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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 #2

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Summary

This study examines variations in tropical lower stratospheric temperatures in a number of reanalysis datasets (ERA-Interim, MERRA and NCEP). The study addresses several issues: (1) dynamical contributions to shorter timescale variations; (2) long-term trends; (3) consistency between multiple reanalyses; (4) consistency between different methods for estimating dynamical contributions in a single reanalysis. I have no major issues with the scientific analysis in the paper, although it would be nice to also have a comparison of the momentum budget estimate for MERRA and a discussion of whether this is consistent with ERA-Interim. My issues are rather with the overall clarity of what the study aims to do and why, and what the reader should expect to take away from it. Because of the multiple foci, I was left feeling that whilst there are some interest-

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ing results, the manuscript lacked a coherent narrative that clearly highlights the novel contributions and key messages. Many related topics have been covered in previous studies and since the attempts to explain why differences arise are limited, the results read at times like a bland documentation of characteristics of reanalyses. Although such a documentation could be of use to other reanalysis users, my view is that it is of limited value to the ACP community in its present form. I think that the manuscript could be significantly improved by providing more background motivation and well defined goals in the Introduction and clearer outcomes and links to the wider literature in the Conclusions. I have made some suggestions for how to improve the manuscript below and these points would need to be addressed before I would recommend the paper for publication in ACP.

Recommendation: Major revisions

General points

P3L20-21 ‘the purpose of this paper is to document common features and differences therein in several modern reanalyses.’ – is this the main goal? If so, what is the new information coming out and how does it fit into the existing literature? See also P4 L8-12.

P14L14-20 It seems a bit contradictory as to what to take away from the paper. In some respects, the proxy does a remarkably good job of capturing the interannual variability, and there is some consistency between the reanalyses as to the long-term trends (e.g. the v’T’ estimate from ERA-I and MERRA). But this result for ERA-I casts some doubt on how these trends should be interpreted. So what is the overarching message?

Section 3.4 This section is again giving rather mixed messages. After making the case in section 3.3 that the momentum balance and eddy heat flux methods disagree in their trends for ERA-Interim. This section gives a detailed description of the seasonality of the hemispheric trends in the eddy heat flux proxy and states that there are strong consistencies between MERRA and ERA-I. However, it then goes on to say

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that the momentum balance estimate for ERA-I has different seasonality to the others. There is no estimate of the momentum balance calculation for MERRA, so we cannot know whether this proxy might be consistent across the two reanalyses. The reader is therefore left wondering what to take away from these calculations, other than a strong sense of skepticism and some confusion. These contrasting findings need to be brought together more clearly in a summary paragraph or the conclusions.

Conclusions: This needs a paragraph which brings all the findings together and links them back to the literature. What is the key message a reader should take away? This is not clear to me in the current manuscript.

At several points in the manuscript the authors make reference to the limited dynamical output available from model intercomparison projects. However, the potential value of their simple analytical approach for modeling studies is not strongly emphasised. Do the findings here suggest that a significant amount of information can be extracted from the limited available data? If so, that should be made clear in the conclusions.

Specific comments

P3 L4 Thompson et al. (Nature, 2012) show that models forced with all known forcings also do not simulate the observed cooling in the tropical lower stratosphere.

P3 L4 ‘Consequently, trends in stratospheric dynamics . . . may play a major role for temperature trends’ – the GCMs referred to in the previous sentence could simulate dynamical trends due to increasing CO₂ alone, and indeed most of them do, so this statement is not clear.

P3 L2-8 It might be worth clarifying the potential for dynamical and radiative contributions to regional and global stratospheric temperature trends. i.e. dynamical contributions small on global scale.

P3 L11 delete ‘what’

P3 L25 four uses of different/differences in one sentence!

P3 L26-28 ‘The dynamically forced upwelling in the tropical stratosphere links temperatures and dynamics via radiation.’ Would it be clearer to make this statement by saying that temperatures reflect the balance between dynamical and radiative processes?

