Piezoelectric versus conventional osteotomy in orthognathic surgery: a systematic review stratified by timing of neurosensory assessment and by what was timed.

Abstract:

Purpose Piezoelectric osteotomy is promoted because it should spare the inferior alveolar nerve, yet comparative studies disagree. We asked whether timing of assessment and what was timed explain it.

Methods Systematic review per PRISMA 2020. PubMed/MEDLINE, Web of Science and LILACS were searched to 15 September 2026 for studies comparing piezoelectric or ultrasonic with conventional instrumentation in sagittal split, Le Fort I or bimaxillary surgery. Scopus screening is incomplete and no Scopus-only record contributes; Embase is unlicensed and CENTRAL unsearched. One reviewer screened; re-screening the PubMed set recovered five eligible studies missed on the first pass.

Results Twenty-one comparative studies were identified, six absent from previous syntheses; one tabulated as comparative in the latest review was excluded as single-arm. Eight of nine assessments at three months or earlier favoured piezoelectric osteotomy, including both studies with extractable counts (risk ratios 0.52, 95% CI 0.31 to 0.87, and 0.47, 95% CI 0.23 to 0.94); the ninth, the only analysis adjusted for amount of mandibular movement, favoured the saw. From six months, three assessments found no difference and three favoured piezoelectric osteotomy. Operating time followed what was timed: shorter for the osteotomy in three studies, longer or unchanged for the procedure in four.

Conclusion The operating-time contradiction is explained by what each study timed, as three primary reports state. The neurosensory result is a direction, not a finding: recovery appears faster, evidence of a lower final rate is not unanimous, and the only study at low risk of bias finds none. That distinction belongs in consent.

Keywords: orthognathic surgery; sagittal split ramus osteotomy; piezosurgery; inferior alveolar nerve; systematic review

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Introduction

Piezoelectric instrumentation entered craniofacial practice on a specific mechanistic claim: selective cutting of mineralised tissue spares adjacent neurovascular structures, so the inferior alveolar nerve should be less often injured during sagittal split ramus osteotomy than with a reciprocating saw, bur or chisel.

The claim is plausible and the stakes are high. Neurosensory disturbance is the commonest complication of sagittal split osteotomy, patients report it as the most troubling, and a proportion of it is permanent: a 2026 systematic review and meta-analysis of 47 studies and 5,406 patients puts the pooled one-year incidence of persistent disturbance at 21% (95% CI 13% to 32%) [2]. That review identifies older age, greater mandibular advancement and intraoperative nerve exposure or manipulation as risk factors, and finds sex, skeletal class, fixation method, third molars and concomitant genioplasty not to be associated. **It does not examine the osteotomy instrument at all** — which is where the present question sits.

The evidence has been contradictory for nearly two decades. Landes reported inferior alveolar nerve sensitivity preserved in 95% of piezoelectric cases versus 85% of conventional cases at three months, p=0.0003, with blood loss lower by more than 200 mL [1]. Bertossi reported

98.2% versus 92.7% preservation at six months [3]. Brockmeyer, using quantitative sensory testing in a split-mouth randomised design, found advantages across three parameters [4]. Against these, Köhnke found the piezoelectric osteotomy substantially faster — 17±6 versus 25±9 minutes, p<0.001 — but no neurosensory difference from six months onward [5], and Monnazzi found none at six months [6]. Sobol is routinely cited alongside these as a third null result, and is not one: time to functional sensory recovery did not differ (94.5 versus 101.5 days, p=0.20), but the highest sensory grade was reached by 18 of 20 piezoelectric sides (90%) against 11 of 20 conventional sides (55%), p=0.01, with an adjusted hazard ratio of 2.3 (95% CI 1.1 to 4.9), p=0.04, favouring piezoelectric osteotomy [7].

Previous syntheses have not resolved it. The dedicated meta-analyses date from 2017 and 2022 [8–12], and the most recent work on the neurosensory outcome, published in 2025, is a scoping review [13]: it maps the studies and their measurement methods, which is useful and which we rely on here for data from reports we could not access, but by design it does not pool estimates, weight by risk of bias, or grade certainty. A separate 2025 meta-analysis has settled the blood-loss question in orthognathic surgery, reporting a weighted mean difference of −81.73 mL (95% CI −97.30 to −66.16) in favour of piezoelectric instrumentation across six studies [14]; we therefore do not re-analyse blood loss and treat it as established.

The synthesis closest to the present question is a 2022 systematic review and meta-analysis of neurosensory dysfunction after sagittal split osteotomy with saw versus piezotome [15]. It searched four sources including LILACS and grey literature to September 2020, included six studies and 284 participants, rated the randomised evidence at high and the non-randomised evidence at critical or serious risk of bias, and found no significant difference at one week (risk ratio 0.99, 95% CI 0.90 to 1.08) or at three months (risk ratio 0.39, 95% CI 0.09 to 1.75). Its authors observe that follow-up beyond three months may reveal faster physiological recovery, and report longer operating time with the piezotome.

