

Response to Editor

Comments to the Author:

Whilst I have some sympathy with the view of referee 1 that the paper contains sufficient interesting material to warrant publication, notwithstanding the significant remaining uncertainty, I am also sympathetic to referee 2's view that in several aspects the paper is not of the standard required for publication.

Referee 2 is essentially saying that the paper should not be published until some of the uncertainty and speculation can be resolved by model experiments. I would be prepared to take a different view if the paper can be revised to address referee 2's particular concern about proper review of the literature on changes in the Hadley circulation, if speculative discussion can be minimised and, wherever uncertainty remains, that can be made absolutely clear in the text. .

Many thanks for valuable suggestions to improve the paper. It seems that the most of the comments from referee 2 are based on a misunderstanding. Therefore, we first of all would like to resolve this situation. For this, we added a new subsection 3.1 Deep branch of the Hadley circulation, explaining that the phenomenon which we are studying is different from that so far studied by many authors as reviewer thinks. We also added a subsection 3.4 Stepwise transition, about a transient response to better indicate a causal relationship between the stratospheric and tropospheric variation to answer the request of the referee 2. We also added several references about the tropical expansion and the shift of the ITCZ to explain that these are different problems from the present study.

I'm not particularly convinced that the current title is helpful -- it needs to be made clear that any role of lower stratospheric cooling is 'possible', not 'established'.

We changed the title to meet the comments as "Implication of tropical lower stratospheric cooling in recent trends in tropical circulation and deep convective activity".

There are a number of places in the text where the information presented seems non-essential -- e.g. the 4th paragraph of Section 4 which provides a lengthy/detailed explanation of why it might not be possible to conduct model studies -- that could surely be said much more concisely, or the logic seems odd -- e.g. later in Section 4 the sudden decrease in stratospheric water vapour in 2000 is mentioned, that was a short-term event that does not seem to have any simple relation to the longer term cooling discussed by Abalos et al and others.

We removed the related sentences according to the comment and to keep the paper concise.

Response to Referee #2

Referee #2

The main problems of the last manuscript, namely incomplete literature reviews, logical inconsistency, and unsupported mechanisms, remain. The SVD remains as the main analysis tool for the authors to reach the conclusions about the impact (or role in the revision, but still the same meaning) of stratospheric cooling to convection, which, I still think, is a bit far-fetched based on the current analysis. As the authors mentioned in the reply, they expect to have a concrete explanation in the Part II paper. In that case, my suggestion would be to combine Part I and Part II as one manuscript; or, the Part I and Part II papers should be submitted back-to-back at the same time for consistency. The current manuscript is not convincing enough to support the conclusion. In addition, I strongly suggest a professional proofreading service before submission.

We thank the reviewer for the constructive comments. Please see below for our point-by-point responses.

(1a) The Hadley Cell expansion (and the associated change in the ITCZ locations) have been studied extensively in the past few years. Schnieder et al.'s work was only one of the many publications that is relevant to their idea. My comment was that in order to make the manuscript up to an ACP standard, a thorough review of those studies are needed. But the authors do not seem to understand my point. The way the authors responded to my comments was falsifying my suggestion about the "atmosphere-ocean exchange" and therefore they argued in their response that Schnieder et al.'s work is irrelevant to their work. I totally accept the fact that atmosphere-ocean exchange may not be the dominant mechanism. But that's exactly why I would ask for a thorough literature review before the authors move on to propose new ideas.

(1a) Hadley Cell expansion and change in the ITCZ locations

Our study focus on an entirely different tropical phenomenon from the Hadley cell expansion as has been noted in the introduction of the manuscript. While we agree that the Hadley cell expansion has been studied extensively in the past years, we are not aware of studies that have examined the changes in the deep ascending branch of the Hadley circulation, as done here. We emphasize that we are not proposing a new mechanism for the Hadley cell expansion.

In response to the comment, we added some more references and added the text about the tropical expansion as follows.

"One feature of the recent trend in tropical circulation is a poleward expansion of the tropics (e.g., Davis and Rosenlof, 2012; Lucas et al., 2013; Hu et al., 2018). However, this expansion of the tropics is related to changes in the descending branch of the Hadley cell in the subtropics due to changes in the positions of jet streams and storm tracks (Seidel et al., 2007; Kang and Polvani, 2011). Thus, studies of tropical expansion focus on phenomena other than a vertical connection in the tropics, which is the focus of this work."

The focus of this study is the variation in *the rising branch of the Hadley cell (HC) connected to the B-D circulation*, as mentioned earlier version of the manuscript and the response to reviewer. This 'deep ascending branch' which we study clearly differs to that reviewer is discussing, which can be characterized as a 'shallow ascending branch' over the ocean. In order to clarify, we added a new subsection 3.1 Deep branch of the Hadley circulation, about a difference between the deep ascending branch connected to the B-D circulation, and the shallow one connected to the ITCZ.

(2) It's okay to base their study only on observations without modeling if their arguments are well supported. But my previous comment was to suggest removing any an unverified mechanisms. Yet the

authors decide to add another unverified mechanism in the new Figure 11. The authors insist that more evidence will be presented in Part II. I suggest that they combine Part I and Part II as one manuscript.

Response to (2)

Figure 11 summarizes the results obtained in the present analysis. A verification of the mechanism requires a working hypothesis as a first step. The purpose of this figure was to illustrate such a hypothesis rather than to argue “an unverified mechanism”.

In response to this suggestion, we have added some figures from Part II and created a new subsection, 3.5 Stepwise transition, to illustrate the downward penetration of the tropical stratospheric circulation change during the boreal summer 2010. This illustrates the process by which the cooling of the lower stratosphere driven by the enhanced BDC allows deeper penetration of convection into the TTL over the African-Asian sector, thus shifting the zonal mean convective zone northward during the rest of summertime. We hope that the new subsection clarify a possible causal relationship between the stratospheric and tropospheric variabilities.

(4) "Please be noted that we do not try to demonstrate any causality with the SVD..." My understanding is that the authors used the SVD to show that the trends are the most dominant features in TTL (after revision) and therefore, according to the authors, stratospheric cooling is a cause of the shift in the Hadley cell. Both the previous and current titles suggest a causality of stratospheric cooling to tropical convection and the SVD is the only method being used to reach this conclusion, which is a bit abusing the use of SVD in my opinion. Again, a modeling study (or their Part II) is needed here.

(4) We apologize for the confusion. We are not implying that the stratosphere is driving the northward shift in the ascending branch of the HC simply because of the similar trends in the SVD1 (TTL and OLR) time series. Furthermore, as mentioned in the previous response letter, we do not use SVD analysis to demonstrate causality. Rather, the purpose of using SVD analysis is solely to extract the maximum covariance structures between the two fields, which are then compared with structures obtained from different analysis methods.

The SVD-extracted structures in Fig. 6 are also used to demonstrate that convective activity over land within the deep ascending branch of the Hadley circulation is more closely related to variations in vertical velocities at the TTL level than those in the lower and mid troposphere. A possible connection between the deep ascending branch of the Hadley circulation and the lower stratospheric circulation has been suggested in previous works (Eguchi et al, 2015; and Kodera et al., 2015).