Reply for comments of Anonymous Referee #1

Thank you for reviewing our manuscript.
The authors understood three major points pointed out by reviewer#1.
Along the comments and suggestions from two reviewers, almost of them were corrected and modified, including figures.

Specific (major) comments

First, I wonder what the overall contribution from this study is and how significant it is. The authors state that they investigate changes in convection during SSW using the NICAM data (l. 27, p. 6805). It will be much more useful to clarify what is the scientific question(s) that remains unanswered in the previous studies and is targeted in this investigation.

The statements in and around the paragraph read as the target question is "are the previous observational and GCM results reproduced by the NICAM for the SSW?". An- other possibility is that the authors aim to diagnose the thermodynamic budget of the changes in the tropical troposphere ("how do the changes in the tropical troposphere including clouds occur thermodynamically"), which is difficult with observational or GCM data. In any case, however, it may not be sufficient in originality and significance if the authors just describe the changes (some part of the present results repeat/confirm their previous results) and show that they are thermodynamically consistent. (This is related to the next point.)

Reply: We added the following sentence for the explanation of the present purpose at l.22-24, p.3 of the revised manuscript (attached as Supplement).

"Further, to investigate the adiabatic and diabatic parameters in the TTL are necessary to clarify a coupling process between the stratosphere and troposphere through the thermo-dynamic balance."

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Second, I do not well understand, from the paper, what is the key dynamical or physical process(es) for the convective changes to occur during the SSW. It seems that this study still lacks presenting direct evidence for such a key process(es). For example, what does determine the particular locations and timing (time scale) of the convective changes in response to the SSW (or wave-driven upwelling in the tropical LS). Exploring such a key process(es) will be an essential point that is worthwhile for a serious investigation on this topic.

Reply: The region where the convective response is apparent is the region where the deep convective activity is seen in climatology. However, it is difficult to say exact region for a particular event, because of the internal variability of the atmosphere. For discussion of regional response, ensemble mean simulation is indispensable, which will be able to do in near future.

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It also seems uncertain to me how the upper level ice (cirrus) clouds in the TTL occur in period 2. Can you be more specific about how the destabilization in the TTL prompts the cloud formation by referring to a direct or existing result(s)?

Reply: Upward motion around the tropopause produces cooling and decrease of static stability in the upper TTL. As a consequence, deep convective activity extends higher in the TTL as indicated by vertical velocity in the Figure 1.

A time-latitude section of ice clouds is added to the revised manuscript as Figure 2d.

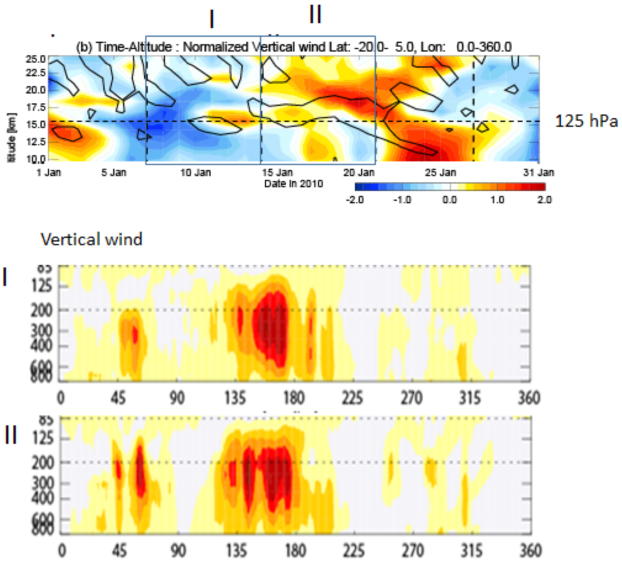

Figure 1: (top) Time-height section of normalized vertical wind (color shading) and negative value of static stability are shown by contours. (bottom) Longitude-Pressure section of vertical wind during periods i and ii.

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Why do such clouds tend to be absent in period 3 even when the LS temperatures are low (Fig. 2a,b)?

Reply: The cirrus clouds were not absent at period 3 (January 21-27). It is shown in Figure 2d of the revised manuscript.

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Third, I sometimes feel it difficult to follow the authors' logic flow (or terminology) in places through the manuscript. I think that the authors will need to better distinguish the followings: refer to existing results in the literature, describe direct results from the materials, derive consequences of present and/or existing results, and make suggestions/speculations.

