Response to Reviewers

We thank the three referees for their thorough reviews, in response of which we have re-conducted the EOF analysis using the monthly anomalies of the FUB data rather than the 5-month running averages of deseasonalized FUB data. We have also removed one ensemble from the manuscript. Our point-by-point responses are listed below in blue font while the comments of the reviewers are in black font. In addition, the URLs in our responses are in purple font.

Reviewer 1

Summary

In this study, the authors focus the possible impact of the ENSO on the QBO cycle using the observations and the model simulations from GISS E2.2. However, the modulation of the QBO cycle by the El Nino and La Nina is not consistent among the model configurations. The authors concluded that the model physics are important to simulate the impact of the ENSO on the QBO period. In general, the authors well explored the possible impact of ENSO on the QBO period. However, I also found some relevant concerns, which should be well addressed. Therefore, I suggest a major revision at the present time.

Thank you very much for your constructive criticisms. We will address your comments point by point as below.

Major comments

1. The relationship between the ENSO and QBO should be well reviewed in the introduction. The possible impact of ENSO on the QBO amplitude and phase should mentioned.

The second paragraph in the introduction contains the following sentences: "Remarkably, an observational study conducted by Taguchi (2010) demonstrated that the downward propagation of the QBO tends to speed up during El Niño and slow down during La Niña while the amplitude of the QBO tends to be smaller during El Niño and larger during La Niña, respectively. Using radiosonde data from 10 near-equatorial stations distributed along the Equator, Yuan et al. (2014) found that the ENSO modulation of the QBO period is more robust than that of the QBO amplitude."

Thus, the manuscript had already adequately addressed the issue with regard to "The possible impact of ENSO on the QBO amplitude and phase…".

Further, ENSO and QBO phase coincidence should also be mentioned. Before 1980s, El Nino tends to appear during EQBO, and La Nina tends to appear during WQBO. After 1980s, El Nino tends to appear during WQBO, and La Nina tends to appear during EQBO (DOI: 10.1175/JCLI-D-19-0087.1; 10.1175/JCLI-D-20-0024.1).

We have changed "The QBO and the ENSO defy linear relationships (Angell, 1986; Xu, 1992; Garfinkel and Hartmann, 2007)." into "The QBO and the ENSO defy linear relationships (Angell, 1986; Xu, 1992; Hu et al., 2012) as highlighted by that fact that while the QBO and ENSO indices are negatively and positively correlated before and after 1980s, respectively (Garfinkel and Hartmann, 2007; Domeisen et al., 2019; Rao et al., 2020c) they are virtually uncorrelated over the longer periods from 1953 to recent times (Garfinkel and Hartmann, 2007; Geller et al., 2016b, see their Figure 5 for details)

2. Previous studies also found that the relationship between ENSO and QBO is not universal among models (DOI: 10.1175/JCLI-D-20-0024.1). This paper only emphasizes the possible impact of ENSO on the QBO, is there a possibility that the QBO can impact the ENSO occurrence and amplitude.

Concerning the QBO influence on the atmospheric circulation in boreal winter, Fig. 13 in DOI: 10.1175/JCLI-D-20-0024.1 shows that none of the CMIP5/6 models can fully capture the physical processes that show up in the JRA reanalysis. Each model is flawed in one way or another.
We are actively investigating those issues right now. Aside from a handful of papers (e.g., Gray et al., 1992) we are unaware of prior studies on whether/how the QBO exerts influence over the occurrence or amplitude of the ENSO.

3. Further, this paper is too long and too dispersal and include too many contents. This paper is not aimed to evaluate the model configurations. However, the comparison between AMIP and CMIP simulations and that between SP and AP physics accounts for a large portion of the paper. I suggest to remove the experiments that fail to reproduce the impact of ENSO on the QBO period. Including those results that do not simulate a significant difference between the QBO periods during El Nino and La Nina, the paper is not convinced at all.

We have removed the Coupled-OMA-AP ensemble to streamline the paper.

There exists no model that is capable of realistically simulating the impact of ENSO on the QBO period with all relevant physical processes being simulated correctly. To reflect this reality, we have changed the title of the manuscript from "ENSO Modulation of the QBO Periods in GISS E2.2 Models" into "Exploring the ENSO Modulation of the QBO Periods with GISS E2.2 Models".

Here are some of our perspectives on why we need to include both the SP and AP models:

(1) In Table 2, the first member of the Coupled–NINT–SP ensemble simulates a reasonable difference in the mean QBO period between El Nino and La Nina episodes. However, the difference is not statistically significant. In parallel, the second member of the Coupled–NINT–AP ensemble shown in Table 2 simulates a smaller difference in the mean QBO period between El Nino and La Nina episodes. However, this difference is statistically significant. Why?
Fig. 14 in the preprint demonstrates that the first member of the Coupled–NINT–SP ensemble simulate more realistic variability of SSTs while the second member of the Coupled–NINT–AP ensemble fails to capture the variability of SSTs on decadal/interdecadal scales. Thus, the ratio of the ENSO signal to non-ENSO noise from the latter run is unrealistically large, underlying why the smaller difference in the mean QBO period simulated by Coupled–NINT–AP between El Nino and La Nina episodes is statistically significant.

