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Structured Dental Therapy and Anger Management among High School Students Aged 13–16 YearsA Quasi-Experimental Study

An exploratory descriptive report from study records

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ABSTRACT

Background: Structured dental therapy has been proposed for school-based anger management. **Objective:** To describe questionnaire patterns and response completeness in records from a quasi-experimental study targeting students aged 13–16 years. **Methods:** Documented aggregates from 40 experimental-labelled and 40 control-labelled files were synthesized (160 forms; 5,600 item positions). A and B denote questionnaire positions within files. Complete ten-item raw blocks were analyzed without imputation. **Results:** There were 5,492 resolved cells (98.07%) and 108 unresolved cells (1.93%). Experimental Part 1 means were 18.22 ± 3.86 (n=36) and 12.43 ± 2.44 (n=28); Part 2 means were 21.95 ± 3.48 (n=37) and 15.90 ± 3.31 (n=29) in A and B, respectively. Complete-file mean B−A differences were −5.46 ± 4.20 (n=26) for Part 1 and −6.22 ± 4.55 (n=27) for Part 2. Corresponding control-file differences were zero. Control agreement was 1,370/1,383 jointly resolved cells (99.06%). **Conclusion:** Experimental files showed lower second-form raw sums. Uncertain student linkage, conflicting chronology, incomplete authorized scoring and possible school–group confounding prevent a causal effectiveness conclusion.

Keywords: structured dental therapy; adolescent anger; school-based research; quasi-experimental study; descriptive analysis; response completeness

1. INTRODUCTION

Anger-management research in schools must identify the emotion or behaviour being measured and the programme students actually receive. A lower questionnaire sum is an observable result; its interpretation depends on valid measurement, comparable assessments and a defensible comparison group.

Adolescence is a relevant setting for research on emotional regulation because development and social context influence how students respond to demanding situations. Experimental work has distinguished emotional reactivity from the capacity to regulate a response and has shown that regulation success varies with age and the social nature of the stimulus (Silvers et al., 2012). This distinction matters when evaluating a programme intended to improve anger management: reduced anger intensity, reduced outward expression and increased control are not interchangeable outcomes.

The present research concerns a programme described as structured dental therapy in high-school students aged 13–16 years. The study plan adopts a non-equivalent comparison-group framework. The documented record set contains questionnaires labelled experimental and control, but the school records, the delivered intervention and the timing of assessments determine how those labels can be interpreted. The target population includes age 13; the first-form headers actually represented ages 14–16.

An empirical report can remain useful when its initial effectiveness question cannot yet be answered. Describing the response pattern, the available outcome denominators and the conditions that limit inference prevents an apparent numerical contrast from becoming an unsupported clinical claim. The present article therefore reports the documented aggregates and their analytical scope. It does not convert file positions into verified pretest and posttest observations or reconstruct missing participant responses.

2. REVIEW OF LITERATURE

2.1 Anger measurement in adolescents

The State-Trait Anger Expression Inventory-2 Child and Adolescent, STAXI-2 C/A, is described by its publisher as a 35-item self-report assessment for ages 9–18 years that covers state anger, trait anger, anger expression and anger control (PAR, n.d.). The authorized edition and manual govern the scale structure and interpretation. A preliminary coding of response positions is not equivalent to applying that scoring system, and a total across all items must not be assumed to represent a single anger construct.

2.2 School programs and emotional regulation

Daly et al. (2015) randomized 37 high-school students to a 16-week yoga programme or regular physical education. They reported a greater improvement in emotion regulation in the yoga group, while the proposed mediating variables did not explain the observed change. Khalsa et al. (2012) evaluated an 11-week school yoga programme and reported domain-specific findings, including anger control. These studies establish a reason to investigate clearly defined school programmes; they do not supply an effect estimate for the dental therapy procedure.

2.3 Oral activity and adjacent experimental evidence

Laboratory studies of chewing provide adjacent evidence rather than direct tests of the present programme. Scholey et al. (2009) used a crossover experiment with 40 adults and reported favorable stress-related outcomes during gum chewing. Johnson et al. (2011), in a 30-participant crossover study, did not reproduce the attenuation of self-rated stress or cortisol, although alertness increased. Differences in task, materials, population and endpoint limit transfer from either study to anger management among school students.

