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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of the sudden stratosphere warming on quasi-2 day waves" *by* S.-Y. Gu et al.

S.-Y. Gu et al.

gsy@ustc.edu.cn

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Interactive comment on "Influence of the suddenstratosphere warming on quasi-2 day waves" byS.-Y.Gu et al. Anonymous Referee #1 Received and published: 4 March 2016

GENERAL COMMENTS The paper is an interesting contribution on the appearance of QTDW. TIME-GCM simulations are used to separate the impact of forcing of planetary waves at the surface. Although the simulated SSW is only minor, the data base is evaluated to show an overall decrease of westward PW3 due to reduced instabilities and an increase of westwardPW2 due to nonlinear interactions. I suggest an extended discussion of these results with respect to observations and recommmend: minor revision.

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Discussion paper

Reply: We thank the reviewer for reading through the manuscript and the following are responses to all the comments/suggestions made by the reviewers, and the tracked version is also attached for further interactive discussions.

SPECIFIC COMMENTS 1) Mesospheric instabilities: The simulations bring up a SSW and are sufficient to discuss certain pathways leading to QTDWs. However, the warming is minor and possibly not sufficiently strong to explain observations of major warmings. One of the obvious effects is the missing of instabilities in the stratospheric easterlies during these times. Its potential to generate planetary waves is lined out in Liu et al. (2004), Limpasuvanet al. (2012), Zülicke& Becker (2014) and Sato &Nomoto (2015), for example.

Reply: In our TIME-GCM simulations, the SSW is generated by forcing stationary planetary waves with zonal wave number (SPW1) at the lower model boundary ($\sim$10 hPa or 30 km). An amplitude of 2.8 km for SPW1 is set in case 3 to generate the strong SSW event in our simulation. Although case 3 may not be a major SSW according to standard definition (and this is a limitation of the model configuration with its lower boundary specified at 10hPa), it does cause strong jet reversal (from westerly to easterly) above $\sim$40km. As shown in Figure 7(e), there is indeed a region with negative potential vorticity gradient corresponding to this reversal. In the case of a major warming, the exact altitude of the instability may be different, but the process should be qualitatively similar to the results presented here. We have revised the paper to reflect this point, and added the references suggested.

2) Equatorial instabilities: It should at least be mentioned, that the intensifying equatorial stratopause easterlies may also lead to instabilities and subsequent forcing of QTDW (Limpasuvan et al., 2000).

Reply: We note that Limpasuvan et al. [2000] found the inertial instability could play a role in amplifying QTDW. Nevertheless, the TIME-GCM experiments performed by Liu et al. [2004] showed that the inertial instability does not seem to greatly enhance the

wave response but only causes additional spatial variability in the equatorial region. This is mentioned in the revision.

3) Observations: For the conclusion of the paper I would like the authors to add a discussion observations in the context of W2 and W3 relation to SSWs. Beside of the relatively weak SSW modelled here, it could also be that a SPW2 forcing (a split-vortex SSW) may directly lead to stronger QTDW.

Reply: Following the reviewer's comment, we added discussions in the conclusion regarding QTDWs from TIMED observation. We agree that the SPW2 forcing may provide additional variabilities for QTDW, but this may not necessarily mean stronger QTDW. Since the growth of QTDW is sensitive to the background wind, and only the modest mean wind is favorable for the propagation and amplification of QTDW. Nevertheless, it will be interesting in future studies to compare the QTDW activities during split-vortex and displacement-vortex years (e.g., 2006 and 2009), which may show new light on the inter-annual variability of QTDW.

TECHNICAL CORRECTIONS With respect to common use of wordings, I suggest the following: a) use "sudden stratospheric warming" instead of "sudden stratosphere warming" b) use "nonlinear advection" instead of "nonlinear advective"

Reply: The words are corrected in the revision.

In the following, all numbering refers to the discussion paper "acp-2015-982.pdf"

line 49: "TIME-GCM" should be defined.

Reply: TIME-GCM is defined in the revision.

line 86: "TIMED/SABER" should be defined.

Reply: It is defined in the revision.

line 96: stratospherIC - see a) above.

Reply: It is changed in the revision.

line 115: If defined before as suggested, "TIMED" need not be defined here again.

Reply: The definition of TIMED is deleted here.

line 197: Here it seems to me that "eastward" and "westward" were confused.

Reply: The summer easterly in propagates from the east to the west, and the winter westerly propagates from the west to the east. Thus, we use westward and eastward instead of easterly and westerly in our manuscript. The sentence is revised as “In the upper stratosphere and mesosphere, the zonal mean zonal wind is easterly in the summer hemisphere and westerly in the winter hemisphere.”

line 261: I suggest to start the sentence not with "And" but "Further, " for example.

Reply: “And” is replaced by “Besides” in the revision.

line 271: If you want to indicate vectors with an over-arrow as you do later, I suggest to do this here, too. Also, because you later introduce another flux, I would add here the index "EP".

Reply: Added in the revision.

line 288: "expansive" –> "extended"

Reply: Changed in the revision.

line 294: "by" –> "at"

Reply: Changed in the revision.

line 273: The sentence "In the northern... region" confused me. I see in your plots that the mesospheric winter easterlies (!) reversed, resulting in weak (!) instabilities in this region. Please, reconsider the text.

Reply: We think the reviewer means line 373 here. The instabilities are indeed not

strong. The sentence is revised to be “In the northern hemisphere of case 3, the eastward wind in the upper stratosphere and lower mesosphere reverses in the polar region (Figure 11c), resulting in weak instabilities in this region.”

line 409: You write of "strong" SPW1 energy, while I see in fig. 13a in 0 - 30 âŮęN at 60-80 km only moderate SPW1 fluxes in comparison to the stronger fluxes at about 60âŮęN. Please, clarify.

Reply: We mean that SPW1 energy is still much stronger in 0-30âŮęN at 60-80 km than that in the southern (summer) hemisphere. The word “strong” is removed in the revision to avoid confusions.

line 411: "advective" –> "advection", as done in the figure captions - see b) above.

Reply: Changed in the revision.

line 415: Add an arrow over the "V" after the "nabla".

Reply: Added in the revision.

line 422: could be deleted because not used.

Reply: Deleted in the revision.

line 423: Delete arrow because it is a vector component only.

Reply: Deleted in the revision.

line 426 - 434: In order to save place for additional discussion, I suggest to delete the text "By adopting... waves." This is for my taste only technical information which does not need to be explained.

Reply: Deleted in the revision.

line 445: You write the amplitude may be "too large" - please, explain why? What did you take for reference?

Reply: We mean the nonlinear advection at lower boundary is much larger than the peak in the mesosphere. The large nonlinear advection value at the lower boundary is due to the large wave sources forced there to compensate for the unrealistic wave decay usually found near the model lower boundary. This is pointed out in the text. WACCM, which simulates the atmosphere from the ground, will be used in the future to avoid the lower boundary effect.

line 446: Please, add the corresponding kilometers, which is the unit of the vertical axes.

Reply: We changed ∼10 hPa with ∼30-45 km in the revision.

line 450: Although this peak in Fig. 13c is not one-to-one at the same position as the one in Fig. 11d, I follow your argument.

Reply: Thanks.

line 466: This is right, and this is what I mean with my specific comment 1). Only the present simulations do not show this instability because the SSW is too weak. Please, mention this because it is important when discussing observations.

Reply: It is mentioned in the conclusion part in the revision. In the future, WACCM will be utilized to further investigate the influence of SSW on QTDW under more realistic atmospheric conditions.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2015-982/acp-2015-982-AC1-supplement.pdf

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