Reply: Along the comment, we rechecked and modified the descriptions in Sections 3 and 4 to make clear the mechanism found by the present study (e.g., l.9-27, p.10).

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Some notable examples are found in the third paragraph in Section 4, whereas other examples also exist.

"Vertical velocity in the LS is mainly driven by extratropical planetary waves," Is there any direct evidence for this statement given in this paper? Or, if this is based on an existing knowledge, then please provide a reference(s).

Reply: This is an existing and basic knowledge of Brewer-Dobson circulation (e.g., Andrews et al. 1987). Reference to the recent studies is added to the revised manuscript. (l.11, p.10 in the revised manuscript)

Andrews, D., J. R. Holton and C.B. Leovy, Middle atmosphere dynamics, Academic press, 1987, p.489.

Ueyama, R., Gerber, E.P., Wallace, J.M., and Frierson, D.M.W, The role of high-latitude waves in the intraseasonal to seasonal variability of tropical upwelling in the Brewer–Dobson circulation, J. Atmos. Sci., 70, 1631–1648, doi:http://dx.doi.org/10.1175/JAS-D-12-0174.110.1175/JAS-D-12-0174.1, 2013.

Abalos, M., W.J. Randel and E. Serrano, Dynamical forcing of sub-seasonal variability in the tropical Brewer-Dobson circulation, J. Atmos. Sci., in press, 2014. (e-View doi: http://dx.doi.org/10.1175/JAS-D-13-0366.1)

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"reflected in the location of the strongest LS upward motion between 20S and 30S," Is there any

result showing that the strongest LS upwelling occurs in these latitudes? Figure3a will not support this claim, as it just plots a correlation.

Reply: The sentence was removed from the revised manuscript.

Generally, the upwelling branch of BD circulation is located at off equatorial summer hemisphere, for this case is south of the equator.

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"but that in the UT was controlled mainly by deep convection (between 10S and 20S)," The word "controlled" will be too strong. The results just show that the upwelling and DH (deep convection) occur at similar locations (Figs. 3a,b and 5). Other results or knowledge will be needed to draw this statement.

Reply: The word was changed from 'controlled' to 'affected'. (l.12, p.10 in the revised manuscript)

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"while the vertical velocity in the TTL was affected by both upwelling in the LS (a branch of the stratospheric meridional circulation) and convection, which is able to reach to the TTL." The phrase ("the TTL vertical velocity was affected by the LS upwelling") sounds awkward. The next sentence makes better sense: "The enhanced upwelling in the LS during the SSW event can intrude deeper into the TTL".

Reply: The sentence was removed.

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Specific (minor) comments

Section 3.1; The authors seem to use the word "cooling" for two meanings: when temperature tendency is negative, and when temperature is lower (e.g., than normal). It is simple and straightforward to use each word only for one meaning. If the authors focus on the temperature tendency, it will be useful to display a plot for that.

Reply: Along the comment, the "cooling (warming)" related with temperature tendency was changed to "negative (positive) temperature tendency".

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l. 27, p. 6808; I'd like to confirm that the serial correlation of the time series is taken into account in calculating the significance of the correlation of the vertical wind (Fig.3a). Namely, the daily mean data for the 30 days should not have an actual degree of freedom of 30 in the statistical test. Unless this effect is considered, the result may be much weaker than the authors expect. Displaying time-latitude sections of the zonal mean vertical wind at a few key levels should be useful to sense its variations.

Reply: Figure 2 shows the time-latitude section of vertical velocity at several altitudes. It is shown that the upward velocity in the southern hemisphere increased after 13 January at 5, 12 and 20 km, although the increasing of upward velocity at the middle troposphere (5km) delayed than that in the upper layers. The vertical lines show the date that the zonal mean vertical wind at 20 km had just started negative as shown in Figure 4d. On the other hand, the temporal variation of latitudinal structure of vertical velocity at 17km (around 100hPa) are not clear. In fact, these features can be seen in Figure 3a, therefore they did not be included in the manuscript but the explanation was added. (l.5-6,p.7 in the revised manuscript)