Table 1. Selected adjacent primary studies and relevance to the present question

Study	Programme and design	Relevant finding and boundary
Daly et al. 2015	37 students; 16-week yoga versus physical education	Emotion regulation improved more in yoga. The intervention was not dental therapy.
Khalsa et al. 2012	School yoga versus physical education over 11 weeks	Selected outcomes favoured yoga, including anger control. Findings were domain specific.
Burckhardt et al. 2018	96 Year 10 students; skills programme and control	No statistical difference between conditions; qualitative feedback was often favourable.
Scholey et al. 2009	40 adults; gum-chewing crossover laboratory study	Reported stress-related benefits under the tested conditions; adolescent anger was not assessed.
Johnson et al. 2011	30 adults; gum-chewing crossover laboratory study	Stress and cortisol attenuation was not reproduced; alertness increased.

2.4 Why direct evaluation remains necessary

Positive accounts of participation and a plausible rationale cannot substitute for a measured contrast. Burckhardt et al. (2018) illustrate this point: students frequently described benefits from a skills programme even though statistical differences between conditions were not found. Across the selected literature, interventions and outcome measures vary. Their results should not be combined into a presumed numerical benefit for the present cloth-based programme.

The selected studies support careful evaluation of programme-specific outcomes and alternative explanations. None resolves the present record-linkage or chronology questions. A proposed autonomic, hormonal or neural mechanism would require direct measurements; these were not outcomes in the documented dental therapy analysis. Likewise, photographs can describe a setting or activity without establishing dose, adherence or effectiveness.

3. NEED AND SIGNIFICANCE OF THE STUDY

The research question has practical interest because a programme delivered within schools needs to be understandable, tolerable and evaluable in that environment. Its value depends on a traceable link between an explicit procedure, actual participation and an outcome measured at known times. The present report contributes by making the available questionnaire pattern and its denominators inspectable, while retaining the distinction between the planned trial question and the results supported by the records.

4. OBJECTIVES AND HYPOTHESES

4.1 Objectives of this report

The objectives were to describe first-form header characteristics; quantify response and section completeness; present raw-section distributions by group label and questionnaire position; summarise arithmetic differences within source files; and examine agreement in the control records. These are descriptive objectives for the existing record set.

4.2 Relationship to the original effectiveness hypothesis

The original study aim was a baseline-adjusted comparison of an authorized anger outcome following the planned intervention. That hypothesis is not tested here. It requires verified student and assessment links, a defined primary scale, documented exposure and an analysis appropriate to the allocation structure. Descriptive differences are reported without accepting or rejecting the causal hypothesis.

5. METHODOLOGY

5.1 Design and source of the present report

This article is an exploratory descriptive synthesis of the study's documented extraction and chapter-level analyses, rather than a new trial or a fresh independent audit of every original scan. The study chapters and the report and workbook identified in them provide the aggregate results (Dental Therapy Study Chapters, 2026; Dental Therapy Combined Analysis 80 Records, 2026; Dental Therapy Master Data 80 Records, 2026). Reporting principles were informed by TREND for nonrandomized evaluations (Des Jarlais et al., 2004).

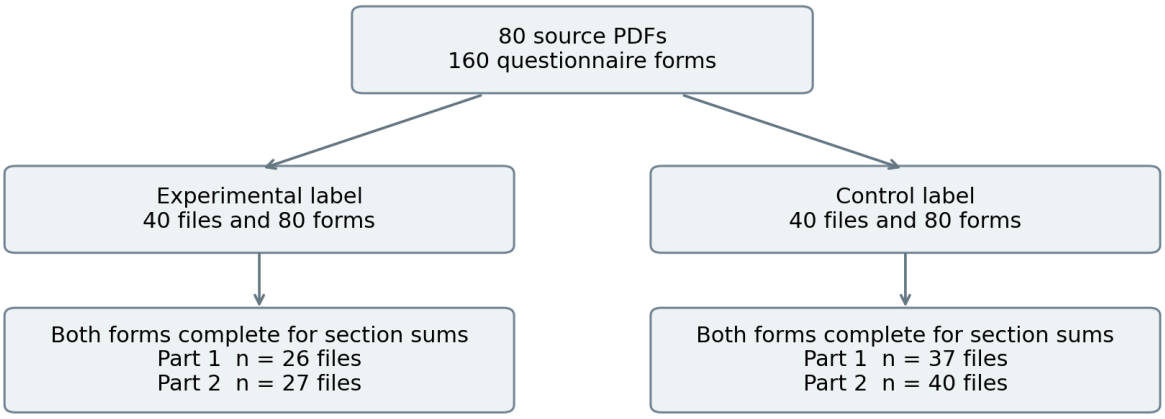

Figure 1. Source-file coverage and complete-section comparison counts

Counts refer to source files and forms. They are not a verified participant recruitment flow or proof of independent allocation.

