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Review on egusphere-2022-1267

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Referee comment on "The role of tropical upwelling in explaining discrepancies between recent modeled and observed lower-stratospheric ozone trends" by Sean M. Davis et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1267-RC1, 2023

Review on:
"The role of tropical upwelling in explaining discrepancies between recent modeled and observed lower stratospheric ozone trends", ACPD, 2022, by Davis, S. M. et al..

In their paper, Davis et al. analyse a suite of WACCM model experiments with, without, and with different sorts of nudging as to whether the simulations can reproduce observed ozone trends from the Ball et al. (2018) study. They find that a misrepresented trend in tropical upwelling in the original simulation setup leads to some of the ozone discrepancies. Particular nudging setups help to get closer to observations with some regard, but surprisingly, the free-running simulation (with nudged QBO) represents the ozone trends best. Overall, I think this is an interesting and well-written paper, the method and the analysis are elaborate and the study reveals some new insights to this topic, which has shaken up the community for several years now. I would be happy to see the paper published in ACP soon. I do, however, have a few remarks that I think are important to consider before publication and two of these, I guess, should be viewed as major points. Revising the paper in that sense should not be too cumbersome, though, please see below.

Major issues:
• Sect. 3.4: The discussion about mixing appears oversimplified to me. What type of mixing is it that you diagnose? Is it parameterised horizontal/vertical diffusion or is it Kyy or some of such diagnostics (see e.g. Abalos et al. (2016, 10.1175/JAS-D-16-0167.1) or Eichinger et al. (2019, 10.5194/acp-19-921-2019))? Moreover, did you consider the diffusivity of the model's advection scheme? I assume this is still included in what you call 'advection'. If that is the case, the results may be somewhat misinterpreted. See Dietmüller et al. (2017, 10.5194/acp-17-7703-2017) for details and for some quantification of this effect. Moreover, please consider e.g. Eichinger et al. (2019, 10.5194/acp-19-921-
2019) for the discussion of the impact of mixing on tracer trends and Dietmüller et al. (2021, 10.5194/acp-21-6811-2021), Orbe et al. (2020, 10.1029/2019JD031631), Ball et al. (2020, 10.5194/acp-20-9737-2020) for discussing the influence of mixing on ozone in the extratropical lower stratosphere. Revise also L390-391 accordingly.

• My other concern (which is partly linked to the above) is the averaging over 60S-60N. It has been shown in several studies, and the present study shows it again, that different processes are at work in the tropics and in the extratropics with regard to LS ozone trends and variability, see for example Dietmüller et al. (2021, 10.5194/acp-21-6811-2021), Orbe et al. (2020, 10.1029/2019JD031631), Ball et al. (2020, 10.5194/acp-20-9737-2020). The tropics are controlled by dynamical upwelling within the pipe, in the extratropics, mixing is important, but extratropical downwelling and possibly other processes can have an impact as well. I understand that the trends from the Ball et al. (2018) study are meant
to be reconciled, but in light of the studies above, I think doing it for 60S-60N is prone to be misleading in interpretation. The relevant figures (mainly Figs. 4 and 6) should be shown for tropics (as in supplement) and extratropics separately and the results should be discussed with respect to the different regions then as well. Also the introduction completely lacks these points, which I think are very relevant for the topic. In Sect. 3.3 this point is already taken up, but only very (too) briefly. I assume that when doing the separation, more than 'roughly half' (see L298) can be explained in the tropics, and it will be very interesting to see how much can be explained in the extratropics.

Minor issues:

