Response to comments of Referee #2

General comment:

This manuscript evaluates the ENSO simulation capability of the coupled Korea Integrated Model (KIM), in which the NEMO ocean model is coupled to the atmospheric component, with the broader aim of extending the model toward extended-range prediction. The authors compare the basic characteristics of ENSO simulated by KIM with those simulated by CMIP6 models that also employ the NEMO ocean model, and attempt to relate the ENSO biases to biases in the tropical Pacific mean state.

I consider this type of evaluation to be an important and necessary step in extending a numerical weather prediction (NWP) model toward climate and extended-range prediction applications. The manuscript also contains useful diagnostics of the ENSO characteristics in KIM. However, I have several major concerns regarding the experimental design, the fairness of the comparison with CMIP6 models, and the quantitative basis for some of the physical interpretations. In its current form, I believe these issues need to be addressed before the manuscript can be considered for publication. Addressing the following issues would help the manuscript move beyond a model-performance report toward a more scientifically rigorous analysis of the physical mechanisms controlling ENSO characteristics in the coupled KIM.

We sincerely thank the reviewer for the careful and thorough evaluation of our manuscript. Following the reviewer's suggestions, we have substantially revised the manuscript. First, we changed the analysis period from 1952-1990 to 1976-2014. Second, we added an analysis of the zonal advection feedback to better characterize the simulated ENSO properties. In addition, we revised the Abstract and the Discussion section to be consistent with these changes and to state the objective of the study clearly. We believe the revisions improve the clarity and quality of the paper. We also hope that our revisions adequately address the reviewer's points. Detailed point-by-point responses to each specific comment are provided below, and corresponding changes are indicated in the revised manuscript.

Specific comments:

1. Fairness of the comparison with CMIP6 models and overstatement of model performance

As I understand the experimental design, the ENSO characteristics of the CMIP6 models are evaluated over 1976-2014, whereas the coupled KIM simulation is evaluated over 1952-1990. The observational analysis based on OISST covers 1982-2020. I understand that the observational period does not necessarily have to be identical to the CMIP6 historical period, since the CMIP6 historical simulations are not initialized to reproduce the observed temporal evolution of the climate system.

However, I have a concern with the way that KIM analysis period is selected. The authors examine several 39-year periods from the KIM simulation and select 1952-1990 because this period has a mean SST climatology that is most similar to observations. This selection makes the subsequent comparison with the CMIP6 models potentially unfair, because the KIM period is effectively selected based on its agreement with observations, whereas the CMIP6 models are evaluated over a fixed period (1976-2014). The selection criterion itself is therefore not

applied consistently across the models. This issue is particularly important because the manuscript subsequently uses this comparison to characterize KIM as having "above-average performance", "outstanding skill", or more realistic ENSO characteristics than other models. These statements appear stronger than what can be supported by the current experimental design.

I therefore suggest that the authors: 1) evaluate KIM over the same 1976-2014 period used for the CMIP6 models, and provide the corresponding comparison. 2) If the authors wish to retain the 1952-1990 period as an additional sensitivity analysis, clearly explain that this period was selected based on its mean-state agreement with observations and avoid using it as the primary basis for ranking KIM against the CMIP6 models. 3) Reconsider and tone down statements such as "above-average performance", "outstanding skill", and "more realistically" unless these claims are supported by a comparison based on common evaluation period and consistent methodology.

We thank the reviewer for this careful and constructive comment. We agree that the original choice of analysis period for KIM introduced an inconsistency in the comparison with the CMIP6 models, and that this inconsistency weakens the basis for some of our statements about KIM's relative performance. In the original manuscript, the 1952–1990 period was selected for KIM because its mean SST climatology was closest to the observations. Our reasoning was that the CMIP6 evaluation period (1976–2014, the last 39 years of the historical simulations) was likewise expected to represent the period whose mean state is closest to the recent observed climate. Because this is the first multi-decadal integration of the coupled KIM, the simulation still exhibits a drift in the mean state toward the end of the integration, and we therefore chose the period with the most realistic mean state. However, as the reviewer correctly points out, this criterion was applied only to KIM, while the CMIP6 models were evaluated over a fixed period. Following the reviewer's suggestions, therefore, all analyses and figures for KIM have been recomputed for 1976–2014, the same period used for the CMIP6 models. The descriptions of the results have been revised accordingly. Regarding the reviewer's second suggestion, the 1952–1990 period is retained only as a sensitivity test and has been moved to the Discussion, where it no longer forms part of the comparison with the CMIP6 models. The Discussion now states explicitly that this period was selected because it is the window in which the simulated SST most closely matches the observations, and that it serves only to confirm that the conclusions are insensitive to the choice of analysis window. The main results remain largely unchanged with the new analysis period, which indicates that our conclusions are robust to the choice of analysis period.

