Review of:
Nonlinear Sensitivity of El Niño-Southern Oscillation across Climate States
By Pontes et al.
Submitted to Climate of the Past

Summary
In this manuscript, the authors aim to clarify the relationship between properties of ENSO variability and the mean background climate in a range of different past and future warmer climate states. The authors employ different past climate simulations (covering the mid-Pliocene, last interglacial and mid-Holocene) and future simulations (ssp585 and abrupt 4x CO2) and present their results with respect to pre-industrial climate simulations. The results show a common mechanism linking changes to the mean atmosphere-ocean circulation to an increase or decrease in ENSO variability. In fact, the authors reveal a non-linear relationship between a change in ENSO variability and the change in the meridional positions of the intertropical and South Pacific convergence zones. Three distinct regimes of ENSO variability change are identified and the processes in the tropical Pacific mean climate leading to the existence of these regimes are explained.

I believe this is a very interesting study with relevant findings. The scientific content is substantial and provides a significant contribution to the field of ENSO complexity. The analysis is sound, and the figures are very clear. I do have a few issues, related to the data selection and to the Introduction, that I would consider major (but should not require too much effort to resolve). Apart from those, I present minor comments and a few technical corrections. I recommend this manuscript to be published in Climate of the Past subject to revisions.

Major concerns
My main issue is the following. Your aim is to analyze nonlinear ENSO sensitivity to the mean climate. I have no issues with that with regards to the past climate simulations. But the future simulations, ssp585 more so than abrupt4xCO2, are transient simulations, and not in equilibrium. You seem to use data from 2015 - 2100 (if I am correct), but both the mean climate as well as ENSO characteristics change quite drastically over that period. I don't think it makes sense to take the mean climate over this whole period and to consider that ENSO characteristics are constant over this period, because they are not. Either you need to convince me that what you are doing is fine (e.g. show me that the mean climate and ENSO properties between f.e. 2015-2060 and 2061-2100 are similar enough for your analysis following some statistical test), or I would suggest you use different simulations or a different selection of years. My suggestion would be to use the simulations from the LongRunMIP project instead of the ssp585. LongRunMIP also encompasses the abrupt4xCO2 simulation, so that works out well. It includes high CO2 'future' simulations, but equilibrated and not transient, so the mean climate can be considered in a mean state, and ENSO can be considered not changing over the last 100 years. Another option is to use extended SSP585 runs, but I don't think many models ran those runs, so that might give you only a small ensemble to use.

I have an issue regarding reproducibility of your results because is it quite unclear what your specific data selection is. Which observational dataset is used? It is not mentioned in the main text, nor in the appendix, nor in the supplement. Do you use monthly data? And how many years? I find 100 years in "Data availability". Are the mean climate features you show based on the mean of those 100 years? For the past periods that is fine. But most ssp585 simulations go from 2015 to 2100; does that mean you only use 85 years? The caption in Figure 1e seems to suggest so. What about the abrupt 4xCO2 simulations though? I am left to find out a lot of details about your data selection from different parts of the paper. I find that a major issue, but I think it will cost you very little time to collect those details in the Methods section.

Lastly, I have some issues with the Introduction. It is clear, but incomplete (in my view). I have three recommendations:

a) First, I find it unclear from the Introduction what the research aims are. Currently, the only stated aim is in L66-67: "to investigate the effect of changes in Pacific mean state on ENSO variability." That is quite vague. Can you explain in more detail: What the main research gap is that you plan to address / what the main research aims are, what your research questions are (and why – relate this to the literature overview), and how you plan to answer those (i.e. more detail on the specific features you will analyze).
b) Too much important background information is explained after the Introduction (i.e. in the Methods and Results). For example, the different types of ENSO are only explained in the Methods – why not in the Introduction? The SPCZ is mentioned first in the Results (L118), but plays an important role throughout the paper – why it is not explained in the Introduction? The most striking is the first paragraph in section 3.2 (L129 – 147). This paragraph is a literature overview, and harbors no results, so should not be included in the Results. Please incorporate it in the Introduction.
c) The literature overview on ENSO variability in the future is incomplete. Cai et al. (2021) present a literature review on ENSO in a warm climate and show (with more confidence than the study you cite now) that ENSO amplitude increases under future warming, but also that equatorial Pacific rainfall intensifies, extreme ENSO rainfall events increase, and EP ENSO variance increases clearly while agreement on CP ENSO variance increase is low. I believe all these results resonate with your findings, so it is relevant to mention in the Introduction. Additionally, while near-term future climate simulations (e.g. until 2100) show an intensification of ENSO variability, equilibrated long-term warming simulations show a robust decrease (Callahan et al. 2021 and Zheng et al. 2022). Both studies employ the LongRunMIP ensemble simulations, that represent equilibrated 2x, 4x, 6x and 8x CO2 increases, both abrupt and 1pct increase type simulations. Almost all these simulations show a robust decrease in ENSO variability. I find that a very significant finding to mention with regards to your study. Additionally (as mentioned in my previous point), I would suggest including these results as they provide suppressed ENSO not in the past but in a long term future, and I would be very interested to see in which of the nonlinear regimes the LongRunMIP results end up.

