We would like to thank you for handling our manuscript. In this document, our reply and the correction policy of the manuscript are described, the corrected manuscript will be submitted you after you check our reply.

Editor Decision: Reconsider after minor revisions (Editor review) (11 Sep 2014) by Peter Haynes
Comments to the Author:
Both reviewers acknowledge that you have responded to their comments on the first version of the paper. However my opinion is that further revisions to the paper are needed to make it suitable for publication in ACP.

Reviewer 2 in particular has made several further comments and in particular notes that in several cases you have given an argument in the response but have not included that argument in the revised paper. Reviewer 2's view is that 'The presented arguments are still very speculative and that needs to pointed out more clearly, in part by including more of the discussion given in the responses in the manuscript.'

Please will you address reviewer 2's further comments in revising the paper further and particular I encourage to include in the revised paper more of the discussion and supporting evidence you have previously given in the responses.

Reply:
Please see the details of our reply in the reply for Reviewer#2.

Reviewer 1 has also made the comment to me that your responses to his/her major comments are acceptable but minimal. Having looked more closely at this I tend to agree. In particular with regard to reviewer 1's first and second major comment, whilst you have demonstrated thermodynamic consistency, you have not really provided an explanation in terms of processes.

Reply:
Slightly detailed discussion on involved processes is added in Summary and discussion section. With a present capability of the model simulation, however, it is difficult to clarify everything at once. We may need to proceed different specified numerical experiments.

Summary and discussion section (p.9, l.23 - p10.l.2)

"The present study investigates stratospheric dynamical impacts on the tropical tropospheric convection during a SSW event from the view point of thermo-dynamic balance in the TTL. As a pilot study, simulation data from a global non-hydrostatic model (NICAM), where moist convection is explicitly represented, was analyzed. The model reproduced the observed processes during SSW: convective activity in the tropical SH was enhanced following an amplification of extratropical planetary wave activity in the winter NH. Vertical velocity and diabatic heating, which are difficult to observe, are intensively investigated."

Summary and discussion section (p.10, l.14 – p.11, l.4)

"Impact of increased stratospheric upwelling can be seen one as decrease of water vapor around the tropical tropopause region, and the other is increase of water vapor around upwelling branch of the Hadley cell in summer hemisphere (15S) (Fig. 5). The former effect around the tropopause is due to a condensation of water vapor by large scale cooling associated with enhanced tropical upwelling in the lower stratosphere (Li and Thompson, 2013). The other effect can be attributed to an increased overshooting and deep convective activity due to decreased static stability in the TTL as a consequence of adiabatic cooling in the lower stratosphere but diabatic heating in the TTL (Fig. 4).

This impact of decreased stability on the convective activity is studied by Chae and Sherwood (2010) with a non-hydrostatic regional model of 28 x 600 km domain. Similar effect is confirmed for the present case. Moreover, the global model also showed the occurrences of more complex processes: large scale organization of convective activity and the associated decrease of water vapor in the tropical NH (Fig. 3). The large scale change in convection took place as mesoscale organization of convective system, such as tropical cyclone or storms (Fig. 5), which can also be found in observational result (Kodera et al., 2014). To clarify such complex processes, we further needs specific numerical experiments, by using an updated version of the global non-hydrostatic model."

Add the following papers to the reference list

Chae, J-H, S. C. Sherwood (2010) Insights into cloud-top height and dynamics from the seasonal cycle of cloud-top heights observed by MISR in the West Pacific region. *J. Atmos. Sci.*, 67, 248–261.

Kodera, K., B. M. Funatsu, C. Claud, and N. Eguchi (2014) The role of convective overshooting clouds in tropical stratosphere–troposphere dynamical coupling, Atmos. Chem. Phys. Discuss., 14, 23745-23761.

Perhaps we have to accept that at present there is no clear explanation, but including more of your responses to reviewer 2 in a revised paper will improve things (by acknowledging uncertainty or mentioning relevant results from previous work).

With respect to reviewer 1's 3rd major comment about ice clouds, your reply is a little confusing. Are you saying that the ice clouds are appearing through the mechanism of convective activity extending higher in the TTL? How are you sure that the ice clouds are not simply occurring because of large-scale cooling and hence condensation?

Reply:

We explained in the above that there are two processes creating cloud in the TTL.