5.2 Recorded setting and population

The extraction covered 80 PDFs with 14 scanned pages each, giving 1,120 pages. Each PDF contained background material and two 35-item questionnaires. Experimental files were indexed E01–E40 and control files C01–C40. These are source-file identifiers. The final number of unique enrolled students cannot be inferred solely from the number of PDFs because some identifiers and demographic entries conflict across forms.

First-questionnaire headers recorded grade 10. Experimental headers named Sai Vidya Mandir and control headers named New Bangalore Public School, with corrections in some school entries. These are recorded labels, not a substitute for a verified allocation or permission register. If school membership determines the study arm, school context and intervention assignment coincide. The analysis cannot treat the students as independently allocated intervention units simply because each has a questionnaire.

5.3 Planned programme and documented exposure

The study plan describes an eight-week, 40-session, five-component cloth-based programme. The Methods chapter does not establish its delivered dose from dated session logs. Consequently, this report gives no numerical attendance, adherence, fidelity or adverse-event estimate. It does not present an inferred sequence of oral procedures as a verified intervention manual. The comparison plan refers to usual school activities; the activities actually received and any additional contact were not quantified in the analysed summaries.

A reproducible account must identify materials, procedures, provider preparation, delivery format, schedule, tailoring and modifications. TIDieR provides a structure for that description (Hoffmann et al., 2014). Programme delivery, possible mechanisms and school context also require distinct process information (Moore et al., 2015). The intervention label alone does not establish any of these features.

5.4 Questionnaire position and preliminary measures

A denotes the first questionnaire stored in a PDF and B the second. Common written dates were 20 July and 20 October 2026, which are 92 elapsed days apart. Some dates and pretest/posttest labels conflict, and at least one file carries the same apparent date on both forms. The report therefore retains A and B rather than imposing a uniform chronological order or changing dates to match the planned eight-week programme.

Readable single selections were assigned codes 1, 2 or 3 according to left-to-right position. Part 1 comprises items 1–10 and Part 2 items 11–20; a complete ten-item raw sum ranges from 10 to 30. The remaining 15 items were retained at item level in the extraction. These preliminary blocks are not reported as authorised STAXI-2 C/A scale scores, norm-referenced categories or diagnostic outcomes.

5.5 Incomplete responses and documented corrections

A sum was available only when all ten responses in the corresponding block were resolved. An incomplete Part 1 block did not automatically remove a complete Part 2 block from the same form. No group-mean replacement, zero substitution, prorating or other imputation was applied. Multiple marks and blank or unclear responses remained separate from accepted numerical values. The incomplete-block exclusions are reported with their denominators, rather than being concealed by a single overall sample size.

The chapter-level audit records one explicit response correction: E27 A item 33 was accepted as response 2 because the original contains handwritten correct/wrong annotations; its former 2|3 marking was retained. It also records that E26 reads ID 9 on both forms and E40 reads ID 18 on both forms. Two Part 3 page-order exceptions, C34 A and C38 B, were mapped using printed item numbers. These documented clarifications do not resolve the other identity or date conflicts.

5.6 Descriptive analysis and denominators

Results are presented as counts, percentages, means, sample standard deviations, medians and ranges. Separate-form means use the complete blocks at each position, so A and B may have different denominators. Within-file arithmetic uses only files with the same block complete at both positions; B−A is the second sum minus the first. Difference distributions and direction counts describe the file arrangement without asserting that each file is a verified repeated measure of one student.

No new participant-level significance tests, confidence intervals or standardised treatment-effect estimates were calculated from the chapter aggregates. Unknown linkage, scoring and allocation conditions prevent a justified effectiveness model. A baseline-adjusted analysis may be appropriate after verification, but it cannot eliminate confounding when school and intervention assignment are inseparable (Vickers & Altman, 2001). In the control-file differences, zero observed variance also makes an ordinary paired t statistic undefined; it does not yield a valid p value.

5.7 Ethics and publication scope

No new participant contact was undertaken for this descriptive synthesis. The reviewed study documentation contains a university request letter and blank forms; these do not establish ethics approval, completed parent permission or student assent for the original study. No approval number, waiver or completed consent process is claimed in this article. Child-research documentation must be assessed through the responsible institutional process in accordance with applicable guidance (Indian Council of Medical Research, 2017). Identifying records and recognisable student photographs are not reproduced here.

