Response to referee interactive comments on "A mid-latitude stratosphere dynamical index for attribution of stratospheric variability and improved ozone and temperature trend analysis" by William T. Ball et al

General comments relevant to both referees:

We thank both referees for very helpful guidance and suggestions in the background, justification and statistical analysis performed. This has led to significant improvements in the quality of the manuscript (which are already apparent in our current revisions and in our response below). Major changes in the revised manuscript have been highlighted in bold text.

We begin by highlighting major updates that both reviewers should be aware of:

i) Figures 10, 11, 12 and 13 have been updated to reflect the use of AR1 instead of AR0. Figure 13 in particular has been completely changed, and numbers added, to clarify the error improvement and a change in the mean values. Figure 12 has been updated to reflect similar style and information as provided in Figure 11.

ii) The point made in the manuscript about no aliasing between regressors being shown by the relative importance plots has been modified. Due to the use of AR1, for temperature, there is a redistribution of the relative importance from the original regressors without the new index to the new one, in addition to the increase in total variance accounted for. However, the fact it does not change the mean value of the regression coefficients in the trend still supports the claim that it does not alias the derived signal. Modifications to the text in the manuscript have been made to reflect this change, and discussion on this point has been added.

iii) A significant amount of background have been added to the introduction.

iv) We have renamed the MLSD index to the Upper-branch Brewer Dobson Circulation (UBDC) index, to reflect a more direct interpretation of what it represents, and for which it should be more easily understood; this changes the title of the manuscript.

A. Karpechko (Referee)
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Paper's main finding is coherence in the variability of stratospheric temperature and ozone in the tropics and extratropics and in the upper stratosphere and lower mesosphere. The authors attribute this coherence to dynamics, specifically to the stratospheric meridional (Brewer-Dobson) circulation, and propose that an index accounting for dynamical effects could be used in multiple regression analysis as additional regressor. They further build such an index using extratropical upper stratospheric temperatures and demonstrate that the index explains considerable fraction of variability in stratospheric ozone and temperatures. Although the authors present interesting analysis, they still have to show how their analysis is related to previous research and high light novel results. The use of regressors accounting for dynamical effects has been discussed in previous WMO Ozone Assessments, discussing their pros and cons. I believe that a more thorough discussion of issues associated with the use of dynamical proxies, as well as relation of the current analysis with previous studies is needed before possible publication in ACP. Please see my specific comments below.

Major comments 1. Various dynamical proxies have been used in past to explain stratospheric variability related to dynamics, see examples in Weiss, et al. 2001; Brunner et al. 2006; Mäder et al., 2007; Wohltmann et al., 2005; 2007 and references therein. While a considerable fraction of variability in both ozone and temperatures can indeed be explained by these proxies, this benefit comes at the cost of attributing variability to processes which are themselves dependent on the variables to be explained (wave propagation depends on the mean state of the stratosphere), i.e. one mixes cause and effect. I suggest that these issues should be discussed in the manuscript. Relevant discussion regarding the use of dynamical proxies for attributing ozone variability can be found in Chapter 2 of WMO ozone Assessment 2011 (Sections 2.1.2 and 2.4).

We agree, and we appreciate the useful set of references that has led to an expansion of the discussion in the manuscript. Further, the new background material simply further highlights the need for such a dynamical proxy, especially in the equatorial region, since previous studies have focused on BDC proxies that operate on inter-annual timescales and longer, and while often being briefly mentioned, the monthly and shorter timescales are usually ignored (except, e.g. Chandra et al., 1986 as mentioned by the second reviewer). Previous studies have also focused on total ozone column, mid-to-high latitudes, and the mid-to-lower stratosphere. This leads to the clear conclusion that, while not a new concept, the development of a proxy accounting for noise-like dynamical events in the upper stratosphere and mesosphere is necessary, and that its application and focus on the equatorial region is new.

