First of all, we would like to thank the reviewers for their important comments. Our point-by-point response is written in blue.

Reviewer 2

The authors provide analysis of the wavenumber frequency spectrum of outgoing long-wave radiation (OLR) and zonal wind data in the tropical atmosphere, comparing CMIP-6 model data with historical observations or reanalysis products, and then looks at the spectra in models with changing climate via CO2 forcing. The analysis is a nice follow-on to the earlier work of Lin, J.-L., G.N. Kiladis, B.E. Mapes, K.M. Weickmann, K.R. Sperber, W. Lin, M.C. Wheeler, S.D. Schubert, A. Del Genio, L.J. Donner, S. Emori, J.-F. Gueremy, F. Hourdin, P.J. Rasch, E. Roeckner, and J.F. Scinocca, 2006: Tropical intraseasonal variability in 14 IPCC AR4 climate models. Part I: Convective signals. J. Climate, 19, 2665-2690, doi:10.1175/JCLI3735.1. that I think needs to be cited here. added. The science is well argued, with quality English and good overall organization. Thank you.

I have serious concerns about interpretation of the results and the conclusions, but my concerns apply to a wide array of similar studies in the field, and the main work I provide to explain my concern is one of my own papers. As such, I sign my review, and I select minor revisions and suggest that the authors are not required to apply my feedback outside of the reference above and the minor comment below.

My concern applies to nearly all papers that assess the spectrum of equatorial waves, including my own and those I have made together with George Kiladis and his research group. So my intention is not to highlight the authors of the present manuscript in particular. Interpreting the background spectrum has been one of the most difficult aspects of understanding the behaviors of convectively coupled equatorial waves, and of the organization of tropical convection more generally. Considering the matter carefully, the background cannot be just some obscure random variability in convection. Any longitude-time representation of OLR anomalies would show that, for example, a kelvin wave that moves around the world does so at different phase speeds through different environments. The same disturbance might move at 20 m/s through the western hemisphere, and slow to less than 10 m/s over the Indian Ocean, and exhibit a full range of phase speeds in between these extremes in other regions or at different times of the year or background states in the historical record. The background wind and background moisture environments evidently modulate the phase speeds of events as they move around the world. Thus even the same disturbance will project to different parts of the power spectrum at different parts of its lifetime. In my paper `https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.3709` I showed that the upper tropospheric wind and geopotential height patterns associated with disturbances in the Wheeler and Kiladis (1999) Kelvin wave band are structurally indistinguishable from disturbances near the MJO band and from disturbances in what are often assumed to be the noise region in between the two bands. The principal difference between them is amplitude and phase speed. Higher amplitude is achieved in the MJO band, along with slower propagation. If the disturbances in between the MJO and Kelvin bands were random noise, we might rather expect them to take on structures like random Gill-type responses to random heating. Instead, these intermediate speed disturbances over the Indian Ocean emerge from faster, zonally broader events in the Western Hemisphere. The result suggests that much of the variance in that band in between is mostly Kelvin-like waves that happen to move at the speed they do because of the background wind and moisture states they happen to intersect.

Another of my previous works, including Roundy, P. E., 2021: World Scientific Series on Asia-Pacific Weather and Climate. The Multiscale Global Monsoon System, pp. 301-314. Chapter 24: The Association between the Phase Speed of the Madden-Julian Oscillation and Atmospheric Circulation. `https://doi.org/10.1142/9789811216602_0024` and a PhD thesis Suematsu, T, 2018: A study on the background sea surface temperature field and moist processes contributing to the realization of the Madden-Julian Oscillation, Ph.D. dissertation, The University of Tokyo.

presented at Suematsu, T., and H. Miura, 2020: Modulation of MJO propagation speed by the fluctuation of large-scale background zonal circulation, 100th AMS Annual Meeting.

concur that upper tropospheric background wind modulates the phase speed of the MJO and convectively coupled waves.

Calculation of the background spectrum necessarily results in power being transferred from regions of high power in the spectrum, down the strongest power gradients in the spectrum, to neighboring regions of lower power. The total spectrum has its strongest gradients at frequencies on the upper end of what we traditionally call the Kelvin band. Thus power is preferentially removed from the Wheeler and Kiladis Kelvin band to higher frequencies and lower wavenumbers, thereby leaving a part of the smoothed spectrum next to that steep gradient with less power. it is therefore no surprise that the region immediately below the region of the gradient will have high power relative to the smoothed background. It's not that waves occurring at lower frequencies are not also Kelvin waves, it's that we highlight those particular Kelvin waves as special because they occur where our artificially defined background spectrum stands most apart from the actual spectrum. Yet it is obvious from my above paper and from direct observation of unfiltered OLR anomalies that disturbances we call Kelvin waves occur at a wide range of slower phase speeds at frequencies higher than the Wheeler and Kiladis Kelvin band.