The absence of a reported monitoring count cannot be interpreted as zero adverse events. This manuscript is a report of documented aggregates and does not certify the conduct or regulatory status of the underlying intervention.

6. RESULTS

6.1 Record coverage and response completeness

The documented extraction contained 5,600 expected item cells: 80 files × two forms × 35 items. A total of 5,492 cells had accepted numerical responses, equivalent to 98.07%. The 108 unresolved cells (1.93%) comprised 88 multiple-mark entries and 20 blank or unclear entries. Experimental files accounted for 91 unresolved cells and control files for 17.

Table 2. Item-level completeness by group label and questionnaire position

Group	Position	Resolved cells	Unresolved cells	Unresolved percent	Complete 35-item forms
Experimental	A	1,378	22	1.57	25/40
Experimental	B	1,331	69	4.93	16/40
Control	A	1,393	7	0.50	34/40
Control	B	1,390	10	0.71	36/40

Each group and position contains 1,400 expected cells. Complete-form counts require all 35 items to be resolved.

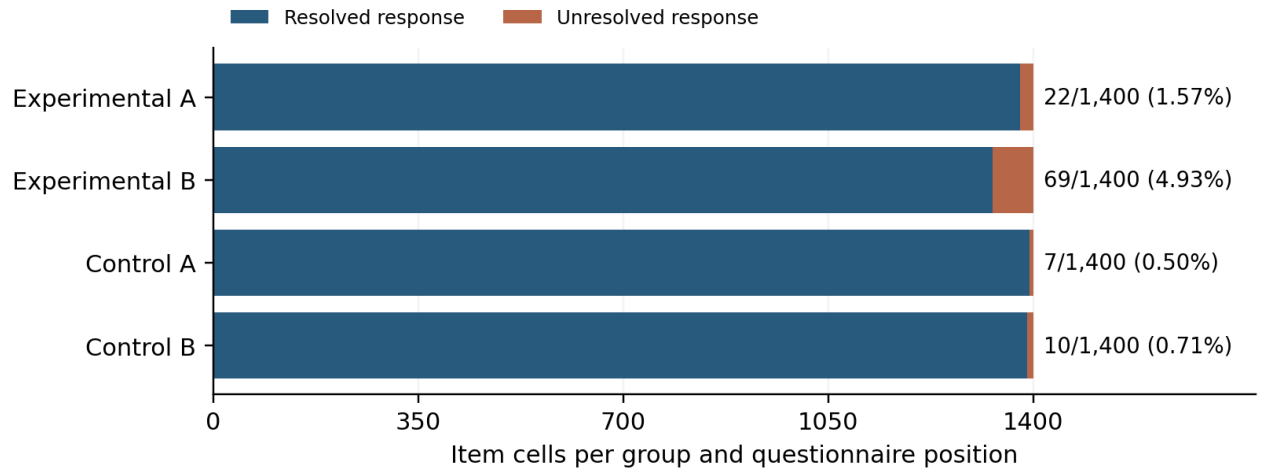

Figure 2. Distribution of resolved and unresolved item cells

Experimental B had the largest unresolved count, 69 of 1,400 cells, and the lowest count of completely resolved forms. The position-specific imbalance is more informative than the pooled 1.93% alone: even a small percentage of unresolved cells can affect many ten-item sums when those cells occur across different forms. These completeness findings concern questionnaire availability; they do not show that the corresponding students missed sessions or withdrew.

6.2 Recorded characteristics and available sections

Table 3. Characteristics recorded in the first-questionnaire headers

Characteristic	Category	Experimental n (%)	Control n (%)
Age in years	13	0 (0.0)	0 (0.0)
	14	10 (25.0)	0 (0.0)
	15	23 (57.5)	20 (50.0)
	16	7 (17.5)	20 (50.0)
Recorded sex	Male	22 (55.0)	19 (47.5)
	Female	18 (45.0)	21 (52.5)

Denominator: 40 first-form headers per group. These are not independently verified enrolment-register characteristics.
The groups differed in recorded age composition. Ten experimental headers recorded age 14, whereas no control header did. All first-form headers recorded grade 10. The broader age range in the title defines the intended population; it does not establish representation at age 13 or across other grades.