• L13: Add (short or half) sentence that tropical upwelling is important for ozone in LS.
• L15: state more precisely the time span you are analysing
• L24: Due to the upwelling trends again, or other reasons too? Maybe state briefly here.
• L27: ... Montreal Protocol in 1987, the atmospheric concentration of ozone depleting substances (ODSs) is declining, and ...
• L55: "replay" simulations. Is that something people know about, I am afraid I do not. Can you explain what it means?
• L74: The terms 'nudging timescale' and 'meteorology frequency' are not self-explanatory, can you modify the wording or explain it more precisely please.
• L80: Would there be a citation for this paradoxicality? If not, can you explain how this comes about?
• Section 2: Partly for the sake of reproducibility, partly for transparency, can you add some information on the following:
– There are some nudging parameters, such as nudging strength, can you provide it, do you vary it, is this a standard setting, have you considered varying it, is there history on that?
– What is the approximate vertical resolution in the UTLS in your setups
– Do you nudge in all altitudes, i.e. in all levels? If not, where not?
– Is nudging performed in grid-point space or in spectral space in WACCM?
– What SSTs and SICs do you use? Ì☐
• L105: I understand now that with 'native' levels, you mean the ERA-I levels, that was not clear to me when I first read it, please clarify. Do you conduct this simulation simply because it is technically possible, and you thought this might have an impact, or is there some other rationale behind this experiment?
• Sect. 2: I think you'll have to argue why you didn't perform a simulation nudged to ERA-5. ERA-Interim is outdated, I think also the (tropical upwelling) trends in ERA-5 are different now. This simulation is clearly missing. Maybe you can argue that MERRA is assimilated in ERA-5, but I am not sure if that is enough, or do you want to keep it for a follow-up study? If a simulation of that kind should be available (or doable on the quick), include it please, if not, at least discuss the possible differences when using ERA-5 for

nudging.
• L111: Usually simulations like that are stuck in one QBO phase, can you state in which one it is here?
• L113-125: Can you be more precise about the diagnostic calculations here. I understand that you calculate w*-based upwelling from the model and v*-based tropical upwelling from the reanalyses, but I don't undestand why you would do that. I think this should be done as consistent as possible. Moreover, for usage of w* please consider our paper Eichinger and Sacha (2020, https://doi.org/10.1002/qj.3876), such that density is correctly chosen for upwelling calculation for the particular w*, this can have an impact on the upwelling trends.
• L146: It is here a little unclear why you use 85 hPa, the standard in literature is 70 or 100 hPa. Later on it makes sense, as you have the best correlation there, but here this choice requires an explanation. Or was this level chosen in previous papers already (and I missed
it)? If so, please state it.
• L231: State why you would expect that.
• Sect. 3.3: I was sceptical at first, when I read that you want to conclude from variability to trends, as the processes that control variability (ENSO, QBO, seasonal cycle, ...) are different from those that control decadal trends (GHG and ODS emissions,...), however, I think the way it is conducted in this paper is absolutely fine. What I would still ask you to do is to write a few words about this, and why you think you can actually conclude from variability to trends in your case, mainly to encourage readers to think about this before doing it.
• Discussion around Fig. 4: Maybe it could help to mention the influence of chemistry (and dynamics) when it comes to explaining the low correlations above around 50 hPa.
• Sect. 3.4 Could you additionally show the impact of chemistry on ozone trends (maybe in appendix or supplement). Would these 3 parts then (in the stratosphere) add up to the total tendencies or what else would still be missing?
• L381: What bird-shaped pattern? You never mentioned this before, where was that?

Technical issues:

• L13: ....that, despite the nudging, the ...
• L17: ...lower stratospheric ozone
• L45: ...variability that strongly determines ozone variability ...
• L60: ...stratospheric ozone decline...
• L145: runs ☐ simulations (and many other times)
• L162: don't ☐ do not
• L182: remove 'also'
• L199: Remove (or replace) 'Interestingly' (everything you write is interesting, right!)
• L200 and in many other places: To my understanding, the lowermost stratosphere is the part in the extratropics that is at around the pressure altitude as the tropical upper troposphere. So in the tropics there is no lowermost stratosphere, but rather just the 'tropical lower stratosphere'. Please check if I am mistaken, if not, change it throughout the paper.
• L210: State what latitudes you are talking about here.
• L230: State what latitudes you are talking about here.
• L257: include 'in the lower stratosphere'
• L260 remove 'are'
• L301: remove 'very'
• L314: don't ☐ do not
• L344-348: Split sentence into 2 or 3 sentences.

- L352: Remove first ’the’
- L368: Remove ’highly’
- L376: variability □ trends and variability
- L395: remains unexplained, or, is still unexplained
- L418: Something seems wrong here, ’dlmmc’.