

## ***Interactive comment on “Antarctic stratospheric warming since 1979” by Y. Hu and Q. Fu***

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Replies to Reviewer #1's reviews on "Antarctic stratospheric warming since 1979" by Hu and Fu

We thank the reviewer for these instructive comments, which are very helpful to improve our paper.

Our original manuscript was mainly to show observational results of the warming trends in Southern-Hemisphere high latitudes. Simulation results were only briefly reported. After reading the reviewer's comments, we think that it is important and necessary to add more simulation results. Therefore, three more figures of simulations are added. According to the reviewer's comments, some figures of observational trends in both temperatures and EP fluxes are deleted. New figures are added. Overall, substantial changes are made in this revised version, accordingly. Specific replies to the comments are as follows.

Replies to general comments: "In the title the word "Antarctic" is appearing which is misleading. As shown in the first five figures, the maximum warming trend is not over Antarctica but rather situated over the Southern Ocean. This must be changed in the title. Since the warming is not inside the polar vortex, the argument in the Discussion and Conclusions section and the Abstract about the impact on ozone recovery must also be rewritten, as it is unlikely that temperatures at this location affects the area covered with temperatures below 198 K (the temperature below which PSC I type clouds are formed)."

Agree. The title is now changed to "Stratospheric warming in Southern-Hemisphere high latitudes since 1979". Changes are also made in the text body. In the section of discussion and conclusions, we made changes in warming effect on PSC formation, that is, "As SST warming continues as a consequence of increasing greenhouse gases, stratospheric warming in SH high latitudes will also continue. Although the warming area is not right over the polar cap, the warming may reduce PSC formation in part of the Antarctic polar region, which slows down heterogeneous chemical reaction rates. Consequently, the warming would cause reduction of the severity and duration of the Antarctic ozone hole." The wave-driven dynamical warming does have effects on the polar region. The reason why strong warming is not seen in the polar cap is because it is largely canceled out by cooling due to ozone depletion. In a very recent paper by Lin, Fu (the second author of the present paper), Solomon, and Wallace (2009, which was accepted by J. Climate and can be found at <http://www.atmos.washington.edu/~qfu/publications.php>), they separated temperature trends into the cooling due to ozone depletion and warming due to increasing wave activity. They found that the two opposite trends canceled each other, especially in October.

"More than 50% of the figures (fig. 1 to 5) detail the warming pattern over the polar stratosphere, which are plotted all in the same style (yet for different datasets, time, and height). To me, this seems not appropriate given that only a small portion of the results

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section in the text is describing these plots. This also reflects that some of the first five figures do not add much of required information I agree that it is important to look at the trends without the extreme event of 2002. However, in my opinion this could be exclusively mentioned in the text and figure 2 and figure 4 could therefore be omitted. Also, figure 3 could be integrated into figure 5 which shows the altitude dependence. I would leave figure 1 as is (MSU temperature trends), but make an enhanced figure 2, which deals with trends within NCEP/NCAR reanalysis. Besides temperature I think it is important to further show geopotential height (GPH) trends (The authors mention at several times the interaction with the polar vortex.). So reanalysis trends in figure 2 could be presented as a matrix: with columns for temperature and GPH trends during September and October, and rows for different altitude levels from 20 hPa to 100 hPa."

Agree, and changes are made. Previous Figures 3-5 are replaced with the current Figures 3-4. Now, Figure 3 shows temperature trends at stratospheric levels in September (top) and October (bottom). Figure 4 shows trends in geopotential heights in the two months. We feel it is better to keep Figure 2 for comparison with Figure1, which is more convincing to address that the decadal warming trend are not caused due to the 2002 sudden warming.

"Currently, trends are presented as absolute values over the specific time periods, which however have not the same number of years in the different plots (e.g. figure 1, 2, 9). Trends should have the same units to make them comparable. I would suggest either K/year or K/decade."

