

GENERAL COMMENTS

The manuscript titled “A Revised Temperature-Dependent Remineralization Scheme for the Community Earth System Model (v1.2.2)” has been revised in response to the reviewers’ comments. However, the revisions are limited, and the manuscript still requires substantial improvement and scientific robustness before it can be considered for publication in my opinion. Several issues previously raised remain unaddressed. I provide additional comments below.

MAJOR COMMENTS

1. In Figure 9, as noted previously, the labels for PI and Tdep remain unchanged. The scatter corresponding to PI still shows higher R^2 and lower cRMSE, which contradicts the conclusions stated in the manuscript. This discrepancy needs to be clarified; otherwise, the results suggest that the skill of the new remineralization parameterization is worse than that of the previous scheme.

In addition, Figure 9 is not well suited for evaluating model performance, as the two metrics shown are independent of each other. Instead, I recommend using a Taylor diagram, which is a standard tool for assessing climate model performance in terms of correlation, root-mean-square error, and the ratio of variances (Taylor, 2001).

Taylor, K.E. (2001). Summarizing multiple aspects of model performance in a single diagram. *JGR: Atmosphere*, 106 (D7), 7183–7192

2. In Figure 11, the authors average over the IAO region, which appears to combine the Indian Ocean and the Arctic Ocean. Is there a specific reason for grouping these two regions into a single category? These regions represent very different oceanic environments—for example, the Indian Ocean is a warm, predominantly tropical basin, whereas the Arctic Ocean is a cold, polar system.

In addition, the authors state that the regional definitions follow Weber et al. (2016); however, that study does not combine the Indian and Arctic Oceans into a single region. Finally, even if the ocean-region names are adopted from Weber et al. (2016), it is strongly recommended that all abbreviations be explicitly defined (as was done for AAZ and ETP) to avoid ambiguity.

3. The authors stated in their response that clarification regarding the use of the last 30 years would be provided in Section 2.3; however, the manuscript does not currently include such an explanation. As I understand it, the analysis involves interannual variability, with results presented as 30-year averages. Nevertheless, climate models inherently exhibit year-to-year variability arising from internal variability and model-specific characteristics, particularly in CESM. I therefore recommend that the associated uncertainties be explicitly represented by showing the ranges of interannual variability, for example using standard deviations. Specifically, Figures 5 and 6 could include latitude-dependent shading to indicate variability ranges, and Figure 11 could present uncertainty ranges (e.g., error bars) for each bar.

4. Finally, Figure 7 requires additional clarification by providing quantitative performance metrics, such as the RMSE between PI and Tdep for each location. Aside from the two Equatorial Pacific regions, PI appears to show better agreement with observations at several sites (e.g., ALOHA, Peru, Arabian Sea). Therefore, further information is needed to clearly demonstrate that the new temperature-dependent remineralization parameterization represents a genuine improvement over the previous formulation, rather than a degradation in model performance.