

Interactive comment on “Total ozone trends from 1979 to 2016 derived from five merged observational datasets – the emergence into ozone recovery” by Mark Weber et al.

Anonymous Referee #1

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Review of the paper by Weber et al.

Overall comment: This is an important paper that should be published. It's important that the excellent prior analysis of ozone trends by this group in Chehade et al. be extended with additional years and documented. I have a number of major comments and questions that I believe should be addressed, and a few minor ones.

Major comments

1) Please clarify the extent to which there are trends in any of the terms used in the MLR, whether there are uncertainties in those, and whether these in turn can influence the calculated ozone trends and their uncertainties. For example, I would argue that we

do not know the trends in eddy heat fluxes in the stratosphere very well (although we may know the year to year variability, we do not know the longer term trends on decadal or multi-decadal time scales). So one question is: are there trends in the eddy heat flux terms that characterize the BDC components in the MLR? How uncertain are those trends? Could they (or do they) then influence the ozone trends that are the primary subjects of interest here? Papers on the uncertainties and differences between ERA and MERRA might be a useful point of reference here, but only a starting point; I think quantitative analysis is needed. The same could be said for trends in the solar term, for example. I am concerned that these could considerably influence the conclusions drawn, and should be discussed and documented.

2) Please clarify which terms in the MLR regression could conceivably involve feedbacks. For example, it is possible that changes in ozone play a role in the strength of the BDC (and this could happen not only on longer time scales, but also interannually). Has this been considered? Could it be important? If, for example, part of the BDC trend term is caused by ozone changes, then is your calculation of the 'ozone trend' potentially in error? By how much?

3) There are many studies providing evidence that the Antarctic ozone hole has influenced the strength of the southern annular mode (AO) in some seasons. Here is another potential feedback. The same questions apply as in item 2) above.

4) The fit to the interannual ups and downs in the ozone time series is pretty good, implying that on an interannual basis the terms involving dynamical variability are fairly well captured. Thus, the ups and downs are certainly not random noise – they are due to known and characterizable phenomena. The paper ought to discuss this, and make reference to the work of Shepherd et al., NatGeo, 2014, who combined a model with data to improve on the analysis. Based on the Shepherd paper, it does not seem reasonable to allow these variations to inflate the uncertainties on the ozone trend terms. Instead of doing the approach of combined MLR, would it not be more consistent to remove them first, and then examine the trends in the remainder. Terms involving inter-

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annual dynamical variations could conceivably be removed by detrending your index, and then regressing the detrended series to the ozone time series, and then doing MLR with the remaining terms you have. How would this or a similar approach affect the ozone trends, and in particular, their uncertainties? If this could significantly reduce the uncertainties in ozone trends (as I suspect, and as I think Shepherd et al. support), then that should at a minimum be stated since uncertainties are a key emphasis of the paper, and we need to know how robust the ozone trend uncertainties really are.

5) While the ILT and PLT trend approaches have certain advantages, they are allowed to float independent of each other. So what they lack is a grounding in the physics and chemistry that must link the processes that deplete ozone to those that make it recover. The advantage of EESC is that it has that grounding. The ILT 'advantage' over the PLT as suggested by your figure has a lot to do with the choice of year for the separation, which is a little arbitrary since there are uncertainties in EESC, and it does vary with height as well as latitude. I think the advantages and disadvantages of each approach should be discussed more clearly.

6) I'm concerned about some of the statements regarding the aerosol fits, for several reasons. a) First, it's not obvious that the total SAD is the best predictor, because the distribution of aerosol and the distribution of ozone losses need not coincide. At a minimum, the paper should test what happens if the SAD amount at 70 mb is used instead. b) Second, the statement that you used the Mills et al. aerosols but don't get significant correlation with polar ozone (suggesting a conflict with Solomon et al., 2016) misses some key points. As indicated in Solomon et al., 2016 and discussed in greater detail in Ivy et al. (GRL, 2016), the dependence of aerosols on ozone loss will depend upon temperature; in a warm year even a big volcano doesn't have a big effect so the dynamics is important in setting the stage. To capture this, you might need some kind of mixed predictor including aerosols and temperature, but that's not what you used so it's misleading to say that you don't get a high correlation. c) Third, the key parameter emphasized in Solomon et al., 2016 was the area of the ozone hole, which is not the

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variable you have evaluated here.

Some minor comments

7) Page 2, line 44 what is the reference for this number for changes in ODS levels?

8) Page 3, line 73. Similar but not the same? What is different from Chehade? Please summarize what you changed, and why.

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