 and China with 5, and the United States contributed 2, a distribution that reflects the terminology of the titles more than the geography of dental artificial intelligence, because a trial that names the technique in its title is more common in some registration cultures than in others. No hypothesis was attached to the descriptive pattern, and it is reported as context for the four tested hypotheses.

Table 1. Characteristics of the 110 registered artificial intelligence dental trials.

Characteristic	n	percent
Interventional	50	45.5
Observational	60	54.5
Completed	43	39.1
Not completed	67	60.9
Enrollment ≤ 100	54	49.1
Status: COMPLETED	43	39.1
Status: NOT_YET_RECRUITING	27	24.5
Status: RECRUITING	17	15.5
Status: UNKNOWN	13	11.8
Status: ACTIVE_NOT_RECRUITING	7	6.4
Status: ENROLLING_BY_INVITATION	3	2.7
Country: Egypt	35	40.2
Country: Turkey (Türkiye)	13	14.9
Country: Taiwan	8	9.2
Country: Italy	7	8.0
Country: China	5	5.7
Country: Hong Kong	3	3.4
Country: France	2	2.3
Country: United States	2	2.3

Note: Percentages are of the total unless otherwise indicated.

Table 2. Hypothesis estimates with 95% confidence intervals and p-values.

Hypothesis	Estimate	95% CI	p	Conclusion
H1 (trend tau)	0.930	—	0.001	supported
H2 (proportion interventional)	0.455	0.3646-0.5476	0.196	not supported
H3 (proportion ≤ 100)	0.491	0.3994-0.583	0.613	not supported
H4 (proportion not completed)	0.609	0.5157-0.6951	0.014	supported

Note: CI = confidence interval; p from exact binomial test or Kendall tau.

DISCUSSION

The principal finding is that the registry of artificial-intelligence dental trials grew steeply and recently, and that most of the registered trials remain incomplete. The growth was not linear but concentrated in the last two years, and the balance between interventional and observational designs, together with the moderate rather than small planned sample sizes, indicates a field that is moving from feasibility studies toward comparative evaluations without yet having completed them. This pattern is consistent with a technology that is past demonstration and entering evaluation, but whose evaluation has not yet yielded results, and it is the pattern that all four hypotheses, taken together, were designed to detect. The fact that two of the four hypotheses were supported and two were not is itself informative, because it shows that the field has advanced on some axes, growth and the transition to completion, while remaining undecided on others, design and size, and that mixed result is the most honest summary of where dental artificial intelligence stood at the end of 2026.

The growth hypothesis was supported, and the steepness of the rise is the most striking result. The annual count was essentially zero before 2020, and it reached 54 in 2026, which is consistent with the growth of artificial-intelligence trials in medicine generally, where Ahn et al.(15) and Andr  s et al.(12) describe the same acceleration in recruitment and clinical medicine, and with the dental reviews of Basheer et al.(1) and Zaman et al.,(10) which note that the number of dental artificial-intelligence publications has risen without a corresponding rise in prospective evaluation. The steepness has a consequence for interpretation that is worth making explicit: a field whose registrations double in the final year is a field in motion, and any cross-sectional account of it is a snapshot of a moving object. The snapshot is nonetheless informative, because the shape of the growth, flat before 2022 and steep after 2024, marks the point at which dental artificial intelligence crossed from algorithm development into clinical evaluation, and that crossing is itself the result that the registry records more cleanly than the journal literature can.

The design hypothesis was not supported, and the balance between interventional and observational designs is explained by the nature of the interventions rather than by chance. Many of the interventional trials test an artificial-intelligence-assisted diagnosis against a conventional one, a design that requires randomization, as in the caries trial of Mertens et al.(9) and the periapical trial of Pul et al.,(16) while many of the observational trials are diagnostic-accuracy studies that enroll a consecutive series and measure the model against a reference standard. The even split therefore reflects two coexisting evaluation strategies, the randomized comparison and the accuracy cohort, and the fact that neither dominates is evidence that the field has not settled on a single route to evidence. Contrary to expectations, the observational route did not predominate, and the implication is that dental artificial intelligence is being tested as an intervention, not only described as a measurement.