(2) Table 2 also shows that the Coupled–NINT–AP ensemble runs generally simulate larger differences in the mean QBO period between El Nino and La Nina episodes than the Coupled–NINT–SP ensemble runs. Does this mean that the Coupled–NINT–AP model is better than Coupled–NINT–SP? On the one hand, the answer is "No" because Fig. 8 and Fig. 9 show that the ENSO amplitude simulated by Coupled–NINT–SP is much more realistic than that simulated by Coupled–NINT–AP.
On the other hand, the answer is "Yes" because Fig. 13 shows that the OLR amplitude associated with the ENSO from Coupled–NINT–AP is much more realistic than that from Coupled–NINT–SP.

This report serves as a stepping stone to further model improvements which will in turn provide more insights into how the ENSO modulates the QBO.

4. The paper should provide a section named "Data and method" or something like. Without a data description and experiment introduction, this paper reads weird and readers fail to find the experimental setup.

Done as suggested.

Other comments

1. L30, L43-45: There are too many papers concerning the possible impact of ENSO and QBO on the climate (DOI: 10.1175/JCLI-D-19-0663.1; 10.1029/2020GL089149; 10.1175/JCLI-D-20-0960.1). I suggest to include more recent publication in the citations.

Done as suggested.

2. L59-60: There are also some studies that focus on the possible impact of the QBO on ENSO. The authors should present some review.

Concerning the possible impact of the QBO on ENSO, the preprint has adequately cited three papers: Gray et al. (1992), Huang et al., (2012), and Hansen et al. (2016).

3. L145-146: The ENSO amplitude in observations and CMIP models are not identical (DOI: 10.3878/AOSL20140055). If you use the same criterion, will the results be not convincing.

DOI: 10.3878/AOSL20140055 investigated the frequency and amplitude of cold tongue and warm pool ENSO events in CMIP5 models. Since most CMIP5 models simulated a smaller ENSO amplitude as compared with the observation, it was wise for its authors to define an ENSO event by using the criterion of whether a selected index (i.e., Niño3 index or ENSO Modoki index) was greater than 0.5 or less than -0.5 times its standard deviation. That being said, the frequency of simulated ENSO Modoki was inflated artificially and understandably in DOI: 10.3878/AOSL20140055 (refer to their Figure 2(c) and Figure 2(d)).
Zhao and Sun (2022) pointed out that most of the CMIP6 models have an amplitude comparable to or even greater than that is seen in observations (see their Fig. 1). Now we can afford to use the realistic, thus better, criterion.

4. L153-154: Other studies also performed the EOF analysis for the QBO wind profiles (See Figure 3 in Rao and Ren 2018CD, doi: 10.1007/s00382-017-3998-x).

This reference couldn't be added because it doesn't include the information that two leading pairs of empirical orthogonal functions (EOFs) and principal components (PCs) account for more than 90% of the vertical structure variance".

5. L168: Here is programming language. I suggest to use the science language: φ =atan(PC2/PC1)

According to Wikipedia, atan2 is a well-grounded mathematical function. Specifically speaking, the webpage points out that atan2 ranges from −π to π while arctan ranges from −π/2 to π/2, which is the very reason why we have adopted atan2.

6. L233: You should provide a detailed introduction for the calculation steps in a method section.

Done as suggested.

7. L254: modulates of => modelate (remove "of")

Corrected.

8. L259: Section 3.1, and 3.2: Should move to a method section.

Done as suggested.

9. L404-406: I also download historical runs from the GISS-E2 models for CMIP6. But I did not see the spontaneous QBO. Is the model in this paper same configured as for CMIP6?

E2.1 and E2.2 are two CMIP6 models that were optimized for the lower and middle atmosphere, respectively. E2.1 has only 40 vertical layers up to 0.1 hPa, thus cannot simulate the QBO. Note that the outputs of E2.1 were submitted to the CMIP6 archive earlier than those of E2.2. You had probably downloaded the CMIP6 historical runs simulated by the E2.1 models before we uploaded the E2.2 outputs to ESGF.

10. Section 4.1: Should move to the method section.

Done as suggested.

11. L439-441: Can you explain why the relationship is not stable?

We have added a paragraph immediately before the last paragraph in the manuscript to explain it.

12. L475: help => helps

Corrected.