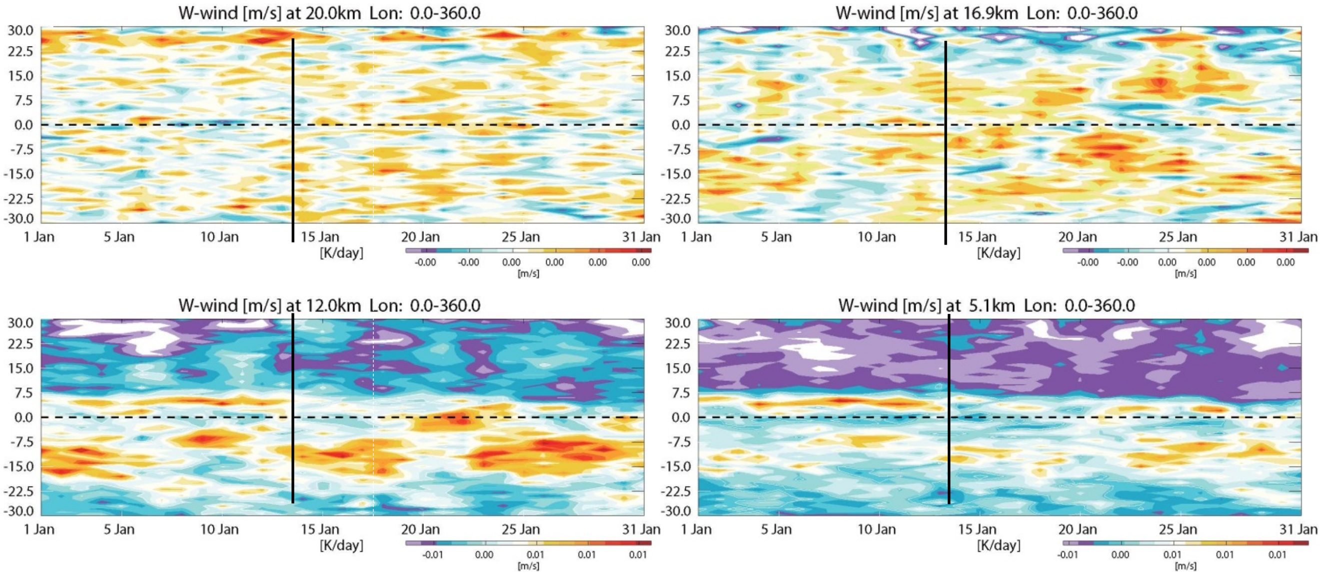

Figure 2: Time-latitude sections of vertical velocity at 20 km (top-left), 17 km (top-right), 12 km (bottom-left) and 5 km (bottom-right) from January 1 to 31. The vertical lines show the date.

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l. 5, p. 6810; It does not seem convincing to me why the authors choose the 20S-5N range for Fig. 4. Figure 3a seems to imply that anomalous vertical wind has strong latitudinal dependence (including sign changes) in these latitudes. Taking the latitudinal average in 20S-5N will lead to a strong cancellation of such anomalous vertical wind signals. Time-latitude sections of the vertical wind will be useful again.

Reply: Because the convection accompany downwelling area in its neighborhood, to get a mean upward velocity of a large scale circulation such as of Hadley circulation, average is taken over the tropics of the summer hemisphere.

However the figures were modified by the average between 20S and equator, because of focusing to highlight the southern hemisphere change.

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l. 16, p. 6810; How do the authors find these terms "major"? It will be needed to say something like "we find that these terms make major contributions in the equation in our preliminary calculations (not shown)."

Reply: It is the basic knowledge [text book of Andrews et al.,1987] and the order analysis also shows that the two terms are dominant.

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l. 19, p. 6810; The equation should be for the zonal mean temperature tendency. If so, please denote some symbol for the zonal mean.

Reply: Along the comment, the equation is modified. (l.15, p.8 in the revised manuscript)

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l. 2, p. 6811; The authors point out the close correspondence between the adiabatic cooling and heat flux time series (Fig. 4c,d). How is this result consistent with the study of Polvani and Waugh (2004, J. Climate)? They showed the importance of cumulating or averaging the poleward eddy heat flux (in time) in the lower stratosphere for obtaining high correlations of the heat flux with the 10 hPa NAM index.

Reply: The eddy heat flux is used as a proxy of wave forcing. Roughly speaking, the eddy heat flux is related with zonal wind "tendency" or temperature "tendency". On the other hand, to analyze the variation of polar vortex, they need to

INTEGRATE eddy heat flux.