Table 4. Outcome availability under section-specific completeness rules

Group	Part 1 A	Part 1 B	Part 1 both	Part 2 A	Part 2 B	Part 2 both
Experimental	36	28	26	37	29	27
Control	37	40	37	40	40	40

A and B columns count complete section forms; both columns count files in which the section is complete at both positions. Each group contains 40 source files.
Only nine experimental files and 31 control files had all 35 items complete in both questionnaires. The section rules retained more information: 26 experimental files contributed Part 1 comparisons and 27 contributed Part 2 comparisons. Restricting every analysis to fully complete 70-response files would discard available blocks, but using complete blocks also means that different outcomes can represent different subsets.
Part 1 availability declined from 36 to 28 experimental forms and Part 2 from 37 to 29. The control counts were 37 to 40 and 40 to 40. These changes in analytical availability must not be described as a verified participant retention rate. Neither completeness nor equal group file counts establishes exchangeable study arms.

6.3 Raw section sums by questionnaire position

Table 5. Complete-section descriptive statistics

Part	Group	Set	n	Mean	SD	Median	Range
1	Experimental	A	36	18.22	3.86	17.5	10–27
1	Experimental	B	28	12.43	2.44	11.5	10–18
1	Control	A	37	20.59	5.32	19.0	13–30
1	Control	B	40	20.48	5.25	19.0	13–30
2	Experimental	A	37	21.95	3.48	23.0	13–28
2	Experimental	B	29	15.90	3.31	16.0	10–21
2	Control	A	40	21.85	3.70	22.0	15–30
2	Control	B	40	21.85	3.70	22.0	15–30

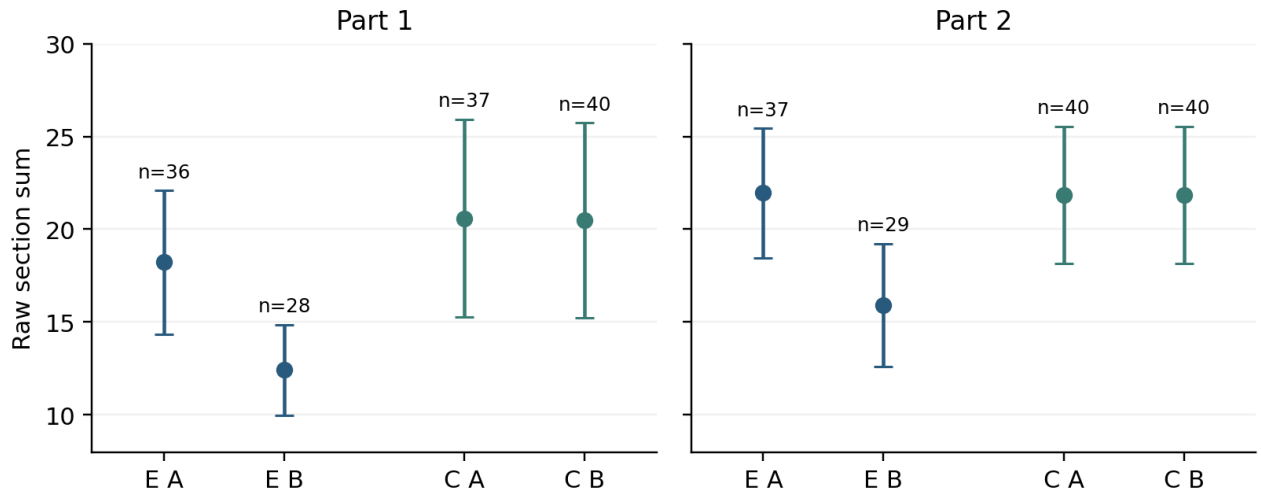

Figure 3. Separate-form raw-section means and standard deviations

Points are means; bars show ±1 SD, not confidence intervals. E=experimental, C=control. Positions are not connected as verified longitudinal observations.

Experimental B means were lower for both sections. Part 1 means were 18.22 in A and 12.43 in B; Part 2 means were 21.95 and 15.90. The corresponding medians were also lower. Control Part 2 means and medians were identical at both positions. Control Part 1 means differed slightly because the complete-form sets contained 37 and 40 forms, despite unchanged sums in all complete-file comparisons.

The table describes distributions of complete forms. Subtracting these separate means does not yield a verified average student change because the underlying subsets and links are not identical. The next analysis uses the narrower set of files complete at both positions.

6.4 Arithmetic differences within complete files

Table 6. Within-file raw-section comparisons

Part	Group	Files	Mean A	Mean B	Mean B−A	SD B−A
1	Experimental	26	17.81	12.35	−5.46	4.20
1	Control	37	20.59	20.59	0.00	0.00
2	Experimental	27	21.85	15.63	−6.22	4.55
2	Control	40	21.85	21.85	0.00	0.00

These are complete-file arithmetic comparisons, not verified student-paired treatment effects. Values are rounded to two decimal places.