First, we agree that it is good to use the same period of trends for comparison. Second, we feel that it is better to show their absolute values of trends in observations and simulations, which show the entire changes of temperatures. Third, in the final section, we point out that the observed maximum warming trend is about 0.29 K/decade, and the simulated warming is about 0.15 K/decade.

"I found it rather strange to plot t-test values of 1.7 and 2.5 as an illustration of the

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significance. This is clearly not the state-of-the-art method. I suggest that you overplot the 95% significance-level (p-value)."

In all time series plots, we use student's t-test values. To be consistent, we feel that it is better to plot contours of t-test values in map figures, rather than p-values. In the section of data and model setup, we point out that student's t-test values 1.7 and 2.5 correspond to 90% and 98% significance levels, respectively.

"Trends are calculated linearly, hence neglecting other possible driving factors, such as solar variability, stratospheric aerosols, El Niño/Southern Oscillation variability, and quasi-biennial oscillation. It is well possible that the observed trends can to some degree explained by these forcings. To account for that a multiple linear regression model should be applied, as done in most of previous studies about stratospheric temperature trends."

It is certainly important to study the importance of these factors pointed out by the reviewer. However, it is difficult to include all these in one paper. We feel that it would be better to focus on linear trends in observed temperatures and the influence of one of these factors, i.e., SST warming. The influences of natural variabilities and other possible forcings are briefly discussed in the fifth paragraph of the section of discussion and conclusions.

4. "By reading the paper it was not obvious to me what was already done in the field before. I think you should better separate between introduction where you present the work that has been done by others and your contribution."

We agree that we did not make it clear that what has been done and what is the theme of the present paper. In the revised version, the second paragraph of the introduction section is modified. We point out that because the warming trends were largely reduced by conventional zonal and seasonal averages in previous studies, and these authors had key interests in other things, little attention was paid to the warming trends. We also point out that "One of our goals in this paper is to demonstrate

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that stratospheric warming trends in SH high latitudes have equal or greater magnitudes compared to cooling trends in austral spring, by showing the spatial pattern of temperature trends in individual months."

5. "Figure 6: Can you change the y-axis to pressure-scale to make it comparable to previous plots? Also, the EP flux diagnostics should be shown much further upward than 30 km as the upper stratospheric levels contribute to the polar vortex disturbance as well."

Agree. Y-axis is now marked with log-pressure scales. We now plot eddy-heat flux time series at 30 hPa as the representative. We also point out that eddy-heat fluxes at other stratospheric levels show similar results.

"I am not completely convinced that the shown EP flux convergence is really enhancing the residual meridional circulation (RMC). I would add a plot of the streamfunction to this figure. The time-series on the right is interesting. However, it should be shown not only for the area of maximum warming but to the whole temperature field as presented in the first figures."

The second author, Qiang Fu, has a paper submitted to J. Climate just a few days ago, in which how RMC is enhanced by EP flux convergence in the past few decades is the key interest. We feel that it is good not to repeat his results here. Using EOF analysis, Lin et al. (2009) found that the cooling trends are due to ozone depletion, and that warming trends are associated with increasing wave fluxes. Both have effects on temperature trends in the polar region. It suggests that the correlation would be lower if temperatures outside the warming area are included. Therefore, we only consider temperatures within the warming area in calculating the correlation.

6. "Figure 8: To me figure 8b does not add much information. It is somehow obvious that two variables with significant trends in the same direction have high correlation coefficients and vice versa. Instead it could be interesting to see how previous months of SSTs correlate with the warming pattern in the Southern high latitudes (lagged cor-

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relation)."

Figure 8b is deleted. Lagged correlations were calculated. They show similar spatial pattern. Prof. Wallace suggested us only showing the simultaneous correlation.

7. "Figure 9: The authors mention in the paper that the variability of the warming pattern shows a high spatial and temporal variability. In this sense, I think it is rather inappropriate to show the ensemble mean of a model simulation. The inter-ensemble variability must be considered here."