13. L495-497: Please also see Domeisen et al. 2019 (RG) and references therein.

Done as suggested.

14. L531-532: Most CMIP models simulate a smaller ENSO amplitude as compared with the observations (DOI: 10.3878/AOSL20140055). This model is different and simulate a stronger ENSO.

DOI: 10.3878/AOSL20140055 studied the CMIP5 models, most of which simulate a smaller ENSO amplitude as compared with the observations. Concerning the CMIP6 models, Zhao and Sun (2022) revealed that most of the models have an amplitude comparable to or even greater than that is seen in observations (see their Fig. 1). We agree with you that the E2.2 model simulates a stronger ENSO.

15. L588-593: This paragraph describes the ERA5 reanalysis and should be moved to the method section.

Done as suggested.

16. L597: during in => during

Corrected.

17. L605-606: This sentence repeats many times. You can provide a section describing the data and methods. There is no need to repeat the methods time by time.

Done as suggested.

18. L732-735: The ENSO amplitude has so larger a bias. To what extent can we trust the results?

We have removed the Coupled–OMA–AP runs from the manuscript.

19. L743, Figure 14: This figure is redundant and fail to connect with the topic of this study.

This figure is closely connected with the topic of this study, showing that whether the ENSO can significantly and realistically modulate the QBO period is dependent on whether the spectra of SSTs are properly simulated. Figure 14 indicates the ratio of the ENSO signal to the non-ENSO noises. Note that erstwhile Figure 14 has become Figure 13 now.

20. L760: Do you mean that none of the results are robust in this study? I also did not see that the QBOi also explore the ENSO modulation of the QBO cycle.

We have added a sentence to explain why our results are robust.

I also did not see that the QBOi also explore the ENSO modulation of the QBO cycle.

Please take a look at the following website:
https://users.ox.ac.uk/~astr0092/Experiments.html#widget2
and refer to Kawatani et al. (2019).

References

Gray, W. M., Sheaffer, J. D., and Knaff, J. A.: Hypothesized mechanism for stratospheric QBO influence on ENSO variability, J. Geophys. Res., 19, 107–110, https://doi.org/10.1029/91GL02950, 1992

Kawatani, Y., Hamilton, K., Sato, K., Dunkerton, T. J., Watanabe, S., and Kikuchi, K.: ENSO Modulation of the QBO: Results from MIROC Models with and without Nonorographic Gravity Wave Parameterization, J. Atmos. Sci., 76, 3893–3917, https://doi.org/10.1175/JAS-D-19-0163.1, 2019.

Rao, J., Garfinkel, C. I., and Ren, R.: Modulation of the Northern Winter Stratospheric El Niño–Southern Oscillation Teleconnection by the PDO, J. Climate, 32, 5761–5783, https://doi.org/10.1175/JCLI-D-19-0087.1, 2019.

Richter, J. H., Anstey, J. A., Butchart, N., Kawatani, Y., Meehl, G. A., Osprey, S., and Simpson, I. R.: Progress in simulating the quasi-biennial oscillation in CMIP models, J. Geophys. Res.-Atmos., 125, e2019JD032362, https://doi.org/10.1029/2019JD032362, 2020.

Zhao, Y. and Sun, D.-Z.: ENSO asymmetry in CMIP6 models, J. Climate, 5555–5572, https://doi.org/10.1175/JCLI-D-21-0835.1, 2022.

Reviewer 2

General comments

Thank you for your insightful expertise. We will address your comments point by point as below.

The work by Zhou et al. deals with the ENSO modulation of the QBO phase speed in the GISS model. This seems strongly related to the parameterized gravity wave forcing, which is not described in much detail (line 93 and following). Basic information should be provided (e.g., how convection changes the parameterized wave spectrum) and possible model-dependence of the results stressed.

Done as suggested.

The text is well-written, but there are some repetitions which could be avoided (see specific comments). For example, the data processing used for calculating ONI and the QBO could be given only once in the methods.

Done as suggested.

The observational analysis part could probably be shortened.

The revision has made clear why we should conduct this analysis.

There are several long sentences which are not very readable. It is in some cases not easy to follow the reasoning or motivation or some of the analysis, please improve the connections between paragraphs where needed.

Done as suggested.

It could be useful to provide more information on the ENSO characteristics in coupled experiments (e.g., references or number/intensity of events), briefly mentioned around line 530 and shown in the spectra plots.

Done as suggested.

Opposite results for some simulations (line 738 and following) are interesting but should be discussed further: is this suggesting a very important role for internal variability, at least in simulations?

Done as suggested.

Differences in some of the plots are small, it would be good to add some significance estimate.

Since the sample spaces consist of monthly data, we cannot know the effective sample sizes of the El Niño and La Niña sample spaces. Thus, it is a bit hard to rigorously conduct a significance test.