Cai, W., Santoso, A., Collins, M. *et al.* Changing El Niño–Southern Oscillation in a warming climate. *Nat Rev Earth Environ* **2**, 628–644 (2021). https://doi.org/10.1038/s43017-021-00199-z

Callahan, C.W., Chen, C., Rugenstein, M. *et al.* Robust decrease in El Niño/Southern Oscillation amplitude under long-term warming. *Nat. Clim. Chang.* **11**, 752–757 (2021). https://doi.org/10.1038/s41558-021-01099-2

Zheng, Y., Rugenstein, M., Pieper, P., Beobide-Arsuaga, G., and Baehr, J.: El Niño–Southern Oscillation (ENSO) predictability in equilibrated warmer climates, Earth Syst. Dynam., 13, 1611–1623, https://doi.org/10.5194/esd-13-1611-2022 , 2022

Minor comments

I feel that L39-40 "which has been … instrumental period" feels out of place here. I suggest removing it here (just end the sentence) and combine it with "with strong impacts worldwide" in L36. So, end L36 at "eastern Pacific (ref)." then insert: "It has been the most important driver of year-to-year climate variability during the instrumental period, with strong impacts worldwide."

Subscripts and superscripts need to be included consistently. CO2 should be CO_2 in L60, L73, L74, L76 and throughout. You write 21st, 4th and 6th in L66-68, but 21st in L75. Choose a style and apply consistently. In L152 R2 should be R^2.

L60 "both interglacials" – I understand you mean LIG and MH. However, the mid-Pliocene simulations you use from PlioMIP2 are also an interglacial period. I feel that is a detail worth mentioning (maybe in the Methods where you introduce PlioMIP2, explain that the simulation protocol is tuned to the KM5c interglacial).

L80-82: I think you need to rewrite this sentence. C-index is an index that represents the CP-ENSO variability, but now it reads as if C-index is the name of a type of ENSO variability (which is, in fact, CP ENSO).

L91: only the ssp585 data is detrended? I would say, for consistency, to apply detrending to all simulations (although it will likely not affect the results substantially).

L103-105 I would like to see included in the text how many models you end up using per scenario/past period after the selection. From the supplement it seems that you end up with quite some models for the future simulations, but only a relatively small selection for the past periods. I find that relevant information to include here.

L112 WES feedback could merit a citation

L113 "the increased difference in inter-hemispheric warming" -- this is not something that is necessarily shown by your results, is it? the SSTs in a-c mainly show a reduced meridional gradient. What I think you mean is the heat asymmetry between both hemispheres, correct? I know from your previous work on the Pliocene (Pontes et al 2022) that this heat asymmetry, stemming from planetary albedo differences related to

vegetation, ice sheets, or orbital differences, drives the northward shift of the ITCZ. For the LIG and MH, I am left to deduce the cause of this inter-hemispheric difference from a sentence in the Introduction (L60-62): “higher boreal summer insolation”, which causes a difference in interhemispheric warming. This is a lot of deduction work for a reader, so I think this merits a sentence here explaining in more detail what is meant by 'increased difference in interhemispheric warming' and clarify this is based on previous findings and not on results in this work.

L118 regarding ‘equatorward’ and relating to the other reviewer’s comments: to me it is quite clear that the ITCZ in the Pacific is always located in the Northern Hemisphere, so I understand your usage of equatorward and poleward here. However, that might not be obvious to all reader, so maybe you can clarify this point.

L124-126 “In this context … mean climate” -- I am not convinced by this formulation. I think there are two different formulations in this sentence, one more related to physics (which I suggest keeping), and the latter more related to how ENSO is defined. Since ENSO is defined as an anomaly to a mean climate, it indeed would follow that when a mean climate is different, ENSO will be different. But that to me feels quite arbitrary and does not reveal any new information on the physical link between the mean state and ENSO variability, which is what you are trying to establish in this study. The first part is more interesting: the strength of the mean atmosphere-ocean circulation can modulate ENSO variability. If possible, I would expand more on that, e.g. a strong mean circulation (La Niña like mean state) inhibits the development of anomalies, effectively suppressing ENSO variability, while a weak mean circulation (El Niño like mean state) is more favourable for the development of anomalies that can lead to ENSO events.