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Fig. 2; It will be useful to match all panels in Fig. 2 with those in Fig. 1 in terms of the quantities plotted, and axis ranges. Some panels could be separated into another figure.

Reply: The authors think that we did not need to be same figures at both Figures 1 and 2, because Figure 1 only shows the characteristics of the atmospheric change when the SSW occurs in real atmosphere.

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Fig. 2c; Is it possible to show cloud fractions of deep convective and cirrus clouds separately?

Reply: Figure showing the variation of ice cloud (Figure 2d) was added together with the explanation to the revised manuscript.

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Why do some plots use height (km) and others use pressure (hPa)? It is simpler to use either one, if possible.

Reply: Both the pressure and altitude labels were put on the vertical-axis in each figure.

Reply for comments of Anonymous Referee #2

Thank you for reviewing our manuscript.
The authors understood the comments from reviewer#2.
Along the comments and suggestions from two reviewers, almost of them including figures were corrected and modified.

The possible effects of a sudden warming on tropical deep convection are studied using the global non-hydrostatic model NICAM. A specific feature of the model is that it resolves the mesoscale and does not use a convective parametrization. The authors point out enhancements of deep convection seemingly associated with the sudden warming event and show that diabatic heating associated with cloud formation plays an important role in the head budget of the TTL during the studied period.

Even though the claims made by the authors are overall sensible (higher convective cloud tops aided by the adiabatic cooling due to enhanced TTL upwelling associated with the sudden warming), I'm not fully convinced by the presented results. Most arguments are quite speculative.

For example, the cooling tendency in the second half of January for the most part only reaches down to ~17 km (Fig. 4a) – is this really low enough to influence deep convection? How does cloud top height respond (neither shown nor discussed)?

Reply: Chae and Sherwood (2010) suggested that the variation of static stability near the tropopause due to the stratospheric upwelling can influence the clouds even their cloud height peaks only near 12 km. This is consistent with the present study.

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.

The deep convective response was seen in diabatic heating rate associated with cloud microphysics (Fig. 5a, b), which is a useful measure of the existence of cloud around the TTL. It is difficult to detect the cloud top by the cloud ice mixing ratio.

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At 14.3 km (Fig. 4e) I can't make out any significant changes to the temperature tendency during or after the sudden warming.

Reply: As described in the manuscript, the temperature tendency in the TTL is nearly zero because the diabatic heating derived from ice cloud formation and the adiabatic cooling associated with the upwelling cancel each other out.

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General comments:

The authors make the point that treating convection explicitly is important (6805/6806) in the present context, but the NICAM simulation analyzed here uses a grid spacing of 14 km. This is still almost an order of magnitude coarser than what is required to start to resolve individual convective plumes. The realism of the simulated convection, especially related to the more sensitive cloud top region, is therefore questionable.

Reply: Kodama et al. (JGR, 2012) assessed cloud signals in a 14 km mesh NICAM simulation, based on previous studies demonstrating that the gross behaviors of clouds can be statistically reproduced, although individual clouds are not sufficiently resolved at this mesh size.

Here, the main point is not a presentation of an individual cloud, but explicitly calculate diabatic heating by cloud formation without using cumulus parameterization.

In conventional GCMs, diabatic heating due to convection is parameterized. That is, the vertical distribution of heating rate is externally determined through a diagnostics of the large-scale field. In this sense, stratospheric impact can be largely parameterization dependent. Therefore, the impotence is first of all to calculate the diabatic heating consistent way together with the vertical velocity. In this sense the use of a global non-hydrostatic model without cumulus parameterization is more realistic than the use of conventional GCMs, even the resolution is still coarse.

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Furthermore, the model does not fully resolve the stratosphere (top at 38 km) and the realism of the simulated Brewer-Dobson circulation, in particular its upwelling branch near the tropical tropopause is therefore unclear. No discussion related to either of these issues is presented, nor can anything related be inferred from the presented results (except indirectly through comparing panels 1a and 2a, although the levels between ERA-Interim and the model don't match up). How does the basic TTL structure compare between the simulation and observations for the studied time period?