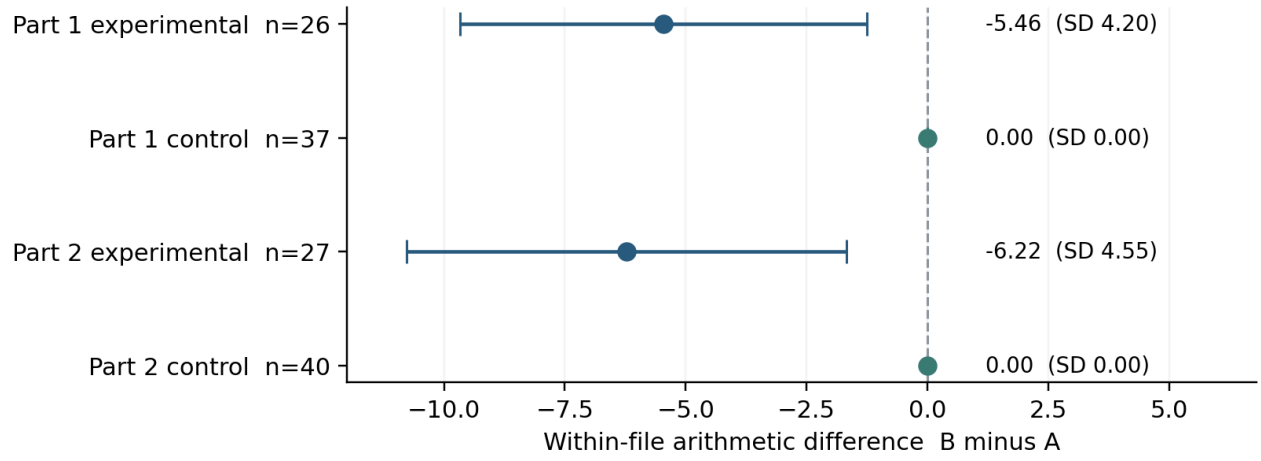

Figure 4. Mean within-file differences and their observed dispersion

Error bars show ±1 SD of differences. They are not confidence intervals. A zero SD describes identical observed section differences in the eligible control files.

For experimental Part 1, the median difference was −5.5 and the range was −14 to +3. Twenty-three of 26 file comparisons were lower in B and three were higher. For Part 2, the median was −7 and the range −13 to +5; 23 of 27 were lower, one unchanged and three higher. The presence of higher values in some files is retained rather than treating the experimental pattern as uniform.

All 37 complete control Part 1 comparisons and all 40 control Part 2 comparisons had equal sums. A stable sum does not require every item response to be the same, so response-level agreement was examined separately. Direction counts are not responder rates: an intervention response classification would require verified participants, a valid outcome and a justified response threshold.

6.5 Repetition in the control records

Among 31 control files with all 35 responses resolved at both positions, 29 had identical complete response patterns. Across the 1,383 cells resolved at both positions, 1,370 agreed (99.06%). The remaining 13 comparisons differed. This distinction between equal sums and identical item patterns is important when assessing the source records.

Table 7. Control agreement using four different denominators

Comparison unit	Denominator	Equal or identical	Percent
Part 1 sums complete in both forms	37 files	37	100.00
Part 2 sums complete in both forms	40 files	40	100.00
All 35 items complete in both forms	31 files	29	93.55
Jointly resolved item cells	1,383 cells	1,370	99.06

