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Comment on wcd-2021-43

Anonymous Referee #2

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-RC2, 2021

Review of Ivanciu et al

The study by Ivanciu et al investigates the effects of future changes in both ODS (leading to ozone recovery) and GHG concentrations on the stratospheric circulation, and consequent effects on surface winds and oceanic circulation. The study adds to the existing literature on the individual and combined effects of ODS and GHG changes, mostly confirming past results on stratospheric(-tropospheric) circulation changes. A new component of the work by Ivanciu et al is the use of a coupled ocean-atmosphere model with a relatively well resolved ocean, including nesting of some regions to eddy-resolving resolution. The paper includes the investigation of southern hemispheric ocean circulation changes induced by ODS/ozone and GHG changes. Thus, the study presents analysis of circulation changes spanning from the stratosphere to the ocean. The results on the stratospheric circulation are presented in detail, and given that they mostly confirm past work the section could be condensed at places (see general comment below). The authors claim to have found a new result in that the GHG-induced temperature/zonal wind changes bear hemispheric asymmetry. However, the physical interpretation of the asymmetry given in the paper is in my opinion misleading (see major comment). Therefore, I overall can recommend the paper for publication in WCD only after major revision. Note that my review focuses mostly on the stratospheric/atmospheric part of the analysis in the paper, as the oceanic analysis is beyond my expertise to judge.

Major comment

The authors stress at multiple places in the study the result of hemispheric asymmetric GHG-induced changes in temperature, winds and wave activity, and claim for example that "GHGs bring an important contribution to the total spring temperature changes at this

height. This contribution is largely underestimated if zonally-averaged fields are investigated (Fig. 2b), as the cooling pole offsets a large part of the warming pole" (line 367-369).

This statement seems to imply that the GHGs exhibit an asymmetric forcing, that is missed if the zonal average is considered, seemingly implying that there are distinct different physical processes at work over the eastern versus western hemisphere. As the authors do state later in the paper, the asymmetries are related to dynamical processes, namely planetary wave activity. Clearly, the asymmetric features the authors refer to are the signature of a planetary wave (of wavenumber 1). As such, the asymmetric response is simply the result of changes in PW activity, and thus one single hemispheric phenomenon and not distinct processes in the two hemispheres. The PW signature noted in the paper is an interesting result, but it should be interpreted clearly as a change in planetary wave amplitude (and possibly phase) that is brought upon by the GHG-induced changes to the circulation and likely non-linear changes of the propagation conditions of PW 1, rather than implying that the hemispheric asymmetric response is an inherent response to the GHG-forcing directly.

The well established (linear) theory of (stratospheric) general circulation dynamics decomposes the atmospheric flow into a zonal mean component and deviations from this flow, i.e. atmospheric waves, that propagate on the zonal mean "background". The interaction of the planetary waves and the background zonal mean flow is the dominating process that forms the stratospheric circulation. Employing this framework, the results can be interpreted more easily and related to the known mechanism of stratospheric dynamics: The temperature anomalies suggest a reduction of PW amplitude and slight phase change (see Fig. 5b / e). This is consistent with reduced vertical wave propagation (as measured by the eddy heat flux, Fig. S5) in Oct/Nov, and consequently reduced EP flux divergence (see Fig. S6), driving a reduction in the residual circulation below (Fig. 10), consistent with the well known "downward controlled" wave driving of the (steady state) residual circulation. How the reduction in PW activity comes about is another interesting research question, that probably would involve more in-depth analysis and might not be straight forward to answer due to the highly non-linear nature of wave - mean-flow interactions.

The authors employ the 3-D Plumb flux to investigate the changes in wave activity, and find again opposing signals in the hemispheres - which again is consistent with a PW 1 signature. The 3-d wave activity flux is a useful tool to study local effects of wave propagation of smaller (higher wavenumber) waves, but a planetary wave is by definition a planetary phenomenon, and therefore the 3-d wave activity flux is in my opinion not suitable to study planetary wave propagation. Further, it cannot be easily used to explain the changes in the residual circulation, opposed to the conventional EP flux divergence.

