

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 #1

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The global warming hiatus has drawn attention to climate anomalies during the early 2000s. The authors examined OLR anomalies during 1999-2016 and identified a decrease in zonal mean OLR near the climatological ITCZ north of the equator during the period. Their SVD analysis showed that it is part of a global tropical mode with a principle component dominated by a trend during the satellite era after 1979. They further discussed the changes in vertical velocity, near-surface wind and SST. The coherent change patterns the authors identified—the intensified ITCZ and SVD mode with a pronounced trend—are quite interesting and potentially important.

The paper may eventually be publishable in ACP but the current manuscript suffers

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from poor organization interrupted by unsubstantiated claims on a wide range of issues (some examples below), some marginally related to the central theme of the paper. As a result, it was very difficult for this reviewer to follow the presentation and figure out what are the robust results and how they are related. I urge the authors to focus on the robust results in support of the major conclusions and delete unsubstantiated speculations. A streamlined, focused presentation is essential for this work to become publishable. Few would have the patience to finish reading an overreaching manuscript that mumbles along.

Major comments 1. Fig. 6c. Zonal-mean vertical velocity change does not seem robust judging from the vertical structure and the relationship with OLR. (a) Vertical velocity change around 10N, where the authors identified a decrease in zonal mean OLR, changes signs three times within the troposphere. The vertical structure is inconsistent with the first-baroclinic mode structure that dominates the tropical troposphere (e.g., Fig. 6d). (b) Strong upward anomalies between the equator and 5S are inconsistent with overall positive OLR anomalies in the region (Fig. 6a). Something seems wrong here. The same problem applies to the SVD mode showing a strong trend in Fig. 8.

The SVD mode with a strong trend features large OLR anomalies in the nonconvective subtropical regions (Figs. 8b,c), raising the question of to what extent the OLR pattern is associated with deep convection and deep vertical motion. Since the focus of the paper is on deep convection, I suggest using precipitation data throughout the paper, instead of OLR.

The discussion of the seasonal transition in climatology (Fig. 6, right) is out of blue and the physical relationship to the multi-decadal change in deep convection is ambiguous at the best. I suggest deleting the discussion of the seasonal transition to avoid confusion and streamline the paper.

2. The connection to the Brewer-Dobson circulation is dubious as it currently stands and should be deleted. ACP readers expect robustly tested results, not unsubstantiated

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speculations. Figure 11 is bizarre: are the narrow bands in vertical velocity in Fig. 11a real, and the meridional dipole in the middle panel of Fig. 11b is averaged out in the meridional mean displayed in the upper panel of Fig. 11a. This is just one example that some speculations do not seem to hold water.

3. At the top of page 8, the authors admitted “a discussion of statistical significance … is practically impossible”. This may be true but the mutual physical relationship among different fields is a minimum test an analysis needs to pass. My major comment 1 questions whether some of the results are robust and physically consistent.
4. Related to my major comment 1, JAS OLR change (Figs. 6 left, 8-9) is not zonally uniform; specifically the decrease in 10-20N is zonally confined in the African-western Pacific sector. This raises the question of whether the zonal mean even makes sense.
5. If the first three sections are hard to go through, section 4 (Summary and discussion) is impossible to comprehend, full of wild, poorly connected speculations. I urge the authors to summarize robust results that make physical sense first and only then, make some reasonable discussions that would be helpful for future research.

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