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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A comparison of the momentum budget in reanalysis datasets during sudden stratospheric warming events" *by* Patrick Martineau et al.

Anonymous Referee #3

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General Comments: This paper examines and compares the momentum budget during sudden stratospheric warming (SSW) events using eight reanalysis data sets. Their results provide some insights into the uncertainties of the budget equation during SSWs, especially the contributions of the QG and non-QG terms, the spread or the discrepancies in terms of the regions and the periods. It is also very useful to know that the spread is much reduced in the latest reanalysis products.

The authors suggested that the largest discrepancy originated mainly from the Coriolis torque (in abstract, line 13, page 7 and section 5). Momentum flux convergence is mentioned as the second term which presents non-negligible spread. I am concerned

the word "originated". This gives an impression that if we fix fv, we would get SSW right. However, the origin of the uncertainties must be in the wave forcing rather in the zonal mean meridional velocity, given the meridional circulation is driven primarily by wave forcing.

Also, what their results actually suggest is that the largest discrepancy is associated with the residual term R, the last term in equation (1.1). This can be seen clearly in their figures 5-7. The standard deviation associated with R is slightly smaller but comparable in magnitude to that of fv. However, the mean state of fv is one magnitude larger than R. Thus, R rather than fv has the largest discrepancies. I suggest that the authors make this point clearer by simply stating that the resolved part of the discrepancies is mainly associated with fv.

I am concerned with their definition of high-agreement and low-agreement SSW events. Those events were defined by the standard deviation of the Coriolis torque averaged from 45-85N. fv is not even an effective measure for SSW events. There are times (within $\sim$5-15 day average window) when zonal mean fv is large but there is no SSW. I do not think that it is appropriate to define the strength of a SSW event (i.e the strongest or weakest) or the associated discrepancies just by using fv. Again, this is because both SSW and the changes in fv and their associated uncertainties are consequences of wave mean-flow interaction.

Other than the above points, the paper is well written in general. I suggest publication with some effort to improve the clarity of the expressions. More specific comments are provided below.

Specific comments:

1) Line 15, page 1: "the onset of SSW events, a period characterized by unusually large fluxes of planetary-scale waves from the troposphere to the stratosphere". This sentence holds true only if the period is $\sim$40 days (Polvani and Waugh 2004). The correction between the wave fluxes (or v'T') would become much reduced if the averaging

period is only 5-15 days, which is used in this study (i.e. figures 7-8 and figures 11-12). At these shorter time scales, stratospheric internal variation becomes important. This is precisely why the models cannot predict the timing or the initialization of SSWs. The authors must be careful when they discuss their results and when they related to the EP flux divergence to those from the troposphere.

2) Line 20, page 1: "The strongest SSWs being subject to larger discrepancies among reanalyses". This sentence gives one impression that there is an accepted definition of "the strongest SSWs". Naturally, the readers would think that these events produced the warmest temperature or strongest easterly winds. Is this true?

3) Line 15-16, page 3. It is better to state that the previous assessment was mainly for the extratropics. In the tropics where the QBO becomes important, higher vertical and horizontal resolution should lead to much improved dynamical consistency.

4) Line 21-22, page 4. The last term R also accounts for non-conservative processes, such as Rossby wave breaking (RWB). During SSW, planetary-scale RWB can play an important role. Interestingly, the largest error is associated with R rather than fv term.

5) Lines 12-16, page 7. Now I understand that the definition is based on the largest discrepancies in the Coriolis torque. This needs to be made clearer in the abstract when you mentioned the strongest SSWs because there is no such a definition in terms of the known or accepted description of the SSWs. Also, see my general comments for further concerns.

6) Line 2, page 8. "The evolution of geopotential height contours". Please include the values here (not just in the figure caption) and justify why those values are used to describe the polar vortex. Ertel Potential vorticity should be a much better quantity for this purpose and why not to use EPV?

7) Figures 3-4. It is really hard to qualify the spread or discrepancies based on the color bar used.

8) Line 7, page 15. "Terms that are left of the QG from of the momentum equation provide much smaller forcing for zonal wind tendency during SSW events ... Their differences from one reanalysis ". I disagree for the following reasons. 1). The two QBO terms are of the opposite sign in general (see figure 7). If they are added together, the sum would have a comparable magnitude when it is compared with the other terms. 2). It is well-known that the SSW events often involve breaking of finite amplitude waves. Such an effect cannot be accounted for by 2.5 resolution pressure level data. Please reword the part to avoid the possibility of misleading the readers. See my general comments for further information.

9) I am not sure whether or not figures 8 and 9 is needed. Would it be more concise or informative if the figures were combined as one and show the two groups: the latest versus older generation reanalysis products?

10) Lines 18-28, Page 22. I suggest that the authors to check would the same spread or results be obtained using the residual term R and its standard deviation to define HASSWs and LASSWs. Same applies to figures 11 and 12.

11) Line 14, page 26. See general comments. The results do not suggest that the discrepancies in those non-QG terms are smaller than the QG-terms.

12) Line 23, page 26. "Most of the residual in the stratosphere is correlated to uncertainties in the Coriolis torque". This is very interesting and somehow expected. My explanation is as follows. In the upper stratosphere, gravity wave breaking and finite amplitude wave activities appear regularly there but their propagation cannot be well captured by 2.5 degree pressure level data. Their effects on the polar vortex or zonal mean zonal wind would be included in R or the vertical momentum flux term especially when the QG-terms are calculated by using variables such as u and v, as it is done by this study. On the other hand, when the EP flux divergence is included as in the transformed Eulerian mean equations, the variation of wave forcing would be better resolved by the data used. This is because Del F accounts for the vertically propagating

wave not just the meridionally propagating waves. This is confirmed by figure 11. The figures shows, at 3 hPa, the temporal evolution of the zonal mean wind tendency follows better with the EP flux divergence, less so in terms of fv. Thus, I would think that it is the uncertainties associated with non-resolved wave forcing caused the spread in fv, rather than the other way around.

Minor comments: 1) Line 29, page 1. Too many citations here for motivation. 2) Line 5, page 2. Two daughter vortices -> two vortices. 3) Line 6, page 2. Please be more specific about the differences. Otherwise, delete the sentence as it adds no information. 4) Line 9, page 2. “the general signature”. What is it? Please be more specific. 5) Line 25, ., -> , 6) Line 16, high stratosphere -> upper stratosphere.

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