Given that the above works also demonstrate that background zonal wind is a major player in determining the phase speeds of observed anomalies, it follows that the location in the spectrum of the strongest gradients in power will change depending on the mean background winds in the models. For example, models with stronger Western Hemisphere upper tropospheric equatorial westerly winds will have Kelvin waves that move eastward more rapidly than in models that have weaker winds in that region. A model with stronger upper tropospheric easterly wind over the Indo Pacific warm pool will also have slower MJO and Kelvin signals.

My main contention, therefore, is that the authors should consider at least including a caveat with respect to their interpretation of the normalized power spectrum, and also with respect to where Kelvin waves can be found in the spectrum. It is probable that models that have faster Kelvin waves at the upper end of the Kelvin wave range have background winds in the Western Hemisphere that are stronger than other models, because such fast Kelvin waves typically appear there and would be faster under such conditions. We agree that the background wind can have a large effect on the Kelvin wave phase speed, and we have added this to the paper. We also cite the Roundy (2020a) and Roundy (2020b) papers when we discuss the various interpretations of the meaning of the background spectrum. Thank you for your detailed explanation.

Minor comment:

The boxed regions for filtering in Figure 4 can be quite small. The smaller the band, the more the Gibbs ringing phenomenon will play in. This effect may dilute some of the relevant signal in the filtered data. In response to the other reviewers, we now focus on OLR, which led to the precise boundaries to change. However, the band is still relatively small, as for this band all of the models we include simulate the KW (except the INM models, which effectively lack a KW). A broader region would lead to including regions in spectral space outside of the KW for at least one specific model, so that this band is more representative than other bands checked.

That being said, we have explored using a larger band. Specifically, Figures 1-3 below show the KW plots from Figures 5,7,8 in the article compared to plots for a broader KW band. The conclusion remains the same: according to both OLR and U250, KW are projected to intensify.

Signed, Paul Roundy

Reviewer 3

General comments:

The paper entitled "Projected Future Changes in the Equatorial Wave Spectrum in CMIP6" authored by Bartana et al. discussed the simulation of Madden-Julian Oscillation (MJO) and convectively couple equatorial waves (CCEWs), including Kelvin waves (KW) and equatorial Rossby (ER) waves, by comparing the 13 Coupled Model Intercomparison Project phase 6 (CMIP6) models against data from ERA5 U250 and NOAA OLR. In practice, the authors applied frequency-wavenumber power spectra of Outgoing Longwave Radiation (OLR) and zonal wind at 250 hPa (U250) to illustrate the signal intensity of simulated MJO and CCEWs and analyzed the differences between observations and model simulations. They also considered the projected changes based on the CMIP6 SSP245 and SSP585 scenarios. The authors found that most models have underestimated the power of MJO and KW but overestimated the power of ER compared with observations. In the future warming scenarios, the models with larger biases of MJO in the present-day climate tend to have a weaker intensification; and those with smaller present-day biases tend to have stronger MJO signals in the future warming scenarios. By contrast, opposite results occur for KW signals.

Overall, this paper is well written and the methods used for conducting model evaluation are reasonable. However, it seems to me that the current version looks more like a technical report rather than a research article. In the revised manuscript, the authors should provide an in-depth discussion of the potential factors that might contribute to the diversity of model biases in simulating CCEWs before it can be accepted for publication. My specific comments are listed below:

Specific comments:

1. As shown in Fig. 4 for the differences in power spectral density (PSD) of U250 between model simulations and observations, the diversity of biases exists. For instance, at least 3 models (BCC-CSM2-MR, CESM2, UKESM1-0-LL) clearly overestimate the KW PSD while 7 models (CNRM-CM6-1, CNRM-ESM2-1, EC-Earth3, FGOALS-g3, INM-CM4-8, INM-CM5-0, MPI-ESM1-2-LR) underestimate the KW PSD. An in-depth discussion of the potential factors (e.g., model resolution and cumulus parameterization scheme) attributing to such diversity is needed to enhance the novelty of this study. We added a short discussion in the text, and a more detailed discussion of the details of the models as Online Resource 1. That being said, we are unable to pin down exactly how different convection schemes affect KW amplitude, as there are still numerous tuning knobs for each scheme and it isn't always clear what the net effect on e.g. convective entrainment actually is. That being said, all of the models (the INM models the exception) have at least a reasonable KW which allows us to focus on future changes. We have added to the discussion section that future work should consider how the details of the convective paratmerizations affect CCEW amplitude.