That observation is the starting point of the present review rather than its conclusion. Three things distinguish what follows. First, the 2022 search closed in September 2020, so it could not include the comparative studies published since, nor the 2025 scoping review whose extraction table we examine here. Second, its analysis pools across assessment times within each window rather than treating the time of assessment as the variable of interest; we stratify by it, because the direction of the published findings changes at that boundary. Third, its point estimate at three months (0.39) is compatible with an early advantage that its confidence interval cannot resolve with six studies, which is precisely the question a larger and time-stratified corpus can address.

Two questions remain open. First, whether the neurosensory disagreement is structural — a difference in *when* function was measured rather than in the instrument. Second, why operating-time results contradict one another so sharply, with reports of piezoelectric osteotomy being twice as fast [3], no different [1], and significantly slower [16].

Materials and methods

Protocol

PRISMA 2020. The protocol was specified before the searches were run but has not been registered in PROSPERO.

Eligibility criteria

Population: bilateral sagittal split ramus osteotomy, Le Fort I osteotomy or bimaxillary orthognathic surgery.

Intervention: piezoelectric device or ultrasonic bone scalpel. **Comparator:** reciprocating or oscillating saw, bur and/or chisel.

Outcomes: primary — neurosensory disturbance of the inferior alveolar nerve, analysed separately at ≤3 months and at ≥6 months. Secondary — osteotomy time, total operating time, bad split, oedema, pain, vascular injury. Blood loss was not re-analysed [14].

Designs: randomised trials including split-mouth designs, and comparative studies with both arms.

Excluded: animal and cadaveric studies [17, 18], finite-element analyses, technical notes without a comparison group, narrative reviews, and studies of piezoelectric use outside orthognathic osteotomy.

Information sources and search

Information sources. PubMed/MEDLINE (118 records), LILACS (4) and Web of Science Core Collection (141) were searched to 15 September 2026 and all retrieved records were de-duplicated and screened at title and abstract level. Scopus was searched on the same date and returned 149 records; those have been retrieved but de-duplication and screening are in progress, and no Scopus-only record contributes to the present synthesis. Reference lists of included studies and of previous syntheses were traced by hand. The full strategy for each database, with its execution date and record count, is reported verbatim in Supplementary Table S1.

Embase was not searched because it is not licensed by our institution. This is a recognised limitation: Embase indexes European and conference literature not covered by MEDLINE, and its Emtree vocabulary retrieves records MeSH does not. We mitigated it with Scopus and Web of Science, which overlap substantially with Embase for journal-published clinical studies, with LILACS for Latin American literature, and by hand-tracing all reference lists.

Search strings. In PubMed/MEDLINE, `(piezosurgery OR "piezoelectric surgery" OR "ultrasonic osteotomy" OR "piezoelectric") AND ("sagittal split" OR orthognathic OR "Le Fort")` returned 118 records; restriction to randomised controlled trials returned 6 and to clinical-trial filters 12. Three comparative studies were not retrieved by that string and were found by other routes: Kokuryo 2018 [19] and da Costa Senior 2020 [20] through the reference list of a previous scoping review [13], and Ruiz Valero 2021 [21] through a narrower neurosensory string restricted to 2019–2021. A fourth, Spinelli 2014 [22], **was returned by that string and was missed at title and abstract screening**; it was recovered in September through the reference list of an included report. We record both facts because the first shows that a single string is insufficient in this field and the second shows what single-reviewer screening costs, and because two of the first three had appeared in no previous synthesis.

Selection, extraction, appraisal

Screening, selection and extraction were carried out by a single reviewer; duplicate independent screening was not undertaken and no inter-rater agreement statistic is available. One eligible comparative study present in the PubMed set was missed at this stage and recovered later [22]; the correction is reported in full rather than absorbed silently, and re-screening of the PubMed set by a second reviewer is the first item of further work. To reduce single-reviewer error, every value entering the synthesis was checked against the source article where the full text was obtainable and against the indexed abstract where it was not, and the provenance of each value is identified in the tables. Extraction recorded the number of **nerves or sides at risk** rather than patients, the exact neurosensory instrument, the timing of each measurement, and the operational definition of "disturbance".

One record required adjudication: Cascino et al. describe their study as randomised in the title but as a retrospective comparison in the abstract [23]. The discrepancy could not be resolved from the indexed record. We treated the study as non-randomised in the primary analysis, which is the conservative choice, and pre-specified a sensitivity analysis excluding it.

Risk of bias: RoB 2 and ROBINS-I. Certainty: GRADE. Operator blinding is impossible in this comparison; we treated performance bias as unavoidable, while requiring blinded outcome assessment for a low-risk rating in that domain.

Synthesis

Random-effects models (DerSimonian–Laird) with risk ratios for dichotomous and mean differences for continuous outcomes; heterogeneity by Q, I^2 and τ^2.

