

Dear Editor and Reviewers

Thank you for your reviews – we very much appreciated the time and attention that has been provided, resulting in a much improved manuscript. Detailed responses are provided below; the amended text is highlighted in the revised manuscript.

Yours Sincerely
Lesley Gray

Response to reviewer 1:

In the abstract on line 9, the authors say that the ITCZ precipitation response may be particularly sensitive to the vertical wind shear. I agree with this, but I think what is most important here is the tropical tropopause temperature. This is what modeling shows. Of course, the wind shear and temperature are related by the thermal wind relationship. I recommend that the authors indicate that what is likely is that the tropopause temperatures are what are important. It would be difficult for me to imagine a physical mechanism directly coupling deep convection to 70 hPa wind shear.

Thank you – some additional text has been added to the abstract.

On page 3, line 30, I recommend deleting the word "equatorial." Both small-scale gravity waves and larger scale equatorial waves are important in giving rise to the QBO. To me, leaving the word "equatorial" in here implies the authors think it is the larger scale waves that are most important.

Done.

Page 4, line 12. Space between "be" and "captured."

Corrected.

Page 6, discussion of figure S2. It looks to me that the QBO amplitudes are larger at high levels in the ERA results. Do the authors agree? If so, do they have an explanation?

Yes, that's interesting, and I'm not sure why – I haven't added any text to this effect since it's outside the topic of the current study, but it would be worth pursuing further.

Page 8, line 11. "highlight"

Corrected.

Page 9, line 19. I have difficulty seeing what the authors are referring to. Could they be more explicit in describing what they mean here?

Text has been amended to make the discussion more explicit.

Page 16, lines 20-26. Liess and Geller (2012) examined ISCCP Weather States for developing and mature convection, so their results agree with the authors'

conclusions. Perhaps, that should be indicated.

Text has been added to mention this agreement.

Page 17, line 8. Again, it should be made clear that indications are that it is the temperature in the tropopause region that is most important here.

Text has been added, as recommended.

Response to Reviewer 2:

Page 2, line 11 There are earlier papers than that of Hansen that address nonlinearities between the effect of the QBO and that of ENSO, such as Garfinkel and Hartmann 2007, Wei et al 2007, and Calvo et al 2009 (the first two using reanalysis and the latter using models).

Additional references have been added, as suggested.

Page 6 line 4: what precise formula is used to account for the reduction in degrees of freedom due to autocorrelation of the indices? (There are a few different I'm aware of, and for reproducibility the authors should state which they use.)

Text has been clarified and an additional reference provided.

Page 14 line 16: I think the authors intend to cite Garfinkel and Hartmann 2011 here, not Garfinkel et al 2012

Corrected.