2. Why did the authors use different data periods (e.g., 1979-2020 for U250, 1974-2021 for OLR, and 1850-2014 for CMIP6 historical run) in assessing biases instead of using the same period as in most previous studies? We now use a common period throughout the paper.

3. The author defined the wavenumber-period window of KW at 3.5k5 and 7T10 days (red rectangles in Figs. 2-4 and 6), which seems to be significantly narrower than previous studies (e.g., Wheeler and Kiladis (1999)). Are the results and conclusions sensitive to the wavenumber-period window chosen? Please see explanation in response to reviewer 2. In short, we picked a relatively small region where all of the models we include simulate the KW (except the INM models, which effectively lack a KW). A broader region would lead to including regions in spectral space outside of the KW for at least one specific model.

4. Lines 79-80, "but a stronger influence in others (Samarasinghe et al, 2021)." Please elaborate further. Example added.

5. Lines 109-110, "Upper level divergence and divergent outflow leads to teleconnections in mid-latitudes" should be corrected as "Upper level diver-

gence and divergent outflow lead to teleconnections in mid-latitudes" Corrected.

6. Lines 146-147, "The MMM shows a generally good representation of both the MJO, KW and ER as compared to observations," should be corrected as "The MMM shows a generally good representation of the MJO, KW and ER as compared to observations," Corrected.

7. Lines 196-199, "Looking at the MMM spectrum, there is a general overestimation of power at lower frequencies (including MJO and ER), and underestimation at higher frequencies. This bias towards low frequencies is similar to that for OLR.". If you look at Figure 4a (MMM - observation), the power of MJO is underestimated (magenta in Fig. 4a), not overestimated. Corrected.

8. Lines 217-218, "Most models have a too-weak MJO bias in U250, similar to the bias in OLR." should be corrected as "Most models have a too-weak MJO in U250, similar to the bias in OLR." Corrected.

9. For ease of comparison, please unify the order of subfigures between observational data and MMM in Figures 1 and 3, and Figures 2, 4, and 6. Corrected.

10. In the descriptions of Figures 7 and 8, please specify what variable (OLR?) are used to calculate the correlation coefficients. We changed the figures so that both parameters are included.

Thank you.

Reviewer 4

The authors calculated space-time power spectrum of equatorial OLR and U250 from 13 CMIP6 simulations and analyzed the results in the context of future changes of convectively coupled equatorial waves and the MJO. They also made several scatter plots of the quantities derived from the power spectrum and discussed them. Although the manuscript is logically written, the scientific merit of the results is questionable due the lack of physical insights into them. The manuscript offers a straightforward documentation of the results with little efforts to provide plausible explanations of why. Why do KW and MJO intensify? Given the rich literature on the dynamics of the convectively coupled equatorial waves and the MJO, the authors should at least discuss their results in the context of theories proposed in previous papers or similar studies that used other models. I do not see any attempt toward that direction in the manuscript. We have added such a discussion: "These results support previous work that has found that the MJO will strengthen due to enhanced frictional moisture convergence, nonlinear wind-induced surface heat exchange, and vertical advection of moist static energy (Jiang et al, 2020; Arnold et al, 2015; Liu et al, 2013). This previous work focused more on the MJO than the KW, however we find the KW intensification to be more robust. Future work should consider why these mechanisms act to preferentially intensify the KW more than the MJO, and why they do not act to intensify other modes such as inertia-gravity waves or equatorial Rossby waves. One possible mechanism is that the strengthening of the subtropical jet in response to climate change leads to more eastward propagating wave-modes as compared to westward, and to the extent that Kelvin waves are excited by subtropical variability propagating into the tropics, this should lead to more KW at the expense of other wave-modes. Ongoing work is aimed at testing this hypothesis, and results will be reported in a future publication."

