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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 #1

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General comments

The authors investigate the impacts of the January, 2010 SSW on the tropical troposphere (especially on the convection) by describing changes in this region during the event simulated by a global non-hydrostatic model, NICAM. This study is an extension of the authors' previous works on this topic: they here examine the data from the NICAM (explicitly representing convection), while previously analyzing observational and GCM data. After confirming the previous results, this study shows that the changes in the tropical troposphere (temperature, vertical wind, convection, etc.) during the SSW are generally related to each other in a consistent manner.

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Interactive Discussion

Discussion Paper

Although the topic is interesting and the use of the NICAM data is promising, I find several difficulties, such as in originality/significance, key process(es) for the coupling, and presentation, as described below. I would therefore recommend “minor revisions” before publication of this manuscript.

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?”. Another 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.)

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.

It also seems uncertain to me how the upper level ice (cirrus) clouds in the TTL occur

ACPD
14, C1118–C1122, 2014

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

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.

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).

"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.

"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.

"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".

Specific (minor) comments

ACPD

14, C1118–C1122, 2014

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.

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.

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.

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).”

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

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.

Fig. 2; It will be useful to match all panels in Fig. 2 with those in Fig. 1 in terms of

ACPD
14, C1118–C1122, 2014

the quantities plotted, and axis ranges. Some panels could be separated into another figure.

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

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

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

ACPD

14, C1118–C1122, 2014

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Interactive Discussion

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