[L148-163: The analysis period of the coupled KIM is 1976–2014, the same period used for the CMIP6 historical simulation. Figure 1a shows the global-mean, annual-mean net radiative flux at the top of the atmosphere (TOA), defined as incoming solar radiation minus the sum of outgoing longwave and reflected shortwave radiation, from the coupled KIM simulation and the ERA5 reanalysis. The net radiative flux in the coupled KIM converges toward zero after about 50 years of integration, indicating a substantially reduced energy imbalance in the

coupled system compared with the early stage of the integration. From the late 1970s onward, however, the net radiative flux becomes slightly positive. This is comparable to the ERA5 estimate, indicating that the simulated TOA energy imbalance during 1976-2014 is broadly consistent with the observed one. Figure 1b presents the annual-mean SST averaged over 60°S–60°N from the coupled KIM simulation and OISST. Consistent with the positive TOA net radiation flux, the simulated SST exhibits a pronounced warming trend after the 1970s. Over the evaluation period, the simulated warming rate is comparable to that of OISST. The simulated SST, however, is systematically warmer than the observations by about 0.4K. This mean state bias may arise from differences in the model's transient response to external forcing relative to the real climate system, including differences in adjustment timescale and effective climate sensitivity. It may also partly reflect the fact that the long-term coupled simulation has not yet been fully optimized for historical transient forcing—a common challenge when adapting an NWP-focused model for extended climate integrations. Because key aspects of coupled atmosphere–ocean variability, including ENSO, are highly sensitive to the background mean state, this warm bias should be kept in mind when interpreting the results.]

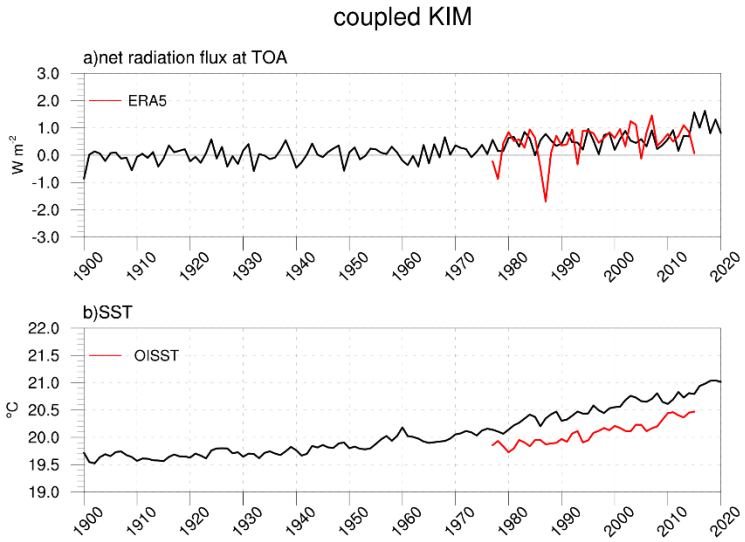


Figure 1: a) Annual-mean, global-mean net radiative flux at the top of the atmosphere (TOA) and b) annual-mean sea surface temperature (SST) averaged over 60°S–60°N in the coupled KIM simulation over the period 1900–2020. The TOA net radiative flux is defined as incoming solar radiation minus outgoing longwave and reflected shortwave radiation. The ERA5 and the OISST time series for 1982–2020 are overlaid in red in (a) and (b), respectively, aligned with the model evaluation period of 1976–2014.