Therefore, I recommend that the authors revise the interpretation of their results throughout the paper, namely interpreting the asymmetric response as a change in planetary wave activity. Employing a wavenumber decomposition of the fields and calculating the wave (EP) fluxes for the individual wavenumbers would help to build a consistent interpretation of the simulated changes in the stratospheric circulation.

This extends to the discussion, where it is stated that "contrasting results can be explained by intermodel differences in simulating the strength of the GHG-induced change within each pole, which lead to different levels of compensation between the warming and the cooling pole."

I agree in that there are (likely) large intermodel differences, but the interpretation here would rather be that different models simulate the changes in planetary wave activity differently. This is again likely a result of the highly non-linear nature of wave - mean-flow interaction, so that for example slight differences in the model basic state might lead to a difference in the response of planetary wave / polar vortex dynamics. This might be somewhat analogous to the northern hemisphere, where models simulate a large range of different responses of the polar vortex to increasing GHG concentrations, and the reasons for this are still not entirely clear (e.g., Wu et al, 2019, GRL).

Minor comments

General comments:

1. The authors compare one set of all-forcing simulations with interactive chemistry to a simulation in which the ozone field provided for CMIP6 is prescribed. This is a fair and useful comparison (as the CMIP6 ozone is used by many models), but the comparison of the simulations mixes two effects: 1) interactive versus prescribed ozone and 2) using an entirely different ozone field (model's own ozone versus CMIP6 ozone). Those different effects are acknowledged at a couple of places in the paper (e.g., line 414), but I would recommend to clarify this from the beginning (e.g. line 173; line 229) to not mislead the reader into thinking this comparison would quantify the effects of the interactive nature of ozone alone. The authors might even want to consider to rename the "prescribed O3" simulation into "CMIP6-O3" to avoid this misinterpretation. Apart from the interactive nature of ozone in one set of simulations (which is certainly more realistic than prescribed ozone in general), it is hard to judge which ozone field will represent future ozone changes more realistically - so the comparison does rather tell us about the possible uncertainty in climate projections arising from a different treatment of ozone. The authors might want to consider to rephrase a few sentences to acknowledge this fact more clearly (e.g., line 964:: "GHG effect is more dominant when the ozone field is prescribed." - this likely depends on the exact ozone field that is prescribed, rather than on whether it is prescribed or calculated interactively).

2. The paper presents extensive analysis and diagnostics, and is as such very long. The

authors might want to consider to condense the material at a few places (in particular Sec. 4.1.1-4.1.3; and in Introduction, Discussion/Summary, see specific comments below). It might help to shift the focus on the new results, e.g. on the (mechanism for) GHG-induced PW attenuation and weakening of circulation in spring in Section 4.1.

Specific comments

- line 54: to my knowledge the evidence of effects of ozone depletion on carbon uptake are very weak and the link is not established (see e.g. WMO O3 assessment, Chapter 5, 2018).

- Introduction general: The description of background on the Agulhas leakage is very detailed - maybe it could be condensed a bit to the main points? Likewise the section of ODS-induced ozone depletion is very detailed - could condense.

- line 144 ff: Maybe rephrase - there have been many studies on past and future ozone and GHG effects (see e.g., WMO 2018), but what is new here is the in-cooperation of a (high-res) ocean model together with a CCM with interactive chemistry. This is stated in the following paragraph(s) (line 168) - consider to put this up front and combining the paragraphs to clarify.

- line 197: It would be helpful to add the boundaries of the nests in one of the Figures.

- line 242: Please include "quasi-geostrophic" to clarify that this is the QG version of the EP flux.

- line 250: Mostly the convention is to add a star (*) to the streamfunction as well to avoid confusion with the Eulerian mean streamfunction.

- line 287: consider reformulating to: "... CH4 cause an overall ozone increase, and N2O an overall ozone loss."

