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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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.

Anonymous Referee #2

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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 ar-

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

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. 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?

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)? 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?

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Specific comments on Figures:

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

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.

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

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

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.

Specific Comments on Text:

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

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

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

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

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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

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

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

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

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

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

6810, line 21: delete "vertical"

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