We thank Anonymous Referee #1 for the detailed comments and suggestions. Below we provide a point by point reply to the comments, with the original comments in italics and our reply in normal font.

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 insurmountable, 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.

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.

Thanks for the additional references that we plan to include in the revised version of our manuscript.

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)).

We have now decided to include also temperature trends from the RICH adjusted radiosonde data set in our study. See our reply to Susan Solomon's comments for a figure comparing temperature trends from ERA-Interim, RICH and MERRA. The temperature trends calculated from RICH observations (Haimberger et al., 2012) agree very well with the reanalysis trends, strengthening our conclusions. As we focus on 50 hPa, as this is the altitude region most relevant for Arctic ozone depletion, MSU and SSU satellite observations will not be directly applicable to our study: the broad MSU weighting functions maximize at lower altitudes while SSU is sensitive only at higher altitudes.

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?

The regression model uses linear least squares, with trends calculated for individual months separately (i.e., no Fourier terms). Errors are estimated from the variance of the residuals without autoregression. We will include an updated description in the revised manuscript.

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

We will include a reference to Morgenstern et al. for the CCMVal-2 models. We will also try to describe the EMAC simulations more clearly. In the sensitivity runs, there is full chemical, radiative and dynamical feedback for the fixed gases. E.g., N2O does not only act as a greenhouse gas, but also as a source for NOy affecting ozone chemistry. Similarly, CH4 has direct radiative effects, it affects stratospheric water and ozone chemistry. For all simulations, temperature changes affect ozone concentrations. Results for the sensitivity runs are most clearly seen for annual mean global mean temperature trends (Table 3): Holding well mixed greenhouse gases (i.e., CO2, CH4 and N2O) fixed at their 2000 surface mixing ratios results in modelled temperature trends of only -0.06 +/- 0.02 K/dec as compared to -0.30 +/- 0.02 K/dec in the standard run. On the other hand, holding ODS (ODS defined here as the halogen source gases, not including N2O) fixed at 2000 mixing ratios lbut increasing CO2, CH4 and N2O as in the standard run leads to a global mean temperature trend of -0.32 +/- 0.02 K/dec, larger but only slightly larger than in the standard run. At least in the EMAC calculations, future lower stratospheric temperature trends depend mostly on the increase in CO2, CH4 and N2O while the decrease of ODS play a much smaller role. The discussion of these in our revised manuscript will be changed to make this point more clear.

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.

Okay, thank you for the suggestions. We will take this into account and try to improve the language.

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.

The main motivation of our study is to investigate the possibility for a cooling of the cold Arctic winters that would have important implications for Arctic ozone depletion. Arctic winter / spring temperatures in the stratosphere are largely controlled by wave activity, e.g. as expressed by the 100 hPa eddy heat flux. In our study we find that when taking out the effect of the wave activity (through linear regression with the heat flux) the Arctic stratosphere has been cooled in past decades at a rate that is large enough to significantly affect Arctic ozone depletion. Sinnhuber et al. (2011) have estimated that a cooling of about 0.8 K/dec could offset the effect of the expected halogen decrease over the coming decades. Here we find a radiatively induced cooling of -0.52 +/- 0.49 K/dec for DJFM based on ERA-Interim for 1980 – 2011. While there are indications for an increase of the strength of the BDC, the inter-annual variability of the eddy heat flux is large, and a significant radiatively induced cooling would imply that a future cold winter (a winter with a small eddy heat flux) would become colder than a past winter with a similar eddy heat flux. To test to what extent this radiatively induced cooling really continues into the future we have analysed the set of CCMVal REF-B2 models together with our own EMAC model. We find that for the past the models reproduce the observed radiatively induced temperature changes (but not the dynamical and overall temperature trends, because there is a large scatter in eddy heat flux trends), albeit underestimating the magnitude of observed temperature trends (e.g. in JJA when the inter-annual variability is low, the modelled temperature trend is only about half as large as the trend from ERA-Interim or RICH observations). The model projections of future Arctic temperature trends at 50hPa indicate a continued radiatively induced cooling by roughly 60% of the magnitude as calculated for the past decades We will try to state this more clearly in the revision manuscript.

SPECIFIC COMMENTS

Thank you. All comments will be taken into account in our revised manuscript.

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

Okay.

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

Okay.

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

Okay.

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

Okay. We will include a reference to WMO (2011).

P6710, L5. Reference/definition for CCMVal2.

