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Interactive comment 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.

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Overall, this is a useful effort to improve statistical estimation of stratospheric ozone and temperature trends and interannual variability by accounting for a source of short-term (month-to-month) dynamical variability in tropical stratospheric data sets. The presentation is excellent and the figures are state-of-the art. However, the value of the adopted technique for trend estimation and its ability to "explain" a larger fraction of the variance in the observations is somewhat overstated, in my opinion. Some important revisions are needed prior to publication.

Main comments:

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Discussion paper

(1) A major claim of the paper is that inclusion of the mid-latitude stratosphere dynamical (MLSD) index can reduce the uncertainty "on all multiple linear regression coefficients ... up to 45% and 25% in temperature and ozone, respectively." First of all, the accuracy of these reduction estimates is questionable because, as mentioned on p. 11, line 12, "we do not consider use of any autoregressive modeling." In other words, serial correlation (autocorrelation) of the residuals of the MLR analysis is not accounted for. It is possible that serial correlation of the monthly residuals is increased when the MLSD index is used because the month-to-month variability is reduced. Have the authors tested whether this is the case? Accounting for any increased serial correlation would increase the uncertainty estimates. For example, application of a "pre-whitening" technique (e.g., Tiao et al. [1990]; Garny et al. [2007]) would ensure that the residuals are approximately white noise thereby yielding more reliable uncertainty estimates. Please re-do the analysis in this manner to provide such a test and yield more accurate (larger) uncertainty estimates. Second, even without accounting for serial correlation, the difference in the ozone and temperature trend results with and without the MLSD term shown in Figure 13 is not very impressive. For the sake of clarity, consider only the yellow curves in the figure. The error bars for the with MLSD (thick curves) and without MLSD (thin curves) cases overlap. These are presumably 2σ error bars, right? If not, then the overlap is even larger. The error bars are roughly the same size at most levels. At 2.5 hPa, the ozone error bar appears to be about 25% smaller for the with MLSD case, which is consistent with the authors' statement. But it is not a very significant difference considering the sizes of the error bars and the large variation in the trend estimates from one pressure level to the next. For most of the other levels, the difference in size of the error bars is hard to discern.

(2) The other major claim of the paper is that use of the MLSD index in a regression analysis can "explain much larger fractions of the total variability." I am not sure that the word "explain" is appropriate. The dynamically induced variability is being accounted for in the MLR analysis but it is not really being explained. For example, the

see-saw temperature and ozone variations between the tropics and extratropics are in many cases associated with minor and major polar stratospheric warmings in the winter hemispheres. The latter are modulated by a number of external forcings including the QBO and the solar cycle. A true explanation of the variability would therefore need to account for the external forcings that are controlling the rate of wave absorption events, which in turn produce the ozone and temperature fluctuations. I also disagree with the terminology "total coefficient of determination", which is used in place of explained variance (R^2) in the text. The words "determination", "explained", and "attribution" are all misleading if the sources of the dynamical fluctuations are not identified. Please revise the introduction and conclusions section to make this clear.

Minor comments:

(3) I agree with the other referee that the history of the ozone and temperature variations that are discussed in the paper and their application to trend analyses is not adequately summarized in the paper. The first report of the existence of such global stratospheric temperature oscillations with a change in phase between low and middle to high latitudes was by Fritz and Soules [1970]. Some stratospheric dynamicists still refer to these oscillations as the "Fritz-Soules effect". See also, e.g., Andrews et al. [1987] for general discussions of their dynamical origin. Another observational study by Chandra [1986] could also be referenced.

(4) In Figure 1 (and maybe other figures), the definitions of the diamonds in the upper right corner seem to be incorrect and are opposite to those given in the caption.

(5) P. 7, line 7. adiabatically

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Discussion paper

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