P4L1 ‘to processes not directly coupled to dynamics’ e.g. ?

P4L9-10 ‘The TLS time series is considered to be of high quality, but may suffer also from smaller artifacts.’ References for these statements?

P4L29 – P5L5 These are all relevant, however, we never learn which of these, if any, are most important for the discrepancies described later in the results section.

P5L23 ‘tropical means in this work’ – later on (P11L1) is it stated that the tropics are defined up to 35°. Is this the tropical mean referred to here? If so, it would be helpful to make this clear.

P5L24-25 ‘where variations in T_E are a consequence of variations in radiatively active tracers.’ Could changes in surface temperature also affect T_E? These might not be coupled to radiatively active tracers.

P6L19 It’s not clear to me what negative time means in this context - can you elaborate on this.

P6L9-12 You should state which altitude range this refers to, since it might not be true at all levels.

P7L7-8 Out of curiosity, does the choice of resolution make much of a difference for the non-linear terms? e.g. if you regrid ERA-Interim to 2.5° do the values change much?

P7L24 ‘should be very similar’ – this is cryptic. Have you checked that they are similar? If so, you should say they are similar, if not then this is speculative.

P8 L2 Li and Thompson (JGR, 2013) also use a similar proxy to relate midlatitude dynamical forcing to tropical cirrus clouds and tropical temperatures in Era Interim.

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P8 L6-7 Is it important that this metric will integrate across the turnaround latitudes during some seasons and will therefore include a part of the tropical upwelling region?

P8L10-11 I consider 25° to be in the subtropics. Does this therefore imply that the subtropics don't matter for tropical upwelling at 70hPa, since you state that you results are insensitive to whether or not the subtropical component from 25-40° is included? If so, is this surprising given the suggested important role of the subtropical wave driving for simulated long-term trends in tropical upwelling (e.g. Shepherd and McLandress, 2011)?

P8L22 Does the shorter optimal timescale for NCEP suggest a fundamental difference from the other reanalyses, e.g. a greater occurrence of shorter vertical wavelength perturbations, or some fundamentally different radiative behavior? Or is this within the uncertainty range?

P9 L26-27 Does the use of fixed bounding latitudes rather than the turnaround latitudes make any difference to the results? Past studies have shown that diagnosis of trends in tropical upwelling in reanalyses can depend upon the use of fixed latitudes vs. turnaround latitudes (e.g. Seviour et al., 2012).

P10L12-13 It would be worth emphasizing that any changes in T_E due to circulation driven changes in ozone would be included in this estimate.

P11L12-15 You should clarify here that the step-wise changes follow the two major volcanic eruptions, I think.

P12 L11 What is the effect? State that the trends are slightly smaller without the volcanoes included.

P13L1-5 This is interesting, but it begs the question as to whether there is any understanding of where the difference in the annual mean trend in NCEP might be coming from?

P14L22-23 by 'generally good agreement' are you referring to the shorter timescale

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variability? Please make clear in the text.

P14L24-27 This agreement seems like cherry picking. There is no explanation as to what determines the periods for which the two methods do agree and the periods for which they don't agree, and more importantly why.

P17L5-7 This is written as though this fact invalidates the estimates of the trends in the proxy timeseries, but does this have to be the case? Surely the trends in the residuals could be driven by different processes (e.g. volcanoes) which don't have to be represented consistently in MERRA and ERA-I for the long-term dynamically driven trends to be consistent? You need to better explain what you mean by this sentence.

P18L14-16 But since both calculations are subject to errors, should we believe any of the estimates discussed here? Particularly given that the reanalyses assimilate much common data.

Figure 1: You should change the figure labels to show 80:11 actually means 1980:2011.

Figure 5: It's interesting that MERRA indicates a significant warming trend from the NH in JJASO, which partly balances the SH driven cooling. This isn't apparent in ERA-Interim, so is worth pointing out.

Figure 8: There is a lot of information in this figure – could it be simplified. I don't for example know what the difference is between the triangle and square lines in panels (a) and (b).

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

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