Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-585-RC2, 2017

Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Detectability of the Impacts of Ozone Depleting Substances and Greenhouse Gases upon Stratospheric Ozone Accounting for Nonlinearities in Historical Forcings" *by* Justin Bandoro et al.

Anonymous Referee #2

Received and published: 5 October 2017

This paper certainly uses a sledgehammer to crack a nut, I am afraid. It seems to construct a point of ozone trends assumed to be linear, even though the trend is non-linear and by doing so seems to mix two slightly different types of non-linearity.

The first type of linearity (non-linearity) focusses on the temporal behaviour of the ozone time series. However, I do not know of anybody who assumes that the full behaviour of the time series is linear. This is exactly the reason why two carefully chosen periods are fitted with (independent) linear trends, or why people assume a fit to (E)ESC. In addition, non-linear (quadratic) terms have been used to consider early

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starting points (pre-1980) of the time series (most recently in Langematz et al., 2016, citing the earlier work as well), clearly acknowledging the complexity of the long-term trend.

The second type of linearity (non-linearity) focusses on the attribution problem. Are the attributed variabilities in ozone a sum of the different terms or not. MLR uses the implicit assumptions that the different factors are a sum, which is presumably a good approximation when different terms in the equation a largely independent of each other. However, it has been realised that this is not always the case and that we have to think carefully of how to choose our proxies (a lot of work is covering this question).

The paper does not clearly separate the two issues and in a way circumnavigates its own problems by choosing two different vertical regimes (nothing wrong with this). However, it would be interesting to know how the method would fair in a more holistic approach.

In summary, I believe the paper to be a nice little exercise in advanced statistics. It is certainly worth publishing after mayor revisions, but the paper needs to simplify its message should clearly acknowledge that the problem of linearity is well recognized (in both aspects – the temporal behaviour and the summing-up of contributing terms). Testing the limits of linear assumptions is always interesting, but it can be done in simple ways with idealised model simulations, alleviating the need for very fancy statistical models. However, I admit that this is a personal preference and that the paper will be a nice contribution to this discussion when revised.

Some more specific comments:

Abstract, line 26: One "the“ too many . . .

Page 3, paragraph 1: strange discussion - non-linear versus piecewise linear (fit EESC), see comment above. This discussion and scoping of the paper needs to change most.

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Page 4, line 14: I assume you talk about the absolute value, otherwise I suggest “regression coefficient significantly different from zero“.

Having the NAT run with no QBO worries me slightly – the authors mention the fact, but I would hope for a slightly more critical assessment of this shortcoming, given that many people try to eliminate the QBO signal in their trend estimates.

You say: “. . . and there are post-2005 differences between the historical WACCM model simulations and SWOOSH data that are relevant to the interpretation of the D&A results.” I certainly agree. However, I would hope for a clearer discussion of what the implications are.

You say: “The decadal variability is of key interest in D&A studies, since it constitutes . . .” What indications do we have that the modelled decadal variability is similar to the observed? Many models show distinct attenuations of amplitudes when free running (compared to SD runs). Is this of no concern for WACCM, or are there no sizeable differences for the free running model compared to the SD configuration?

You say: “. . . simple linear regression line is not an adequate representation of ozone changes over the entire observational record (1984-2016).” As I mentioned above, nobody is stating this (any more, see comment above). Please clarify this.

You use spectral filters to construct a comparison of variability on different time scales. I would prefer simple power spectra comparing the variability. The filtering you do, makes me feel uncomfortable, give that you have one time window up-to 20 years with a time series of ∼33 years.

Figure 3: typo in title

Langematz, U., Schmidt, F., Kunze, M., Bodeker, G. E., and Braesicke, P.: Antarctic ozone depletion between 1960 and 1980 in observations and chemistry–climate model simulations, Atmos. Chem. Phys., 16, 15619-15627, https://doi.org/10.5194/acp-16-15619-2016, 2016.

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