Okay. In addition to the SPARC CCMVal report we will include the suggested reference to Morgenstern et al.

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

Okay.

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

Okay. We will change the text accordingly.

P6710, L21. "confidence interval"

Okay.

P6710, L23 Why "quasi"? Define

Okay, we will define this as "near-global (60°S-60° N)" as in Randel et al. (2009).

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?

We will include a reference to Newman and Rosenfield (1997) for radiative time scales; see also our reply to Susan Solomon's comments.

P6711, L1. "DJF"

Okay.

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

Okay, we will note this.

P6711, L9. "The correlation. . ."

Okay.

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.

Okay. We will change the description accordingly.

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

Yes. We will try to state this more clearly in our revised manuscript.

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

We think the larger radiatively induced cooling in spring is likely due to the larger ozone trends in this season, see our new Fig. 3 our the reply to Susan Solomon's comments.

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

In Sinnhuber et al. (2011) we have found that temperature changes in early winter have about the same effect as temperature changes in late winter and spring for the integrated ozone loss in spring. Thus it is really the average temperature over DJFM rather than over DJF or March alone that matter for ozone depletion.

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

Okay. Our intention was to note that this is only marginally significant.

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

Okay.

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

Thanks, we agree, and will state this more carefully.

.
P6714, L5: "indicates"

Okay.

P6714, L7: Note that the multi-model mean trend error is low as you have canceled out much of the interannual noise (e.g. see Young et al. 2013).

Okay, we will note this in the revised manuscript.

P6714, L22: "occurs"

Okay.

P6715, L3: "(cf. Fig 3b)" ?

Thanks. We will just write "(Fig. 3b)".

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

Okay.

REFERENCES:

Thank you for the suggested additional references. We will include these in the revised version of our manuscript

Lin, P., Fu, Q., Solomon, S., & Wallace, J. M. (2009). Temperature Trend Patterns in Southern Hemisphere High Latitudes: Novel Indicators of Stratospheric Change. Journal Of Climate, 22(23), 6325–6340. doi:10.1175/2009JCLI2971.1

Morgenstern, O., Giorgetta, M. A., Shibata, K., Eyring, V., Waugh, D. W., Shepherd, T. G., et al. (2010). Review of the formulation of present-generation stratospheric chemistry-climate models and associated external forcings. Journal of Geophysical Research, 115, D00M02. doi:10.1029/2009JD013728

Ray, E. A., Moore, F. L., Rosenlof, K. H., Davis, S. M., Boenisch, H., Morgenstern, O., et al. (2010). Evidence for changes in stratospheric transport and mixing over the past three decades based on multiple data sets and tropical leaky pipe analysis. Journal of Geophysical Research, 115(D21), D21304. doi:10.1029/2010JD014206

Wang, L., & Waugh, D. W. (2012). Chemistry-climate model simulations of recent trends in lower stratospheric temperature and stratospheric residual circulation. Journal of Geophysical Research, 117(D9), D09109. doi:10.1029/2011JD017130

Young, P. J., Thompson, D. W. J., Rosenlof, K. H., Solomon, S., & Lamarque, J.-F. (2011). The seasonal cycle and interannual variability in stratospheric temperatures and links to the Brewer-Dobson circulation: An analysis of MSU and SSU data. Journal Of Climate, 24, 6243–6258. doi:10.1175/JCLI-D-10-05028.1

Young, P. J., Rosenlof, K. H., Solomon, S., Sherwood, S. C., Fu, Q., & Lamarque, J.-F. (2012). Changes in Stratospheric Temperatures and Their Implications for Changes in the Brewer–Dobson Circulation, 1979–2005. Journal Of Climate, 25(5), 1759–1772. doi:doi: 10.1175/2011JCLI4048.1

Young, P. J., Butler, A. H., Calvo, N., Haimberger, L., Kushner, P. J., Marsh, D. R., et al. (2013). Agreement in late twentieth century Southern Hemisphere stratospheric temperature trends in observations and CCMVal-2, CMIP3, and CMIP5 models. Journal of Geophysical Research. doi:10.1002/jgrd.50126

Additional references

Haimberger, L., Tavolato, C., and Sperka, S., Homogenization of the global radiosonde temperature dataset through combined comparison with reanalysis background series and neighbouring stations, J. Clim., 25, 8108-8131, 2012.

Newman, P. A. and Rosenfield, J. E., Stratospheric thermal damping times, Geophys. Res. Lett., 24, 433–436, 1997.