Specific comments

18 'gravity waves parameterized interactively' -> 'interactive GW parameterization'?

We have changed "…and its gravity wave sources parameterized interactively" into "…but with its gravity wave sources being parameterized interactively".

The propagation and dissipation of the parameterized gravity waves indeed interact with the thermal and motion fields of the model atmosphere in all CMIP5/6 models. Here, we emphasize that the generation of the parameterized gravity waves should also be dependent on the thermal and motion fields of the model atmosphere in climate models.

41 may refer at QBO zonal asymmetry

We have added the following clause ", which likely results from the zonal asymmetry of the QBO winds (Hamilton et al., 2004)".

61 all -> how many. When first introducing 'T' for truncation, please clarify what it means

Corrected.

80 using which model?

The information has been added.

114 may add a reference like Naujokat, 1986

Naujokat (1986) was cited at the end of the next sentence, i.e., in L117.

131 please add a reference, e.g. Salby, 2012

Done as suggested.

148 are you referring to the ONI for ERSST or the simulations?

Corrected.

171 I do not see why 'now', as to me this is unrelated to the previous paragraph

We have removed it.

206 I'd say that N1 and N2 do not result from calculations

Corrected.

207 you could introduce as done for A the meaning of the overbar for both quantities

Done as suggested.

344 What else could be done, since you stated that you are not considering the amplitude already?

We have removed this useless sentence.

424 To avoid confusion with capitalised psi for phase speed, please ensure the latter is uppercase elsewhere.

Here we have replaced ψ with ε.

436 Not sure to understand this sentence

Revised.

450 do you need to scale by the respective variance?

In L447-448, the preprint mentioned "both the observed and simulated QBO can be very well represented by the trajectory of $(PC_1(t), PC_2(t))$ in a linear space spanned by the first two orthonormal EOFs."
Since EOF1 and EOF2 are orthonormal, further scaling is not needed. In other words, the variances of EOF1 and EOF2 have been absorbed into $PC_1(t)$ and $PC_2(t)$, respectively.

462 Why the focus on Coupled-NINT-Ap? Can you motivate and remind the reader about this configuration?

We have added the following sentence in the previous section: "Since coupled models encounter more difficulties in simulating the ENSO modulations of the QBO (Serva et al. 2020, see their Fig.4 for more details), we first look into the ensemble simulations from the Coupled–NINT–AP model, which incorporates the most up-to-date cloud parameterization schemes."

543 La Niña does not have a well-defined peak, suggest rephrasing

Corrected.

554 in any season? in both hemispheres?

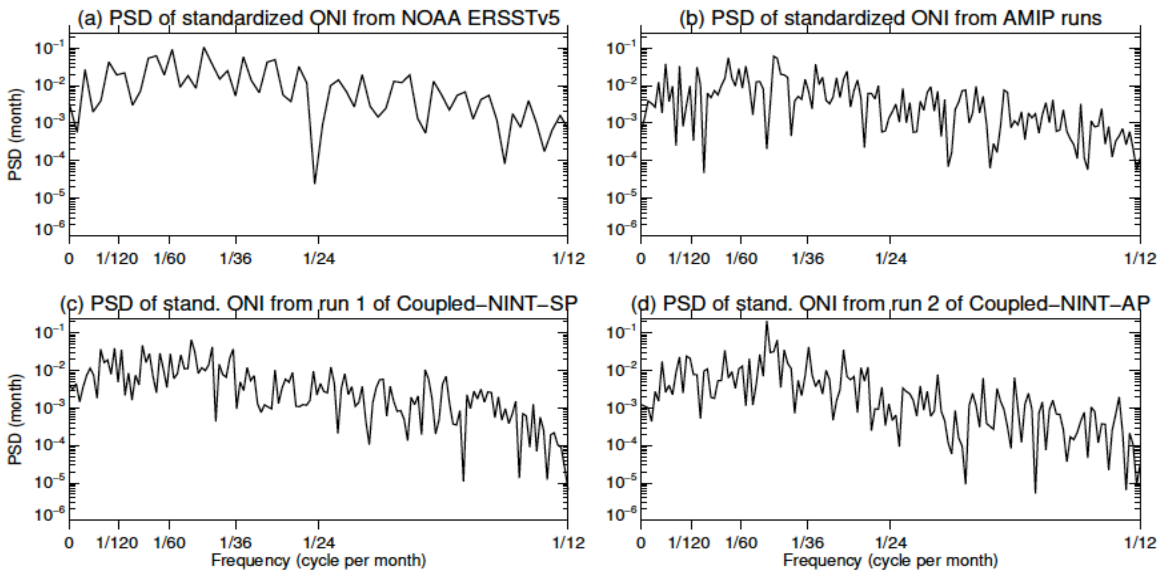
We didn't further divide the ENSO into the cold-season-matured and the warm-season-matured ENSO as shown in Figure 1 in Rao and Ren (2014). Instead, we look at those figures from the holistic/composite point of view.

