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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of the tropospheric flows to a major Southern Hemisphere stratosphericwarming event using NCEP/NCAR Reanalysis data with a PSU/NCAR nudging MM5V3 model" *by* K. Wang

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Before I begin my review, it is important to mention that I was a previous reviewer on this manuscript when it was submitted in what appears to be identical form to another journal. I submitted a detailed review to the journal and suggested ways in which both the methodology and manuscript could be improved. Since there has been no change between the current manuscript and the one I reviewed previously my concerns about the manuscript still stand. The study has several major methodological flaws and the figures provided in my copy were almost illegible. Additionally in this version there

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appears to be some problems with the submitted PDF as it caused two different PDF readers to crash when attempting to print.

I repeat my suggestions here as they appeared in the previous review. I disclose here, as I also did in my previous review, that I am one of the lead authors of a paper which provides much of the motivation for the present study (Charlton et al., 2005) and that I would be happy to talk to the author about the suggestions I make and about the comments made in our own previous work. Unless these concerns are addressed, I cannot recommend final publication in ACPD.

The comments below are reproduced from a previous review, but with line numbers revised for the current manuscript.

Major Comments

- **Motivation**

  In the introduction, the author describes their motivation for the study. In particular they observe that (p7955 l28):

  "Charlton et al. [2005] concluded that because the variance in geopotential height at 60S occured in the troposphere and stratosphere simultaneously in the spring of 2002, the forcing did not necessarily come from below."

  This is a fundamental misunderstanding of our statements in the 2005 paper. It leads the author to pose the following question (p7956 l11):

  "Were tropospheric flows critical to the occurrence of the dramatic 2002 SH stratospheric warming?"

  I don't think that we would dispute the idea that the troposphere played a large role in the 2002 SH event. What we actually said in our 2005 study was:

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“(p599) It is therefore possible that at certain critical times, specifically during the splitting of the vortex, that the troposphere and stratosphere act dynamically as a single system; the tropospheric master/ stratospheric slave model of their interactions breaks down. In these circumstances, the exceptional peak in the EP flux at 100 hPa in Fig. 9 is, at least in part, a symptom of the exceptional stratospheric warming above rather than indicative of the cause. This notion does not contradict the importance of the role of asymmetries in the underlying topography as an essential prerequisite for the observed, large-scale variability of the troposphere/stratosphere system and therefore for the occurrence of stratospheric warmings in the system.”

This is far from asserting that the troposphere is unimportant in this event, it merely tries to point out that a large region of the atmosphere (between 300 and 1hPa) is varying apparently in concert for large parts of the evolution. We also suggest that this may indicate that the evolution of the stratospheric flow would be sensitive to **both**conditions in the troposphere and stratosphere during this time.

I feel very strongly that the manuscript in its current form misrepresents our paper and current thought on stratosphere-troposphere links in general to say that there is a debate about if the troposphere was or was not important in the splitting process. We explicitly hypothesise that the splitting of the vortex may be related to local cyclogenesis in the Drake Passage noting that (p598):

“According to this provisional hypothesis, the split was driven from below when an event in the lower atmosphere met with a favourable asymmetric stratospheric circulation.”

- **Suitability of model for experiments**
  I have serious concerns that the model used is suitable for the experiments described in the manuscript. The model in question, MM5V3 ,is used in a configuration which has a model top at 10hPa. This precludes any dynamics in the upper stratosphere which may have been key in producing the major warming. In particular, downward propagation of zero-wind lines from the upper stratosphere is not possible. The author is aware of these limitations, but does not provide a satisfactory explanation for the use of a model with

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this low top. While Simmons et al. (2005) do note that their forecasts with a model with a top at 10hPa were 'reasonably skillful' they also do not show any of the results from this model in their study. Given that much is made later of the precise, two dimensional structure of the polar vortex at 30hPa the model must simulate this region accurately. It is not simply the case that an 'accurate simulation of dynamical processes compared to those delivered using a higher model top' is required. The author needs to prove that the model is capable of simulating the dynamics of the major warming in sufficient detail to be useful for the experiments. This would involve examining and comparing the details of the flow at 10hPa and below in the zonal mean and wave components with suitable representations of the real event from, for example, reanalysis data.

- **Suitability of methodology**
  My largest concerns with the paper lie in its use of nudging techniques to prove that the troposphere plays an important role in the split of the polar vortex. Firstly, there is little explanation of the nudging techniques used, how they work and what the resulting impact is on the tropospheric flow compared to an un-nudged simulation. Is the flow contrained at all gridpoints or just at gridpoints near the lateral boundary? More explanation is required here, given that the reference used is un-published.

  Secondly, the two main sections of the paper deal with cases where the troposphere is strongly constrained to be in a particular state and then the effect of this state on the stratosphere is examined. Many of the runs look at cases where the flow is constrained below 50hPa and the analysis of stratospheric state is performed at 30hPa. According to the level boundaries listed on page 6 this would mean that there are 2 model levels, and 20hPa between the constrained and analysed regions. It is hardly surprising that the conditions at 50hPa have a strong influence on the flow at 30hPa, in fact it would be a concern for the model if this were not the case. The results from section 3.1 would seem to suggest, in contradiction of the conclusions, that the initial state at 30hPa has a relatively large impact on the 30hPa forecast, given that the flow is constrained below 50hPa. A similar result is shown in section 3.2 when different stratospheric initial conditions

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are shown to have some impact on the resulting stratospheric forecast despite the strong constrint of conditions below 50hPa to be like those of 2002.

Thirdly, if the author seeks to investigate the impact of the tropospheric state on the stratosphere then they must keep the stratosphere (likely to be between 300hPa and the model top in the polar regions) identical between different experiments. This is extremely difficult in an evolving situation like that of the SH 2002 major warming when the position of the tropopause is extremely variable. However, a good starting point might be to compare runs constrained below a surface known to reside in the troposphere throughout the model run at all latitudes, for example 300hPa. It is not appropriate to say that the region between 300hPa and 50hPa, which contains large portions of the stratospheric polar vortex is ’tropospheric’.

- **Suitability of diagnostics**
  The diagnostic techniques used in the study are very limited and the graphics produced are of extremeley poor quality for a publication of this type. Little can be gained by simply comparing plots of temperature at 30hPa between the three runs. A fuller set of diagnostic which looked at the zonal mean state, along with more dynamically relevant quantities such as potential vorticity would be appropriate. Additionally, taking differences between the two integrations would assist the reader in understanding what is changing between the runs and examining the flow at levels other than 30hPa would lend confidence that the results show a robust change to the stratospheric circulation.

  The graphics in my copy of the paper are extremely poor, contain lots of unnecessary non-data ink and are too small to make detailed comparisons between the different sets of runs. The wind vectors are impossible to read and interpret.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 7953, 2008.