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Interactive comment on "Causes of interannual variability of tropospheric ozone over the Southern Ocean" *by* Junhua Liu et al.

Anonymous Referee #2

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Review of Liu et al., Causes of interannual variability of tropospheric ozone over the Southern Ocean

The manuscript by Liu et al. presents an analysis of a series of runs with the Global Modelling Initiative (GMI) CTM driven by MERRA re-analysis to look at the inter-annual variability of ozone in the middle to upper troposphere in regions of the southern hemisphere. To investigate the contribution of stratospheric input on ozone, a diagnostic tracer of stratospheric ozone is included. To estimate the role of inter-annual variability in emissions, the difference between the full simulation and a simulation with constant emissions is used. Multiple linear regression and correlations are used to estimate the contribution of these influences on the year-to-year variability in the model ozone. The study finds a significant contribution of the stratosphere to ozone variability in the upper troposphere, even deep into the tropics, a finding that furthers our evolving understand-

ing of the significant role stratospheric input can have on ozone in the troposphere.

The paper is well written and clearly presents a well thought out analysis. I do not have any significant concerns with the material presented. My one methodological concern is the approach to quantify the contribution of stratospheric ozone (stratO3) and the interannual variability in ozone precursor emissions (emissO3). For example, for the South Atlantic region Figure 6 presents the multiple linear regression (MLR) of stratO3 and emissO3 against the model ozone anomaly. The combination of these two factors can reproduce a high degree of the interannual variability of the model ozone, up to nearly 76% for December at 270 hPa. To separate the contribution of stratO3 and emissO3, the correlation of the stratO3 term from the MLR against the original model ozone timeseries is calculated. Then the contribution of emissO3 is calculated from the correlation of the emissO3 term against the residual that results from removing the stratO3 contribution. During the original MLR analysis the stratO3 and emissO3 terms were simultaneously fitted to the ozone anomaly, but the contribution of stratO3 and emissO3 is calculated by correlation sequentially. The end result is that while the combined stratO3/emissO3 regression explains 76% of the variance for December at 270 hPa (Figure 6), individually stratO3 accounts for 61% and emissO3 accounts for 40% (Figure 7). Given the process of simultaneously fitting the stratO3 and emissO3 terms during the MLR, is not the correct way to calculate their individual contributions to regress these terms individually against the original timeseries? I would argue that if correlation of stratO3 accounts for 61% of the variance, then emissO3 should account for approximately 15% since the combination of the two accounts for 76%. The process seems to work in the extreme where one component explains all of the variance – the south Atlantic at 270 hPa in August, for example – but for cases where both components contribute substantially the approach of regressing the second term against the residual seems to give an inflated estimate. This could be because the process of calculating the residual by removing the contribution from the first term has also removed a large fraction of the variance? And since there is no correct order to which of the two terms is fitted first and which is fitted second, they both should be correlated against the same (original) timeseries. Following this approach one could argue that emissO3 explains a certain fraction of the residual variance, but one could not directly compare the stratO3 and emissO3 correlations.

The change in methodology argued for above may have some impact on the conclusion of the relative importance of stratO3 and emissO3 for certain regions at certain times but I do not see how it would fundamentally alter the conclusions of the paper.

My other comments are mostly minor and related to specific parts of the paper. They are detailed below.

Lines 103-104. A minor quibble that part of the treatment of lightning NOx is discussed here, where it is stated that the global total is fixed at 5 Tg-N/year, and part is discussed at Lines 146-148. It would help the reader to rework a bit these two parts to combine them in one place.

Lines 103-104. If lightning NOx emissions are held constant, how do you derive the interannual variability in lightning NOx that is used in the correlation shown in Figure 14. It must be the variability over a particular region, but I am not sure I found where that is discussed.

Lines 143-145. I guess it is obvious that the run with constant emissions fixed at the year 2000 levels means that the annual cycle of year 2000 emissions repeats. Sorry for another quibble, but it would help remove any doubt if the wording were more explicit.

Lines 159 - 162. Here the stratO3 tracer is discussed. When it is stated that the stratO3 tracer is 'removed in the troposphere with the same loss frequency...' is that the same loss frequency as Ox and how exactly is Ox defined? Would you know the global average tropospheric O3 lifetime that you would derive from the loss frequency you used for stratO3?

Line 222 . '...represents [the] fraction of tropospheric ozone from [the] stratosphere...'

Line 237. I would suggest removing 'of' from 'Within the Atlantic, despite of the...'

Lines 259 – 261. Here it is mentioned that the interannual variability in the GMI simulation is larger than in the GMAO assimilated ozone for the two tropical regions. Is there any additional information that could be provided as to why this may be the case? Perhaps some comparisons from the Wargan et al. (2015) paper against independent observations or the role of emissions in the assimilation that is mentioned at Lines 277-279? This would seem to be an important component of the comparison if one is to have confidence in the analysis of interannual variability presented later in the paper.

Lines 367 – 369. The statement on the relative contribution of emissions to ozone variability at 270 and 430 hPa will probably need to be revisited if the method of attribution is revised as argued for above.

Lines 454-456. On Figure 12, it would be interesting to see the same fit of ozone with lightning at 430 hPa as is shown for 267 hPa.

Line 484-485. 'Figure 14 compares the model residual after removing the contributions from StratO3 and EmissO3...' and I would raise the same concern that the analysis is overestimating the contribution of lightning to explaining the variance in ozone.

Lines 552 – 556. Because the correlation of lightning with ozone variability is negative, the authors suggest deep convection is having a negative effect on ozone in the upper troposphere by lofting clean surface air. I agree that could definitely be a possibility, but can you rule out that the correlation is signalling some other effect? Perhaps circulation changes that are associated with the interannual variability in deep convection?

Lines 825-829. The colour scale on Figure 1 indicates it is ppb and it should be DU as I understand it.

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