Other specific comments are:

p8 l24: 'which may balance the third and fourth terms' -- it seems unsatisfactory to leave this as 'may' -- surely you could verify this balance from the model?

Reply: remove 'may'.

p10 l18: Clarify this reference -- is the paper that has now been submitted to ACPD as a companion paper?

Reply: revise as "Kodera et al., 2014" and add it to the reference list.

In making revisions to the paper please can you make a list of the changes you have made in response to the comments of reviewer 2 and to my own comments above and provide reasons if you have chosen not to make changes.

I expect to consider the revised version of the paper myself without sending it again to the referees.

Reviewer #2

The authors have responded to the reviewer comments and made some corresponding changes to the manuscript. However, I still have some comments that should be taken into account before publication.

In particular, I feel that most of the discussion given in the response to my earlier review comments should be incorporated in some form or another in the manuscript. For example, the Chae and Sherwood (2010) paper seems crucial for some of the arguments (see author response), but is not referenced, and no corresponding discussion is included in the revised manuscript. Similarly for the Boville and Chen (1988) reference.

No discussion related to the grid size of the model is included (see earlier review and author response).

By the way, I'm not sure I agree with related argument in the author response, which basically states that a poorly resolved cloud is more realistic than a parametrized one - to me that's unclear, they may both be equally unrealistic and in certain cases the parametrized cloud may be more realistic than its poorly resolved counterpart.

The presented arguments are still very speculative and that needs to be pointed out more clearly, in part by simply including more of the discussion given in the responses in the manuscript.

Reply:

According to the comment, the text has been modified and added as follows, one in Introduction section and the other in Analysis data section.

1) Section 1 (P.3 line 20-29)

"These results suggest that convection plays an important role in the stratosphere–troposphere coupling in the tropics. In these models, however, the effects of convection were not explicitly treated, but were represented by cumulus parameterization. Thus, dependence on the parameterization was unavoidable. Furthermore, examination of thermodynamic processes in the TTL using the higher resolution GCM is desired to clarify the coupling process between the stratosphere and troposphere, where the thermo-dynamic balance is complicated. In the present study, convective change during a SSW event that occurred in January 2010 is investigated using a high-resolution global simulation data from a global non-hydrostatic model, NICAM (non-hydrostatic icosahedral atmospheric model) (Satoh et al., 2008, 2014), in which a cumulus parameterization was not employed. This allows discussion of the dynamic and thermodynamic changes in the TTL without any prescribed relationship between dynamics and moist convection."

2) Section 2 (p.4, l.10).

"Kodama et al. (JGR, 2012) assessed cloud signals in NICAM simulations and demonstrated that the gross behaviors of clouds can be statistically reproduced with 14 km mesh size, although individual clouds are not sufficiently resolved. The main purpose of the present study is not a realistic presentation of an individual cloud, but investigation of diabatic heating processes in the TTL without any prescribed relationship between convection and dynamics as assumed in a cumulus parameterization. The use of 14 km horizontal mesh size and 38 km model top is marginal to a TTL study, which will be followed by numerical studies with more suitable setups (Satoh et al. 2014). Nevertheless, the BD circulation and the seed of the SSW events included in the initial data, which was interpolated from an objective analysis, led to spontaneous occurrences of reasonable atmospheric circulation and convection in the model for the simulation period, as shown in Section 3"

p.4, l.11

"The impact of low model top on the planetary wave propagation has been studied. If model top is simply lowered, large difference occurs in the troposphere as well as in the stratosphere. However, if some readjustment of the wave dissipation is made, lower stratospheric circulation becomes more or less realistic [Boville and Chen, 1988]. Present study mainly concerns circulation lower than about 25 Km, so that NICAM model of which model top at 38 km can be used. "

Add the following papers to the reference

Boville, Byron A., Xinhua Cheng, 1988: Upper Boundary Effects in a General Circulation Model. *J. Atmos. Sci.*, 45, 2591–2606.

Satoh, M., H. Tomita, H. Yashiro, H. Miura, C. Kodama, T. Seiki, A. T. Noda, Y. Yamada, D. Goto, M. Sawada, T. Miyoshi, Y. Niwa, M. Hara, T. Ohno, S. Iga, T. Arakawa, T. Inoue, H. Kubokawa, 2014: The Non-hydrostatic Icosahedral Atmospheric Model: description and development. Prog. Earth Planet. Sci., accepted.