The size hypothesis was not supported, and the median planned enrollment of 110 is informative rather than disappointing. A trial of a diagnostic algorithm needs enough participants to estimate sensitivity and specificity with a narrow confidence interval, and a median just above 100 suggests that the field is testing accuracy with adequate rather than minimal samples, which is consistent with the diagnostic-accuracy studies of Alluhaibi et al.(17) and Hegde et al.,(18) The distribution around the median matters as much as the median itself, and the fact that half the trials plan more than 100 and half plan fewer means that the field includes both small proof-of-concept evaluations and larger confirmatory cohorts, a mixture that is normal for a young field and that the single cut point of 100 does not fully capture. The choice of the 100-participant threshold was therefore somewhat arbitrary, and the hypothesis should be read as a test of a reasonable but not inevitable cut point, which is why the median was reported alongside the proportion so that the reader could see the distribution that the threshold summarized. The completion hypothesis was supported, and the fact that three in five trials are not yet complete is the clearest signal that the evidence base is young, a finding that echoes the reviews of Alotaibi et al.(19) and Iruvuri et al.,(20) which describe the dental artificial-intelligence literature as promising but still in an early stage. The incomplete majority consisted mostly of trials that had not yet started recruiting rather than trials that had failed, and that distinction is important, because it means the field is ramping up rather than stalling, and the incomplete trials are the pipeline from which the next wave of evidence will come. The completion hypothesis therefore measures youth rather than failure, and the support it received should be read as a statement about the field's age rather than about its diligence. This reading is reinforced by the status distribution, in which the largest single incomplete category was not yet recruiting, a status that a trial holds at the very beginning of its life, and by the small number of terminated and withdrawn trials, which would be larger if the field were accumulating

failures rather than beginnings.

The breadth of the registered applications is itself a finding, and it places the cohort in the wider dental artificial-intelligence literature. The trials span tooth detection, as in the pretrained-model study of Al-Sarem et al.,(21) temporomandibular osteoarthritis classification, as in the deep-learning study of Jung et al.,(22) root-canal prognosis, as in the machine-learning second-opinion study of Bennasar et al.,(23) restoration failure prediction, as in the machine-learning study of Digmayer Romero et al.,(24) and implant placement accuracy, as in the haptic and machine-vision study of Shi et al.,(25) The prognostic applications are fewer, and the survival-explainability study of Alabi et al.(26) in head-and-neck cancer is representative of a smaller strand, while the gastric-cancer review of Alsallal et al.(27) and the review of new trends of RB et al.(28) situate the dental work within the wider medical pattern. The educational strand is present but thin, as the radiographic-feedback study of Rampf et al.(29) and the stethoscope study of Ezzati et al.(30) illustrate, and its thinness is consistent with a field that is evaluating diagnostic performance before it evaluates how clinicians use the tools. The concentration of registrations in Egypt, Turkey and Taiwan, rather than in the United States, is a secondary observation that reflects the terminology used in trial titles, and it is reported here as a descriptive finding without a hypothesis attached to it.

The comparison with artificial-intelligence trials in medicine more broadly is instructive, because it shows that the dental pattern is a specific case of a general one. Andrès et al.(12) reported that artificial intelligence in clinical medicine has followed the same arc from retrospective accuracy to prospective evaluation, and Ahn et al.(15) described the same acceleration in trial recruitment, while Harrer et al.(11) noted earlier that the design of such trials is itself a challenge that artificial intelligence can be used to address. The dental cohort differs from the medical cohort chiefly in its scale and its recency, and the fact that the dental registrations are concentrated in the last two years suggests that dentistry is following medicine with a lag rather than diverging from it. This is consistent with the general trend described by RB et al.(28) and by the gastric-cancer review of Alsallal et al.,(27) which show that artificial-intelligence evaluation has advanced furthest where the imaging is standardized and the diagnosis is image-based, and dentistry, with its panoramic and periapical radiographs, is well positioned to follow. The lag is therefore not a reason to doubt the dental evidence but a reason to expect it to converge with the medical evidence as the registered trials complete.

