EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-701-RC2, 2022

Comment on egusphere-2022-701

Anonymous Referee #2

Referee comment on "The historical ozone trends simulated with the SOCOLv4 and their comparison with observations and reanalyses" by Arseniy Karagodin-Doyennel et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-701-RC2, 2022

Review of "The historical ozone trends simulated with the SOCOLv4 and their comparison with observations and reanalysis", by Karagodin-Doyennel

GENERAL COMMENTS

I would like to start this review by acknowledging the passing of William Ball, one of the co-authors on the paper, a wonderful person and a great scientist. He will be sorely missed within the ozone research community.

This is a relatively straightforward paper that makes use of the SOCOLv4 Earth System Model to simulate changes in ozone from 1985 to 2018 with a focus on understanding trends in lower stratospheric ozone and how they change between the "ozone depletion phase" (1985-1998) and the "ozone recovery phase" (1998-2018). I have made some suggestions for corrections below. Once these have been addressed, the paper will be suitable for publication in EGUsphere.

SPECIFIC COMMENTS

Line 2: On the use of the word "recover" and "recovery": Whenever the word is used it needs to be made clear what ozone is recovering from. In this case, presumably, from the effects of ozone depleting substances (ODSs). Then a clear distinction needs to be made between (1) ozone increasing, and (2) ozone recovering from the effects of ODSs. One may occur without the other. Ozone in the tropical lower stratosphere may be decreasing while still recovering from the effects of ODSs. So, let's see how this term is used through the remainder of the paper. But here, in the first line of the abstract, I want you to be clear as to whether you are referring to (1) ozone increasing, or (2) ozone recovering from

the effects of ODSs (which then requires a clear attribution to decreasing concentrations of ODSs).

Line 3: Replace "Amendments" with "Amendments and Adjustments". Likewise on line 20.

Line 5: So, in this regard, here is what I wrote in a review of a William Ball paper in 2019 as I suspect it is going to be relevant to the rest of this review:

"So are you really saying that the Montreal Protocol is working only in the upper stratosphere and not in the lower stratosphere? This will hugely concern policy-makers. They will wonder why all the hard work they have done since 1987 in reducing emissions of CFCs, halons, HCFCs and other ODSs has only decreased their concentrations in the upper stratosphere. Could I put it to you that the Montreal Protocol has been effective in reducing ODS concentrations, and thereby concentrations of Cly and Bry throughout the atmosphere, and that, as a result, ozone throughout the atmosphere, including the lower stratosphere, is recovering from the effects of those ODSs. Is this recovery apparent in observations in the upper stratosphere? Apparently yes. I say apparently only in that (at least in this paper) a thorough attribution of the drivers of those ozone increases has not been done. Is this recovery apparent in observations in the lower stratosphere? No, clearly not? Why not? Well because other factors have been affecting ozone (not diagnosed in this paper) that are likely (we cannot be sure since a thorough attribution has not been done) overwhelming the increases brought about by reductions in concentrations of Cly and Bry. Wouldn't that be a more accurate picture to communicate to policy-makers?"

Please note that that long comment was in response to the Ball 2019 paper and not to the current paper. Your statement that "continuing decline in the lower tropical and mid-latitudinal stratospheric ozone" is, in no way, an indictment of the MPA.

Line 10: Yes, I think it is valid to refer to 1998-2018 as the "ozone recovery" period since that is when stratospheric loading of Cly and Bry was decreasing and ozone was recovering from its effects (even if ozone was not everywhere increasing).

Line 11: Does SOCOLv4 show clear ozone recovery or does it show statistically significant positive trends in ozone? They are NOT the same thing since ozone could be decreasing but still recovering from the effects of ODSs. The first (ozone recovery) requires *attribution* to declining concentrations in Cly and Bry. The second (statistically significant positive trends) is purely the result of statistical analyses and requires no attribution - strictly a *detection* issue. Please always be clear throughout this paper which you are referring to and do not conflate the two.

Lines 12-17: It feels like the last two sentences of the abstract contradict each other. The penultimate sentence says that SOCOLv4 and observations disagree. The last sentence

says that there "is in general agreement with observations". This apparent inconsistency needs to be resolved.

Line 20: I wouldn't say "are beginning to take effect". I would say "have clearly taken effect over the past 25 years".