Here , we don't mean the ENSO teleconnections/effects on a global scale. Since we are dealing with the ENSO modulation of the QBO, we view those figures in a very narrow sense that is described in the *Wikipedia* as follows:
El Niño–Southern Oscillation (ENSO) is an irregular periodic variation in winds and sea surface temperatures over the tropical eastern Pacific Ocean…
The spatio-temporal complexity of the ENSO was detailed in Timmermann (2018) and is beyond the scope of the manuscript.

588 please explain why you discuss this now. ERA5 should be introduced in the Methods section.

Corrected.

1311 figure could be improved by using logarithmic ordinate and/or putting spectra in a single plot



The above figure uses logarithmic ordinate. It doesn't help us to contrast the ratios of signal to noise between various pair of panels. In addition, when we use various colored lines to put spectra in a single plot, some overlapping of the lines makes it difficult to visualize them.
Thus, we decided to use the original format.

Technical corrections

133 NOAA undefined

NOAA was defined in L123 in the preprint.

143 CDC=CPC?

Corrected.

209 repeated v and t

The repeated ν and t have been removed.

221 be consistent in the use of lowercase psi

For consistency, we denote ψ' as the monthly QBO phase speed. Then we average ψ' over the time span (i.e., the number of months) of each ENSO event and denote Ψ' as the mean QBO phase speed during an ENSO event.

267 Why 'according'? Unclear

https://www.merriam-webster.com lists the meaning of "according to" as
1: in conformity with
2: as stated or attested by
3: depending on

By “according to” we mean “depending on”.

288 NCCS undefined

Corrected.

338 'baseline'

This part has been moved into a new section “Data, models, and methods”.

434 'Andrews'

Corrected.

441 maybe 'configuration'?

Corrected.

632 'nether'?

Corrected.

688 'Earth'

Corrected.

1146 more shades in Fig 5 right would be better?
Or maybe using the same levels to ease comparison

The contour intervals are halved in all panels in Fig. 5 so that more shades in the right ones could show up.

1275 the varying levels across plots should be fixed

Contour levels across plots are identical. We simply added the maximum and minimum values in each panel as extra annotations to give more information.

Additional references

Naujokat, 1986 https://journals.ametsoc.org/view/journals/atsc/43/17/1520-0469_1986_043_1873_auotoq_2_0_co_2.xml

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Rao, J. and Ren, R.: Statistical characteristics of ENSO events in CMIP5 models. Atmos. Oceanic Sci. Lett., 7, 546–552, https://doi.org/10.3878/AOSL20140055, 2014.

Taguchi, M.: Observed connection of the stratospheric quasi-biennial oscillation with El Niño–Southern Oscillation in radiosonde data, J. Geophys. Res., 115, D18120, https://doi.org/10.1029/2010JD014325, 2010.

Timmermann, A., An, S.-I., Kug, J.-S., Jin, F.-F., Cai, W., Capotondi, A., Cobb, K. M., Lengaigne, M., McPhaden, M. J., Stuecker, M. F., Stein, K., Wittenberg, A. T., Yun, K.-S., Bayr, T., Chen, H.-C., Chikamoto, Y., Dewitte, B., Dommenget, D., Grothe, P., Guilyardi, E., Ham, Y.-G., Hayashi, M., Ineson, S., Kang, D., Kim, S., Kim, W., Lee, J.-Y., Li, T., Luo, J.-J., McGregor, S., Planton, Y., Power, S., Rashid, H., Ren, H.-L., Santoso, A., Takahashi, K., Todd, A., Wang, G., Wang, G., Xie, R., Yang, W.-H., Yeh, S.-W., Yoon, J., Zeller, E., and Zhang, X.: El Niño–Southern Oscillation complexity, Nature, 559, 535–545, https://doi.org/10.1038/s41586-018-0252-6, 2018.

Reviewer 3

The authors have done a lot of detailed work to compile these results. The study covers both model and observational parts. However, much of the text describes the figures extensively without giving the reader a clear road map that how these results differ importantly from previous studies. The repetition of text can be seen at some places. My suggestion is for the authors to perhaps shorten the description of results to reflect only their essential messages.

Thank you for your incisive comments, which led to a significantly improved manuscript.

I have the following major concerns before any recommendation on the manuscript:

1. Sometimes, the description of results is too extensive and needs to be shortened. The description related to the mechanics in the results sections should be moved in the discussion section. The repeated text requires curbing. Sometimes, the figures are described randomly within the results section.

   Done as suggested.