L131-132 “whose meridional positions affect ENSO dynamics” – I think this statement merits a reference

L151-152 “the ENSO-convection centers relationship” – propose to change this to “the relationship between ENSO amplitude change and the displacement of the convection centers”

L160 (regarding the previous paragraph): I agree with the other reviewer that it seems like the abrupt4xCO2 results are important for establishing a quadratic relationship over a linear one. However, I think you already showcase that even without the abrupt 4xCO2 simulations, a quadratic fit is better than a linear fit for all ENSO indices (namely in supplement figure S4). It would be worth it to mention this result in the main text.

L161-L165: Are these findings based on the results from all models or only the selected subset? Do these results relate to the curves shown in Figure 2? Please clarify

L163-165: the description of the quadratic model could be moved to the Methods

L165-L169: you use 10 models for the ssp585 scenarios. is the difference between 6/10 agreeing or 7/10 agreeing what you call 'non robust increase' and 'significantly increased' ..? also, I am confused where you state 'in our subset of models' and in the

next sentence 'if selecting models based on …'. Isn't this subset you mean already only those models that pass this criteria test?

L195 "SST variability" I think you mean that there is a small model spread in the mean SST in the Niño3 region. A small range in SST variability would suggest that most models agree on the amplitude of ENSO, which is not what you seem to imply.

L213 (regarding previous paragraph): following these results, I would be interested to see the ENSO amplitude change as a function of the change in easterlies variability. It would probably show a linear relationship where a weaker (stronger) ENSO relates to a weaker (stronger) easterlies variability. However, it would not show that the weaker ENSO / weaker easterlies variability is two different regimes: where the convection centers are shifted poleward or strongly equatorwards. Your results and conclusions could harbour a recommendation to the ENSO research community to not just look at this ENSO change - easterlies variability relationship, but also investigate the shift in convection centers, if one truly wants to know which regime in ENSO suppression is occurring. I think that is a valuable recommendation to state here on in the Conclusions).

L231 "three key background conditions": but these are background states with regards to a PI control, so I would suggest calling it something like 'anomalous background states', or 'three distinct regimes deviating from the PI control'.

L231 related to this, you follow up with mentioning these three regimes. For clarity for the reader, I would suggest numbering these regimes, e.g. in L232 1) A poleward migration …, L236 2) A moderate equatorward shift… and L239 3) A strong equatorward shift (remove "finally")

L238: "…dynamical coupling." – I would add ", and thus dampened ENSO variability." To that sentence for clarification.

L249-250: "Nonetheless, … selection criteria." – I find this a strange read, and it should/could include a suggestion for the community. Propose to rewrite: "In this study, we applied two model selection criteria to reduce the influence of such biases, and we recommend others to employ similar model selection steps when investigation ENSO complexity."

L261-263: "This may … biodiversity loss." – I find this a highly suggestive sentence which seems to be included to increase the relevance of this study. I would suggest removing it. The study is relevant in its own right. I would recommend including a different take-away (more related to the study results), or just remove this sentence altogether.

Figures:

Figure 1: Am I correct in assuming panel e shows all model simulations, and not just the models selected based on the criteria mentioned in the methods? I think it makes sense to include all models here, since you are just looking at mean climate and ENSO and not

yet the relationship between the two. However, I would clarify in the Methods that you include all models for this result, and only move forward with a smaller subset for the later results.

Figure 1: you might want to extend the arrows with the correct units to all panels (a-d) so that it is clear from the figure itself which panels show wind stress, and which show surface wind. Now, from the panels itself it is not clear (although the caption clarifies).

Figure 2a: Is it really necessary to include this figure with the Niño regions..? Consider leaving it out.

Figure 3a: am I correct in assuming that 'control' here means PI control data, and all the other data is the grouped results of all the past and future simulations? if yes, are all model simulations included, or only the subset of models that pass the criteria test..? Please clarify

Figure 3a: 'strong equatorward' is defined as >9deg. looking at fig 3b, to me that seems like only 1 simulation belonging to abrupt4xCO2. Is that correct? Because that would imply that your findings on the strong equatorward rainfall-SST relationship is based on 1 model and 1 simulation, making it hard - I think - to connect meaningful conclusions to that result.

Figure 3c: if I am correct, the triangles and squares in panel c are the wrong way around. Please change accordingly

Figure 4: I would suggest naming these panels (a), (b) and (c).

Technical corrections

L29: El Niños → El Niño events
L31: how tropical → how the tropical
L41: likely the → likely that the
L61-62 “in the Northern Hemisphere” can be removed (“boreal” already implies this)
L66: in Pacific → in the Pacific
L130: position → positions
L141: coupling, important → coupling, which is important
L205: In this context, we found → We find
L228: The results described above → Our results
L236: Moderate → A moderate
L274: Simulation → Simulations
L281: the pre-industrial climate → pre-industrial climate simulations