[Figures 2 and 3: ENSO pattern – the main results remain unchanged after the analysis period was changed. However, the sentence in L222, "overall, the coupled KIM simulates the CT intensity more realistically than most of the other models." has been removed. With the revised analysis period, the CT becomes warmer, and this statement is no longer supported.]
[Figures 4 and 5: ENSO intensity and skewness – With the revised analysis period, the ENSO

amplitude simulated by the coupled KIM becomes closer to the observed values, while its skewness changed weakly negative. The corresponding text has been revised accordingly. Note that this part has been further revised in relation to Comment #2; the detailed changes are described in our response to that comment]
[Figures 6, 7, 8 and 9: ENSO seasonality – the main results remain unchanged after the analysis period was changed. These figures and the related text have been further revised in response to Comment #3; please refer to our response to that comment for details.]
[Figure 10: ENSO teleconnection - the main results remain unchanged after the analysis period was changed to 1976-2014.]
[Figure 11: Portrait diagram summarizing ENSO metrics – changing the analysis period did not substantially alter the results shown in Fig. 11, except for a slight degradation in the simulated SST climatology. We revised the summary of ENSO simulation performance under different analysis periods to be consistent with the main text.]
[L478-482: Model drift may, however, affect the mean state metrics, so we tested the robustness of the results to the choice of analysis period. We repeated the analysis using the model years 1952–1990, a period selected specifically because the simulated SST most closely matches the observed SST over that window (see Fig. 1b). This selection criterion was not applied to the CMIP6 models, so the test is used solely to assess robustness and not as a basis for model comparison. The overall results remained largely unchanged, which indicates that the conclusions are robust to the choice of analysis period.]

Regarding the third suggestion, we have reviewed each of the flagged expression.
[L217 in the original manuscript: "more realistically.." – removed]
[L190: "more realistically.." – retained, as this refers to a specific pattern comparison supported by Fig. 2 rather than an overall ranking claim.]
[L450: "above-average performance" – removed and revised to "performs comparably to the multi-model average for most ENSO related metrics".]
["outstanding skill" – this expression no longer appears in the revised manuscript.]

Another important issue is that the coupled KIM has substantially higher resolution than the CMIP6 models included in the comparison. Therefore, some of the differences in ENSO characteristics may reflect the higher model resolution rather than the specific physical improvements or configurations of KIM. This difference in resolution should be explicitly acknowledged when interpreting the comparison.
We fully agree with the reviewer and recognize this as an important point. The coupled KIM was run at high resolution because KIM was originally developed as a high-resolution numerical weather prediction (NWP) model, with its dynamical core and physical parameterizations designed and tuned for high-resolution applications. Running the model at a coarser resolution comparable to the CMIP6 models would therefore have required substantial retuning of the physics and would not have represented the model configuration as developed. We nevertheless acknowledge that the higher resolution may contribute to some of the

differences between KIM and the CMIP6 models. In the revised manuscript, we explicitly acknowledge this resolution effect.

[L485-493: Another important difference is horizontal resolution. The coupled KIM was integrated at a higher resolution than most of the CMIP6 models considered (Table 1). The higher resolution reflects the origin of KIM as an NWP model, whose key physical parameterizations were developed and tuned for high-resolution applications (Hong et al., 2018). Integrating the coupled KIM at high resolution was therefore considered appropriate for evaluating the intrinsic performance of the coupled KIM. However, model resolution is well known to exert a strong influence on ENSO simulation (Wengel et al., 2021; Liu et al., 2022). The performance of the coupled KIM shown in Fig. 11 therefore likely reflects the combined effects of its higher resolution and the improved atmospheric and oceanic physics of the coupled model, which cannot be separated in the present study. Disentangling these effects, for example through sensitivity experiments at resolutions comparable to those of the CMIP6 models, will be an important subject for future work.]

