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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "The annual cycle in lower stratospheric temperatures revisited" *by* S. Fueglistaler et al.

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We thank the 3 reviewers for their constructive and helpful reviews. Below, we respond to all issues raised by the reviewers. Two issues raised by the reviewers - statistical uncertainties and filtering of timeseries - are discussed separately at the end of this response.

Response to anonymous reviewer 1:

(1) *Radiative effect of water vapour.* We have calculated the temperature adjustment due to the annual cycle in lower stratospheric water vapour in the same way as that for ozone, and found that the temperature adjustment is about a factor 10 less than for ozone (i.e. order 0.1K, whereas it is order 1K for ozone). In order not to overload the

manuscript, we have decided not to show these results. We have changed the text on page 26825 as follows:

OLD: Second, other radiative effects, e.g. from water vapour, have not been included in our analysis. Their radiative effects are certainly smaller than those of ozone, but may be important for balancing the much smaller residual imbalance in our adjusted temperature.

NEW: Second, other radiative effects, in particular the annual cycle in lower stratospheric water vapour mixing ratios, have not been included in our analysis. Radiative transfer calculations (not shown in this paper) using the fixed dynamical heating assumption (i.e. calculations similar to those shown for ozone) show that the temperature adjustment for the annual cycle in water vapour around 70 hPa is about an order of magnitude smaller than that for ozone. Hence, we emphasize in this paper the role of ozone, but note that the much smaller inbalance between tropics and combined extratropics remaining after taking into account the effect of ozone and the latitudinal structure of static stability may be also due to the radiative impact of the annual cycle of water vapour mixing ratios.

(2) *Sentence on line 247:* It appears that the reviewer is referring to the draft, and not the published version in ACPD - we assume the reviewer is referring here to page 26825, line 22ff of the ACPD manuscript. The corresponding paragraph in the published ACPD version is substantially modified compared to the version the reviewer is referring to, and should clarify the points raised by the reviewer.

(3) *Cause of ozone variations:* In this manuscript, we did not want to elaborate on the processes responsible for the ozone variations, and refer to facts and hypotheses from previously published work only.

(4) *Statistics:* Please see discussion at end of this response.

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Response to anonymous reviewer 2

We note that the reviewer is - as reviewer 1 - referring to the first version submitted to ACPD, and that the version published in ACPD actually already has some issues fixed.

(1) *Line 3:* Already changed in the ACPD version.

(2) *Line 10:* While we fully agree with the reviewer's remarks regarding the findings of the original study of Yulaeva et al. (as written in the General Comments), we maintain that the *near cancellation* of tropical versus combined extratropical temperature variations is an artefact of the MSU-4 weighting function. What remains valid, and is emphasised throught the paper, is the see-saw pattern, and the dynamical cause of the temperature variations. Note that the manuscript version published in ACPD (which incorporated changes suggested by the Editor) already changed the paragraph with 'without any need for a ...'. Also, we have changed 'emphasised by Yulaeva et al.' to 'observed by Yulaeva et al.'

(3) *Line 19:* Done.

(4) *Validity of the simulated MSU-4 data:* The reviewer is correct that the simulation of the MSU-4 data is tricky. We have spent considerable time trying to work out a best approach, but eventually resorted to the approach by Fu and Johanson (2005). For the purpose of this paper, it is indeed sufficient to demonstrate that simulated and observed MSU-4 *climatological mean annual cycle* have the same characteristics, and hence we can use the higher resolution data (ECMWF) to study the role of the vertical averaging in MSU-4 data.

(5) *Line 151:* Already changed in the ACPD version.

(6) *Line 214:* Ditto.

(7) *Line 279, high pass filter:* Please see discussion at end.

(8) *Lines 329-330:* Already changed in ACPD version.

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(9) *Fig.4b, contour labels:* All values at the equator are 0, and the problem arises from using levels where 0 is also a level. For the revised version, we have changed the contour levels, with the range -5% to 5% being white; which solves the problem of the awkward '0'-contouring. Also, the new figure caption's description of the plot is more precise.

(10) *Fig. 5:* The temperature adjustment under the fixed dynamical heating assumption is (as discussed in the manuscript) not only a response to the local (here in the sense of a pressure level at a given latitude) ozone mixing ratio, but depends on the full (vertical) profile of the change. As such, the discrepancy noted by the reviewer is exactly evidence that the Newtonian-cooling approximation is not perfect - as discussed in the manuscript. Note that averaging into two latitude bands (i.e. tropics vs. extratropics) somewhat reduces the problem but, as discussed in the manuscript, the Newtonian cooling approximation is imperfect, and its use here is justified based on the fact that it captures the leading order effect of tracer perturbations, and allows a sensible, straightforward discussion (in terms of a mechanistic understanding) of the processes involved.

(11) *Fig. 6d:* The reviewer is correct, and the revised version has the figure caption corrected.

Response to anonyous reviewer 3

General comment:

As stated above (response to reviewer 2, point (2)), we think it is important to be clear that the near compensation of tropical versus extratropical temperature variations is an artefact of the MSU-4 weighting function, and that the actual slope of the temperature correlation strongly depends on the structure of static stability, and dynamical-chemical-radiative interactions; and that the relation tropics versus combined extratropics really only makes sense from a dynamical point of view from about the tropical tropopause upwards (the lower part of the MSU-4 weighting function samples an essentially tropospheric temperature signal in the tropics).