Pre-specified subgroups: (a) timing of neurosensory assessment, ≤3 months versus ≥6 months; (b) assessment instrument — subjective report, Semmes-Weinstein monofilaments, two-point discrimination, quantitative sensory testing; (c) for operating time, whether the osteotomy alone or the entire procedure was timed.

Split-mouth studies [4–6, 16] were analysed preserving within-patient pairing. Treating the two sides of one patient as independent overstates precision, and we report whether previous syntheses of this literature did so.

Results

Included studies

Table 1. Comparative studies of neurosensory outcome. Every figure attributed to a primary report was verified against the full text of that report, with one exception: Bertossi 2013 is closed access with no institutional route available to us, and its figures were verified against the published abstract (marked ‡). Rows attributed to the scoping review [13] rather than to a primary report are marked †.

Study	Design	n	Assessment	Timing	Result
Landes 2008 (a) [1]	Prospective comparative with **historical controls** (piezoelectric 2005–2006, controls 2000–2005; mean age 21±3 vs 27±5 years)	50 piezo vs 86 conventional overall; blood loss and time compared within the bimaxillary subgroup only (**26 vs 65**)	Subjective sensation, light touch, pin-prick, static and moving two-point discrimination, all scored qualitatively by one unblinded examiner	3 months	Sensitivity preserved 95% vs 85%, p=0.0003 — **the mean of five qualitative tests, not a per-patient rate**. Blood loss 541±150 vs 773±344 mL, p=0.001. Bimaxillary procedure time 227±73 vs 238±61 min, p=0.5. **Isolated Le Fort I 96±32 vs 77±32 min, p=0.03, favouring the saw**
Landes 2008 (b) † [13]	Prospective	90	Light touch, pin-prick, two-point discrimination	3 months	2% vs 16% lacked recovery
Bertossi 2013 ‡ [3]	Described by its authors as a randomised controlled clinical trial; published in a technical-strategies section	110 in two groups; **group sizes not reported**	Inferior alveolar nerve sensation; method not specified in the abstract	6 months	Sensation retained 98.2% vs 92.7%; **denominator (patients or sides) not specified**. Mandible per side 3:31–5:02 vs 7:23–10:22 min; Le Fort I 5:17–7:55 vs 8:38–15:11 min — **ranges, with no dispersion and no significance test reported**. Blood loss given as categories (<300 mL vs 400 to >500 mL), not measured
Spinelli 2014 [22]	Prospective split-mouth, side allocated by alternation (not randomised); one senior surgeon, one examiner	12 patients / 48 osteotomies; neurosensory outcome on **24 mandibular sides per arm**	Five-grade subjective sensory scale	1 day, 1 week, 1, 3 and 6 months	Normal sensation 75% vs 38% at 1 month (**p=0.003**), 84% vs 58% at 3 months, 97% vs 72% at 6 months; no difference at 1 week (p=0.68). Blood loss per side 237.5±86.1 vs 311.7±97.0 mL, p=0.0367. **Whole-procedure time per side 163.6±41.2 vs 105.3±36.9 min, p=0.0018 — piezo 35% slower**
Monnazzi 2014 [6]	Prospective double-blind, split-mouth	20	Semmes-Weinstein	6 months	**No significant difference**
Brockmeyer 2015 [4]	Split-mouth randomised, **patient- and assessor-blinded**	37 at baseline; **32 at 1 month, 23 at 6 months, 8 at 1 year**	Quantitative sensory testing, DFNS protocol	Baseline, 1 month, 6 months, 1 year	Warm detection p=0.046; mechanical allodynia p=0.002; vibration detection p=0.030, all favouring piezo —

Study	Design	n	Assessment	Timing	Result
					but these are **main effects from two-way ANOVA across the pooled timepoints, and no post hoc test reached significance**; the authors describe the one-year result as a trend
Köhnke 2017 [5]	Randomised, split-mouth	50 patients / 100 osteotomies	Two-point discrimination, light touch	6–12 months	**No difference after 6 months** despite an early advantage. Osteotomy time 17±6 vs 25±9 min, p<0.001. Nerve exposure 68% vs 82%, n.s. (the report gives 81% in its abstract and 41/50 in its table). The two sides were operated by two different surgeons, and the difference between surgeon and technique was significant, p=0.036
Nabiev 2014 [24]	Comparative, non-randomised	127 (102 conventional vs 25 "improved method")	Electric pulp testing, blink reflex, CT, questionnaire	Not stated	Reports fewer disturbances with the improved method. **The improved method combines the piezoelectric device with dedicated splitters, so the instrument is confounded with a co-intervention**; no figures given in the record and the full text is in Russian. **No data extracted**
Shirota 2014 [25]	Retrospective, non-randomised, **analysed by ANCOVA** adjusting for sex, age, instrument, amount of setback, operating time and fixation method	59 (piezo 29 vs Lindeman bur and chisel 30); mandibular setback only	Semmes-Weinstein monofilaments	3 months	**Against piezoelectric osteotomy.** Semmes-Weinstein score significantly **higher** — that is, more disturbance — with piezosurgery, p=0.008; blood loss **not influenced** by the instrument. Authors conclude piezosurgery reduced neither blood loss nor neurosensory disturbance

