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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 #3

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The Bandoro et al. manuscript presents a modified approach to detection and attribution that is able to account for the non-linear temporal behaviour of the forcing terms - in the case of stratospheric ozone analysed here, the rise in the atmospheric concentration of ozone depleting substances (ODSs) until the late 1990s followed by a slow decline. The authors clearly and convincingly present a comparison of their new approach with the more widely used approach of assessing signal-to-noise using linear trends and demonstrate the difficulties that arise when the magnitude of the forcing is not linearly changing with time. I have no significant concerns with the methodol-

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Discussion paper

ogy or the presentation, though I will admit to having little background in detection and attribution.

My one, I believe relative inconsequential, concern is how the NAT-h timeseries was constructed. On Page 7, lines 6 – 8, the authors state:

'In addition, we were able to isolate the response to volcanic aerosols, solar cycle, and the QBO, by differencing the sum of ensemble mean anomalies of the FIXED GHG1960 and ODS1960 simulations from the individual ensemble anomalies of ALL2.'

I can see how the arithmetic of the construction of NAT-h should work; by adding together the effects of GHGs (from the ODS1960 simulations) and ODSs (from the FIXED GHG1960 simulations) and removing these signals from the ALL2 timeseries. My concern is whether the volcanic aerosol effect will be correctly represented in the NAT-h timeseries. The response to large eruptions, such as Pinatubo, will depend critically on the concentrations of reactive chlorine in the stratosphere. Under the low chlorine loading of the ODS1960 simulation the increased volcanic aerosols will produced an increase in ozone in the mid-stratosphere, while under the higher chlorine loading of the FIXED GHG1960 the Pinatubo eruption will produce some increases in the mid-stratosphere and more significant decreases in the lower stratosphere. Given the spatial variability in the response, and regions of the atmosphere where the response to enhanced aerosols in the ODS1960 and FIXED GHG1960 simulations will be of opposite direction, I would think that it would be difficult to imagine that the actual representation of the effects of volcanic aerosols in the NAT-h timeseries would be correct.

I suggest this is probably not a significant concern because the region of the lower stratosphere from 100 to 40 hPa is below the region where volcanic aerosols have the largest impact on the reactive nitrogen chemistry, although it is the region where the opposing responses of halogen and nitrogen chemistry to aerosols is important. And I am not sure how such a relatively rare event as large volcanic eruptions would contribute to noise in long-term signals. Although I would like to stress that there are reasons to believe the NAT-h simulation is not a good representation of the effects of volcanic aerosols on ozone that one would find in a proper natural historical simulation.

Aside from this footnote to the NAT-h simulation, my other comments are minor and are given below.

The first five paragraphs in the introductory text, over Pages 2 and 3, bounce around a bit from topic to topic, in particular the fourth paragraph (Page 3, Lines 1-9) that discusses linear trends for D&A in the middle of a general discussion of stratospheric ozone. Personally, I found it a bit difficult to follow the thread through the introduction.

Page 6, Lines 17-30 – The term 'emission' is frequently used through this section when I think the more accurate term would be concentration. For example, at Line 27 there is the statement '...while another keeps GHG emissions fixed at their 1960 conditions...' which would suggest the atmospheric concentrations continued to increase after 1960.

Page 6, Line 28 – Do the time-varying concentrations of ODSs in the FIXED GHG1960 simulations affect the radiative forcing of these coupled simulations?

Page 9, Lines Lines 11-18 – Here you argue for conducting the analysis of the lower stratosphere for both the global and extratropical regions. Given the discrepancy with SWOOSH observations for the tropics after 2005, I think this is fully warranted. My point would be about the possible explanation for the discrepancy in tropical ozone being related to the behaviour of volcanic aerosols. If the small volcanoes had significantly impacted ozone and was not properly accounted for in the database of specified aerosols used in the modelling, wouldn't the effect be most pronounced in the extra-tropical lower stratosphere? Reactive chlorine levels would be much higher than in the tropical lower stratosphere and since many of these small eruptions were in the extratropics I would think the aerosols would also be more prevalent in the extratropical lower stratosphere.

It must also be kept in mind that the agreement of different observational databases for the tropical lower stratosphere is not terribly good. See Section 2.2.4.3 of the WMO 2014 Ozone Science Assessment for the discussion of differences in post-2000 trends for the lower tropical stratosphere.

Page 12, Lines 13-18 – On the underestimation of variability in the upper stratosphere, part of the discrepency may be due to observational uncertainty as different ozone datasets have some significantly different representations of the magnitude of the solar cycle – see Maycock et al., Atmos. Chem. Phys., 16, 10021-10043, 2016. Chemistry-climate models also tend to have solar cycle variations in ozone that are towards the lower end of observational estimates – see Chapter 8.5 of SPARC CCMVal (2010) (SPARC Report on the Evaluation of Chemistry-Climate Models, V. Eyring, T. G. Shepherd, D. W. Waugh (Eds.), SPARC Report No. 5, WCRP-132, WMO/TD-No. 1526).

Page 16, Line 10 – the statement that 'NAT-h is nudged to reanalysis temperature and wind fields.' seems a bit misleading as it makes it sound like a 'Specified Dynamics' simulation where nudging is applied everywhere. The statement should be more specific to the nudging used here to produce the QBO.

Page 17, Line 8 – I am unclear what is meant by '...the noise data set N(x,p,t), which is constructed by concatenating the NAT, NAT-h and CTL simulations.' Does this mean a single timeseries was create by splicing all three of these simulations together, thus creating a $\sim$1250 year timeseries? If so, how would the resulting timeseries be used with the S/N analysis that begins in 1984?

Page 21, Line 18 – there is a erroneous bracket at '.. with methods 1 and 2 (respectively.'

Page 21, Line 28 – there is a word missing at '..method 2 yielded markedly S/N ratios...'

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