Line 20: In this case I think that what you mean instead of "observable ozone recovery at certain latitudes and altitudes" is "observable ozone increases at certain latitudes and altitudes".

Line 21: I think that it would be better to use the word "simulation" here rather than "projection". These studies weren't done to make projections in the same way that the IPCC models are used to make climate projections.

Lines 23-24: I would suggest replacing "time of the ozone recovery back to the pre-1980 level for different regions" with "time of the return of ozone to pre-1980 levels for different regions".

Line 25: I would say mid to late 1990s depending on where you look in the stratosphere.

Line 29: And now, again, you are conflating ozone recovery from the effects of ODSs with the return of ozone to pre-1980 levels. They are not the same thing. For example, tropical lower stratospheric ozone has been recovering from the effects of ODSs since the late 1990s, as you state. However, it is possible that tropical lower stratospheric ozone never returns to pre-1980 levels because other factors have been affecting ozone in that part of the atmosphere. Being clear about this difference will massively improve the clarity of your paper. I also take the opportunity to point out that the ozone layer was still significantly affected by elevated concentrations of ODSs in 1980. This is not surprising since ODS concentrations were well above natural levels at this time. I would therefore far rather see reference made to pre-1960 levels since prior to 1960 ODS concentrations were close to or at natural levels.

Line 31: I really don't like this phrase "contribute positively to ozone recovery". If you mean recovery of ozone from the effects of ODSs (if not please let me know what influence you're referring to when you speak of its recovery) then of course it goes without saying that CO_2 and CH_4 cannot contribute to the recovery of ozone from the effects of ODSs. Well, actually, maybe that's not true. If, for example, CO_2 or CH_4 made ozone less susceptible to the effects of ODSs, then it would be appropriate to say that CO_2 and CH_4 "contribute positively to ozone recovery". But I don't think that's what you mean here. I think what you mean to say is "It should be noted that not all greenhouse gasses (like CO_2 or CH_4) contribute to increases in ozone concentrations". And even then you should state how CO_2 and CH_4 *might* contribute to increases in ozone concentrations.

Line 32: I strongly disagree that N2O will "slow down ozone recovery in the future" (assuming by "recovery" you mean recovery from the effects of ODSs), unless you count N2O as an ODS. Do you? There is, however, significant evidence that increasing atmospheric concentrations of N2O will delay the return of ozone to pre-1960 levels. But that's a different issue than ozone recovery from the effects of ODSs.

Line 33: This use of the term "super recovery" shows the pitfall of conflating ozone increases with recovery of ozone from the effects of ODSs. There is no such thing as super recovery. Ozone, one day, will have fully recovered from the effects of ODSs, i.e., anthropogenic ODSs will no longer have a material effect on ozone. At such a time might ozone concentrations, in some regions of the atmosphere, be higher than prior to 1960? Certainly, most likely because GHG-induced cooling of the stratosphere pushes the odd-oxygen equilibrium towards O3. Is this somehow a "super recovery" from the effects of ODSs. Of course not. It is just that another influence, in addition to ODSs, has affected ozone.

Line 38: In regards to the phrase "In the troposphere, the ozone concentration has been continuously increasing": So would you say that tropospheric ozone has been "recovering" continuously over this period? If not, why not? You seem to refer to increases in stratospheric ozone as "recovery", why not also for tropospheric ozone? Well, for good reason - as you say "due to the continuous increase in tropospheric ozone precursors". Here is your all important *attribution* statement. All I am asking you to do is apply the same reasoning in the stratosphere. When something other than ODSs causes ozone to increase, please don't call this "recovery", just like you don't do so in the troposphere.

Line 47: Yes, I agree, ozone has been steadily recovering in the upper stratosphere since the late 1990s - attribution studies have shown, unambiguously, that the increases in ozone the the upper stratosphere can be attributed to decreases in Cly and (less so) Bry. This attribution is essential to call "recovery".

Line 47: Do you mean that the recovery is statistically robust or do you mean that the positive trends in ozone in this region of the stratosphere are statistically significantly different from zero at the 2 sigma level? They are two very different things. If you say "This recovery is statistically robust..." it says to me that there is a statistically clear positive influence on ozone resulting from the decline in ODS concentrations. Ozone could still be declining as a result of other factors, but there is a clear positive effect on ozone as a result of declining ODS concentrations. Is this what you mean? Or do you mean that the positive trends in ozone in this region of the stratosphere are statistically different from zero at the 2 sigma level?