Also, the choice of U250 as opposed to OLR as the variable for investigating the future changes seems inappropriate. The authors should use OLR or precipitation to examine convectively-coupled waves (the focus of the current study) in isolation from dry waves. Also considering the expected rise of the tropopause height, using a fixed pressure level (250-hPa) seems to be not ideal. We have performed the future-historical calculation for OLR and now include it as an equal to U250. Note that the projected rise in tropopause height is around 20hPa (Figure 7 of Zelinka and Hartmann, 2010), which is less than the vertical resolution archived by the CMIP. So regardless of which level we choose to show, there will always be some ambiguity associated with the pressure levels chosen.

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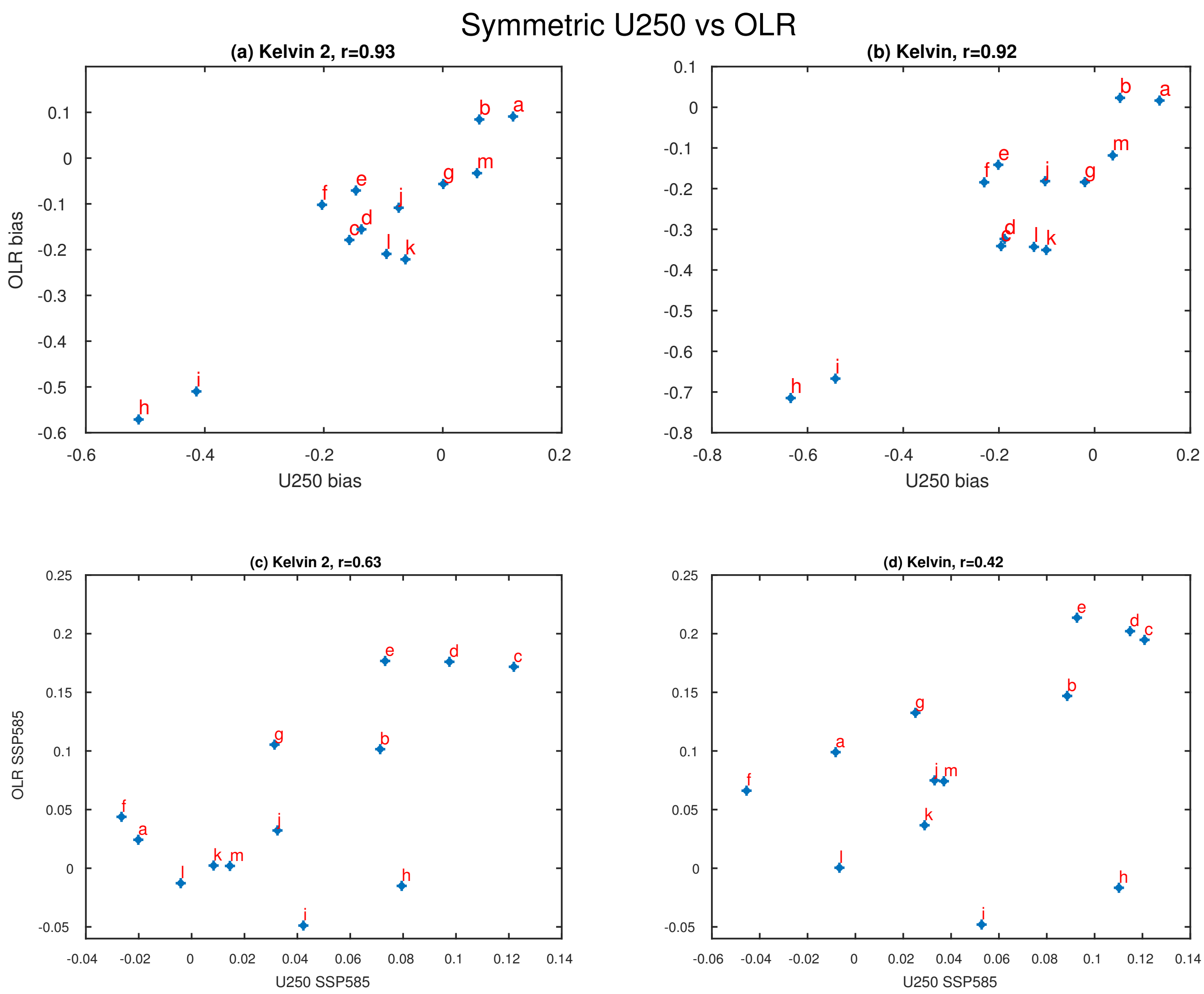

Fig. 1 correlations between biases in OLR and in zonal velocity at 250 hPa. ω-k values: (a) KW 2: $2 \leq k \leq 7$, $5 \leq T \leq 10$ days; (b) KW: $3.5 \leq k \leq 5$, $3.5 \leq T \leq 7$ days. Letters correspond to the labeling of the models on Table 2 in the article.