- line 304: CMMI -> CCMI

- line 345: "top of the tropospheric westerly jet ": in literature, this is often referred to as "upper flank of the subtropical jet", which is possibly a more specific phrasing.

- line 346: Are the effects linear (i.e. GHG+ODS = GHG&ODS) ? In particular for dynamics that are highly non-linear, this cannot a priori expected. Maybe add a short note?

- Fig. 4/5, line 359: Please add also climatological contours to Fig. 4 (as is done in Fig. 5), to simplify interpretation of the changes (are pattern attenuated or amplified?).

- line 360: "due changes" -> "due to changes"

- line 432: consider rephrasing - the STJ does not extend into the stratosphere, but the wind changes up to the top of the stratosphere are rather changes in the polar vortex. For example consider: "... an acceleration of the STJ and enhanced westerly winds throughout the stratosphere"

- line 455: a planetary wave is better characterized by the wavenumber-decomposed EP flux rather than local wave activity changes, as it is planetary (and thus non-local) by nature (see major comment).

- line 465: I strongly disagree with this interpretation. The "critical layer control" mechanism explained in Shepherd and McLandress is caused by a forcing-induced wind change (GHG warm the upper troposphere, thus inducing the wind changes by thermal wind balance), while it is not obvious why GHG forcing would lead to asymmetric changes in the zonal wind (GHG are well-mixed, so their purely radiative effect has to be zonally symmetric). Rather, the asymmetric zonal wind changes are a signature of the planetary wave itself.

- line 477: In this case I agree with the interpretation: in the case of ozone forcing, there is a clear thermodynamic stating point (ozone changes drive temperature changes), that subsequently modifies the background wind through thermal wind balance; the PW propagation responds to the changed background winds. Again I strongly would recommend to include the (wavenumber decomposed) EP flux diagnostics instead of the 3-d wave activity flux in order to interpret the dynamical changes related to planetary waves.

- line 505: The introductory sentence of the section clarifies that only the residual circulation is analyzed here rather than the entire BDC - why not renaming the section to "Residual circulation" ?

- line 515: should read "north of ..", right ?

- line 543: I would recommend to show Figure S6 of the total EP flux divergence in the main paper, as it provides a more quantitative link between wave activity to the changes in residual circulation (see major comment).

- line 550: I agree with this interpretation. Could overlay wind changes and EP flux changes to highlight this effect more quantitatively.

- line 571: So if the mechanism of GHG-induced residual circulation changes were to be investigated in more detail here, you would need to analyze EP fluxes / residual circulation outside the polar cap, right? I leave it to the authors if they want to go into more detail here.

- line 601 ff: consider moving this paragraph to the discussion section? This is rather speculative as it is hard to judge whether indeed the higher resolution ocean model leads to more realistic simulation of SST-related effects.

- line 671: I assume you refer to an uncoupled simulation? Please clarify.

- line 766: Do you see signs of this effect on the long-term trend in the simulations? Is it possible to quantify the effects of the Agulhas leakage on the Atlantic circulation trends?

- line 795: Possibly a scatter plot of wind stress versus ACC (similar to Fig. 14) could help to quantify this effect?

- Discussion / Summary general comment: The Discussion does in large parts summarize the results already; thus the Summary does duplicate the Discussion section to a large degree. I would recommend to either combine the two sections into one "Discussion and Summary" section, or avoid duplication by only focusing on the critical points in the discussion.

- Section 5.3: possibly combine with 5.2, as the wind, residual circulation and wave activity changes are closely coupled and some discussion is duplicated.

-Discussion of ocean circulation changes: I wonder on the effects of having the "nests" of high-resolution ocean model - could the only partly high-resolved ocean induce artefacts, or would you expect even more compensation if the whole ocean was simulated at the resolution of nests? Consider adding a remark on this.

- line 980: consider citing recent studies that investigated those effects of interactive ozone.

- line 990: remove () around Li et al 2016.