Brazilian Dental Science

Subject: [BDS] Editor Decision

Dear Maulida Hayati, Nurhayaty Natsir, Aries Chandra Trilaksana:

Your submission Analysis of Fracture Resistance and Surface Characteristics of Bioactive Ceramic Cluster Filler and Bulk Fill Fiber-Reinforced Composites in Endodontically Treated Teeth to Brazilian Dental Science, has been revised and according to reviewers' comments, there are questions to be addressed and/or points to be clarified/corrected.
Please answer the reviewers considerations point-by-point in a separate document and also please make all the corrections in the text highlighted in yellow.

Deadline: 30 days

Thank you for considering Brazilian Dental Science for publishing your research.
We are looking forward the revised version of you manuscript.

Sincerely,

Reviewer A:
Recommendation: Revisions Required

Comments to the Author

1. The title reflects the subject of the study. This manuscript presents a clear and clinically useful message. It is well written in terms of clarity, style, and use of English language. however Materials and methods are insufficient in detailed. The

discussion section explains adequately the purpose of this study in the context of published information. The conclusions accurately and clearly explain the main results. The length of the manuscript is ideal. All tables and figures are of good quality and relevant to the subject. All references are appropriate and current.

Materials and Methods

1. Restoration Method Clarification

There appears to be a procedural difference in the restoration methods between the groups. Both were restored using the incremental technique; however, the increment thickness varied—TMR-Z Fill 10 was applied in 2 mm increments, whereas EverX Posterior was applied in 4 mm increments. It is recommended that the authors provide a clear rationale for this variation, as it may influence the mechanical behavior and fracture resistance of the restorative materials.

2. Sample Preparation and Method Consistency

The authors are encouraged to include a more detailed description of the sample preparation process for the fracture resistance test. It should be clarified whether the same incremental restoration technique was consistently applied across all samples. In addition, further explanation regarding the preparation procedure used for the AFM (Atomic Force Microscopy) analysis is necessary to ensure reproducibility and to understand how the restoration method may have affected surface morphology.

3. Fracture Pattern Analysis and Intact Teeth

The authors should elaborate on the procedure used for fracture pattern analysis, particularly explaining how intact teeth were assessed or utilized as a reference group. This clarification is important to accurately interpret the classification of fracture types and to contextualize the comparison between restored and intact specimens.

Results

1. Placement of Observations in Discussion

The statement “EverX exhibited higher variability due to fiber exposure, whereas TMR-Z Fill demonstrated greater uniformity from ceramic cluster fillers” should be moved to the Discussion section, as it represents an interpretation of the findings rather than a description of results.

2. Consistency of Group Nomenclature

The authors should ensure consistency in naming the experimental groups

throughout the manuscript. Terms such as EverX, EverX Posterior, TMR-Z Fill 10, and TMR-Z Fill appear to be used interchangeably; it is recommended that a single, consistent form be applied for clarity and uniformity.

3. Clarification of Fracture Pattern Descriptions
It is suggested that the authors provide a more detailed explanation of the fracture patterns to help readers better understand the intended definitions of “restorable” and “non-restorable.” Additional clarification linked to Figure 1 would enhance interpretability and strengthen the discussion of the results.

4. Clarification of EDS Presentation (At% vs. Wt%)
The authors’ decision to present Figure 4 in both atomic percentage (At%) and weight percentage (Wt%)—despite showing similar results—should be justified. A brief discussion on the reasoning or significance behind including both forms of data would improve the manuscript’s coherence and scientific depth.

Reviewer B:
Recommendation: Revisions Required

Comments to the Author

Reviewer Comment
I have reviewed the manuscript reporting an in vitro comparison of two restorative materials in endodontically treated teeth using mechanical testing and surface/interface characterization (AFM, SEM–EDS, FTIR). The topic is clinically relevant, and the authors’ attempt to combine multiple analytical techniques is commendable. However, several methodological and interpretive issues substantially limit the clinical translatability and the strength of the conclusions. In its current form, I recommend major revision.

