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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of tropical lower stratospheric cooling on deep convective activity: (I) Recent trends in tropical circulation" *by* Kunihiko Kodera et al.

Anonymous Referee #2

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The authors identified a northward shift of the ITCZ from observations and they related this shift to circulation changes in the stratosphere as a result of stratospheric cooling under global warming.

I like the fact that the authors tried a number of correlation analyses between the ITCZ shift and the changes in the tropospheric/stratospheric circulation. However, if I have not missed anything, I found some logical flaws in their arguments and that the mechanism proposed in Figure 13 is not rigorously supported by observations or theories. I wonder if it could be due to a problem of writing and presentation. Therefore, I recommend a major revision in the first revision round, so that the authors may clarify their

ideas, modify the title, write a more in-depth literature review, redo the data analysis, and remove unsupported claims.

The following critical comments may be considered during their revision.

1. The authors argued that changes in the equatorial stratospheric upwelling drove the ITCZ shift. This argument needs to be supported substantially:

(a) The movement of ITCZ under climate change has been well studied; see Schneider (GRL, 2017, https://doi.org/10.1002/2017GL075817) and references therein. These work have already demonstrated the importance of atmosphere-ocean exchange for determining the location of the ITCZ but the authors have not thoroughly review these observational and theoretical studies. (By the way, the authors mentioned the Hadley cell expansion in Introduction. They should clarify whether the Hadley cell expansion and the ITCZ movement are related.) Whatever the authors have come up with on explaining the ITCZ movements, their explanation (e.g. the stratospheric-led SST changes claimed in this paper) must eventually address those atmosphere-ocean exchange mechanistically and quantitatively.

(b) The logic that they turned their attention to stratosphere is a bit difficult to follow. They first pointed out that changes in convection extended into the tropical tropopause layer. Then they went further to assert that it was the changes in the stratospheric circulation that allowed the convection to get deeper into the tropical tropopause layer. However, there could be a lot of tropospheric causes (e.g. SST changes alone) that made the convection stronger and deeper regardless how the stratosphere changes. Without eliminating all those tropospheric causes first, it is hard to imagine why the stratosphere needs to be involved.

2. All proposed mechanism must be supported quantitatively but there is no detail how much fraction of the ITCZ movement may be caused by the proposed stratospheric changes. In fact, as a scientific paper, there is almost no number in the text that quantify any effects being studied. At most of the time, the authors present some correlation

coefficients as hints. But correlations or covariances cannot be used to imply causality between different variables. Causality should be at least shown by model simulations, which elucidates by how much changes in the stratospheric circulation would cause how much movements of the ITCZ. The models described on page 10 were not designed to isolate the stratospheric effects on the ITCZ movements. In addition, very little has been learned from those models: the model results actually do not quantitatively support their proposed mechanism shown on Figure 13. For this reason, the conclusion of the current manuscript that changes in the equatorial stratospheric upwelling drove the ITCZ shift appears to be speculative. Without a definitive modeling study, the proposed mechanism involving stratosphere, Section 3.4, and Figure 13 are deemed inappropriate.

3. Not only the causality should be quantified, the statistical significance of the observed quantities must also be established. Currently, there is no statistical test. Actually, the authors decided to skip the statistical test because "a discussion of the statistical significance of the relationships between variables that exhibit large trends in a short data record is practically impossible". This statement is hard to understand, because (i) if there were large trends in two time series, then it is almost certain that the correlation between these two time series is statistically significant because the noise is relatively small compared to the trends. Did the authors actually mean "weak trends"? And (ii) without establishing the statistical significance, the covariances of the stratospheric and tropospheric variables presented in the manuscript may just be artifacts.

4. A more serious problem perhaps is their use of the SVD analysis as a way to probe the mechanism. SVD is never intended for causality attribution. SVD can only extract the maximal variability from the data. The 1st mode of the correlation matrix can only be understood as the largest correlation in the tropospheric and stratospheric circulations that share the same secular trend over the past years. (Whether the secular trend is driven by anthropogenic warming is a separate question.) The SVD itself has no im-

plication on whether the stratospheric circulation is driving the trospheric circulation, or vice versa. They may be independently responding to the same forcing (e.g. anthropogenic warming), and thus the covariance between the stratospheric and tropospheric trends can be almost certainly statistically significant, as discussed in Comment 3. But such trends do not help to "prove" the proposed mechanism.

5. When deriving the proposed mechanism, the authors correlated an index for stratospheric tropical upwelling (I_ω) with tropospheric variables such as temperature, SST, and OLR. Figure 9a clearly showed that I_ω was a sum of a decreasing trend plus an oscillatory component. Were the correlation coefficients shown Figure 9b-9d caused by the trend or by the oscillatory component? (The maximal lag of 5 months shown in Figure 9e can only be related to the oscillatory component.) If it is caused by the trend, then, as Comment 4 suggested, the causality is not proven. Indeed, the authors may simply fit each of $\bar{\omega}$, $\bar{T}$, and OLR with a straight line $y = c_1 * time + c_2$ and plot c_1 to see whether similar Figure 9b-9d could be reproduced. If yes, then there is no direct evidence that I_ω is the cause of the tropospheric changes.

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