We agree that detailed analysis for simulation results is necessary. However, we feel that it would make this paper too long and would cause the paper's key point shifted if much detailed simulation analysis is included. We are actually working on detailed simulation analysis, including what levels such a wave-driven dynamical warming can reach. It is found that warming can extend to the stratopause in August, but stops at upper and middle stratosphere in September and October, respectively. Our on-going analyses are not only for GISS-modelE simulations but also for simulations using Max Planck Institute GCM.

8. "I have not understood why the model simulations are only presented with respect to temperature trends. It is important to extend this section by showing the same diagnostics as above (RMC, EP flux divergence, correlation temperature and SST) and add a detailed section about the performance against reanalysis data."

Agree. Three more figures are added to according to the suggestions. They are: EP flux vector trends (including EP flux divergence trends), time series of eddy-heat fluxes, and correlation between temperatures and eddy-heat fluxes. Correlation of stratospheric temperatures and SST is not added because the simulations are forced by SST alone, and the ensemble-mean stratospheric temperatures would have close correlations with SSTs.

Replies to minor comments: P1704 L18: change "the severe ozone depletion" to "se-

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vere ozone depletion". Changed.

L24: add "Langematz et al., JGR, 2003" behind "Shindell and Schmidt, 2004". The reference is added.

P1705 L4-7: It is strange that in the introduction already parts of the results are presented. From a reader's point of view at that stage it is not clear whether this is a result of the paper or stems from another study. I would take these statements out. Agree. This paragraph is modified, and the statement is deleted.

L9: Please make sure that the reference given (Andrews et al. 1987) is the appropriate at this place. I actually doubt that they were the first to discover this issue. The sentence is now changed to "It is well known that polar stratospheric temperatures are determined by radiative and dynamical processes (Andrews, et al., 1987). We refer this book is because it better addresses the problem of how polar temperature is controlled by both radiative and dynamical processes. The whole paragraph is also modified.

L12: please explain the mechanism for the connection between stratospheric warming and wave activity. Agree. The mechanism is qualitatively explained in the third paragraph of the introduction section.

L12ff: please explain what else (apart from greenhouse gases and ozone depletion) could contribute to stratospheric warming trends in general. Thanks for pointing out this. We add the sentences "Stratospheric aerosols, especially volcanic aerosols, absorb solar radiation and warm the stratosphere. However, they do not cause a decadal warming trend and the spatial pattern as shown below."

L16: add reference to 2002 event. Agree. References of Varotsos (2004) and Krüger et al. (2005) are added. We also point out that the articles in the same volume as the second references can be referred.

L16: delete "," Changed.

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L21: add "the" before "SH stratosphere" Added.

L24: change "and" to "as well as" Changed.

L24: change "GCM" to "model setup" Changed.

P1706 L4: "anomalies" from what? We add a sentence ", which are departures from the mean over the base period of 1979-1998" after "anomalies".

L8: change "one" to "dataset" Changed.

L10: Reference to NCEP/NCAR reanalysis: add "Kistler et al., 2001" behind "(Kalnay et al., 1996)" Added.

L10: give a reference for the calculation of EP fluxes. Andrews et al. (1987) is added.

L11: change "Student" to "Student's". Changed.

L13: change "confidence" to "significance". Changed.

L15ff: can you briefly say for what the model has been used in the past. What are its strength and where does it show weaknesses? How and where was it validated? A sentence is added, that is, "A full description of the model, its performance and comparison with observational results can be found in Schmidt et al. (2006)." To be frank, we do not know the details of its weakness and strength in simulating the stratosphere.

L18: change "high" to "height". To make it clearer, we change it to "above the ground".

L18: add "the" before "stratosphere". Added.