Study	**Design**	**n**	**Assessment**	**Timing**	**Result**
Akbar 2017 [26]	Comparative split-mouth, each patient operated with both instruments	24 patients / **96 osteotomies** (48 maxillary, 48 mandibular)	Nerve function; method not specified in the record	**Not stated**	Nerve damage less frequent with piezo, p=0.009. Swelling less, p=0.041. Blood loss lower but **not significant**, p=0.091. **Surgery time longer with piezo, p=0.003**
Koba 2018 [27]	Retrospective comparative	44; mandibular setback only	Paraesthesia incidence	Immediately postoperative and 3 months	No difference immediately; **significantly less paraesthesia with piezo at 3 months**. **Osteotomy time and blood loss both significantly lower with piezo.** A few cases did not recover in either arm; neither blood loss nor osteotomy time correlated with palsy
Kokuryo 2018 [19]	Retrospective cohort	67 patients (ultrasonic 35, conventional 32); 70 vs 64 sides	VAS, light touch, two-point discrimination, thermal	3 months	Disturbance 16/70 sides (22.9%) vs 28/64 (43.8%). **RR 0.52 (95% CI 0.31 to 0.87)**; recovery significantly more common with ultrasonic
Ruiz Valero 2021 [21]	Retrospective observational	23 vs 13	Sensory testing by region	Not stated	All patients disturbed immediately; **time to recovery significantly shorter with ultrasonic, p=0.0001**; chin and lower lip most affected
~~da Costa Senior 2020~~	**Excluded** — see note below	376	—	36 months	**Not a comparative study of instrumentation**
Sobol 2022 [7]	Prospective **non-randomised** split-mouth	20 patients / 40 sides	Functional sensory recovery (MRC grades)	12 months	Time to functional sensory recovery 94.5 vs 101.5 days, **p=0.20**; but grade S4 reached in **18/20 (90%) vs 11/20 (55%), p=0.01**, and adjusted **HR 2.3 (95% CI 1.1 to 4.9), p=0.04**, both favouring piezo
Cascino 2021 [23]	Design discrepant	100 (50/50)	VAS and standardised neurosensory tests	6 months	Fewer discomforts and less analgesia with piezo; all recovered slight sensitivity
Raj 2022 [16]	Prospective split-	10 patients (5	Ordinal grading	1 day, 1 week, 1	Favours piezo at all

Study	Design	n	Assessment	Timing	Result
	mouth	mandibular, 5 maxillary)	scale	month	three points (p=0.007, p=0.004, p=0.04); at 1 month 10/10 piezo sides at best grade

The pattern by timing

Grouping the studies by when the outcome was measured produces an ordered result that no individual study makes visible.

At three months or earlier, all but one study favour piezoelectric or ultrasonic osteotomy: 95% versus 85% sensitivity preserved [1]; 2% versus 16% lacking recovery [13]; **22.9% versus 43.8% of sides affected, RR 0.52 (95% CI 0.31 to 0.87)** [19]; **7 of 24 versus 15 of 24 sides affected at one month, RR 0.47 (95% CI 0.23 to 0.94)** [22]; significantly less paraesthesia at three months [27]; significant advantages at day 1, week 1 and month 1 [16]; and significantly faster recovery [21].

The exception matters more than the majority, and we put it first rather than last. Shirota et al. compared 29 piezoelectric with 30 bur-and-chisel sagittal split setbacks and found the Semmes-Weinstein score **significantly worse with piezosurgery at three months, p=0.008**, concluding that the instrument reduced neither blood loss nor neurosensory disturbance [25]. It is retrospective and non-randomised, as are four other studies in this table. It is also **the only study in the corpus that adjusts for the amount of mandibular setback**, which the 2026 meta-analysis of persistent disturbance identifies as one of three consistent risk factors [2], and for operating time and method of fixation. We do not offer that as a reason to prefer its result: it is a small unrandomised study carrying the same confounding by period of adoption as the others. We offer it because it turns an unexplained discrepancy into a testable one — **no other study in this literature adjusts for how far the mandible was moved.**

At six months or later, the picture is mixed rather than uniformly null. Three results show no difference: Monnazzi at six months [6], Köhnke from six months onward despite an early advantage [5], and Sobol on its primary endpoint — time to functional sensory recovery, 94.5 versus 101.5 days, p=0.20 — at twelve months [7]. Two results do not. Sobol's own secondary analysis favours piezoelectric osteotomy on the proportion reaching the highest sensory grade, 90% versus 55%, p=0.01, with an adjusted hazard ratio of 2.3 (95% CI 1.1 to 4.9) [7]; Bertossi reports 98.2% versus 92.7% sensation retained at six months, with no significance test given [3]; and Spinelli reports normal sensation in 23 of 24 piezoelectric sides (97%) against 17 of 24 saw sides (72%) at six months [22]. Sobol falls on both sides of the line by itself, because its primary and secondary endpoints disagree. We record that plainly because we had ourselves first classified Sobol as a null study on the strength of a secondary source, and reading the primary report showed that it is not; the stratified pattern is real but weaker than it appears from abstracts.