1) Timing not reported: The manuscript does not clearly specify the interval between restoration placement and testing/analyses. This directly affects

polymerization maturation, water sorption, interface behavior, and mechanical outcomes.

2) Polishing protocol insufficiently detailed: Surface roughness outcomes are highly dependent on finishing/polishing, yet the protocol is not described in enough detail to ensure reproducibility.

3) The use of LSD for post-hoc testing raises concerns due to inflated Type I error; more conservative approaches (e.g., Tukey or Bonferroni/Holm adjustments) are recommended. Why did the authors choose this analysis?

4) The roughness section appears contradictory: reporting $p > 0.05$ yet presenting significant post-hoc comparisons. Post-hoc pairwise comparisons should only be performed when the overall ANOVA demonstrates significant group differences ($p < 0.05$). Running post-hoc tests after a non-significant ANOVA ($p = 0.084$) is statistically inappropriate, as it increases the likelihood of false-positive findings (inflated Type I error rate).

5) Interface gap paradox and clinical trade-off not adequately discussed: The material described as “bioactive” exhibited significantly wider interface gaps (8.40 ± 0.82 µm vs 3.60 ± 0.43 µm), which typically implies poorer marginal adaptation and potential risks (microleakage, staining, secondary caries). The manuscript must reconcile this with claims of enhanced interfacial integration and discuss the clinical significance of this trade-off.

6) Fracture mode reporting needs a systematic approach: Although the manuscript mentions “restorable vs non-restorable” patterns, there is no systematic classification (e.g., adhesive/cohesive/mixed; or standardized fracture mode criteria) and no formal statistical comparison of fracture mode distributions. Why was a more detailed classification system not used? Standard schemes (adhesive/cohesive/mixed; or above/below bone level) would allow better comparison with existing literature.

7) Inconsistency between EDS Data and Claims of Remineralization: The manuscript presents an important inconsistency between the elemental analysis results (EDS) and the interpretation advanced in the Discussion regarding the remineralization potential of TMR-Z Fill 10. According to Table 4, fluoride (F) was

clearly detected in EverX Posterior (1.9 At% / 2.0 Wt%), whereas fluoride was not detected nor reported for TMR-Z Fill 10, whose elemental profile is described primarily by the presence of zirconium (Zr) and silica (Si). This distinction is further reinforced by the Figure 4 caption, which explicitly states that the EDS spectrum confirms a "silica-fluoride" profile for EverX, while TMR-Z is characterized by zirconium.

Despite this, the Discussion states that "taken together with our EDS data and prior reports on bioactive composites, these spectra support the interpretation that TMR-Z promotes mineral deposition at the interface." This conclusion does not appear to be directly supported by the presented EDS results, as no fluoride signal was identified for TMR-Z, whereas EverX demonstrated clear fluoride availability. Moreover, the interpretation of remineralization for TMR-Z seems to rely predominantly on the emergence of an FTIR shoulder at ~878 cm^{-1}. However, the manuscript itself acknowledges that this band is classically assigned to the v_2 carbonate mode in apatites. While carbonate incorporation can be associated with biological apatite, it does not constitute direct evidence of fluoride-mediated remineralization or ion-exchange–driven mineral deposition, particularly in the absence of corroborating elemental data.
Please clarify why EverX, despite confirmed fluoride presence and acknowledged remineralization potential in the EDS discussion, is interpreted as exhibiting "less stable" interfacial mineral formation compared to TMR-Z.

8) Precision in bioactivity terminology: Several statements use definitive language about remineralization/mineral deposition by TMR-Z Fill 10 (e.g., "enhanced remineralization" in the Abstract/Conclusion and "promotes mineral deposition at the interface" in the Discussion). However, since the study relies on single-timepoint EDS and FTIR performed after restoration completion, the data primarily reflect material composition and spectroscopic signatures at the interface, rather than demonstrating an active, progressive remineralization process over time. I recommend revising these claims to more conditional wording (e.g., "potential to support remineralization," "may promote mineral deposition," "designed to support bioactive interactions"), clearly distinguishing material properties from time-dependent interfacial processes, and briefly acknowledging that direct evidence of remineralization would require longitudinal/time-resolved measurements.