Other comments:

1. I disagree with the authors on line 23 of page 2 ("... relatively easy to identify the causal relationship ..."). A causal relationship between the SSW and the tropical convective activity would mean that without the SSW the enhancement in convective activity would have not taken place. It seems to me that the only way to actually infer such a causal relationship in the modeling framework the authors use, is to somehow remove the SSW in a sensitivity simulation and show that the hypothesized effect due to the SSW is absent in this sensitivity simulation.

Reply:

Here, "relatively easy" means relatively easy compared to a longer timescale variation in which atmosphere-ocean coupling process should be considered. To make the difference of the time scale clearer, MJO is eliminated as follows.

"It is, therefore, relatively easy to identify the causal relationship between the stratospheric and tropospheric variation, which can be separated from long-term variability such as El Nino–Southern Oscillation (ENSO), or the Quasi-Biennial Oscillation (QBO)."

We also added in the text that the numerical experiments such as proposed by the reviewer have already been made using a conventional GCM to investigate the impact of the SSW (Please, see the answer for the previous question)

2. I still think that a better assessment of how anomalous the effects are is needed. For example, in Fig. 5: what is the chance that the enhancement in

(b) simply represents quasi-random variability?

Reply:

Due to a limited capability of the calculation, it is difficult to make a long run with a global non-hydrostatic model. However, it does not represent random noise, but it reproduces observed feature (c.f. Fig.4 of Kodera et al., 2014).

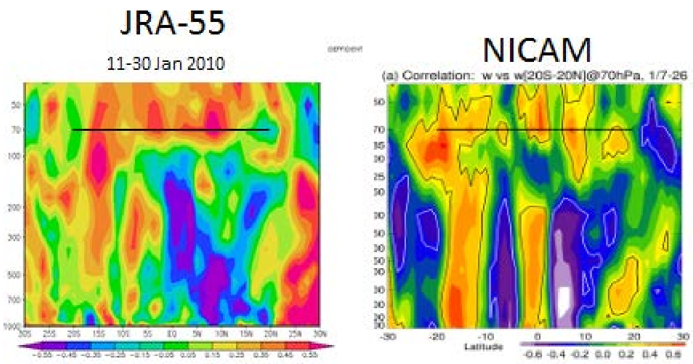
The sentence (p.10, l.7-8) was modified to indicate the similarity with observation.

"Particularly large variation is found over in Fig.5 over the southwestern Indian Ocean, southwestern Pacific and coastal region of Australia similar to the observation (Fig.3 in Kodera et al., 2014)".

3. Fig 3a is still included even though the authors acknowledge that statistical significance is unclear -- shouldn't the panel then simply be removed?

Reply:

It is difficult to estimate the statistical significance with limited data. However, we can still compare with the observational result how the model simulation to see whether it is realistic. The following figure is corresponding observational correlation map in Fig.3.



page 2, line 10: The (full) temperature tendency in the TTL is pretty much zero (see Fig. 4e), at least no discernible trend due to the SSW is obvious. So I find this statement confusing. Maybe what should be pointed out here is that the diabatic heating from cloud formation contributes significantly to the heat budget (as does that due to upwelling).

Reply:

According to the comment, the sentence is modified as follows.
"Although the upwelling in the TTL was correlated with that in the stratosphere, the temperature tendency in the TTL changed little due to a compensation by diabatic heating originating from cloud formation."

page 6, related to Fig. 2d: I'm having a hard time to see evidence for the anomalies associated with the SSW (i.e. comparing period (iii) to the previous ~2 weeks) - please comment

Reply:

According the comment, the sentence is divided to make the description on ice cloud clearer.
"The region of minimum OLR shifted southward from 5S to 12.5S after 14 January. The upper level clouds (ice clouds) shifted also southward but extended farther southward (15S) compared to convective clouds (Fig. 2d)."

page 2, 18: increase in what? intensity, frequency …?

Reply: modify 'increase of the intensity'.

page 8, line 15: should be w' (not asterisk) in last term

Reply: revise w' (not asterisk).

page 16, last sentence: values should include units

Reply: add the unit [10^{-6}kg/kg].