

Interactive comment on “Radiative and dynamical contributions to past and future Arctic stratospheric temperature trends” by P. Bohlinger et al.

Anonymous Referee #1

Received and published: 17 July 2013

This study uses a mixture of reanalysis data and chemistry-climate model (CCM) output to investigate the radiative and dynamical drivers of Arctic temperature trends, both for the recent past (1980-2011) and projected future (2000-2049). A strength of the study is that it looks at the trends by month and seasonal, and several observationally-based and CCM studies have pointed to a seasonally varying nature of the stratospheric circulation trends.

However, in general I find that the present work is not placed in the context of other relevant studies, does not justify the data it uses, lacks sufficient detail for the methods, and does not clearly describe what it is trying to achieve. These issues are not insur-

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mountable, but I think they need addressing before being considered for publication. I have expanded on these general concerns below, followed by some additional specific comments and corrections. I hope that these are useful for the authors.

GENERAL CONCERNS

1. Context for the study: The introduction (as well as subsequent sections) does not sufficiently draw on the growing body of work on stratospheric circulation changes. I suggest the authors start with Lin et al. (2009), Ray et al. (2010), Young et al. (2011; 2012) and Wang and Waugh (2012), also drawing on the references in the introductions of those papers. Also, the results of the present study need to be put in the context of other work.

2. Trends from reanalysis: As was mentioned by Prof Solomon in her review, I have concerns about using reanalysis products for trend studies. As I'm sure the authors are aware, changes in the observing systems (e.g. moving to a new satellite) can introduce spurious jumps in reanalyses and thus impact any trends. As well as the MSU4/MSU TLS satellite data, there are radiosonde observations for the lower stratosphere. While I realize the strengths in using consistent EP flux and T data, it would at least be good to see how the reanalysis trends compare to a variety of observations over the same time period (i.e. not just an ad hoc comparison to Randel et al., (2009)).

3. Description of regression: There is no description of the regression model currently. Is it a linear least squares model? Is it fitted by month, or are there Fourier terms? How are the errors defined? Is autoregression of the residuals taken into account?

4. Description of the CCMs: For CCMVal2, Section 3.1 should draw on the published articles that describe the simulations/models etc. (e.g. Morgenstern et al., (2010) for the simulations). The description of the EMAC runs is not very clear. For instance, the word “basically” (P6713, L2) needs to be removed from section 3.1, and the sensitivity studies (section 3.4) need better explaining. E.g. does CH₄ produce stratospheric water? Does N₂O chemistry effect ozone concentrations? Without more detail I am

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unconvinced about the attribution of the trends with the sensitivity studies (e.g. what about stratospheric water vapor changes?).

5. Clearer/more scientific language: Overall, I think that the manuscript would benefit from a careful re-reading, perhaps by a native English speaker. Three specific phrases/wordings that are used throughout the manuscript are not very clear to me: (1) "...for a given dynamical situation..." - does this mean "in the absence of a dynamical trend" or some such? (2) "cold winters" – does this mean winters where the temperatures are such that PSCs can form? Please be specific. (3) "...attribute future..." and "...predict..." – these are applied to model output in the manuscript, where "projection" would be more correct – the simulations are not forecasts, but a projected set of conditions for a given set of inputs.

6. What's the take home message? Although this is last my list here, this is really my biggest criticism of the current manuscript – I'm really not clear on what the overall result is, what it might mean, and how it fits in the context of other studies.

SPECIFIC COMMENTS

P6708, L7: Define CCM (and you don't need to say "CCM models")

P6708, L21: "The expected decrease..."

P6709, L17: Clarify "measurements" – i.e. not a direct measure of the BDC, but temperature

P6710, L2. "...ozone depletion" – reference?

P6710, L5. Reference/definition for CCMVal2.

P6710, L6. "...ERA-Interim, we use the output from these models to project..."

P6710, L10: Can't attribute future trends – but can look at future *modelled* trends

P6710, L21. "confidence interval"

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P6710, L23 Why "quasi"? Define

P6710, L24-26. Need a citation here. Are radiative time scales on the order of 100 days in the lower strat? Is it really in radiative equilibrium?

P6711, L1. "DJF"

P6711, L6: None of the trends in Fig. 2 are significant – you should note this.

P6711, L9. "The correlation..."

P6711, L11: The correlation does not "allow you" to do the regression, it is just consistent with a linear relationship between T and EP-flux.

P6711, L16-: How are you determining the dynamical (and radiative) trend? Is it using the temperature that is linearly congruent with EP-flux?

P6711, L26: Could this indicate a change in seasonality of the BDC? See Young et al. (2012) and refs. therein.

P6711, L28. If March is so crucial, why look at DJFM rather than just M?

P6712, L1: "a significant mean cooling of ... K decade⁻¹." (delete last half of sentence)

P6713, L7: "...were not included, in accordance with the REF-B2 specifications." (Or "...unlike the REF-B2 specifications")

P6713, L26. The ERA error bars and the model spread are not quite the same thing. The former is the error comes from not only interannual variability, but also from how well/badly a straight line models the trend. The model spread is not so much related to IAV, but – under the assumption that the models are drawn from a statistical ensemble that includes "reality" – says something about the range of trends we might expect for the given forcings.

P6714, L5: "indicates"

P6714, L7: Note that the multi-model mean trend error is low as you have canceled out

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much of the interannual noise (e.g. see Young et al. 2013).

P6714, L22: “occurs”

P6715, L3: “(cf. Fig 3b)” ?

P6716, L5-6: “. . .we find that a projected future increase if CO2 would contribute. . .”

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 6707, 2013.

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