Reply: 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. The shortcoming of the low model top is found as very rapid decrease of the eddy heat flux to negative values in Fig. 4c, while observed heat flux decreased more gradually. Negative values in model heat flux suggest a reflection of the planetary waves from above, which should be due to low model top. In the present study, we focus on the initial phase of the stratospheric impact (until 27 January), so that this may not cause a serious problem.

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

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The occurrence of an MJO event during the analyzed time period is mentioned with the remark that the simulated MJO was weaker than the observed one.

Was it so weak as to not show up in any of the presented analyses (e.g. the cloud fraction in Fig. 2c)?

Reply: Figure 1 shows time-longitude sections of OLR along the equator : (left) model simulation and (right) observation.

The period ii corresponding to an enhanced stratospheric upwelling, is indicated by solid horizontal lines in each panel. In the case of the observation, convective active region shifts slowly eastward from Maritime continent to Western Pacific from the beginning. In the case of the model simulation, eastward propagation starts only at the end of period ii, or the beginning of the period iii.

In both cases, equatorial convection is suppressed during period ii, because the convective active regions shift southward off equatorial southern hemisphere. The present study focuses mainly on the meridional variation of convection in the southern hemisphere. This cannot be explained by single eastward travelling convective signal, such as MJO.

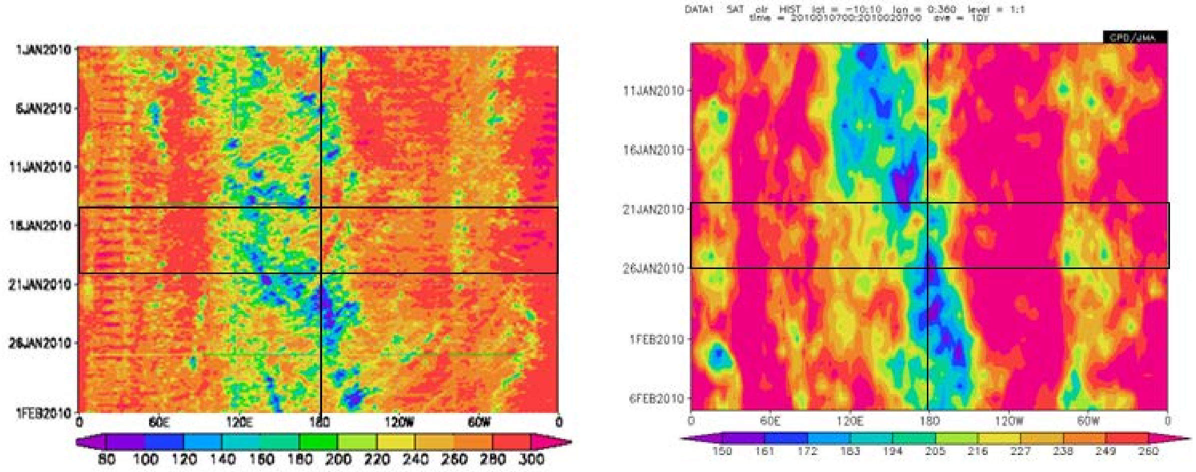

Figure 1. Longitude-time section of OLR from NICAM (left) and NOAA (right). Period (ii) is indicated by two horizontal lines..

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What is the likelihood that the enhanced deep convection in period (iii) could be related to MJO activity?

Even if an MJO influence can be ruled out for period (iii), the MJO signatures earlier in the season could be used to put perspective on the anomalies found in period (iii). In other words, how comparable is the hypothesized sudden warming influence on tropical deep convection to the MJO or other tropospheric variability?

Reply: As stated in the above, the eastward propagation of convective active region in the model starts only at the period iii at the reorganization stage of modified convective activity. The activation of convection around dateline is different from canonical lifecycle of MJO, which suggests that the enhanced convection is affected by the stratosphere upwelling event than MJO. As described in the manuscript, a positive feedback process further makes convection enhance.

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Specific comments on Figures:

Fig. 2c: is this the column integrated cloud fraction, or at a specific level?

Reply: Rewritten as “column integrated cloud fraction”.

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Fig. 3a: These correlations should be heavily influenced by memory in the time series (auto correlations) and the shortness of the time period should lead to sampling issues. The shown correlation field is very noisy. Why not show a similar composite structure as in panels (b) and c)? Why is the Eulerian mean vertical velocity used here as opposed to the TEM version, which would be more physical (especially in the TTL and lower stratosphere)? The latter point is also relevant for Fig. 4b.

