

Comment on wcd-2021-43

Anonymous Referee #1

Referee comment on "Twenty-first-century Southern Hemisphere impacts of ozone recovery and climate change from the stratosphere to the ocean" by Ioana Ivanciu et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-43-RC1>, 2021

This manuscript examines the impact of ozone recovery and increases in greenhouse gases over the 21st on the Southern Hemisphere, including changes from the stratosphere to the oceans, using ensembles of chemistry-climate model simulations with differing ozone and GHG fields. The manuscript contains several interesting results that are suitable for publication in Weather and Climate Dynamics. However, before publication I think the presentation needs to be more concise (and in one aspect more precise), and the analysis of the ocean changes needs to be broadened.

Major Comments

1. The paper is very long, and needs to be more concise. This is especially the case for the Abstract and the Introduction. For example, it is not until page 5 of the Introduction that what is actually done in the paper is mentioned. I suspect you will lose readers before they get to this stage. While the review is good, much of it is not needed to justify the analysis done.

Also the discussion section repeats much of what is in the results section, and I think could easily be removed (with maybe a few sentences comparing to previous studies included in the results section).

2. Some of the text discussing the differences of the INTERACT_O3 and PRESC_O3 runs is misleading, or at least could easily be mis-interpreted. For example in both the abstract (line 9-10) and Conclusions (line 1049) there are statements about significance differences between the INTERACT_O3 and PRESC_O3 simulations that will read as if these differences are due whether the ozone field is prescribed or calculated interactively. While interactive vrs prescribed may be causing some differences, no evidence is provided to show this is the major cause. Given the differences in the ozone fields shown in Fig 1, I

actually think this the major cause of the differences between runs and not interactive vrs prescribed ozone. This is mentioned in section 5.7 and the conclusions, but more focus is put on the prescribed versus interactive aspect in these discussions, and in other places the writing is such to strongly indicate it is due to ozone not being interactive in the prescribed ozone run.

Note, I don't understand the statement on line 981 "the fact that the differences in some fields are as large as the impact of ozone recovery, it is likely not the only cause".

I don't disagree that fact ozone is no interactive in prescribed run will likely be making a cause, but to make strong statements on this you would need to do PRESC_O3 runs that use ozone from the INTERACT_O3 (as has been done in many previous statements). Until you do this I don't think you can make statements on the importance of the ozone being interactive.

3. I find the choice of ocean diagnostics a little puzzling. I know that the Agulhas leakage is an important aspect of the ocean circulation, but I am not sure it is the most important aspect to focus on and I think before focusing on a regional aspect (e.g., Agulhas leakage) I think there should be some discussion of these more hemispheric aspects.

In fact there have several recent studies that have shown the impact of ozone depletion on the meridional overturning circulation, sea surface and interior temperatures, and sea-ice (e.g. Li et al 2016, Ferreira et al 2015, Seviour et al 2016, 2019). These aspects and studies should be discussed in the Introduction, and some analysis of these fields should be included. Doing this would not only present a better view of the ocean changes, but would also enable comparison with these studies (esp the Li et al 2016 and Seviour et al 2016 studies which also examined aspects of the impact of using prescribed ozone). For example, are the differences in these aspects between INTERACT_O3 and PRESC_O3 consistent with these studies?

Minor Comments:

Line 4: "... a unique coupled ..." Why "unique". I guess at one level all models are unique, but I don't see what is specifically unique about model used here.

Line 300: I think the evolution of TCO (Fig S1) should be shown in the main text, and as well as showing INTERACT_O3 the evolution of other runs should be shown. As well as interpreting differences between runs shown here this will have put the ozone evolution in context of other CCMs studies.

Line 310-314: It is stated that the ozone fields show a similar vertical and seasonal structure of ozone recovery, but when I look at fig 1 I see big differences in magnitude and vertical structure in SON months, and in the following sentences some of these large differences are discussed. So I think it is not correct to say similar unless you describe what is meant by this.