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Atmospheric Chemistry and Physics Discussions

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

**Anonymous Referee #1**

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This study shows that the seasonal variation in tropical T4 temperature trends is highly anticorrelated with the seasonal variation in high-latitude temperature trends, and that this anticorrelation is as large as -0.97 if ozone-induced variations in SH high-latitude temperature are first removed. This suggests that these seasonal variations are primarily dynamically-induced.

Overall I think the results presented here are potentially of considerable interest, but there I several issues which I think need to be addressed before the manuscript is publishable in ACP.

Major Comments:

Some of the material presented here is taken from Lin et al. (2009) without full ac-



knowledgement. 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 almost 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.

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.

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 therefore 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.

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. 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.

Specific comments:

Ln 42, Delete 'a' before 'rising'.

Ln 78: T4 also has a contribution from the upper troposphere in the tropics.

Ln 92-97: I think any similarity between T4 trends in the NCEP reanalysis and MSU



in the high latitude stratosphere of the Southern Hemisphere does not necessarily indicate that the eddy heat flux trends are reliable. MSU radiances are assimilated into the NCEP reanalysis, so the two datasets are not independent. Eddy heat flux is not a directly assimilated quantitiy and is much more likely to be sensitive to model biases, and in particular trends in eddy heat flux are likely to be unreliable.

Ln 101 and 102: 'ensembles' should be 'ensemble-members'

Ln 128, 129: I don't agree with this – strengthening of the BDC could be identified using variables other than T4.

Ln 134: A large warming can't be explained by a cancellation of the dynamical warming and radiative cooling – the dynamical warming must be larger.

Ln 167: Replace 'herein' with 'here'.

Ln 168: Delete 'the' before '150 hPa'.

Ln 189: Replace 'linearly correlated' with 'associated' (correlation tells you nothing about the relative magnitudes of two quantities). Replace 'anomaly' with 'anomalies'.

Ln 212-214: If you retain the heat-flux-based regression in the paper, you should show this comparison between results derived using the two methods.

Ln 241: Replace 'Mach' with 'March'.

Ln 247-249: No justification is given here – the authors give the impression that they alredy know what the dyanical warming trend should be before doing the analysis.

Ln 308-309: 'GCMs... suggest that the BDC is intensifying'. Replace with 'GCMs.... suggest that the BDC is expected intensify in response to increasing greenhouse gas concentrations', or something similar.

Ln 313-135: The observations only contrast with published model results if the trends are inconsistent over the observed period allowing for internal variability. There is a lot of internal variability in dynamical activity in the Northern Hemisphere stratosphere, so I suspect that this difference is not significant.

Ln 318-321: The authors argue that the CMIP3 models have limited stratospheric resolution and therefore don't capture the change in the BDC – therefore their simulated stratospheric temperature changes are radiatively-induced. The authors presumably trust the NCEP renalaysis to reproduce stratospheric climate change, since they use the stratospheric eddy heat flux trends from this model, but the NCEP renalaysis model has an upper boundary at 3hPa. 11 of 21 CMIP3 models have a higher upper boundary. An alternative hypothesis to explain the difference between the tropical stratospheric temperature trends derived here and the CMIP3 ensemble mean stratospheric temperature trends is that most or all of the dynamically-induced variations are associated with internal variability. The authors could easily test this hypothesis by plotting the 5-95% range of T4 trends in each month in the CMIP3 runs with ozone depletion.

Ln 321-325: Again the authors should plot the range of trends simulated in the CMIP3 models before drawing conclusions regarding consistency or inconsistency with observations.

Ln 454-455: 'The area where the trend is significant at the 90% confidence level by a Student's t test is shaded'.

Figure 9: This figure is not needed, since it just reproduces the data in the previous figure.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21819, 2009.