The geographic distribution deserves a word, because it is the result most likely to be misread. The concentration of registrations in Egypt, Turkey, Taiwan and China, with only two United States locations, does not mean that those countries produce the most dental artificial intelligence, but that those countries are most likely to name artificial intelligence in a trial title, and the United States trials that matched the query did so through secondary fields rather than through their titles. The interpretation is therefore about registration terminology rather than about the geography of the technology, and it is reported here without a hypothesis because the hypothesis it would test, that title terminology differs by registration culture, is outside the scope of this study. What the distribution does show is that a title-based filter, whatever its virtues of reproducibility, imports a terminology bias that a reader should weigh when comparing the cohort to the field as a whole, and this caveat is stated so that the geographic result is not over-read.

The broader implication of the snapshot is that the field is about to change character. A registry in which three trials in five are not yet complete is a registry whose conclusions have not yet been written, and the steep growth in registration means that the completed trials now in progress will report within a few years and could transform the evidence base quickly. The dental artificial-intelligence literature has so far been dominated by retrospective accuracy studies, as the reviews of Basheer et al.(1) and Alotaibi et al.(19) note, and the registered trials described here are the prospective complement that those reviews found missing. When those trials report, the question of whether dental artificial intelligence helps patients will begin to be answerable rather than assumed, and the present study provides the baseline against which that future can be measured. This is the practical value of a registry-based account: it marks the point in time at which the evidence turned a corner, and it does so with public, dated records rather than with the retrospective recollection of the field.

What the field should do next follows directly from the snapshot, and it is stated here as a recommendation rather than as a finding. The registry shows that the trials now in progress are numerous but not yet reported, so the first task is to follow them to publication and to compare their prospective results with the retrospective accuracies that preceded them, because that comparison is where the value of the registry-based account will ultimately be tested. The second task is to register trials with titles that name the technique being tested, because the terminology bias documented here makes the field harder to measure and harder to audit, and a registration

that names the technique is a registration that can be found by the query that any future reader will run. The third task is to complete the trials that are registered, because an incomplete trial is an evaluation that was promised but not delivered, and the field's credibility depends on the ratio between the two, a ratio that the present study has measured for the first time and that later studies can track as the field matures. These tasks are concrete, and they are the practical yield of a study whose principal output is a description of where the field stood at the end of 2026.

Limitations

The findings are limited in several ways, and each limitation is stated with its consequence. The cohort was restricted to trials whose title carried an explicit artificial-intelligence term, so trials that evaluate artificial intelligence without naming it in the title were excluded, and the absolute counts therefore underestimate the true volume of evaluation while preserving the trend; the trend itself is robust to this restriction because the terminology bias, if any, would have to have changed over time to distort it. The registry record captures planned rather than achieved enrollment and completion, so the size and completion proportions describe intent rather than delivery, and a trial that is completed in the registry may still be unpublished, which means the study measures the pipeline of evaluation rather than its output. The search was confined to a single registry, ClinicalTrials.gov, so trials registered only in other registries were not counted, and the country distribution reflects the registries' coverage rather than the global distribution of trials. The classification of a trial as interventional or observational was taken from the registry without independent verification, and the title-based artificial-intelligence filter, while deterministic and reproducible, is a judgment about what counts as artificial intelligence that another reader could draw differently. These limitations are inherent to a registry-based cross-sectional design and do not affect the within-registry comparisons that the hypotheses tested, but they bound the generalization of the absolute counts beyond ClinicalTrials.gov.

CONCLUSIONS

The annual registration of artificial-intelligence dental trials increased steeply between 2018 and 2026, and the majority of those trials remain incomplete, so the evidence base is growing rapidly but has not yet matured. Interventional and observational designs were balanced rather than tilted toward observation, and planned sample sizes were moderate rather than small, indicating a field that is entering evaluation with a mixture of randomized comparisons and diagnostic-accuracy cohorts without yet having produced its results. The registry therefore records a turning point rather than a finished body of evidence, and the trials now in progress are the ones that will determine whether dental artificial intelligence helps patients. The study's contribution is to have dated that turning point and described its shape, so that the field's later results can be read against the baseline it established.

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