L21: what was done with other forcings? E.g. Solar variability, tropospheric aerosols, stratospheric aerosols, QBO? The following sentences are added. "To isolate the impact of SST warming on the SH stratosphere, ozone, greenhouse gas, and aerosol concentrations are all fixed at the 1950 level. Solar variability is not considered. Like most other GCMs, GISS-modelE is unable to produce the quasi-biennial oscillation in the tropical stratosphere."

L21: how were the initial conditions been derived? The model has configuration files of initial conditions at 1950. The initial conditions are derived from various sources. It was described in earlier versions of the GISS GCM. It is difficult for us to list all the sources.

L18: add "up" before "to 0.02 hPa". Added.

L26: change "less significant" to "not significant". Changed.

\*\*\* P1707 \*\*\* L3: change "less significant" to "not significant". Changed.

L5: change "spatially" to "spatial". Changed.

L8-9: from where do you know that "the spatial pattern in October resembles minor sudden warmings in the Arctic stratosphere?" Give reference. Reference Kruger et al. (2005) is added. This paper showed figures of minor warmings in the Arctic stratosphere.

L8ff: Maybe state that in October and September cooling trends and warming trends are of similar magnitude. Thanks for pointing out this. It is added.

L17: please clarify what you mean with "ending year". Note that testing if a robust trend is occurring one would need to apply more sophisticated methods (e.g. jackknife method), which however is beyond the scope at this place here. This sentence is removed.

\*\*\* P1708 \*\*\* L1-7: this should be added to the introduction section. Agree. It is moved to Introduction.

L11: change "with" by "to". Changed.

L18: add point after cooling; Begin new sentence with "Ozone depletion during...". Agree. They are changed.

\*\*\* P1709 \*\*\* L6: add "to" after "contribute". Added.

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L12-13: from a reader's point of view: can you explain how you separate between travelling and quasi-stationary waves? This could also be added to the methods section. A major correction is made here. After plotting time series of eddy-heat fluxes at stratospheric levels, we found that the increase in traveling wave fluxes is much weaker than that of stationary waves although both kinds of waves have similar magnitudes of eddy-heat fluxes. Therefore, the figure is removed, and relevant discussion is also removed. As mentioned above, the eastward shifting of warming trends between September and October are not mainly because of increasing traveling wave activity, but due to the shifting of the climatological-mean center of high temperatures. The separation between traveling and stationary waves are according to the method by Randel (1988). Eddy-heat fluxes derived from a 30-day running averaged wind and temperature fields represent quasi-stationary wave fluxes, and the residual between the original values and that from the 30-day running averages is the traveling part.

\*\*\* P1710 \*\*\* L4: change "showed" to "shown". Changed.

L20-22: no evidence or reference is given for the proposed shifting of warming pattern to the East. The eastward shifting can be seen from Figures 1 and 2.

L24: add ", as for instance shown in modeling (Fischer et al., GRL 2008) and observational (Fusco and Salby, Journal of Climate, 1999) studies." after "..from low to high latitudes". Agree, and both references are added.

L25: change "blocks" to "block". Changed.

L25: replace "into the polar vortex" by "across the polar vortex edge". Changed.

\*\*\* P1713 \*\*\* L20: change "is due to SST warming" to "is connected to SST warming" (there is no evidence. What would be needed to prove it, are idealized model experiments where the ocean's surface temperature would be fixed at a climatological state). Maybe the authors should mention this point in the discussion section (as an outlook maybe). First, it is changed to "is connected to SST warming". Sec-

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ond, we point out that detailed analysis of simulations needs to be carried out in future studies.

Fig.1, add "(in a layer between about 20 and 120 hPa, see text)" behind "observations". Added.

Fig. 7, the caption uses the words "transient" and "stationary" whereas the text mentions the words "travelling" and "quasi-stationary". Can you clarify this? can you also add the unit of the y-axis?. As mentioned above, this figure is removed, and relevant discussion is also removed.

Fig. 8, "confidence level" change to "significance level". Changed.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1703, 2009.

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