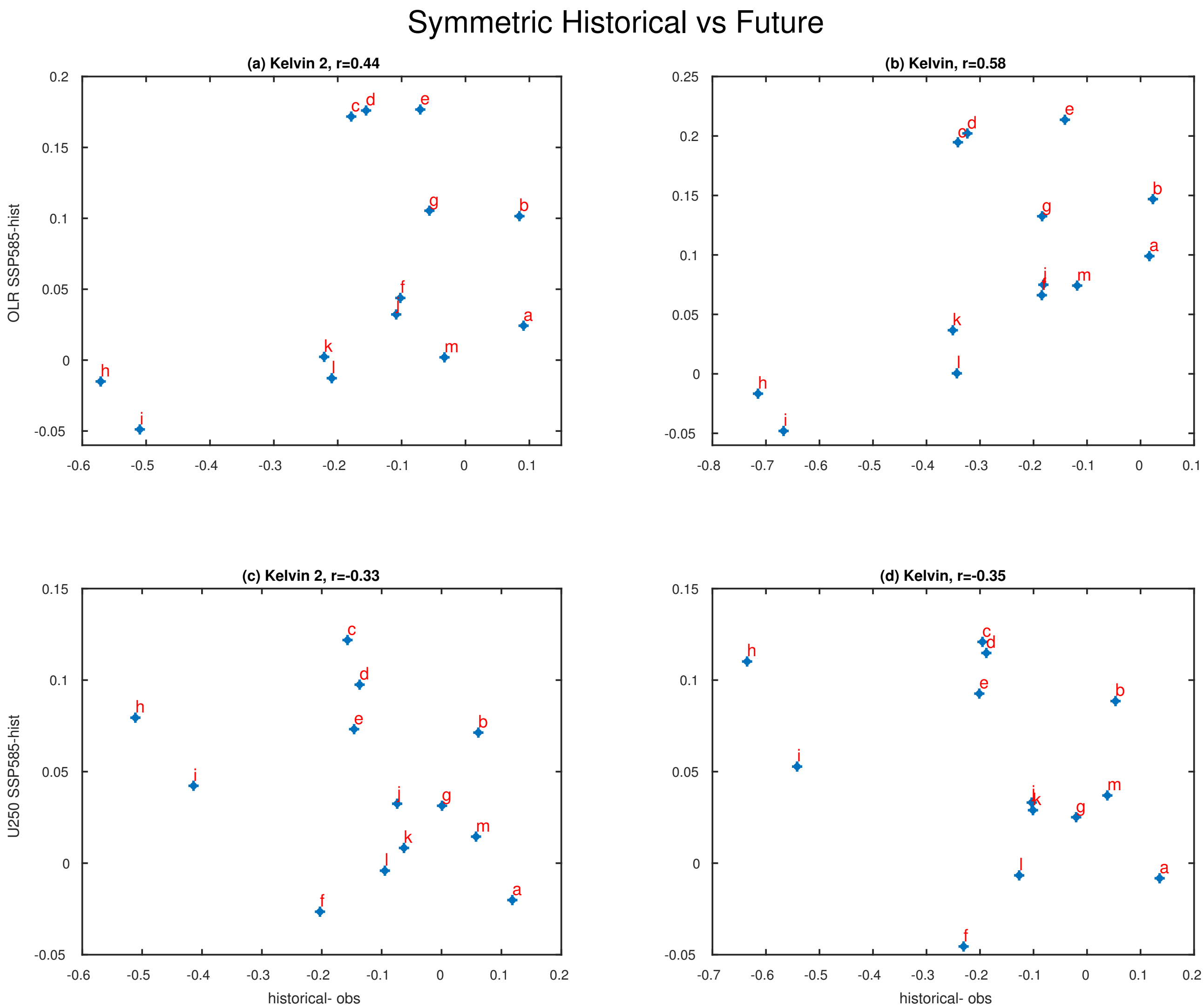

Fig. 2 Correlations between U250 future forecasts and historical bias of the models:(a-b) SSP585 vs history, and (c-d) SSP245 vs history. ω-k values: (a, c) KW 2: $2 \leq k \leq 7$, $5 \leq T \leq 10$ days; (b, d) KW: $3.5 \leq k \leq 5$, $3.5 \leq T \leq 7$ days. Letters correspond to the labeling of the models on Table 2 in the article.