References:

1. *Brunner, D., J. Staehelin, J.A. Maeder, I. Wohltmann, and G.E. Bodeker, Variability and trends in total and vertically resolved stratospheric ozone based on the CATO ozone data set, Atmos. Chem. Phys., 6 (12), 4985-5008, doi: 10.5194/acp-6-4985-2006, 2006.*
2. *Mäder, J.A., J. Staehelin, D. Brunner, W.A. Stahel, I. Wohltmann, and T. Peter, Statistical modeling of total ozone: Selection of appropriate explanatory variables, J. Geophys. Res., 112, D11108, doi: 10.1029/2006JD007694, 2007.*
3. *Wohltmann, I., M. Rex, D. Brunner, and J. Mäder (2005), Integrated equivalent latitude as a proxy for dynamical changes in ozone column, Geophys. Res. Lett., 32, L09811, doi:10.1029/2005GL022497.*
4. *Wohltmann, I., R. Lehmann, M. Rex, D. Brunner, and J. Mäder, A processoriented regression model for column ozone, J. Geophys. Res., 112, D12304, doi: 10.1029/2006JD007573, 2007.*
5. *Weiss, A. K., J. Staehelin, C. Appenzeller, and N. R. P. Harris (2001), Chemical and dynamical contributions to ozone profile trends of the Payerne (Switzerland) balloon soundings, J. Geophys. Res., 106(D19), 22685–22694, doi:10.1029/2000JD000106*
6. *WMO: Scientific Assessment of Ozone Depletion: 2010, Global Ozone Research and Monitoring Project, 52, 516, 2011.*

2. There are also problems with using temperature as a proxy representing extratropical wave dynamics. Stratospheric temperature is controlled by a number of processes, such as horizontal and vertical advection, diabatic heating, and not all variability is necessarily directly attributable to extratropical wave forcing. Constructing an index by maximizing correlation, as is done in this study, also maximizes the risk of mixing statistical noise with physical processes. That is why using proxies more directly related to wave activity could be a better choice. While I agree that wave activity proxies such as EP-flux divergence are difficult to calculate, one can try, for example, heat flux evaluated at 100hPa (e.g. Newman et al. 2001), which is quite easy to calculate.

Again, we agree with this assessment and indeed we will make it clear that we are mixing in physical processes from different specific sources (see previous response) in the introduction. Further, it would make sense that if wave-driving from the troposphere at mid-latitudes is one of the main drivers of variance in the equatorial upper stratosphere, then the use of EP-flux divergence (EPFD), or the heat flux at 100 hPa would represent a more physical proxy. We note, however, that while

Newman et al. (2001) were successful in representing short-term dynamical fluctuations in the stratosphere with EPFD, they did not investigate effects above 10 hPa. During the analysis of our original manuscript, we investigated the relationship between 100 hPa heat flux and equatorial ozone and temperature in the upper stratosphere, but were unable to find any clear agreement, even if we considered a lag to account for the time waves take to propagate into the stratosphere and force a response. We revisited this following the reviewer's suggestion (above) and considered the correlation between indices of the NAO, AAO, ENSO, QBO, and 100 hPa heat flux (v'T') averaged between 60-90 S, 60-90 N, 45-75 N and 45-75 S. We further divided months out to consider Dec-Feb and Nov-Apr for the northern hemisphere, and Jun-Aug and May-Oct for the southern hemisphere, and either the original timeseries or detrended and deseasonalised. We compared all of these cases with the MLSD index, which has high agreement locally to the respective hemisphere and with the equatorial upper stratosphere and mesosphere. Considering just the R^2 values (i.e. correlation coefficient squared), we found agreement exceeding 0.15 only in three cases: for DJF 60-90 N after deseasonalising and detrending the data, and 0.18 and 0.19 for DJF 45-75 N with and without deseasonalising and detrending (the third value shown is also similar for a 1 month lag); there was nothing clear in the southern hemisphere, with values all close to zero. This indeed suggests that there is possibly some relationship between heat flux at 100 hPa, and we concede this was a simplistic set of checks. However, the three results showing some coherence with the MLSD index account for very little of the variance we see in temperature above 10 hPa. The source of the variance warrants further investigation beyond this manuscript, as our analysis clearly shows a relationship with changes in temperature in the upper stratosphere and mesosphere related to what appears to be a wave-forcing like response in the EPFD and stream functions (i.e. Figs 5 and 6).