2. In the observation part, the authors are only confirming the finding of Taguchi (2010) with the same methodology. I could not observe any advantage of their observation analysis in compare to Taguchi (2010).

   We adopted a different methodology from that of Taguchi (2010).
   Since our preprint was designed to avoid criticizing Taguchi (2010), the advantage was not perceptible to you. Our revision has made clear why our method is advantageous.

3. This study is using 5-month moving averaged deseasonalized data. Is such huge smoothed data (a nearly half-year window) suitable to study the gravity wave generation? I suggest using the monthly data instead of the 5-month moving average. Using the monthly will also be an advantage of your study against Taguchi (2010). A separate section is required for the data and method; currently, there is mixing of data information and result description.

   Done as suggested.

4. All the figures are plotted very causally and not suitable for publication. There is a lot of scope for improvement in almost all the figures. Please see the specific comments for details.

   Please take a look at our uploaded figures rather than the ones inserted in the preprint. We will explain this issue in more detail.

Specified comments & suggestions:
L47-48 "the tropospheric subtropical jet (Garfinkel and Hartmann, 2011a, 2011b)..", can be updated with more recently citation (e.g. DOI: 10.1029/2022JD036691).

Done as suggested.

L48 "the boreal summer monsoon (Giorgetta et al., 1999)", can be updated with more recently citation, Yoden et. al 2023 which shows the QBO modulation on global monsoon system (https://doi.org/10.54302/mausam.v74i2.5948).

Done as suggested.

L103-108: I suggest to add some sentences related to the motivation of this study.

We have added a paragraph before this part.

L121 "We further smooth the deseasonalized zonal winds using a 5-month moving average (for more details, refer to Taguchi, 2010)". It will be better to use the monthly deseasonalized zonal winds instead of the 5-month moving average.

We have adopted your suggestion. Namely, we have employed the monthly anomalies in our observational study.

L 132, I suggest to the authors that the data can be extended for seven more years, i.e., 1953 to 2022.

Anstey et al. (2021) pointed out that the first two EOFs explain no more than 60% of total variance during the 2016 and 2019/20 QBO disruptions. Those two QBOs deserve more studies separately. As to whether outliers should be deleted or not, it is always a controversial issue. We prefer to erring on the cautious side.

L139-142, Why the different base periods? Only one base period can be used, i.e., a de-seasonalized anomaly for the whole time period of the data set.
L149 "identified 21 El Nino and 15 La Nina events between 1953 and 2015". Definitely, using monthly data, the El Nino and La Nina events will increase by two to threefolds. The same can be applied on the model part too.

Those issues are related to climate change and climate variabilities.

1. Why has CPC of NOAA adopted this criterion for many years?
There must be a raison d'être!
Let's conduct a thought experiment by extending the 1pctCO2 CMIP6 experiment (1850-2014) for another one thousand years. What will happen if we only adopt the whole period of model outputs as one base period?
Using current criterion of ENSO events, we will find that in the first 100 years, every month of every year probably falls into La Niña category and that in the last 100 years, every month of every year probably falls into El Niño category because of global warming!
Please refer to Fig. 6 in Latif and Keenlyside (2009) to get a taste of it.

2. Even if we could keep the CO2 concentration at the current level forever, we still need to use the method adopted by CPC of NOAA!
The following is our explanations.
Chapter 8 of Hartmann (2015) details "Natural intraseasonal and interannual variability". Apparently, monthly mean SSTs over the Nino3.4 region does include natural intraseasonal and interannual variability. Adopting your suggestion "Definitely, using monthly data, the El Nino and La Nina events will increase by two to threefolds" will lead to conflating intraseasonal variabilities with the ENSO. It is not acceptable.
Furthermore, adopting the whole period of model outputs as one base period will leads to conflating the PDO with the ENSO. It is not acceptable either. Please also refer to Rao et al. (2019).

You appear to dislike the practice of CPC of NOAA with regard to the definition of the ENSO, i.e., you seem to strongly object the widely adopted method: filtering out the intraseasonal and interdecadal variabilities in order to define the ENSO.

This is surprising given the well-known fact that if Lewis Fry Richardson had applied filtering to his data, he would have fulfilled his dream of numerical weather prediction 100 years ago (Lynch, 2006).

Even now, various national weather centers still conduct various filtering on daily basis (Houtekamer and Mitchell, 1998). In his chapter 7 "Filtering and Data Assimilation", Sullivan (2015) pointed out "it is not bigotry to be certain we are right; but it is bigotry to be unable to imagine how we might possibly have gone wrong".

L143 "CDC" to "CPC"

Corrected.

L144 As suggested in the comments line 121, if authors consider the use of monthly data, then monthly ONIs can be used to define periods of El Niño and La Niña whenever it exceeds the threshold values ± 0.5 K (+ El Niño, − La Niña). Sometimes the SST lies in ENSO phase for 2 to 3 months, and the generation of gravity waves for such a short period will be washed out in the 5-month moving average and cannot be ignored.