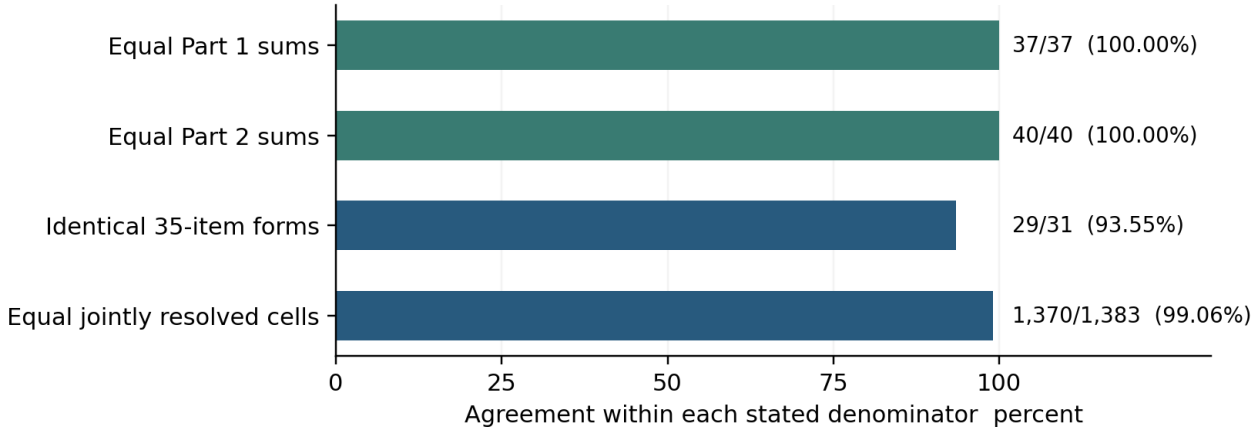

Figure 5. Control agreement across file section and item units

The pattern does not identify why responses repeat. Independent administration, genuine stability, record assembly and duplication cannot be distinguished from agreement percentages alone. The repeated responses require confirmation of independent collection before the control series can be interpreted as a longitudinal comparison.

6.6 Status of the effectiveness inference

The observed experimental pattern is lower second-form raw sums. No ANOVA, paired t-test, treatment p value or clinical recovery category is reported. The results do not establish a baseline-adjusted intervention contrast because identities, chronology, authorised outcome scoring and allocation remain insufficiently verified in the documented analysis. The original effectiveness hypothesis is therefore not adjudicated by these descriptive results.

7. DISCUSSION

7.1 What the observed pattern establishes

Both preliminary sections show a lower experimental B distribution, and the same direction appears in the files complete at both positions. This consistency is a useful descriptive observation. The file comparisons also show variability: three experimental files were higher in each section, and one Part 2 comparison was unchanged. The observed pattern is therefore neither an all-student improvement nor a demonstrated clinical recovery rate.

The two forms of comparison answer different questions. Separate-form summaries describe available responses at each PDF position. Complete-file differences describe arithmetic within the assembled files. The latter reduces one source of denominator mismatch, but does not validate student identity or time ordering. A result can be numerically reproducible while its clinical interpretation remains limited by the provenance and organization of the observations.

7.2 Measurement interpretation

The preliminary raw sections preserve numerical information without assuming a scoring rule that has not been established for the administered forms. This is a narrower claim than reporting an authorized state-anger, trait-anger, expression or control outcome. These domains are distinct in STAXI-2 C/A (PAR, n.d.). A smaller unweighted block sum cannot be labelled improved anger management across every domain without the instrument-specific definition and direction.

No normative conversion or severity threshold was applied. The study therefore does not estimate the prevalence of low, elevated or very high anger, nor the proportion crossing a clinical threshold. It also contains no analyzed parent, teacher, disciplinary or academic outcome demonstrating a change in everyday functioning. Future interpretation should begin with the selected primary scale and its permitted scoring, then distinguish statistical variation from a meaningful difference for students.

7.3 Completeness and selection

The unresolved-item proportion is small in aggregate but concentrated in experimental B. Requiring complete blocks can select a different set of records at each position. If the probability of a complete block is related to comprehension, administration conditions

or the response itself, an available-form mean can differ from the intended participant outcome mean. The present summaries cannot determine the direction of that selection.

Reporting the section denominators is therefore substantive, not cosmetic. Complete-block analysis avoids inventing responses and retains a block when another part is incomplete, but it does not show that omitted responses are missing at random. A later authorized missing-item procedure would need explicit rules and sensitivity checks; it could not repair an uncertain person or date link. Completeness, linkage and measurement validity are separate properties that need separate evidence.

7.4 Setting chronology and alternative explanations

The first-form age distributions differ and group labels appear to align with different schools. If only one school contributes each arm, the comparison includes school environment, teaching routines and recruitment as well as any intervention difference. Adjusting for measured individual characteristics cannot create replicated allocation units or identify the effect of the programme separately from an unreplicated school contrast. The reported file pattern must remain narrower than that causal question.

Common written dates span 92 days, while the plan specifies eight weeks. Those quantities can differ for legitimate reasons, such as delayed assessment, but no reason should be inferred or a date rewritten without a supporting record. Conflicting date labels and repeated identifiers add uncertainty about which observations are comparable. Source verification must be independent of whether a correction makes the intervention appear more effective.

7.5 Relation to the adjacent evidence

The school yoga trials and oral-activity experiments reviewed earlier offer reasons to investigate specific outcomes, but they test different procedures. Their positive and null findings reinforce the need for direct evaluation rather than supplying a presumed mechanism or expected response. The present summaries contain no physiological measurements demonstrating a hormonal, autonomic or neural pathway. A mechanism remains an explanatory hypothesis until it is measured.