Reference: Newman, P. A., E. R. Nash, and J. E. Rosenfield (2001), What controls the temperature of the Arctic stratosphere during the spring?, J. Geophys. Res., 106(D17), 19999–20010, doi:10.1029/2000JD000061

Other comments:

1. P2L1-5: See Major Comment 1. There are plenty of studies using different set of proxies, not only the six proxies listed here.

We have expanded this section to a separate discussion of equatorial and higher latitude MLR analysis, which, e.g. includes other proxies such as the AO and NAO, but also see our response to Major Comment 1.

2. P2L118: I believe there are older references which show influence of dynamics on stratospheric ozone, e.g. Fusco and Salby 1999 and references therein.

We have included this reference in addition to others, and those already mentioned above.

Reference: Fusco, A. C. and Salby, M. L.: Interannual variations of total ozone and their relationship to variations of planetary wave activity, J. Clim., 12, 1619–1629, 1999.

3. P2L22-23: Please note that acceleration of BD circulation leads not only to increase of ozone in the extratropics but also to a decrease in the tropics, thus it is more correct to say that ozone is redistributed, not just increased.

We suggest this can be made clear with the addition of the following bold text:
"The increase in ozone at mid-latitudes comes partly from ODSs reductions, but also because the BDC is expected to accelerate (Garcia and Randel, 2008; Butchart, 2014), which will reduce the time for

ozone depletion to occur and lead to faster transport of ozone from the equatorial region to higher latitudes. This in turn leads to a reduction of ozone over the equator and a prevention of a full recovery over the tropics. ***Thus, the recovery of ozone at mid-to-high latitudes can be understood as being partly due to less ozone destruction by lower ODS concentrations, and partly due to a faster redistribution of ozone-rich air from the tropics."***

4. P3L27-28: I think smoothing removes short-term variability, not long-term. Please rewrite.

The sentence was incorrectly formulated, and should make clear we remove the smoothed time series from the original; thus the following formulation should be clearer.

"…we remove all long-term variability by subtracting a timeseries that has been smoothed, with a 13-month running mean, and then deseasonalised, with monthly values, at each latitude and pressure*."*

5. P8L4-6: Please see Major Comment 2. I think some caution is needed when using stratospheric temperature as proxy for dynamics.

See response to major comments 1 and 2 and revisions of the text.

6. P9L6: 'Verses' -> 'versus'

Done

7. Figure 10: The difference in Fig. 10b between regression results from GOZCARDS and SWOOSH from the one hand and SBUV from the other hand are interesting. It appears like dynamical variability in GOZCARDS and SWOOSH is represented by the other proxies because, after addition of the dynamical proxy, the explained variability changes only little in these data sets, and the total explained variability is quite similar in all four data sets. Do you think it is purely statistical effect or it may be related to the way these data sets are compiled? (Sorry I am not familiar with these data sets.)

This is an entirely valid, and interesting, question. As you suggest, we believe (and have evidence to support) that the answer resides in the way the datasets are compiled. Indeed, looking at individual time series, it becomes clear that the earlier periods in GOZCARDS and SWOOSH at these altitudes contain high variance fluctuations that look more related to the datasets used themselves than real variability; GOZCARDS and SWOOSH use a similar source of data (SAGE II) for this period. Given the variance is on the order of that in the MLSD index, it is likely (and this is now a postulation) that the reduced improvement during this period is due to these high variance artefacts. We are tackling this problem and are due to submit an article relating to this soon.

8. Figure 11: I am puzzled by why the annual R2 for the w/ MLDS regression in the middle panel is larger than any seasonal one. The result from the w/o regression, where the annual R2 looks like the mean of seasonal results, looks more logical, is it not?

As you correctly identified, there was a mistake in how anomalies were dealt with in the regression routine. This has been corrected, and indeed the results now appear more logical.

9. Captions to Figure 11: What is distribution peak? Is it the mode?

The peak is, more precisely, the median and we have added this to the description.