2. Need for more quantitative analysis of ENSO feedbacks and quantitative scientific interpretation

The manuscript attributes several ENSO characteristics of coupled KIM to biases in the tropical Pacific mean state, particularly through zonal advective feedback and thermocline feedback. These are physically reasonable mechanisms, but the current discussion remains largely qualitative. There is a substantial body of previous literature that quantitatively diagnoses ENSO feedbacks, including studies using the same or closely related CLIVAR ENSO metrics. For example, the zonal advective feedback and thermocline feedback can be quantified through terms such as (-u'dT/dx) and (-wdT'/dz), respectively. Despite the availability of theses quantitative approaches, the manuscript does not appear to provide quantitative estimates of the relevant feedback terms, nor does it sufficiently cite the literature establishing these diagnostics. I recommend that the authors substantially strengthen this part of the analysis by: 1) providing quantitative estimates of the relevant ENSO feedback terms and citing previous studies that have established these diagnostics; and 2) comparing the magnitude of the feedbacks between KIM, observations/reanalysis, and the CMIP6 models, where appropriate. If access to the necessary ocean variable from the observations and CMIP6 models is limited, the authors could alternatively compare the KIM results with those reported in previous studies that have quantitatively diagnosed these feedbacks in CMIP models. More generally, quantitative information should be provided throughout the manuscript whenever possible. The current manuscript frequently describes differences in terms such as "larger", "smaller", "stronger", or "weaker", without providing the magnitude of these differences. More quantitative presentation would make the conclusions substantially more rigorous.

We thank the reviewer for this constructive comment. We agree that our original discussion of ENSO feedbacks was largely qualitative. In response, we have substantially revised the ENSO intensity and skewness sections by adding quantitative information of the zonal advection feedback and supporting literature. In addition, throughout the manuscript, we have replaced

qualitative descriptions with quantitative ones wherever possible.

[L235-250: Following the Bjerknes stability index framework (Jin et al., 2006; Kim and Jin, 2011), these feedbacks can be written as $-u' \partial \bar{T}/\partial x$ and $-\bar{w} \partial T'/\partial z$, where u' denotes the anomalous zonal ocean current in the mixed layer, $\bar{T}$ the climatological ocean temperature, T' the ocean temperature anomaly, and $\bar{w}$ the climatological upwelling velocity at the base of the mixed layer. Because subsurface ocean variables and zonal current anomalies are not archived for the coupled KIM, we focus on the zonal advection feedback and estimate it using zonal wind stress anomalies as a proxy. Figure 4b displays the zonal wind stress anomalies regressed onto the Niño3.4 index, multiplied by the climatological zonal SST gradient averaged over the Niño3.4 region. This feedback partly explains the inter-model spread in ENSO intensity. In particular, models with a strong mean zonal SST gradient (Fig. 3b), such as IPSL-CM6A-LR and CMCC-CM2-SR5, exhibit both a strong zonal advection feedback and a large ENSO amplitude. However, CanESM5 has a stronger-than-observed zonal advection feedback but one of the weakest ENSO amplitudes, whereas EC-Earth3 simulates a near-zero feedback but an ENSO amplitude comparable to that of the other models (Fig. 4). These cases indicate that other processes, such as the thermocline feedback and thermodynamic damping, contribute substantially to the inter-model spread in ENSO intensity (Kim et al., 2014; Bellenger et al., 2014). The coupled KIM has the second smallest zonal SST gradient among the models examined (Fig. 3b). Nevertheless, its zonal advection feedback ranks fourth-largest, and its ENSO intensity is close to the observed value. This suggests that the coupled KIM simulates a relatively strong ENSO-related zonal wind stress response.]

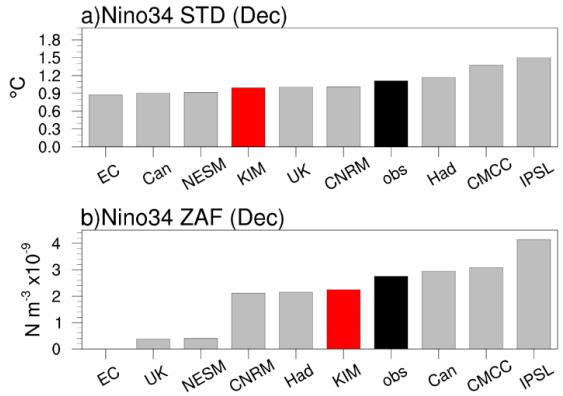


Figure 4: (a) Standard deviation of the December Niño3.4 index and (b) zonal advection feedback proxy, defined as $-\tau x' \partial T/\partial x$, averaged over the Niño3.4 region for observation (black bar), the CMIP6 models (gray bars), and the coupled KIM (red bar).

