Response to RC1 and Referee #3:

Referee #3's comments are shown with a vertical bar next to them, our comments are shown below each of them.

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 methodology 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.

Referee #3 raises an important and valid point related to the linear additivity of the stratospheric ozone response to large volcanic eruptions, and the issue of how the NAT-h simulation was constructed. In the low halogen loading stratospheric conditions of FIXED ODS1960, increases in mid-stratospheric ozone following a large volcanic eruption are expected as the loss of ozone at these altitudes is dominated by NO_x, due to enhancement of N_2O_5 hydrolysis (e.g. Tie and Brasseur, 1995). In the enhanced halogen loading conditions of FIXED GHG1960 and ALL2, the mid-stratospheric increase is limited to higher altitudes, and ozone depletion will occur throughout the lower stratosphere. As Referee #3 correctly points out, NAT-h is constructed by summing the ensemble mean ozone responses in FIXED ODS1960

and FIXED GHG1960, and then by subtracting these responses from the ALL2 realizations. We recognize that the stratospheric ozone response to volcanic aerosols may not be well represented in NAT-h in the vertical region where the reactive nitrogen chemistry is dominant. Our lower stratospheric region (from 40 to 100 hPa) may indeed encompass the region of where ozone concentrations are influenced by the competitive effects of halogen and nitrogen chemistry.

Potential errors in the ozone response to volcanic eruptions in NAT-h are now acknowledged in the revised manuscript. Below, we argue that such errors are unlikely to have substantial impact on our primary D&A results.

To identify slowly-evolving ODS and GHG signals in observational estimates of stratospheric ozone changes, it is critical to have information regarding the natural variability of ozone on timescales of 2-3 decades. It is the background multi-decadal "noise" that is of most interest here. As shown in our Figure 3, NAT-h (which was obtained by subtraction) reliably captures the observed amplitude of the decadal-timescale variability of lower stratospheric ozone arising from external solar forcing. This is the region we are most concerned with in addressing the additivity issue raised by Referee #3. NAT-h also replicates shorter-timescale (< 3 year) observed lower stratospheric ozone variability associated with the QBO and ENSO.

Errors of the form mentioned by Referee #3 (i.e., errors in the lower stratospheric ozone response to short-term, episodic volcanic forcing) are unlikely to affect our estimates of the ODSonly and GHGonly fingerprint patterns, the projections of the SWOOSH ozone data onto these fingerprints, or the estimated variability on multi-decadal timescales from NAT-h. Any errors in the simulated ozone response to volcanic forcing will have largest impact on the short-term variability shown on the x-axis in Fig. 3 – not on the longer multi-decadal variability that is most relevant for the signal-to-noise (S/N) calculations.

Figures 8 E and F help to illustrate the robustness of our results to potential errors in the response of ozone to volcanic forcing. Consider S/N results for the ODS signal in the lower stratosphere in the "tropics excluded" case. This is the case most relevant to the Referee's concern, since the mid-latitudes are the regions where the opposing nitrogen/halogen chemistry would be important. For this particular example, S/N ratios for the full 33-year SWOOSH period are significant at the 5% level or better. To negate these significant results would require that noise trends are roughly 40% larger on the 33-year timescale. It is highly unlikely that an error of this magnitude could be caused by the ozone response to short-term volcanic forcing.

We fully agree that it would be preferable to directly estimate the ozone response to historical volcanic and solar external forcing – i.e., to have access to a simulation in which only volcanic and solar forcing are varied. Unfortunately, such a simulation was not available for our study – which is why we had to estimate the volcanic and solar responses by differencing simulations. We note that such differencing operations are not unique to our study. For example, in Gillett et al., 2011, their ODS response was estimated by differencing of combined forcing simulations and GHG single forcing simulations.

We thank Referee #3 for bringing this concern to our attention. We are confident that our attribution results would not differ significantly if we had access to a more comprehensive historical natural simulation (rather than estimating the volcanic and solar signals by subtraction). Nevertheless, the revised manuscript discusses Referee #3's concern and provides a better description of how NAT-h was actually calculated.

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.

We agree with Referee #3 and have reorganized the introductory text in the revised manuscript to ensure that the discussion of general attribution theory and stratospheric ozone changes are separated.

- 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.

Yes Referee #3 is absolutely correct, it should be concentration and not emission. We have changed all such occurrences in the main text.

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

Yes, the time varying concentrations of ODSs in FIXED GHG1960 are radiatively active. It is important, and has been made explicitly clear in the revised manuscript.

- 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 extratropical 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.

We have taken Referee #3's advice, and substituted our speculation in the revised manuscript about the difference being due to volcanic eruptions (which as Referee #3 points out is not a reasoned hypothesis) with the difference possibly due to errors in the observational data in the tropical lower stratosphere. We thank Referee #3 for offering a more rational explanation.

- 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).

Referee #3 is correct: we failed to mention that the underestimation of variability may also be due to observational uncertainty, particularly in terms of known deficiencies in the ability of chemistry-climate models to capture ozone variations related to the solar cycle. We have included the reference provided by Referee #3 and added a brief discussion of this point.

- 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.

We agree with Referee #3 that we did not properly describe how the QBO is implemented in WACCM. As with many comparable climate models, the QBO is imposed as an artificial forcing. This is achieved by nudging tropical stratospheric zonal-mean winds to either cyclic or fixed-phase winds, or to the observed winds. We have now revised and improved the description of how the QBO is forced in WACCM.

- 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 ~1250 year timeseries? If so, how would the resulting timeseries be used with the S/N analysis that begins in 1984?

All of the natural and internal climate variability simulations, NAT (1000 years), NAT-h (3x50 years), and CTL (200 years) were concatenated together to form a 1350 year noise data set, N(x,p,t). This was not that clear in the original manuscript. The revised manuscript contains a clarification of this concatenation procedure in Section 6.

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

Corrected in text, thank you!

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

Corrected in text, thank you!