Two studies in the corpus report events and denominators at side level, and are therefore the only ones from which a risk ratio can be computed: Kokuryo, retrospectively, and Spinelli, prospectively and within patients. Spinelli yields RR 0.47 (95% CI 0.23 to 0.94) at one month, 0.40 (95% CI 0.15 to 1.10) at three months and 0.14 (95% CI 0.02 to 1.07) at six months [22]. Both sets of intervals are computed at side level and **neither corrects for clustering within patient**; in Spinelli the sides are matched pairs and a paired analysis would be the correct one, so the intervals given here are indicative rather than definitive. The remaining reports give percentages, significance levels or ordinal grades without recoverable numerators and denominators. **No pooled estimate by stratum is therefore presented**, and we regard reporting that plainly as preferable to combining figures whose denominators we would have had to assume.

A correction to the existing synthesis

The 2025 scoping review [13] tabulates da Costa Senior et al. 2020 as a comparative study of piezoelectric versus saw in 279 patients followed for 36 months, reporting no difference — and it is the largest study in that table. We obtained and read the primary report in full [20]. It is a **prospective single-arm cohort of 376 consecutive patients, all treated with the same modified buccal sagittal split technique by one three-surgeon team**, examining long-term neurosensory disturbance; its stated aims are the incidence of disturbance and its risk factors, and its findings concern age, extent of advancement and intraoperative nerve manipulation. **Instrumentation is not a comparison in that study, and no group is defined by it.**

Three specific discrepancies follow. First, the design: there is no saw arm and no piezoelectric arm. The piezoelectric device appears only as the instrument used to release the inferior alveolar nerve from the proximal segment when release was required, in 52 of 376 right-side and 72 of 376 left-side operations; every other patient had the nerve released with a freer or found free. Second, the sample size: the cohort is 376. The figure of 279 corresponds to the subgroup with three-year follow-up, which is also where the 36-month figure comes from — and the primary report is itself internally inconsistent here, giving 279 in the methods and 269 in the results. Third, the result: no comparison of instrumentation was performed, so "no difference" is not a finding of that study in either direction.

The point is not bookkeeping. The only association involving the piezoelectric device in that cohort runs the other way — surgical release of the left inferior alveolar nerve, performed with the device, was significantly associated with postoperative hypoaesthesia ($p = 0.014$) — and it is uninterpretable as an instrument effect, because the device was used precisely in those nerves that were adherent enough to need invasive release. The authors attribute the left–right asymmetry to the operating surgeon's handedness. This is confounding by indication in its clearest form, and it is the reason a cohort of this design cannot enter a comparison of instruments in either direction.

We therefore exclude the study, and we record the discrepancy because it bears on how the existing synthesis should be read: the largest apparent contributor to the "no difference at long follow-up" conclusion is not a comparative study of instrumentation at all.

Operating time: the contradiction resolves by what was timed

Table 2. Operating time by what was measured.

Study	What was timed	Piezo	Conventional	Direction
Köhnke 2017 [5]	Sagittal split osteotomy only, stated as incision to segment mobilisation	17±6 min	25±9 min	Piezo faster, p<0.001
Koba 2018 [27]	**Osteotomy time**	lower	higher	Piezo faster, p<0.05
Bertossi 2013 [3]	Cutting time per side	3:31–5:02 min	7:23–10:22 min	Piezo faster; **ranges only, no test reported**
Raj 2022 [16]	Osteotomy, split-mouth	13.28 min	8.62 min	**Piezo slower, p<0.001**
Spinelli 2014 [22]	**Whole procedure per side**, stated as positioning + osteotomy + osteosynthesis	163.6±41.2 min	105.3±36.9 min	**Piezo slower by 35%, p=0.0018**
Akbar 2017 [26]	Time required for surgery	63 min longer	—	**Piezo slower, p=0.003**
Landes 2008 [1]	Whole bimaxillary procedure, cut to suture	227±73 min	238±61 min	No difference, p=0.5
Landes 2008 [1]	Whole **isolated Le Fort I**, cut to suture	96±32 min	77±32 min	**Piezo slower, p=0.03**

These rows do not measure the same quantity, and one study supplies two of them with opposite signs. **Seven of the eight follow one rule: where the report times the cut, the piezoelectric device is faster; where it times the operation, the device is slower or no different.** The single exception is Raj, ten patients timing "osteotomy", and Köhnke attributes that discrepancy to surgeon experience, stating that his own operators had at least a year with the technique [5, 16]. Pooling these rows into a single estimate, as would be natural from the abstracts alone, produces a number without a referent. We report them stratified and do not combine them.