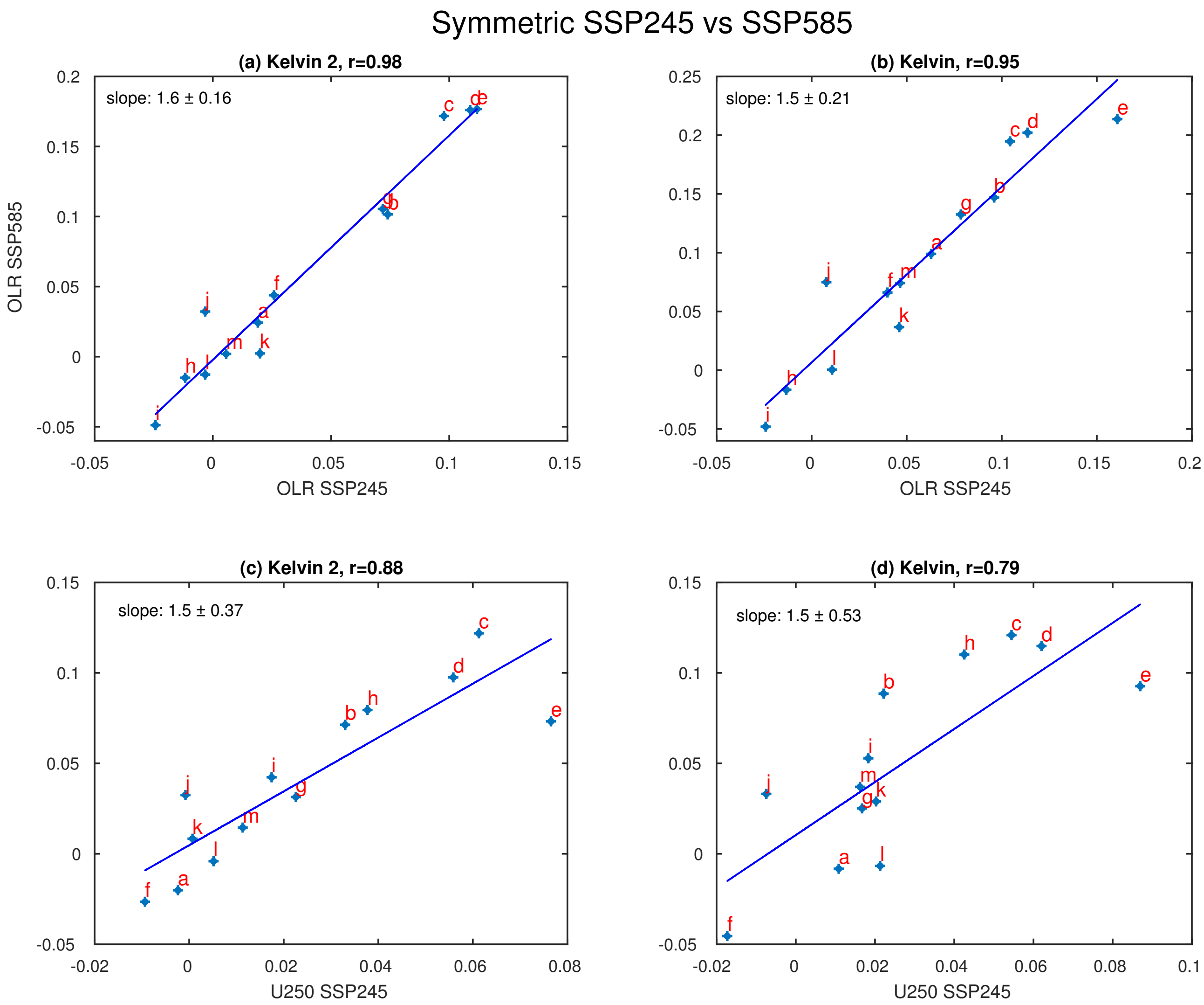

Fig. 3 correlations between U250 SSP245 and SSP585. ω-k values: (a) KW 2: $2 \leq k \leq 7$, $5 \leq T \leq 10$ days; (b) KW: $3.5 \leq k \leq 5$, $3.5 \leq T \leq 7$ days. Letters correspond to the labeling of the models on Table 2 in the article. A best-fit line and its slope, including 95% confidence intervals, are shown for each panel.