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

### **Anonymous Referee #1**

Received and published: 4 July 2014

In this work, the relation between lower stratospheric heat fluxes and tropical lower stratospheric temperatures is examined, in three reanalysis. Not clear if the aim is the inter-comparison itself, of if new knowledge is expected from the inter-comparison exercise. Or if the main aim is the comparison of the two different methods used. What is also unclear is the role of "variability" and "trends" in the investigation (see below). So, although the material is technically sound, the manuscript can be improved by clarifying the aim of the work and its scientific goals. My recommendation is possibly publication after a major revision of the aims and interpretations, following the general and specific comments below.

General comments

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Once the whole paper is read, it seems that the aim is on defying trends in tropical lower stratospheric temperatures that are due to dynamical processes - hence a (high frequency wrt the trend time scale) "variability" proxy is used to characterize dynamical processes. Fine. If this is indeed the case, I would remove "variability" from the title - because it is not an objective of the work to determine causes of "variability in dynamical forcing", something distinct to "trends in dynamical forcing"

Methods are generally well described and sound (scientific quality excellent), however not new: previously used by the Author(s), and variants by others as well, and as appropriately cited. Hence this work is a useful application, while not proposing/exploring new approaches.

The applicative nature of this work is acknowledged by the Authors at page 7, discussion of the proxy. One is therefore lead to ask, what specific aspects of the results and conclusions are new? Unfortunately, this is not clear from the current conclusions.

Given that, it is disturbing that the aim of the work is not well defined. "Variability and trends" may cover a variety of scales - I could deduce those of interest after reading the methods (2.2) section - but the timescale of interest is a scientific aim that should be stated explicitly, best in the Introduction.

Given the applicative nature of this work, and its focus on the dynamical proxy, the calculation of the correlation could actually be better explained. In the Northern hemisphere, the correlation I would expect from eq.(3) page 8 would actually be negative (larger positive heat flux anomaly, stronger circulation, negative tropical temperature anomaly). But this is not the case (figure 1)

The main result seems to be that ERA-Interim and MERRA mostly agree (for the carried out comparison), while NCEP deviates. Is this a new result?

The inter-comparison is useful. Not consistent trends are found in the three reanalysis for the dynamically forced trend. But - unfortunately - no reason/speculation is given

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for the noted discrepancy!

Specific comments

Page 3

LL 15-20: I would suggest to introduce the purpose of the work not with ",.. and . . ." but a proper sentence. Also not clear what is the aim of the reanalysis inter-comparison: Is it the interest of the author only to do the inter-comparison - or do they aim at a gain in learning? If so, what learning?

L 25: "The dynamically forced upwelling in the tropical stratosphere links temperatures and dynamics via radiation" unclear.

Page 5 LL 1-5: Unclear the time scales of the trends considered - and/or "trends" is used in a more general sense, that is including "low-frequency variations" - that may look like a trend if the time-series is not long enough. The paragraph is vague. One is left with asking: Is the "identification of possible reasons for differences in trends" between re-analysis the main aim? So, is the focus on disentangling if some re-analysis might mis-represent some processes? Or is the focus on the origin (radiative versus dynamical) of observed trends?

L 12: Please specify "tropical mean"

Page 6 LL 1-4: Not clear the context of these not shown approximations. "Variations" on which time scales?

Page 9 LL 16-24: The results of this paragraph sounds a bit circular, given that  $\tau \sim 70$  days is selected to obtain maximum correlations between  $W_{star}$  and temperature variations. Please explain better what is expected by construction and what is not.

Page 10 L 21: "is a coincidence" is too strong and misleading. The ozone induced effect highlighted by Fueglistaler et al 2011 is of dynamical origin - hence the strength of the residual circulation is heavily implicated. The residual circulation is indeed behind

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Interactive Discussion

Discussion Paper



the relationship assessed by the authors (as they know well).

Page 17/18 Conclusion, seasonality of the trends: Any new result with respect to Fu et al ACP 2010?

Page 18, LL7-11: This extension of the Fueglistaler 2012 result is interesting. However, it is not clear what is meant with "dynamical response" - are the Authors attributing this dynamical disturbance to Pinatubo? What would be the evidence, based on this work, for this claim? Could the Authors also please discuss how this tropical cooling (and the implied circulation increase) relates to the stratospheric-tropospheric response in the Northern hemisphere claimed (e.g., Robock/Graf/Stenchikov works) to occur after volcanic eruption? And what could be the role of other phenomena unrelated to the effects of the Pinatubo aerosols?

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In general, the figure captions should be more precise, for example:

Figure 1: correlations: are those reported for "no-volc" for the 1995-2011 period as well? Why the correlation goes down, for "no-volc"?

Figures 8: caption has letters for panels, but these letters are missing in the figure. Description of what seen in panels a, b is insufficient. What are the signatures?

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

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