Studies contributing outcomes other than the primary one

Five further comparative studies contribute to secondary outcomes but could not be placed in the timing strata, because their full texts were not accessible and their indexed records do not state when the neurosensory assessment was made: a retrospective comparison of bad splits by instrument [28], two comparisons of cutting technique in orthognathic surgery [29, 30], a study of the severity rather than the incidence of disturbance after sagittal split osteotomy [31], and an evaluation of the ultrasonic bone scalpel in Le Fort I osteotomy [32]. A sixth, the second of the two Landes cohorts [33], appears in Table 1 on the authority of the scoping review alone and has not been read in the original. They are listed here so that the reader can see the full extent of the comparative literature and judge how much of it this synthesis was able to quantify.

The corpus therefore comprises twenty-one comparative studies: sixteen in Table 1, of which fifteen were read in the original or against the published abstract and one rests on a secondary source, plus the five above. Twelve of the twenty-one were assessed for risk of bias; the remainder supply no extractable outcome and are listed for completeness only.

Risk of bias and certainty

Of the twelve comparative studies assessed for risk of bias, one is at low risk — Monnazzi, double-blind and split-mouth, which finds no difference [6]. Six are retrospective or non-randomised comparisons in which confounding by period of adoption cannot be excluded [19, 21, 23–25, 27], because piezoelectric cases are systematically later than saw cases in the same centre and surgical experience accumulates over that interval; Shirota is the only one of them to adjust for any clinical covariate, and is the only one whose result runs against the device [25]. No included study describes blinded outcome assessment for the neurosensory endpoint except the split-mouth trials with instrumented testing [4, 6]; Brockmeyer blinded both patient and assessor and is the best-protected study in that respect, but lost 78% of its sample by one year and reports no significant post hoc comparison [4]. Landes compares a piezoelectric series operated in 2005–2006 against **historical controls** operated in 2000–2005 who were six years older on average, a difference that runs in the same direction as the reported effect; we judge the risk of bias from confounding in that study to be critical, and it contributes no pooled estimate [1]. Bertossi describes a randomised trial but reports no randomisation method, allocation concealment, blinding, sample size calculation or losses, and its full text is not accessible; we judge it at high risk of bias overall [3]. Spinelli allocates the side by alternation rather than at random, with one surgeon and one unblinded examiner using a subjective five-grade scale; its within-patient design controls patient-level confounding well, and we judge it at moderate risk of bias, with the caveat that the six-month column of its saw table sums to 25 of 24 sides [22]. Operator blinding is impossible by the nature of the comparison, so every study carries unavoidable performance bias. Sobol is prospective but not randomised [7], so it shares the non-randomised allocation of the six above without their period effect. Certainty for the neurosensory outcome at six months or later is **low**, and for the operating-time outcomes **very low** given the inconsistency in what was measured.

Discussion

The principal finding is temporal rather than mechanistic: **piezoelectric osteotomy appears to accelerate neurosensory recovery, and the evidence that it shifts the endpoint is weaker and less consistent than the evidence that it shifts the speed.** Eight of the nine results measured at three months or earlier favour it; the ninth, and the only one adjusted for the amount of mandibular movement, runs against it [25]. From six months onward the studies diverge further:

three results show no difference [5–7] and three show a persisting advantage [3, 7, 22], with Sobol contributing to both columns because its primary and secondary endpoints disagree.

We state the strength of the two claims separately, because they are not equally supported. The operating-time claim rests on eight measurements from seven studies, three of which define in their own words what they timed, and it holds in seven of the eight. The neurosensory claim rests on a majority that is large but not unanimous, on two risk ratios neither of which corrects for clustering within patient, and on a corpus in which exactly one study sits at low risk of bias — Monnazzi, which finds no difference. **The first should be read as a finding and the second as a direction.**

If that holds, the clinical argument changes shape. A faster return of sensation over the first months is worth something to a patient, and it is a legitimate reason to choose the instrument. It is not the same as preventing permanent injury, which is how the technique is often presented, and the distinction should be made explicitly in consent.

The operating-time contradiction dissolves once one asks what was timed, and this is not our inference: two of the primary reports say it themselves. Köhnke states that, in contrast to other studies, osteotomy duration was defined as the time from mucosal incision to complete mobilisation of the segment on each side [5] — the cut, not the operation. Landes measured cut-to-suture time for the whole procedure: across a bimaxillary operation he found no difference, but in the isolated Le Fort I — the shortest procedure in his series, and therefore the one in which the osteotomy is least diluted by everything else the operation contains — the piezoelectric device was significantly slower, 96±32 versus 77±32 minutes, p=0.03 [1]. **One study yields both signs, according to what it is timing.** Spinelli is the third report to state its definition, and states the opposite one: operating time was recorded per side with dedicated software and

counted positioning, osteotomy and osteosynthesis; on that definition the piezoelectric device was 35% slower, 163.6±41.2 versus 105.3±36.9 minutes, p=0.0018 [22]. Cutting time is shorter with a piezoelectric device in two studies [3, 5] and longer in a third [16]; whole-procedure time is unchanged in the longer bimaxillary operation, longer in the isolated Le Fort I [1], and longer per side when positioning and osteosynthesis are counted [22]. **Three of the studies say in their own words what they timed, and the direction of the result follows the definition rather than the device.** The discrepancy between Köhnke and Raj is the one that still needs explaining, and the most likely candidates are learning curve and device generation — Raj studied ten patients, a slower cut is what one expects early in adoption, and Landes reports that his own first twenty cases took about 30% longer.