3. ENSO seasonality and the mechanism responsible for the flattened seasonal cycle

The discussion of the "flattened ENSO seasonality" in Figure 5-7 primarily focuses on differences in the tropical Pacific mean state between September-October and January-February. While these comparisons are useful, I am not fully convinced that they identify the process responsible for the flattened ENSO seasonal cycle. In particular, the mature phase of ENSO occurs around November-December, and the results appear to suggest that ENSO

growth during this period is insufficient. Therefore, rather than focusing primarily on the mean-state differences between September-October and January-February, it would be useful to examine how ENSO actually grows throughout the seasonal cycle. I suggest that the authors investigate the seasonal evolution of ENSO growth rate and examine the contributions of the different feedback processes discussed above, particularly the zonal advective feedback and thermocline feedback. For example, diagnosing the seasonal evolution of these feedbacks would help determine whether the weakened ENSO peak is associated with reduced growth during the developing phase, changes in thermocline feedback, changes in zonal advective feedback, or other processes. Such an analysis would provide a much stronger physical explanation for the reported "flattened ENSO seasonality" than comparison of mean-state biases between two selected seasons.

We appreciate the reviewer for this insightful comment. We agree that comparing mean-state biases between two selected seasons alone is insufficient to fully explain the process responsible for the flattened ENSO seasonality. We also agree that the seasonal evolution of the feedbacks governing ENSO growth should be examined directly. Following the reviewer's suggestion, we have extended the analysis by adding monthly evolution of the zonal advection feedback from September to February. The corresponding changes are as follows.

[L290-308: To diagnose the cause of the flat ENSO seasonality in these models, we further examine the monthly evolution of the mean state biases and zonal advection feedback, focusing particularly on the period from September to February. Figures 7a and 7b show the time evolution of the monthly climatological zonal SST gradient and zonal wind stress averaged over the central tropical Pacific (5°S–5°N, 180°–220°E for SST and 190°–240°E for zonal wind stress). Figure 7c shows the corresponding zonal advection feedback $(-\tau x' \partial T/\partial x)$ averaged over 5°S–5°N, 190°–260°E. As noted in Figs. 3a and 3b, the CT bias in many models leads to an overall overestimation of the zonal SST gradient in the central Pacific. However, a distinct seasonal feature of this bias is found; the overestimation is most pronounced during September–December, whereas the gradient becomes generally weaker than observations during January–February (JF) (Fig. 7a). Consistent with the SST gradient, the zonal wind stress exhibits an enhanced easterly stress bias from September to December, whereas in JF, this bias tends to be weaker in most models (Fig. 7b). The seasonal evolution of the zonal advection feedback reflects these mean-state biases (Fig. 7c). In observation, the zonal advection feedback remains at a moderate level throughout September-February, with a relatively large value in October. This result is consistent with previous findings that the zonal advection feedback strongly modulates the seasonal SST growth rate preceding the boreal winter ENSO peak (Chen and Jin, 2022). In contrast, most models overestimate the zonal advection feedback from September to November, followed by a steady decline below the observed level by February. The inter-model spread is large, and a few models simulate a near-zero zonal advection feedback throughout the season. Nevertheless, the dominant tendency is an enhanced feedback in boreal autumn followed by a weakened feedback in late winter. This seasonal distribution of the zonal advection feedback favors SST anomaly growth earlier than observed

and limits it during winter. As a result, it contributes to the 'flattened' ENSO seasonality, in which the amplitude is spread over September-February rather than peaking sharply in NDJ (Fig. 6).]