We have adopted your suggestion. Namely, the monthly FUB zonal wind anomalies are used. We even skipped deseasonalizing the FUB data.

L154-156 Not a justified reason. If we go beyond the 2015 period, the QBO disruptions (2016 and 2019/20) will not have a significant impact on the total variance of the leading two EOFs. In our own analysis for the period 1979–2022, the two leading EOFs account for 94.73% of the total variance (58.07% by EOF1 and 36.66% by EOF2). If authors are worried about these QBO disruptions, then the time period of disturbance can be excluded if lies in El Nino and La Nina sampling.

This has already been answered.

L157 Instead of two data sets (FUB and ERA5), the authors may also think of using only the ERA5 data for all observational analysis.

Pawson and Fiorino (1998) pointed out that the QBO westerlies from both NCEP–NCAR and ERA-15 reanalyses were generally weaker than the radiosonde winds at Singapore (1.8°N, 104.8°E) at 30 hPa and below.
This seems to be also applicable to the QBO winds from ERA5 presented by Pahlavan et al. (2021). Thus, the FUB data are superior.

L168: This is programming language. Proper mathematical expression should use here.

According to Wikipedia, atan2 is a well-grounded mathematical function. Specifically speaking, the webpage points out that atan2 ranges from $-\pi$ to π while arctan ranges from $-\pi/2$ to $\pi/2$, which is the very reason why we have adopted atan2.

L207: If possible, unit "radians/month" to Km/month. Same in sequent text.

We haven't found any relevant reference to address this issue.

L241: This is a clear mixing of analysis description and data information. The data information should be in a separate section.

Corrected.

L336 -345: As mentioned in the above comment (L139–142), why the different base periods to calculate the SST anomalies?

Already answered.

L356: For the reader's convenience, it will be nice to include the Fig.1 EOFs vectors in Fig.4 also (same line format as in the Fig.1).

We have merged Fig.1 and Fig. 4.

L530 -532: "Comparing Figs. 8a and 8b with Figs. 3a and 3b". I suggest to add one more row at the bottom of Fig. 8 for the difference between the model and observed amplitude of QBO during El Nino and La Nina.

Done as suggested.

L 588-590: The description of the ERA5 reanalysis should be moved into the new suggested data and method section.

Done as suggested.

L593: Is the composite difference in Fig.11c passes the statistically significant test (>95% confidence)?

Since the sample spaces consist of monthly data, we cannot know the effective sample sizes of the El Niño and La Niña sample spaces. Thus, it is a bit hard to rigorously conduct a significance test.

L604-628: This paragraph can be rewritten more precisely by focusing on the comparison between ERA5 and models.

Done as suggested.

L687-693: This paragraph seems unfit here and can be shifted to an appropriate place. The discussion part can start with Paragraph 2.

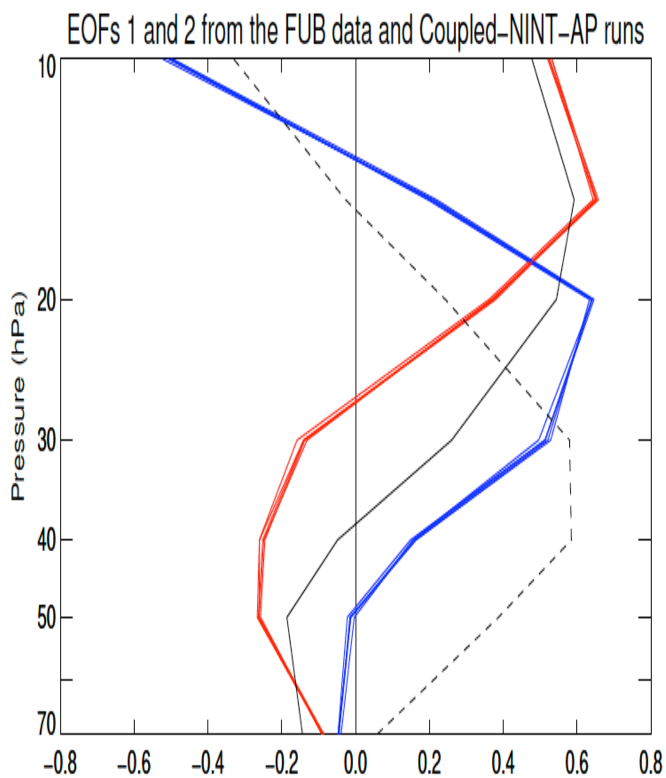
This paragraph is the punchline of our paper, which is also emphasized in the abstract.