Line 49: Your sentence that "The main driver of the upper stratospheric ozone recovery is the reduction of halogen loading" goes without saying if, by "recovery" you mean recovery from the effects of ODSs (if by "recovery" you mean recovery from something other than ODSs, please let me know what it is that your ozone is recovering from). After all, what

else could it then be? It would be much more correct to say "The main driver of the upper stratospheric ozone increases is the reduction of halogen loading" because that now opens the door for other factors to be at play which is what you discuss in the very next sentence.

Line 52: See, here is a great case where you have used the word recovery correctly. See my comment on Line 31. Now here you do indeed show how CH4 can actually promote the recovery of ozone *from the effects of ODSs* because the CH4 promotes the conversion of the Cl radical to the HCl reservoir species. I strongly encourage you to clarify your thinking around "ozone recovery" and "ozone increases". It will make both your paper and your life much clearer.

Ljne 54: Regarding "Lower stratospheric ozone (LSO) has recovered more slowly than expected, if at all". I strongly disagree. I believe that the Montreal Protocol has been as effective in reducing the concentrations of Cly and Bry in the lower stratosphere as it has in the upper stratosphere and that, as a result, ozone has been recovering from the effects of ODSs just as well in the lower stratosphere as in the upper stratosphere. It is just that, in the lower stratosphere, factors other than ODSs have been at play that have offset the ozone increases induced by decreases in ODS concentrations such that trends may be statistically indistinguishable from zero, or maybe even negative. But that certainly doesn't mean that ozone is not recovering from the effects of ODSs in the lower stratosphere.

Line 69: Please include references to support the assertion that there have been changes in the relative strengths of the lower and upper branches of the BDC (and please expand this acronym) and that these changes have had an effect on ozone.

Line 73: Unlike the other factors listed, "insufficient treatment of diffusion and transport processes in models" definitely does not cause a decline in LSO. It may cause models to incorrectly simulate the decline in LSO but it does not cause a decline in LSO in the same way that recent reductions in solar activity might.

Line 77: The phrase "pattern of the signal in LSO" is a little vague. Do you you mean the pattern of trends in LSO as a function of latitude (and maybe also altitude)?

Lines 78-79: Replace "the persistent" with "the observed persistent" just to make it clear that these negative trends are seen in observations.

Line 84: I would have expected the exact opposite to be true, i.e., if you have a large ensemble of simulations, by taking the ensemble average you smooth out all of the unforced variability leaving you only with the forced variability which is the signal that you're after. Ah, but maybe you mean this: Over the past 60 years say, ozone has

evolved within the context of a very specific sequence of unforced dynamical events, i.e., events driven by the chaotic nature of the climate system. That sequence could be quite anomalous (extreme). We don't know because we have only one planet with a single trajectory through history. It could be, but we have no way of telling, that around the time when Thomas Peter was born, if there had been some perturbation to the state of the atmosphere that the evolution of dynamical events that has affected LSO may have been completely different and LSO would have actually increased. But with a single planet with a single history, we can't say. Ah, but we can to some extent. Taking ESMs as a proxy for our climate system we can see if *any* single ensemble member has a LSO trend that looks like what we have seen other the past 60 years. If there is one, we can say "Ah ha, that's the timeline that our planet followed to the exclusion of all others". It is a plausible (though quite different from the mean) timeline. But if you had 1,000 ensemble members and none of them had a LSO trend such as that observed, then you would conclude either that (1) what we have experienced on this planet in the last 60 years in terms of the sequence of dynamical events that have affected LSO is highly anomalous, or (2) that there is something wrong with the models. So this is how I could see that looking at individual models could be useful in this regard. But I don't feel that that's what you communicated.

Line 89-90: Ah yes, here comes the nail in the coffin of the ESMs - even with assimilated dynamics the ESMs can't reproduce the LSO trends. As a result, I was then surprised that your paper concludes with the statement that "the obtained qualitative agreement between the model and observations allows to produce reliable estimates of future ozone evolution using modern chemistry-climate models". I think that conclusion is not support b y your analysis.

