Anonymous Referee #2

Review of Zeng et al. Attribution of recent ozone changes in the Southern Hemisphere mid-latitudes using statistical analysis and chemistry-climate model simulations

This study uses statistical analysis to understand recent changes in ozone up to 25 km at Lauder, NZ and relate them to changes over similar and longer time periods in chemistry-climate model simulations. Overall the manuscript is clearly written and analysis explained well and I think it would be of interest to the ACP community and would recommend its publication after the authors take some mostly minor comments into consideration.

Thanks very much for the reviewer's positive comments.

General comments:

I have concerns about using a chemical measure (O3) for tropopause height to evaluate the O3 field itself. Can you demonstrate that this measure is the same as a PV or temperature based tropopause height over varying timescales?

We agree that PV would be the ideal quantity to define the tropopause. Unfortunately, we don't have observed variables to calculate PV. We have modified our tropopause height definition to that of Bethan et al. (1996), based on the vertical ozone gradient, which is consistent with the PV definition (e.g., Beekmann et al. 1994). We have recalculated the tropopause height and have incorporated the new tropopause height in our regression analysis. Consequently, the trend in tropopause height at Lauder has changed from 14.2 m/yr to 17.9 m/yr. We have also calculated the tropopause height based on the WMO lapse rate definition, and the result is similar to that based on the ozone definition (trend: 16.5 m/yr). There is no significant systematic difference between the calculated ozone tropopause height and the thermal tropopause height at Lauder. We have revised the relevant text (page 3, lines 24-28), figures (2, 3, and 4) and tables (2 and 3) based on the new tropopause definition.

I can understand on shorter timescales how the level of o3 of 150 ppb would reflect dynamical variability in tropopause height (although one could argue that a different concentration _100 ppb might be a better choice) but on longer timescales it seems like the chemical changes (through ODS changes) would also be reflected in this quantity. Can you quantify and separate and include some discussion in the text of this issue?

We have now adopted the new definition of tropopause height based on ozone gradient greater than 60 ppbv/km, constrained to ozone mixing ratios greater than 80 ppbv and exceeding 110 ppbv immediately above the tropopause (Bethan et al., 1996). Longer time-scale O_3 changes will inevitably affect the tropopause movement through their feedbacks to the dynamics, and it is not straightforward to separate the influence from chemical changes and those from dynamical changes. We are confident that the new ozone tropopause definition adopted here is a robust choice, and is also consistent with the thermal tropopause height in this case. Moreover, O_3 changes in the tropopause region over this period (1987-2014) at Lauder are around 17-25 ppbv, which is significantly smaller than the vertical O_3 gradient in this region (typically 50-70 ppbv/km).

What does chemical O3 tropopause changes look like over the full ref-C2 run can you see the impact of ODS changes causing o3 loss and recovery reflected in this quantity? If you can't see this impact it would be a good demonstration that it is essentially a dynamics only representation over both short and long time-scales. If you do it can be quantified and related to the smaller change in ODSs over the Lauder record

Using the revised ozone tropopause definition, the calculated trend in tropopause height from the REF-C2 simulation averages about 1.2 m/yr over 1960-2090, and does show some difference between the ozone depletion (4.1 m/yr in 1960-2000) and recovery periods (0.9 m/yr in 2000-2090). Ozone changes do affect dynamics through feedbacks, so we see a larger than average change in tropopause height during the ozone depletion period. This is just for clarification; we do not intend to discuss this in great detail in the present paper.

I understand that the Lauder record 1987-2014 includes only a modest change in ODSs peaking in the 1990s with little net change so a linear trend might be expected to be flat but it doesn't rule out a some ozone loss in the early period and gain in the later period.

Both REF-C1 and REF-C2 show some loss of stratospheric O_3 from 1990-2000 and recovery from 2000 onwards (Figures 9 and 10). So there is some O_3 loss in the early period of the record and gain in the later period. But it is hard to ascertain such a signal from the O_3 sonde record at Lauder, due to large variability in observed stratospheric O_3. Notes have been added in the text (Page 12, lines 5-10)

Can you explain why the variables in Table 3 are in some cases different than those shown in Figure 3 like surface Temperature which seems to be highly correlated to lower troposphere ozone is not shown in figure 3.

We have added surface temperature as a regression function and related discussions, and have modified Tables 2 and 3, and Figures 2 and 3. (Sections 2.2 and 3.1)

p5 line 17-18 it seems a bit circular argument if you are using o3 to define the tropopause height.

We have changed the definition of tropopause to be based on the vertical ozone gradient, and the statement stands.

p10 line1-3 Can you make the same claim for ODS changes over this time refC1 time period (difference between RefC1 and fODS).

We did not perform a "SEN-C1-fODS" scenario which could be compared to the REF-C1 simulation, therefore cannot directly compare between them. The SEN-C2-fODS simulation cannot be directly compared to the REF-C1 simulation due to differences in model constellation between them.

p10 line 34 p11 line 1-2 it is not obvious why this should be the case, can you do any additional analysis to explain possible mechanisms.

Unfortunately, we cannot explain the response of stratospheric O_3 trend changes to changes in O_3 precursors, which are mainly originated from the troposphere, based on current diagnostics and model simulations. We suspect it is caused by dynamical changes through chemical feedbacks to radiation. We plan to follow up with a more objective analysis. We have added a note to this effect in the text. (Section 4.4; Page 12 lines 14-16)