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Atmospheric Chemistry and Physics Discussions

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9, C8207–C8212, 2009

Interactive Comment

Interactive comment on "On the seasonal dependence of tropical lower-stratospheric temperature trends" *by* Q. Fu et al.

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Reply to the Anonymous Referee #1 by Q. Fu, S. Solomon, and P. Lin

We first thank the referee's valuable comments and suggestions on our manuscript. Here we respond to the referee's major comments. All referee's comments including the specific comments will be answered/addressed in details when we submit a revised version of our paper for publication in ACP.

"Some of the material presented here is taken from Lin et al. (2009) without full acknowledgement. Specifically, half of the panels in Fig 3 are taken directly from Lin et al. (2009) Fig 3, Figure 4 is very similar to Lin et al. Fig 9 (one less year of ozone data is shown here, and the heat flux index is defined slightly differently), and Figure 5 is al-most identical to Lin et al. Figure 10. Since this material has already been presented in an accepted paper it does not need to be reproduced here. Although Lin et al. (2009) is referenced in this manuscript, and the focus here is on the tropics in contrast to the Southern Hemisphere focus of Lin et al., the authors need to more carefully differentiate their results from those of Lin et al, and state more clearly in the introduction how they develop the results of Lin et al."

Some of the material from Lin, Fu, Solomon, and Wallace (2009) was presented for the completeness of the paper so that readers can easily follow it. In the revised version, we will make the relevant part more brief, and more explicitly acknowledge Lin et al. (2009) so that it is clearer to readers what is new in this paper.

"Although the approach taken is very similar to Lin et al. (2009), there are some slight differences in the definitions – e.g. Lin et al. define the eddy heat flux index from 45-90S, whereas 40-90S is used here. Lin et al. (2009) define the eddy heat flux index as a mean of the values from three months (the month considered, and the two previous), whereas only two months are used here. I think it would be less confusing to readers if the definitions used in Lin et al. (2009) were used here, in order to avoid giving the impression that these have been chosen to give the highest correlations over the observed period."

We will clarify in the revised version that our results are insensitive to the eddy heat flux averaged either over 50-80S(N), 45-90S(N) or 40-90S(N). We will use the three months mean eddy heat flux following Lin et al. (2009) in our revised version (a longer averaging period is used on the basis that the radiative relaxation timescales in the lower stratosphere are long). Furthermore, we will define the index as the vertically averaged eddy heat flux between 10 and 50 hPa following Ueyama and Wallace (2009) instead of using the eddy heat flux at 150 hPa as in Lin et al. (2009) or in our original version. Note that although the correlation of eddy heat flux among 150 hPa and upper levels are never lower than 0.9, the vertically averaged eddy heat flux between 10 and 50 hPa is a better representation for the wave breaking in the upper stratosphere.

“The authors use multiple definitions of the dynamical contribution to temperature trends: - SH, November-May: Total temperature trend minus ozone-congruent trend (regression on ozone index times ozone trend). - SH, June October: Regression on eddy heat flux times eddy heat flux trend. - NH, total temperature trend + 0.32 K/decade. Although they put forward reasons for using the various definitions, I do not find these wholly convincing. In the Southern Hemisphere, they argue that there is no trend in eddy heat flux in November, a time when some local warming is observed – and there fore that the eddy heat flux data are wrong in November. However, if they are wrong in November, why do the authors trust these data in the other months? (they do not independently verify these data). Similarly in the Northern Hemisphere they argue that the derived eddy heat flux is unreliable because subgrid-scale gravity waves are important there – but no information on the relative importance of gravity waves is given to support this conclusion. Secondly, although they have perfectly good ozone trend data in the Northern Hemisphere, they do not use this to estimate the radiative contribution to the trends there mainly because they say that there is no ozone hole in the NH high latitudes. Their assumpion of a seasonally constant radiative cooling seems particularly unjustified to me, and the authors do not try to justify this assumption. Lower stratospheric cooling is dominated by ozone – but even if ozone trends were seasonally uniform, which they are not, the corresponding radiative temperature changes would not be seasonally uniform, due to seasonal variations in solar insolation”.

To address the referee’s concern, we will use a unified approach to derive the dynamic contribution associated with the change of the Brewer-Dobson circulation (BDC) to the temperature trends. We found that the improved index defined as the three-month mean eddy heat flux averaged between 50 and 10 hPa can be generally used as a measure of the BDC including SH November and NH. Without assuming a constant radiative trend in the NH, the derived radiative (dynamic) trend in the tropics is slightly smaller (larger). As a sensitivity test, we will also show that the results in the original version are reproduced by using the improved dynamic index but assuming a constant radiative trend in the NH. This indicates that the SH results presented in the original version is robust as we claimed but the assumption of the constant NH radiative trends do have some effects on the results.

“In summary, I would find these results much more convincing if the authors used a consistent definition of the dynamically-induced temperature trends in both polar regions and through all months of the year.”

This will be done in the revised version by defining the dynamically-induced temperature trends consistently as the regression on eddy heat flux times eddy heat flux trend in both polar regions and through all months of the year.

“I think the reanalysis trends in eddy heat flux index are likely to be unreliable in both polar regions and all seasons, and I don’t think the assumption of constant radiative cooling in the NH is justified. Therefore I would suggest defining the dynamical temperature trend as the total trend minus the ozone_congruent part throughout (admittedly this still doesn’t allow for the seasonal cycle in solar insolation, but I think this is the most defensible of their three definitions). If the main results are not robust to such a consistent definition, then I think this would call into question the conclusions drawn.”

The trends by using the improved eddy heat flux index as defined above are quite reliable in both polar regions although they might not be perfect (e.g., in Feb as will be discussed in details in the revised version). Also see the figure below showing the total T4 trends in 12 months over the tropics versus the total T4 trends in high latitudes (without separating the dynamic trends from the total). The open circles are for October, November, and December when there is a large seasonal dependence of the radiative cooling due to the large ozone-depletion-induced radiative cooling in the Antarctica. Without considering the months of October, November, and December, the total trends in the tropics are anti-correlated with the total trends in the high latitudes with a correlation coefficient of -0.97, which enforces the robust relationship identified in our paper as well as the relatively small seasonal dependence of the radiation part in the NH and other months of the SH. This figure and related discussion will be added to the paper.

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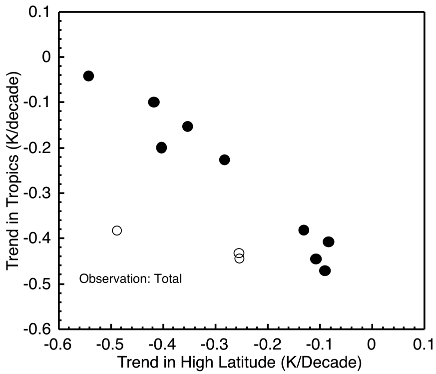

Observed total MSU T_4 Trends in tropics (20^{o}N-20^{o}S) versus those in high latitudes (40^{o}N-82.5^{o}N and 40^{o}S-82.5^{o}S) for 12 months of the year for 1979-2007. The open circles are for October, November, and December when there is large ozone-depletion-induced radiative cooling in the Antarctica.

Fig. 1.

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Interactive Discussion

Discussion Paper