Reply: As pointed out by two reviewers, the calculation period, approximately 3 weeks, is not enough to discuss statistical significance. The figure of vertical velocity anomaly was not included in the manuscript, because the feature of vertical velocity anomaly is same with panel (b). Instead, we emphasized the relationship of vertical velocity between lower stratosphere and troposphere.

We confirmed that W and W* are practically the same in the equatorial region. Therefore it is simpler to use W. Also, a comparison between a longitudinal distribution of vertical wind, or heating distribution in Fig. 5 is easier when W is used.

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Figs. 3b/c: percentage differences instead of absolute differences would provide more insight - I find it hard to obtain a sense of the strength of the anomalies from the shown plots

Reply: The following (attached) figure (Figure 2) shows the anomaly by percentage expression, which is quite similar to the absolute difference.

In this study, we want to discuss the absolute value of anomaly, especially at the lower troposphere, and its distribution change. Therefore, we keep the original Figures 3b and 3c in the revised manuscript (attached as Supplement).

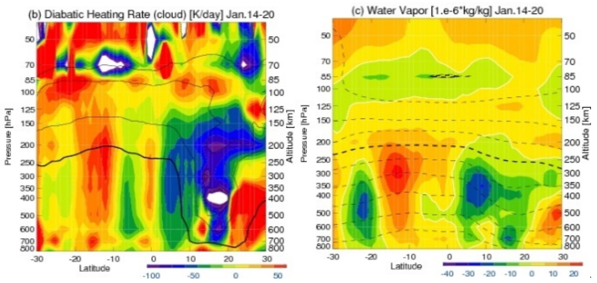

Figure2: Same as Figures 3b,c in the manuscript but the color contour indicates percentage [%].

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Fig. 4a,b: A color scale with near zero values in white (e.g. as in 2b) would make this easier to read.

Reply: Along the reviewers mention, the color shade of Figures 4a,b was changed.

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Fig. 5 and related discussion in text: I can’t see the big difference between the left and right panels that the authors point out.

Reply: The color scale in Figure 5 was changed so as differences become clearer: the convection over the western Indian Ocean clearly enhances at the latter period, and active convection region over the western Pacific extends horizontally as well as vertically.
Further, the upward area extends higher into the TTL at the latter period.

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Specific Comments on Text:

6806, line 14: is this a single or double moment scheme?

Reply: Single moment scheme. The information was added to the revised manuscript (l.12, p.4).

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6806, line 24: "30 days" - the full month of January is shown in Fig. 2, so if the simulation started on December 20 it must have lasted longer than 30 days – please correct

Reply: Corrected to 60-days.

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6807, line 20: shouldn't it be Fig. 1b?

Reply: Fig.4c is correct. Here, we want to show the stratospheric wave activity in the stratosphere in the NICAM simulation.

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6807, lines 21-24: "almost comparable" seems too strong - the simulated anomalies are only 60-70% of the observed ones; a more quantitative statement would be more appropriate

Reply: The word "almost" has been removed.

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6808, line 1: this is hard to infer from the monthly temperature anomalies shown in Fig.2b - cooling refers to the time derivative of what is shown and I'm unable to make out where that is strongest shown in Fig.2b

Reply: As can be seen in Fig. 2a, decreasing tendency of the tropical temperature is largest during the transition period of anomalous warming to anomalous cooling (period ii).

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6808, lines 4/5: I guess this refers to period (ii) (it wouldn't be true for other periods) -should be clarified

Reply: The phrase "in period (ii)" has been added (l.25, p.5).

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6808, lines 11,12: this sounds interesting and should be elaborated on more, possibly including a relevant Figure

Reply: The figure showing a latitudinal variation of ice cloud was added as Figure 2d.

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6809, line 15: "not shown" - isn't this shown in Fig. 2b?

Reply: Corrected.

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6810, line 10: I can't see it reaching down to 10 km, to me it looks like it only reaches down to 17 km; at least the cooling tendencies in the TTL are very small and don't look significant

Reply: As mentioned above, the sentence was modified (l.5-7, p.8).

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6810, Eq.: there are bars missing (or similar notation for zonal mean) and it should be wˆprime in the last term

Reply: Corrected

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6810, line 21: delete "vertical"

Reply: Deleted.