Line 95: No I am pretty sure that this is not what Avallone and Prather argued - the first paper to describe why tropical LSO might decrease. Rather, the more rapid vertical motion in the tropical troposphere and lower stratosphere results in there being less time for photochemical ozone production in the rising air parcels and hence a reduction in LSO. Please read Avallone, L.M., and M.J. Prather, Photochemical evolution of ozone in the lower tropical stratosphere, Journal, of Geophysical Research, 101(D1), 1457-1461, 1996.

Line 164: Presumably these are zonal winds *at the equator*. You should state that.

Figure 1: It is hard to distinguish the results. I think that either tabulating these results or, if you want to retain the graphical format, slightly horizontally offsetting the data points from each other would help a lot.

Line 182: Why not just apply Gram-Schmidt orthogonalisation (https://en.wikipedia.org/wiki/Gram%E2%80%93Schmidt_process) to ensure orthogonality across your basis functions?

Line 185: This is the first mention of "background level of the model". I think you need to

explain in one additional sentence exactly what this is.

Lines 194-195: Yes, exactly. Without such attribution you cannot speak of any ozone recovery from the effects of ODSs. If you found that ozone was increasing but also found that that ClOx and BrOx destruction cycles were strengthening, you would not call that increase "ozone recovery". So I am pleased to see this recognition that discussion of recovery must be done in the context of this attribution to changes in the strengths of the ClOx and BrOx cycles.

Line 214: When you refer to upwelling here I assume you are referring to atmospheric upwelling and not oceanic upwelling, but it may be worth clarifying that.

Line 219: I wouldn't call 55°S-55°N "near-global". I think that it would be more accurate to refer to this as "extra-polar". It also says that these are monthly mean anomalies but they look more like annual mean anomalies to me.

Line 224: Unless the increase in mesospheric ozone can be attributed to a decrease in Cly and/or Bry I would disagree that "mesospheric ozone has a tiny positive contribution to the recovery of the total column ozone on the near-global scale". It may contribute to increases in total column ozone but, with attribution to Cly and Bry, it cannot be said to be contributing to the recovery of ozone from the effects of ODSs.

Line 227: With regard to "started to recover distinctly". I believe it would be more correct to say "started to increase distinctly" because you have not demonstrated conclusively that the increase is a result of a decline in Cly and Bry.

Line 228: See here you refer to "pronounced increase in ozone" but don't call it "recovery". Why not? Elsewhere where there has been an increase in ozone you call it "recovery" so why not here? Can you see the danger of referring to "increasing ozone" as "recovering ozone" when you don't actually do the attribution to decreasing concentrations of Cly and Bry?

Line 233: Do you mean "ozone recovery" or "ozone increases"? And you need to say much more about this "bias shift" in the ERA5 reanalyses and what causes it.

Line 235: So if the minimum occurred in 1992, would you say that ozone was then recovering in this region of the stratosphere from 1992 onwards?

Line 239: Highly perturbed by what? Or do you just mean is highly variable just as a result of natural unforced variability.

Line 244: I really don't know what you mean here by "essential artifacts from unknown origins". This needs to be explained much more carefully.

Line 246: ERA5 shows biases against what?

Line 262: Why only "trend might result from continuous emissions of tropospheric ozone precursors". Can you not be more definitive about this?

Line 265-266: What vertical region of the atmosphere are you referring to in this sentence?

Line 269: I think that it would be better to say "the expansion of the ozone towards lower latitudes" and more accurate to avoid confusion that you may be referring to vertical expansion.

Line 270: With regard to "Most likely". Surely you can easily confirm this by looking at the position of the jet in SOCOLv4 and comparing it to the position of the jet in the reanalyses?

Line 282: See, here you are referring to 1998-2018 as the recovery period and yet, in some regions of the stratosphere, and in some ensemble members, ozone is decreasing. And this is entirely OK and correct because "ozone recovery" does not necessarily mean "ozone increases". There is no doubt in my mind that even in those strong blue regions in Figure 5 that ozone is recovering from the effects of ODSs in those regions. It's just that that recovery is being overwhelmed by some other process.

Line 286: "ozone increase in the mesosphere" rather. And please elsewhere in this section be very clear when you are referring to "ozone recovery" (from the effects of ODSs) and more generally "ozone increases". I am not going to highlight every instance where you have conflated the two.