Two limitations are ours rather than the literature's, and we can be specific about the second rather than formulaic. The protocol was not prospectively registered. And screening was done by one reviewer without duplicate verification: **we know what it cost, because we measured it.** Spinelli 2014 was returned by our own PubMed search on 28 July, was not carried forward at title and abstract screening, and was recovered in September through the reference list of another included report [22]. That prompted a complete re-screen of all 118 PubMed records against our documentation, which recovered **four more eligible comparative studies** [24–27] — including the only study in the corpus whose result runs against the device [25]. **Five eligible records out of 118 were missed on the first pass**, and we report the rate rather than presenting the corrected corpus as though it had always been complete. We also decline to claim that the second pass was exhaustive: it was performed by the same reviewer. Duplicate independent screening of PubMed, and of the Web of Science and LILACS sets screened in the same session, is the first item of further work. Three limitations of the evidence deserve emphasis. **Operator blinding is**

impossible, so outcomes assessed by the operating surgeon warrant more caution than those assessed by an independent examiner or an instrument. **Learning curve is unaccounted for** in every study. **Sample sizes are small** except in the two retrospective series, and the largest study is the one least able to control confounding.

Our contribution relative to predecessors is specific and we state it plainly. The blood-loss question is addressed by a 2025 meta-analysis restricted to orthognathic surgery [14] and we do not re-analyse it, with one qualification we would not have been able to make before the re-screen: of the studies recovered on the second pass, one found blood loss **not influenced** by the instrument in an analysis adjusted for age and operating time [25] and another found the difference **not significant** [26], while a third found it significantly lower with piezoelectric osteotomy [27]. The pooled mean difference of −81.73 mL stands as the best available estimate; the studies that report the outcome without pooling are less uniform than that figure alone suggests. The neurosensory literature was mapped by a 2025 scoping review [13]. We initially relied on its extraction table for five studies; we have since obtained and read four of them in full and verified the fifth against its published abstract, and the extraction table proved accurate in its figures but silent on what conditioned them — what was timed, over how many patients, against what comparator, with what losses and with what denominator. One row, the second Landes cohort, still rests on that table alone and is marked as such. A Spanish-language evidence synthesis published in 2021 and indexed only in LILACS reached a conclusion consistent with ours — that piezoelectric surgery may reduce severe nerve injury, with low certainty for pain, blood loss, swelling and operating time — and, because the journal is not indexed in MEDLINE, it is absent from every English-language review of this question, including our own PubMed search [34]. What we add is fourfold: the correction of a misclassified study that is the largest in that table [20]; two comparative studies absent from previous syntheses [19, 21]; the timing-stratified reading with the two risk ratios the data permit [19, 22]; and the separation of operating-time outcomes by what was actually measured, which three of the primary reports state explicitly and which turns out to predict the direction of the finding better than the device does [1, 5, 22].

Conclusion

The neurosensory advantage of piezoelectric osteotomy in orthognathic surgery is best described as faster recovery rather than a lower final rate of disturbance: the advantage is consistent at three months and absent from six months onward. Operating-time results depend entirely on whether the osteotomy or the whole procedure was measured. Future comparative studies should pre-specify a single instrumented neurosensory outcome at fixed intervals to at least twelve months, report nerves at risk rather than patients, preserve within-patient pairing in split-mouth analyses, and separate cutting time from total operating time.

Declarations

Ethics approval: Not applicable. Systematic review of published literature; no human participants or animals.

Consent to participate / Consent for publication: Not applicable.

Competing interests: The authors declare that they have no competing interests, financial or otherwise, in the products, devices or information discussed in this manuscript.

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Author contributions: BMB conceived the review, designed and ran the searches, screened the records, extracted the data, assessed risk of bias, analysed the data and drafted the manuscript. BRF, RFD and SML contributed to the interpretation of the findings and critically revised the manuscript for important intellectual content. All authors read and approved the final version and agree to be accountable for all aspects of the work.

Data availability: All data derive from published studies cited here. Search strategies, screening log and extraction sheets are available from the corresponding author on reasonable request.

Nota para el autor

La valoración de este tema ha cambiado. En la versión anterior de este borrador se recomendaba considerar sustituirlo por ser demasiado estrecho. **Ya no.** El error de clasificación de da Costa Senior 2020 en la revisión de alcance de 2025 le da a este manuscrito una razón de existir que no dependía de nosotros: la síntesis vigente sobre el desenlace neurosensorial apoya parte de su conclusión en un estudio que no es comparativo.

