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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A global non-hydrostatic model study of a downward coupling through the tropical tropopause layer during a stratospheric sudden warming" *by* N. Eguchi et al.

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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.

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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 $\sim$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: Attached 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.

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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).

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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.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C4447/2014/acpd-14-C4447-2014-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 6803, 2014.

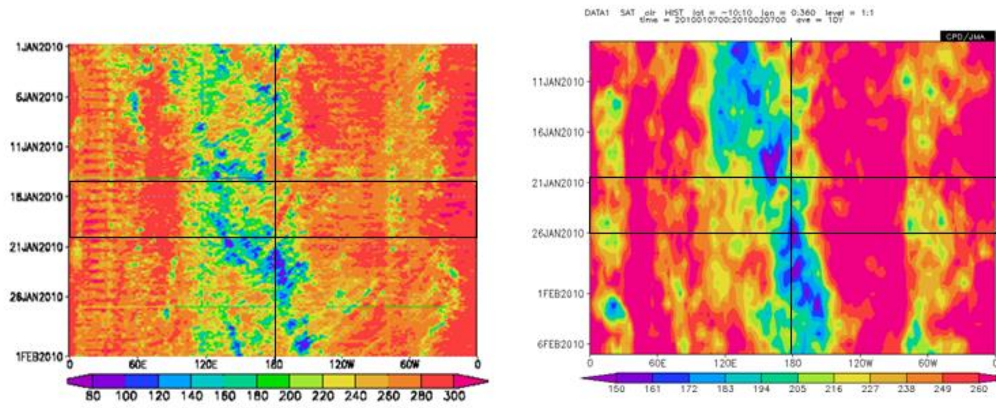

Fig. 1. Longitude-time section of OLR from NICAM (left) and NOAA (right). Period (ii) is indicated by two horizontal lines.

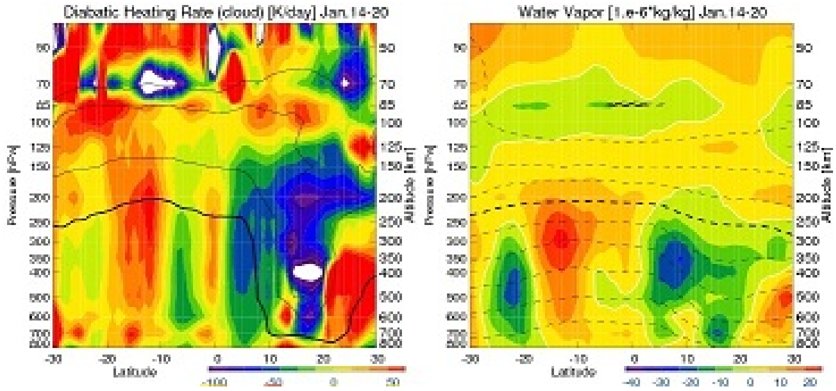

Fig. 2. Same as Figures 3b,c in the manuscript but the color contour indicates percentage [%].