Kawatani et al. (2019) and Serva et al. (2020) pointed out that parameterized gravity waves are either unable to simulate the ENSO modulation of the QBO or harmful to simulating this modulation in high-resolution models. This paper shows that the properly parameterized gravity waves can simulate the ENSO modulation of the QBO even though the horizontal resolution is not high. Since horizontal resolutions in many climate models are not high and those models still employ various parameterization schemes of gravity waves, our results might be useful for the further improvements of these models.

Comments of Figs.
All the figures are plotted causally and not suitable to be considered for publication.

Fig.1 It is too elongated along the x-axis, for the best view, the aspect ratio X:Y should be ~=1:1.3

Note that we have also separately uploaded all individual figures which are plotted as vector images. Vector images can be arbitrarily resized, rescaled, and reshaped infinitely without losing any image quality. Those uploaded figures are ideal in that the professionals of ACP can design the layout of each page in any way without worrying about the issue of losing resolution. The following rescaled figure has the aspect ratio ~1:1.3 (X:Y) as you suggested.



Note that we arbitrarily inserted those figures into the manuscript in such a way that the reviewers can read them clearly. We ignored such things as golden ratio, believing that they are the job of the ACP staff.

Fig.2 This figure is also too elongated along the x-axis, the aspect ratio X:Y should be =4:1.

Same as above.

I suggest to interchange the panels (a) and (b) for the systematic representation, i.e., top (a) should be for El Nino and (b) La Nina and (c) same (El Nino- La Nina).

You look at this issue from an aesthetical point of view.
However, we regard it as a cognitive issue because the pattern of El Nino is almost identical to that of (El Nino - La Nina). The repeated pattern will facilitate our visualization and enhance our memory.

It is an inconvenience to compare all the panels in the current color scale as it is different for different panels. The color bar should have the same scaling on both the positive and negative sides for all panels (here for this figure -45 to 45 Wm-2).

We have adopted the "substance over form" principle.

Had your suggestion been adopted, we would have missed the very important information in each panel: maximum and minimum values of OLR anomalies.

Currently, panel (a) and Panel (b) share some common color bars to the fullest extent. It is very easy for us to compare and contrast them:
(a) From the first order of approximation, the negative OLR anomalous pattern during El Niño is roughly the mirror image of the positive OLR anomalous pattern during La Niña.
(b) Further looking at the maximum and minimum values in panel (a) and panel (b), we can spot the implied asymmetry between El Niño and La Niña. Namely, the amplitude of El Niño is stronger (up to 4.5°C, as measured by the spatially averaged SST anomalies over the eastern equatorial Pacific) than that of La Niña (up to −3°C). This issue has been studied extensively such as Rao and Ren (2014) and Zhao and Sun (2022) concerning the CMIP5 and CMIP6 models, respectively. Please also refer to Timmermann et al. (2018).
Due to the first-order symmetry, it is natural that the contour interval is adopted as two times that used in Panels (a) and (b).

The fine and coarse contour intervals can be used for visualization of smaller and larger signals (e.g. please see Fig. 21 of Hitchman et. al 2020, https://doi.org/10.2151/jmsj.2021-012). The same can apply for other color figures too.
There is no need for more fine contour intervals. Adding several dozens of more contours will make Fig. 2 flashier and less elegant. Einstein pointed out: "Everything should be made as simple as possible, but not simpler".

Fig. 21 of Hitchman et. al 2020, (https://doi.org/10.2151/jmsj.2021-012) is a great figure!
Note that it is a raster image. In other words, if you rescale Fig. 21 many times larger it will become blurred with many pixels showing up. This is the situation where the right aspect ratio X:Y should be taken into account.
When you rescale our uploaded figures, you will never encounter such a problem. We would like the ACP professionals to decide how to rescale our figures.

Fig.3 Same comments as for Fig.2

Same as the reply to the comments on Fig.1.

Fig.4 Same comments as for Fig.1

Same as above.

Fig.5 Same comments on color scale as for Fig.2. The representation of panel numbers should have the same order in the caption of all figures. In Fig. 3, it is before starting the description [see L 1111 (a) La Nina and (b) El Nino] but here it is after starting the description [see L1152 La Nina (a) and El Nino (b)]. The same corrections must be applied for other figures too.
Fig.6 Same comments on color scale as for Fig.2.
Fig. 7. The aspect ratio X:Y should be similar to Fig.5 and 6. Same comments on color scale as for Fig.2.
Fig.8 too compressed along x-axis. the aspect ratio X:Y should be =4:1.
Fig.9 and 11: Same comments as for Fig.2.

Fig. 12, and **Fig. 13**: Same comment on the color scale as for Fig.2. Fig13. the aspect ratio X:Y should be =4:1.

Those similar issues have already been addressed in the above replies.

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