Dos consecuencias prácticas:

1. ~~**Verificar personalmente el original de da Costa Senior 2020 antes de enviar.**~~ **HECHO el 04/08/2026.** Texto completo leído íntegro por acceso institucional (UANDES, ScienceDirect). La corrección queda **confirmada y ampliada**: cohorte prospectiva de un solo brazo, 376 pacientes, misma técnica en todos, sin brazo de sierra ni de piezoeléctrico; el 279 es el subgrupo con seguimiento a tres años —y el propio artículo dice 279 en Métodos y 269 en Resultados—; el piezoeléctrico solo se emplea para liberar el nervio cuando estaba adherido (52/376 derecho, 72/376 izquierdo) y su asociación con hipoestesia está confundida por indicación. La afirmación ya puede hacerse por escrito.
2. **Redactar la corrección sin acritud.** Se señala el hecho, se cita la fuente primaria y se sigue adelante. Un revisor de Springer aceptará una discrepancia documentada; no aceptará un tono de reproche.

El título propuesto sigue siendo válido, pero conviene considerar uno que refleje la pregunta real:

"Does piezoelectric osteotomy prevent neurosensory injury or merely accelerate recovery?"

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Supplementary Table S1. Search strategy by database

Every string is reproduced exactly as it was entered. Strings were translated into the syntax of each

database rather than transcribed unchanged.

#	Database	Platform / edition	Date run	Search string as entered	Records
1	PubMed/MEDLINE	NCBI, via E-utilities	28 Jul 2026; re-run 16 Aug 2026	(piezosurgery OR "piezoelectric surgery" OR	**118** on both dates

#	Database	Platform / edition	Date run	Search string as entered	Records
				"ultrasonic osteotomy" OR "piezoelectric") AND ("sagittal split" OR orthognathic OR "Le Fort")	
2	PubMed/MEDLINE	NCBI	28 Jul 2026	String 1 restricted with AND "randomized controlled trial"[pt]	6
3	PubMed/MEDLINE	NCBI	28 Jul 2026	String 1 restricted with the clinical-trial and randomised-trial filters	12
4	LILACS	Portal Regional BVS, filter[db][]=LILACS, field tw	**16 Aug 2026**	(tw:(piezosurgery OR piezoelectric OR piezoelectrico OR piezoeletrico OR piezoelectrica OR piezocirugia OR "ultrasonic osteotomy" OR "osteotomia ultrassonica" OR "osteotomia ultrasonica")) AND (tw:("sagittal split" OR "osteotomia sagital" OR orthognathic OR ortognatica OR ortognatico OR "Le Fort"))	**4**
5	Scopus	Elsevier, advanced search	**16 Aug 2026**	TITLE-ABS-KEY((piezosurgery OR "piezoelectric surgery" OR "ultrasonic osteotomy" OR piezoelectric) AND ("sagittal split" OR	**149**

#	Database	Platform / edition	Date run	Search string as entered	Records
				orthognathic OR "Le Fort"))	
6	Web of Science Core Collection	Clarivate, all editions, Query Builder	**16 Aug 2026**	TS=((piezosurgery OR "piezoelectric surgery" OR "ultrasonic osteotomy" OR piezoelectric) AND ("sagittal split" OR orthognathic OR "Le Fort"))	**140**
7	Embase	—	**Not searched**	Not licensed by our institution. See Methods	—
8	Cochrane CENTRAL	—	**Not yet run**	—	—
9	ClinicalTrials.gov, WHO ICTRP	—	**Not yet run**	—	—

Notes.

Row 1. The string returned 118 records on both 28 July and 16 August 2026, confirming that it is reproducible without drift over that interval.

Row 4. Terms entered in English, Spanish and Portuguese, without diacritics. None of the four records duplicated the PubMed set. One is a Chilean evidence synthesis on precisely this question (*Int J Interdiscip Dent* 2021;14(1):73-78) that is absent from PubMed because the journal is not indexed there; it has been added to the map of previous syntheses. One further record, a retrospective study of piezo-osteotomy in orthognathic surgery, awaits full text to determine whether it has a comparator arm.

Row 6 (Web of Science). Executed and complete. Records were exported at record level with PubMed identifiers and DOIs, de-duplicated against the PubMed and LILACS sets by identifier, and screened at title and abstract level.

Row 5 (Scopus). Executed; the count is exact. Record-level export from Scopus requires a personal account, which we did not create, so records were read from the results interface. **De-duplication and screening are in progress and no Scopus-only record contributes to this synthesis.** This matters more in this review than in most: because Embase is not available to us, Scopus and Web of Science are the stated mitigation for its absence, and that mitigation is complete for Web of Science only.

Row 7. Embase is not licensed by our institution and was not searched. Embase indexes European and conference literature not covered by MEDLINE, and its Emtree vocabulary retrieves records that MeSH does not. To mitigate this we searched Scopus and Web of Science, which overlap substantially with Embase for journal-published clinical studies, together with LILACS, and we hand-searched all reference lists.