8. PRACTICAL APPLICATIONS

The immediate application is to research practice: retain original markings, document corrections, use outcome-specific denominators and separate programme delivery from outcome measurement. For schools, the report supports considering a carefully specified evaluation with appropriate participant protection; it does not establish the programme as a routine anger-management treatment. The absence of an analysed adverse-event rate also prevents a claim of universal safety. Oral or jaw symptoms require procedure-specific professional assessment; NIDCR guidance describes the relevance of painful jaw symptoms and the value of avoiding excessive jaw loading in affected individuals (NIDCR, n.d.).

9. STRENGTHS OF THE REPORT

The analysis includes both labelled source groups, exposes the denominators behind each summary and preserves the difference between unresolved and numerical responses. It presents the less favourable experimental comparisons and the unusual control repetition. These features make the descriptive account inspectable and provide a basis for verification without changing values to meet an anticipated result.

10. LIMITATIONS

This report relies on the aggregate extraction documented in the thesis chapters and is not an independent re-reading of every original scan. The principal limitations are uncertain unique-student linkage and chronology, unconfirmed authorized scoring, unequal section availability, possible school–group confounding and unquantified delivered exposure. Generalization is also limited by grade 10 records and no age-13 first-form entries. These limitations prevent a definitive efficacy or safety conclusion.

11. RECOMMENDATIONS FOR FUTURE RESEARCH

A subsequent evaluation should begin with a versioned procedure manual and an explicit primary anger outcome, accompanied by the authorized assessment materials. Recruitment, assignment, participant IDs and assessment dates should be recorded prospectively. Multiple independent schools or classes in each condition would allow the allocation structure to be addressed in both design and sample-size planning. Comparable assessment conditions and the actual comparison activities should be documented.

Session delivery, attendance, component completion, modifications and discomfort should be measured separately from questionnaire outcomes. A prospective analysis plan should specify baseline adjustment, handling of missing responses and reporting of effect estimates with uncertainty. Process evaluation can explain implementation and context without treating an unmeasured mechanism as an established result (Moore et al., 2015). Later behavioral outcomes and longer follow up may address whether any verified change has practical and sustained relevance.

12. CONCLUSION

The documented records show lower second-questionnaire raw section sums in experimental-labelled files and unchanged complete-file control sums. These descriptive findings are based on available complete sections and preserve missing responses rather than replacing them. They do not establish that structured dental therapy caused improved anger management. A valid effectiveness assessment depends on verified student and time-point links, authorized outcome scoring, documented exposure and an analysis that respects the actual allocation structure.

13. DECLARATIONS

13.1 Data source and availability

The article reports aggregate results documented in the study chapters and the extraction report and workbook identified there. No new participant data were collected for this manuscript and no synthetic observations were generated. Individual identifying records, protected test items and student photographs are not included. Access to underlying research records is subject to the data custodian’s permissions and applicable participant protections; public availability is not asserted.

13.2 Ethics and consent

The reviewed material does not establish an ethics approval or waiver reference or completed consent and assent for the original study. No such status is asserted. The responsible institution and authors must determine and document the permissible publication scope before submission; a request letter or newly prepared form is not a substitute for the original decision or consent record.

13.3 Authorship funding and competing interests

The authors are Sowmya C. (first and corresponding author), Dr. Bhaskar Tandel (second author), and Resham Aswani (third author). Specific contributions, funding and competing-interest declarations require author confirmation before submission.

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APPENDIX A ANALYTICAL TERMS AND SOURCE TRACEABILITY

Table A1. Definitions retained without changing source data

Term	Meaning in this manuscript
Experimental and control	Researcher-assigned group labels attached to source files.
A and B	First and second questionnaire positions within a PDF; not assumed to be verified baseline and follow up.
Complete section	All ten responses resolved in the specified raw block.
Within-file difference	B section sum minus A section sum in a file complete for that block.
Unavailable sum	The relevant section is incomplete; it is not assigned zero or imputed.
Equal sums and identical forms	Different comparison units: identical section totals need not imply identical item responses.

Numerical sources: Results chapter Tables 6.1–6.8 and 6.10–6.14; chronology and linkage qualifications: Tables 6.9 and 6.15–6.17 and the corresponding Methods and Discussion sections. Figures in this manuscript were redrawn from the documented aggregates. All numerical findings derive from the dental therapy study documentation.