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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability and trends in dynamical forcing of tropical lower stratospheric temperatures" *by* S. Fueglistaler et al.

Anonymous Referee #3

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The manuscript by Fueglistaler et al. discusses the contribution of dynamical forcings to variability and trends of tropical lower stratospheric temperatures at 70hPa. The analysis is based on meteorological reanalyses from ERA-Interim, MERRA and NCEP over the period 1980-2011, together with MSU satellite temperature data. Dynamical forcings are calculated (a) from a simple proxy based on eddy heat flux at 70hPa integrated over the extra-tropics of both hemispheres and (b) the momentum balance evaluated at 30N/S, following Randel et al. (2002). It is shown that both methods to calculate the dynamical forcing describe the inter-annual variability of tropical lower stratospheric temperatures well, with high consistency between ERA-Interim and MERRA, but give very different results for the long-term trends. The paper addresses the important topic of attribution of changes in temperatures and upwelling in the tropical

lower stratosphere and makes important contributions to this topic by evaluating and discussing the dynamical forcing. I recommend publication in Atmos. Chem. Phys. after consideration of the following remarks.

General comments (in no particular order)

1. The temperature proxies based on extra-tropical heat-flux and based on the momentum balance have very different long-term trends. Is there any chance to explore in more detail why the trends are so different, including the very different seasonality of the trends? As discussed in the manuscript, the momentum balance is the theoretically more complete description, but may also be more sensitive to errors in the reanalyses. In any case it would be good to provide a bit more background here on the details of the calculations and a discussion of how the two proxies are theoretically related. E.g., is the term u'w' in the momentum balance neglected?

2. Would it be possible to show seasonal trends also for MSU data for comparison with Figs. 2, 5 and 6? It is noted at several places in the text that 70hPa temperatures are strongly constrained by MSU data. On the other hand MSU data have a relatively broad weighting function and may be influenced by upper tropospheric changes.

3. Tropical mean temperatures, integrated heat fluxes and the momentum balance calculations all use different bounding latitudes: temperatures are integrated over 35N/S, heat fluxes starting at 25N/S and the momentum balance at 30N/S. Although it is noted in the text that results are very similar for different latitudes, it would be reassuring if the authors could show that the long-term trends are not affected by different choices of the latitudes.

Specific comments

p.13390, l.7-16: It is reassuring that the slope is close to 1, but I am not fully convinced that the implication is that changes in the equilibrium temperature are small. Is the 70 day radiative relaxation time derived from an independent analysis, or empirically

derived from the fit? In other words: can changes in the equilibrium temperature be hidden by an effective relaxation time? Anyway it would be good to spell out explicitly which "two time series" (l.12) are fitted here.

Section 2.5: It was not fully clear to me what you do to "filter" or remove the QBO influence. My understanding after 2nd reading is, that you average tropical temperatures over 35S to 35N, as this minimizes the influence of the QBO. Is this right, or do you do any other fitting or filtering to remove the QBO signal? I suggest to spell this out more clearly. Anyway there is much overlap and redundancy between Section 2.5 and Appendix A1. I suggest to combine the information and remove Appendix A1.

p.13393, l.28: Again, does the difference in latitudes (30N/S for the momentum balance, 35N/S for the temperatures) play a role here?

p.13394, l.15: Can you expand the discussing of how/why the two calculations give imperfect estimators of the true dynamical forcing? Which terms (processes) are neglected in the momentum balance? What is the theoretical relationship between extratropical heat flux and tropical temperatures?

p.13394, l.28: I suggest to split this sentence in two, as these are two different ideas.

p.13395, l.25: Better say larger contribution to the trends than contribution to the forcing.

p.13395, l.26: "Positive trend": this is slightly confusing. Do you mean a positive trend in the dynamical forcing, that leads to a negative trend in tropical temperatures as seen in Fig. 5?

p.13399, l.24: what is area weighted here? I suppose it is an area weighted integration of the slopes, not an area weighted slope, that is integrated.

p.13400, l.19: I am confused here about the sign of the correlation. What is correlated here, the dynamical forcing and the tropical temperatures (which are generally anti correlated with colder temperatures for larger extra tropical dynamical forcing), or the

temperature with the temperature proxy (which should ideally be positively correlated)? This applies also to other places in the manuscript. Clearly spelling out which time series are considered would help.

Fig. 6: You may include in the caption to panel (a) that the trends are for 1980-2011.

Technical corrections

p.13388, l.20: "a coefficient"

p.13389, l.16: "Te" should be "TE"

p.13392, l.7 "are removed"

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 13381, 2014.