**Atmospheric
Chemistry
and Physics**
Discussions

Interactive comment on "Analyzing time varying trends in stratospheric ozone time series using state space approach" *by* M. Laine et al.

Anonymous Referee #2

1   General impression

The authors present a new approach to ozone time series analysis, which is different from the usual multiple linear regression methods. The analyzed time series are the combined SAGE and GOMOS data, which are presented in a companion paper by Kyrola et al., where a standard multiple linear regression analysis is applied. The presented state space approach is relatively new and interesting. Generally, the topic of the paper is well suited to ACP.

The main conclusions of the paper are that the presented approach is feasible, and that it provides long-term ozone changes comparable to other methods, with a clear

change from ozone decline before 1997 to ozone increases after 1997. The stated year 1997 is actually not consistent with the presented evidence, e.g. in Fig. 4, which usually indicates the change from negative to positive trends around 1999 (1998 to 2002). This should be corrected.

Overall I feel that the presentation is not clear in important subsections describing the new methodology. Two major parts are especially lacking:

- The introduction and description of the state space approach is good and I can follow and understand it well (section 2.2 of the manuscript).

- However, I was not able to understand at all, how the parameters for fitting the state space approach to the observations were obtained (sections 2.3 and 2.4 of the manuscript). The way in which the fit parameters are obtained, and the Kalman filters and Monte Carlo realizations are done, is not described well at all. To a non-specialist like me (and probably most ACP readers) it does not become clear what is done, and how it is done. After reading the manuscript, it is not possible for me to repeat the authors analysis on the same (or other) data. In my opinion, one major goal of a manuscript presenting a new method should be to enable others to use that method. I think this manuscript fails in this important aspect.

- There is very little contrasting the new state space approach to the standard multiple linear regression. What are the advantages? What are the disadvantages? Does the state space approch do a better job describing the observations than multiple linear regression? Smaller residuals? Larger R^2? More freedom to follow smooth long-term trends than, e.g. a hockey-stick trend?

2 Further major points.

A major point of the DLM analysis seems to be the arbitrary (?) selection of the model error covariance **W**. If **W** is set to zero, according to the authors, the same results as by standard multiple linear regression are obtained. I think this should be actually demonstrated, and/or compared in detail with the Kyrola et al. companion paper. It would also be good to demonstrate how the results change as the authors change the size of their model error covariance **W**, e.g. from their current selection to something much larger (double? five times?).

Do I understand correctly, that the model error covariances related to QBO and solar cycle in **W** are set to zero? Is that because the main interest is in smooth long-term variations? How does varying the solar cycle influence the decadal trends? This should really be discussed more.

Figs. 6 and 7, to me, provide a strong indication that there is a significant difference between the SAGE data (up until 2002/2005) and the GOMOS data (after 2002): At 35 to 45 km, and at 45 to 55 km, and both at 50 to 60N and 50 to 60S, the distribution of the data changes significantly after the introduction of GOMOS, when low values become much more frequent than before. This should definitely be mentioned. Corresponding caveats about the usability of the long-term trend results (e.g. in Figure 4) should be added. Has the performed sophisticated statistical analysis not provided any hints on this substantial change in the statistics of the underlaying data? (By the way: I think it is very good that Figs. 6 and 7 are provided and show the original data and the derived long-term level.)

Eqs. 8 to 13 should be explained/motivated better. After a longer while, I figured out, that essentially they are (probably) used to iteratively determine the new state and the various covariance matrices. Eq. 12 is important, because only(?) it brings in the observations. Is that correct? If so, I think this should really be pointed out, as should be the meaning of and the procedure for using these equations.

Am I correct in assuming that the starting W_1 and V_1 in Eqs. 9 and 10 come from Eq 3? How are the other initial values/ matrices obtained? After reading the rest of section 2.3 and section 2.4, I guess, it is more complex. But I still don't understand it. Especially section 2.4 uses a lot of special technical terms (Gibbs sampling, Metropolis-Hastings scheme, marginal distribution, conjugate priors) that mean nothing to me (and probably to most readers). This part should be improved.

An important contributor to the decadal trends investigated by the authors is the way the solar cycle is handled. It seems to me, that the authors have not paid much attention to the solar cycle (e.g. using 11-year sines and cosines instead of F10.7, in a similar way as they do for the annual cycle). It seems to me that at various levels and latitudes there is a lot of decadal variability left, that might come from uncorrect handling of the solar cycle. Examples are (in Figs. 6 and 7): relative maxima around 1990, 2002, 2011, at 35 to 45 km, 50 to 60N, 10 to 20 N, 0 to 10N, 20S to 10S. I feel that more discussion of "interferences" from the solar cycle on the decadal trends is required. In Fig. 4 it would definitely be good to indicate (e.g. by hatching) which areas are significant, and which are not.

3 Summary

This is an interesting manuscript that presents a new (to me and many others) concept for ozone time series analysis. By separating between an underlying "true" state and the projection of that state into observations with limited accuracy, and by assuming certain properties for the evolution of the underlying state, a different access to things like long-term variations becomes possible. I feel that this is an interesting paper and, in principle, deserves publication in ACP.

However, I also feel that the authors have to improve their presentation, and have to make clearer to lay-people like me, what is actually done (and why it is done). So

without major revisions, I feel the manuscript is not ready for ACP. Because of this, I am also not making any specific comments, because I feel this is only necessary for a much later version of the manuscript.

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