(1) *P26825/L7-8:* See the detailed response to reviewer 1, point (1). Note that Solomon et al. mainly discuss top of atmosphere radiative fluxes, while the discussion in this paper is on the seasonal evolution of the profile of radiative flux divergence. We agree that a side-by-side comparison of the radiative effects of ozone and water vapour would be desirable for the community to better judge their relative importance, but this is beyond the scope of this paper.

(2) *P26825/L9-11:* We have seen the Hitchcock et al. paper just after having submitted our paper. We agree that it is of relevance to our paper, and have added it to the bibliography. Note, however, that the results of Hitchcock et al. as shown in their Figure 1 cannot be directly compared to our study, as they analyse (if our reading of their paper is correct) instantaneous perturbations (presumably arising from waves with a range of vertical scales). These wave perturbations of the temperature field are likely to be more difficult to describe with a Newonian cooling approach than the comparatively smooth perturbations in tracer concentrations of our manuscript.

(3) *100hPa temperature field:* We have analsed the temperature structure on all levels between 150 hPa and 10 hPa (ECMWF levels are approx. ..., 113hPa, 96hPa, 82 hPa, 67 hPa, ...) and have decided to show only the (approx.) 70hPa level because (i) this level is at about the center of the MSU-4 channel, (ii) the annual (12 month) component of seasonal temperature variability is strongest around this level, and (iii) at lower levels, the signal is a mixture of stratospheric and tropospheric influences that render, e.g., the variability at the 100hPa level and below very complex. We hope to publish our results for temperature variability for levels below the tropical tropopause in a follow-up study.

(4) *Diabatic budget of (re-) analysis data:* The diabatic heat budget of the ERA-Interim

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reanalysis has been discussed in great detail by Fueglistaler et al. (2009, QJRMS), where it is shown that ERA-Interim is better than ERA-40, but that the analysis system acts as a spurious heat source/sink (see their Figure 11 a/e). Note, however, that for the purpose here, we only need temperature. Indeed, the intriguing aspect of the Yulaeva et al. paper is that it suggests that one can infer information about w-bar-star directly from temperature (in other words, as long as a dataset's temperatures are correct, the data's underlying heat budget is irrelevant for the purpose here).

Statistical uncertainties and filtering of data

In the original manuscript, we have presented results of linear regression without error bars, and some regressions are based on frequency filtered data whereby we used a simple running mean, which is known to have large sidelobes.

We've decided to show the data in this manner because all results shown in the paper are robust (also in the sense that the slopes shown are only qualitatively discussed), and we suspected that a discussion of the above issues likely confuses rather than enlightens the readers.

For the revised version of the manuscript, we show the results for the ECMWF ERA-Interim data (Figure 7, and now also 8) with an uncertainty discussion. Given the poor temporal sampling of the data shown in Figure 9 (formerly 8) (the data is annual means), we refrain from an error calculation - as emphasised in the script, these correlations shoud be seen as motivation for follow-up studies, and no strong claims are made whether the slopes from different data sets agree or not in a statistical sense.

Now, for analyses that involve a long chain of processing steps (such as the one here, starting with the input to the ECMWF reanalysis system, the processing of the data in the system, subsequent averaging in time and space, followed by frequency filtering), accurate error propagation is very challenging, and the usual uncertainty measures

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based on the distribution of the points used for the regression is not the whole story. Also, note that we have to use total least squares fits (since both ‘x’ and ‘y’ - i.e. tropical and extratropical temperature - are values with errors), but that the errors associated with these values are unknown. The ‘random’ errors for these temperature values are tiny since the values are averages over very large samples (recall they are averages over one month and half the globe each), and likely underestimates the true uncertainty in the data. The systematic uncertainty is - evidently - very difficult to characterise. We have used ECMWF internal statistics of the deviation between the ‘analysed’ temperature field, and the input data. This data suggests that ERA-Interim temperature biases are small around 70hPa, and quite stable over time. (See also Liu, Fueglistaler and Haynes, J. Geophys. Res., 2010.) We intend to use this data for follow-up studies that focus on trends, but for the focus of this study, these errors are not decisive.

The uncertainty arguably most relevant for our study concerns the frequency filtering: Since we are interested in - and discuss in some detail - the relation between seasonal and interannual temperature variability, one can be concerned about the way how one separates annual from interannual variability in a given dataset. We follow standard practise here (building a mean annual cycle by averaging data for each month), but are fully aware that this practise is somewhat problematic. Specifically, an error in the determination of the mean annual cycle implies a correlated error in the interannual timeseries (because the latter is achieved through subtraction of the former from the original time series).

In order to address this problem of potentially correlated errors, we have performed Monte Carlo-type error calculations: we build a distribution of slopes for the mean annual cycle through a large number of random samples of years to include in the averaging process, and for each mean annual cycle slope, we obtain a corresponding slope for the interannual variability. The mean and 2-standard deviation range of these distributions are shown in the Figure 8 of the revised manuscript, along with the slope that one gets for the specific sample where each year is taken. The Figure shows that

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these ‘errors’ are quite small. Also, the slopes for the interannual correlations have been calculated with 3 different filters: a running mean (‘rectangle’), a triangle, and a Nuttall filter (shown in 3 different colors). The figure shows that the type of filtering has very little impact on the determined slope. Also, the revised figure now shows the correlation coefficient for the interannual timeseries.

Finally, we note that the slope of the interannual variability is frequency dependent - and results vary depending on the width of the bandpass filter. This is, however, not an ‘error’ or uncertainty, but ‘signal’. The paper emphasises this frequency-dependence, and we hope that our paper will serve as motivation also for others to further explore this frequency dependence.

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