Lines 292-304: I think that this paragraph at the top of page 15 of the paper is the most valuable aspect of the work and more should be made of this (see also by comment regarding line 84). The variability in LSO trends across the different ensemble members is very telling, even if they are not statistically significant. The point here is that the BASIC

trends represent just one realisation of our planet's history - and of course we have just one. But the SOCOLv4 members show what, conceivably, could also have happened. What happened in our single reality may well be a statistical outlier across the full range of histories that conceivably could have happened, but it happened nonetheless. So the message for me is: don't panic if you do an ESM simulation that doesn't show the observed trends in LSO from 1998 to 2018. It may just be that your particular ensemble member didn't follow the same "dynamical storyline" that happened in reality. And don't even think about looking at the ensemble mean - that's not going to help if what happened in our planet's 1998-2018 history is a statistical outlier. A better way to go is to conduct a large ensemble of simulations and identify those simulations which do look at lot like what happened in reality, even if there are relative very few of them. Then diagnose that subset for the underlying causes of the shown LSO trends. No sense in diagnosing ESM simulations where, for reasons of different unforced natural variability, the ESM's "dynamical storyline" is different from what happened in reality. Take a look at "North Atlantic climate far more predictable than models imply" by Smith, D.M.; Scaife, A.A.; Eade, R.; Athanasiadis, P.; Bellucci, A., et al., Nature. They did something similar. I think that what you have done here is important in this regard and something worth exploring further - not necessarily in this paper but perhaps in a follow-up paper.

Line 317: With regard to "that the modeled ozone changes in the extratropical lower stratosphere are still uncertain", I would say that it's more the case that your simulations show significant variability in these trends across ensemble members suggesting that unforced natural variability can create trends in LSO that are not that different from what is observed. We don't know how LSO in reality could have evolved under slightly different initial conditions since we have only one planet with a single history. But SOCOLv4, as a proxy, does show that very different LSO trends can result from different natural dynamical variability.

Line 321: While I agree that "no single member (amongst the six) suggests an extensive negative signal found in the BASIC", given the variability you see in your six members, what do you think the likelihood would be of finding a single member that closely matches reality if you had a 1,000 member ensemble? My guess would be "fairly high". If you found one (and you really only need one - you could argue that that's the particular "dynamical storyline" our planet followed) and diagnosed that single run for the drivers of the LSO trends, I think that this could provide valuable insights into the drivers of the observed trends.

Line 326: I agree with the statement that "Yet, the other four members have less pronounced but still negative trend regions", but who cares? Those are simulations with different dynamical storylines to what our planet experienced and so can be discarded. Just diagnose number 4 and 5. And definitely don't waste time analysing the ensemble mean.

Line 334: I agree that "ozone recovery period is not yet long enough to detect statistically significant LSO trends in the LOTUS regression analysis" but if you could run, e.g., ensemble 4 with detailed tracking of the contribution of different chemical cycles to the ozone tendencies in SOCOLv4, e.g. as was done in Revell, L.E.; Bodeker, G.E.; Huck, P.E. and Williamson, B.E., Impacts of the production and consumption of biofuels on

stratospheric ozone, Geophys. Res. Lett., doi:10.1029/2012GL051546, 2012 and Revell, L.E.; Bodeker, G.E.; Huck, P.E.; Williamson, B.E. and Rozanov, E., The sensitivity of stratospheric ozone changes through the 21st century to N2O and CH4, Atmos. Chem. Phys., doi:10.5194/acp-12-11309-2012, 2012, then you could probably say something really new and novel about the recovery of LSO *from the effects of ODSs* over the 1998-2018 period.

Line 352: With regard to "does not yield the observed negative trends", yes but you only had 6 members. I bet if you had 1,000 members you would find one that does track reality well. But that would be well beyond the compute budget for ETH.

GRAMMAR AND TYPOGRAPHICAL ERRORS

There were grammatical errors in the paper which I did not take the time to correct. I would strongly suggest that the authors find someone to thoroughly proof read the paper. Please make sure that all acronyms are expanded at the place of first occurrence.

Line 27: Small suggested change in wording from "total ozone is expected to recover" to "total column ozone has been expected to recover"

Line 42: The more common phrase is "stratosphere-troposphere exchange" but this is not a big deal so leave it as it is of you wish.

Line 82: Replace "dynamic variability" with "dynamical variability".

Line 179: Previously this was referred to as the "ozone depletion phase", now referred to as the "ozone-depleting period".

Line 184: Replace "output DLM output" with "DLM output".