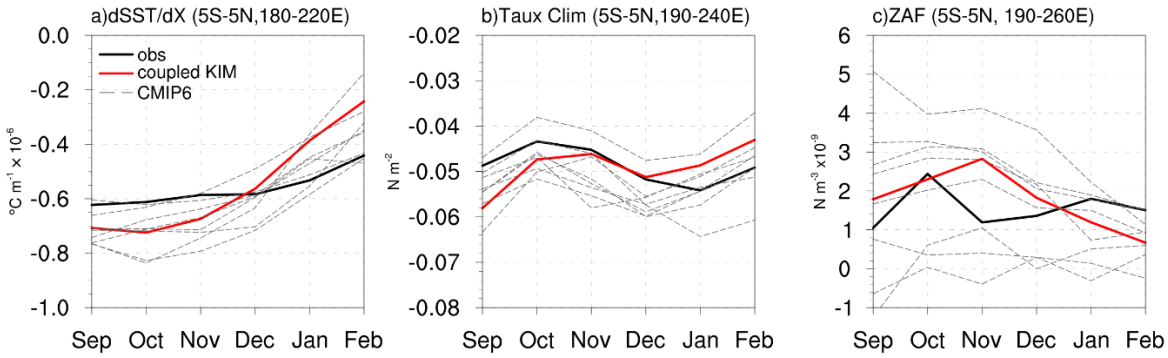


Figure 7: (a) monthly climatology of the zonal SST gradient averaged over 5°S–5°N, 180°–220°E, (b) monthly climatology of zonal wind stress averaged over 5°S–5°N, 190°–240°E, and (c) monthly zonal advection feedback proxy, defined as $-\tau x' \partial T/\partial x$, averaged over 5°S–5°N, 190°–260°E, from September to February for observation (black line), the CMIP6 models (gray lines), and the coupled KIM (red line).

4. Clarification of the scientific objective in the Abstract

The objective of the study is not sufficiently clear from the Abstract. In particular, it is difficult to determine whether the primary purpose of the study is: to evaluate the ENSO characteristics of a long-term historical coupled KIM simulation, or to demonstrate the suitability of KIM for extended-range ENSO prediction. These are related by distinct objectives and require different experimental designs and evaluation frameworks. The abstract should be revised so that the primary scientific objective is immediately clear and is consistent with the actual experiments and results presented in the manuscript. I also have concerns about describing the experiment as a "171-year simulation." According to the methodology, the first 100 years are excluded from the analysis, while the forcing after 2015 is held fixed at the 2014 level. Therefore, the full 171-year integration period does not correspond to the period actually analyzed or to a continuous historical-forcing experiment. I suggest that the authors describe the actual analysis period more explicitly in the Abstract rather than emphasizing the total integration length. If the full integration length is important for model development or spin-up, this can be explained in the main text.

We thank the reviewer for this helpful comment. The primary objective of this study is to evaluate ENSO simulation characteristics of the coupled KIM. The coupled KIM has been developed on the basis of KIM, an NWP model, for extended range prediction. We examined its ENSO characteristics in a long-term simulation to assess whether its current development direction is appropriate. The study therefore focuses on the evaluation of ENSO characteristics, not on demonstrating the suitability of the model for extended-range prediction. We have revised the Abstract to state this objective clearly. Following the reviewer's suggestion, we no

longer emphasize the total integration length in the Abstract. Instead, we explicitly state the 39-year analysis period. In addition, we have revised the Abstract as a whole to reflect the changes made to the main text in response to the reviewer's other comments.

[L5-20: **Abstract.** This study presents a comprehensive evaluation of the El Niño–Southern Oscillation (ENSO) simulation characteristics in the newly established coupled Korea Integrated Model (coupled KIM). To robustly diagnose ENSO behavior, a long-term coupled simulation was conducted and systematically evaluated against eight CMIP6 models that share the Nucleus for European Modelling of the Ocean (NEMO) as their oceanic component. Benchmarking the CLIVAR 2020 ENSO metrics framework, we evaluate diverse aspects of ENSO, including its spatial pattern, intensity, skewness, seasonality, and teleconnections, within the context of the tropical Pacific mean state based on 39-year analysis period. The coupled KIM simulates a warm mean sea surface temperature (SST) bias in the eastern equatorial Pacific. This bias weakens the zonal SST gradient and thereby reduces the excessive westward extension of the ENSO pattern, partly through error compensation. ENSO intensity is reasonably simulated, supported by a zonal advection feedback comparable to the observed value. The seasonal phase locking of ENSO is also realistically represented. However, a more detailed analysis reveals that most models, including the coupled KIM, overestimate ENSO amplitude in autumn and underestimate it in winter. In the coupled KIM, the eastern Pacific warm bias extends westward in January-February, weakening the zonal SST gradient and thus the winter peak of ENSO. This westward extension is associated with the double intertropical convergence zone bias, which is pronounced during this period. The ENSO precipitation teleconnection is well represented, whereas the Northern Hemisphere extratropical circulation response requires improvement. Overall, the coupled KIM performs comparably to the multi-model average for most ENSO metrics.]

Technical Corrections:

1. Definition of Niño regions

The manuscript does not consistently use the Niño-3 and Niño-3.4 regions, and the geographical boundaries of these regions are repeatedly described throughout the manuscript. This makes the results somewhat difficult to follow. I suggest defining all Niño regions, including their longitude/latitude boundaries, clearly in the Methods section. The Results section can then simply refer to "Niño-3," "Niño-3.4," etc. This would improve readability and avoid repeatedly presenting geographical definitions.

We thank the reviewer for this helpful suggestion. Following the comment, we move the definition of Niño3.4 index to the Methods section.

[L126-128: ENSO variability is characterized using Niño3.4 and Niño3 index, which are defined as area-averaged SST anomalies over 5°S–5°N and 170°W–120°W, and 5°S–5°N and 150°W–90°W, respectively.]

2. Lines 275–280: Interpretation of the NEMO vertical diffusion scheme

The discussion attributing the simulated ENSO skewness to the improvement in the NEMO vertical diffusion scheme appears rather speculative if the corresponding sensitivity-test results are not presented. I am not necessarily suggesting that an additional experiment is required. However, if the authors do not intend to provide the sensitivity experiments supporting this interpretation, the statement should be toned down and presented as a possible explanation or hypothesis rather than as an established mechanism.

We agree with the reviewer's comment. In the revised manuscript, the analysis period was changed from 1952-1990 to 1976-2014. With this change, the simulated ENSO skewness changed slightly from about 0.08 to -0.02, and the relative performance of the coupled KIM among the models remained the same. The coupled KIM therefore still simulates ENSO skewness better than many of the CMIP6 models, particularly models that simulate stronger La Nina than El Nino events. However, its performance is not distinctly superior to that of the other models. We therefore concluded that the result does not provide sufficient grounds to attribute the skewness performance to the improved NEMO vertical diffusion scheme. The corresponding discussion has been removed from the revised manuscript.

[Deleted L279-288 of original manuscript]

3. Lines 112–115: Forcing after 2014

The forcing is held fixed at the 2014 level for the 2015–2020 period. This makes the 2015–2020 integration different from a standard CMIP historical simulation and also complicates the comparison with the CMIP6 models. Since this period is not used in the primary analysis, I question whether this information needs to be emphasized in the manuscript.

We agree with the reviewer. Since the 2015-2020 period is not used in the primary analysis, we have removed the corresponding sentence.

[Deleted L115 of original manuscript]

4. The red line in Figure 1b does not appear to represent the 1982–2020 period stated in the caption. Please check the figure and revise the caption accordingly.

We thank the reviewer for pointing this out. The red lines in Fig.1 represent the observed time series for 1982-2020 (ERA5 and OISST). Note that the ERA5 TOA net radiation flux in Fig. 1a was newly added in the revised manuscript in response to suggestion from Reviewer #1. However, they were plotted on the model time axis, corresponding to the model evaluation period of 1976-2014. This alignment was adopted because the model evaluation period (1976-2014) is constrained by the end of the CMIP6 historical simulation in 2014, whereas OISST is available only from late 1981. To compare the model with observations over a period of equal length (39 years), we used the observed period 1982-2020. We recognized that this was not clearly described in the original caption. We have revised the caption to state this explicitly as follows:

[L166-170: The ERA5 and the OISST time series for 1982–2020 are overlaid in red in (a) and (b), respectively. The observational time axis is shifted by 6 years to align with the model evaluation period 1976-2014 (both 39 years).]

5. The units of the model resolutions in Table 1 should be explicitly indicated. It would also improve readability to list the atmospheric and oceanic resolutions separately rather than presenting them in a single filed.

We thank the reviewer for this helpful suggestion. Following the reviewer's comment, we have revised Table 1 as follows. The single "Resolution (Atm/Ocn)" column has been split into two separate columns for the atmospheric and oceanic resolutions. Units are now explicitly indicated.

[Table 1]

6. Units are missing from many of the figures. Please add the appropriate units to the figure captions throughout the manuscript.

We thank the reviewer for this careful comment. We have added the appropriate units to all figures (axis labels and color bars) and to all figure captions throughout the revised manuscript.

[All figures and captions]