

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2022-730

Anonymous Referee #1

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Referee comment on "Montreal Protocol's impact on the ozone layer and climate" by  
Tatiana Egorova et al., Atmos. Chem. Phys. Discuss.,  
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Review of "Montreal Protocol's impact on the ozone layer and climate" by Egorova1 et al.,  
2022, for Atmospheric Chemistry and Physics

### GENERAL COMMENTS

This paper describes the climate and atmospheric benefits of the Montreal Protocol and its Amendments and Adjustments simulated using the SOCOLv4.0 Earth System Model. The analysis is relatively straightforward and adds to a body of evidence accumulated through several similar previously published papers. The work will be of interest to readers of ACP. I believe that the paper will be suitable for publication once my concerns, listed below, have been addressed.

### SPECIFIC COMMENTS

Line 16: Replace 'ozone-depleting substances' with 'hODSs'. You've defined the acronym so you might as well use it.

Lines 20-21: I think that the abstract should concisely summarise what was learned from this study over and above what is know from similar previous studies. What is new here?

Line 24: SOCOLv4.0 is a very good Earth System Model (ESM). But it is not state-of-the-art. State-of-the-art suggests that it is the best ESM, defining a standard that all other ESMs should aspire to. This is not the case. The phrase is anyway ridiculously overused. Just delete it.

Lines 44-46: I find this sentence confusing. I have not seen any analysis, by anyone, including the Ball et al. papers of 2018 and 2019, that suggest that the ozone layer is not recovering from the effects of ODSs. That's not to say that ozone is everywhere increasing. It is not. But "ozone increasing" is not equivalent to "ozone recovering" otherwise we would be talking about tropospheric ozone increases during the 1980s and 1990s as "ozone recovery". Ozone may be decreasing in some regions of the atmosphere, but that certainly does not mean that ozone is not recovering from the effects of hODSs. So I have no idea at all why the "recovery and the efficiency of the MPA are now being questioned" as you state. Sure models and observations of, e.g., declining ozone in some regions of the stratosphere may disagree, but that says something about the deficiency of the models, nothing about the deficiency of the MPA.

Line 50: Just one point that is worth noting and commenting on regarding "observed meteorological fields". If you run the counter-factual simulation (i.e. the world in which there was no MPA) with observed meteorological fields, then this is not a self-consistent simulation since we know that the meteorological fields would have been different had there been no MPA right? So doing an analysis this way is not a very 'clean' attribution. Using CCMs (or ESMs) for this purpose makes much more sense and provides a further motivation for your work. I think it is worth making that clear here. Ah yes, I see you make some of these points below.

Line 83: I think that papers that explore 'the world avoided' because of the Montreal Protocol are interesting. But you haven't made much of a case for why they are required. What is the value in knowing what would have happened if the MPA had not been implemented? As an analogy, I have no desire to know what would have happened if I hadn't decided to come in to the office today. The fact is that I did. The fact is that the MPA was enacted. What is the value of knowing what would have happened had it not been enacted? I think that greater justification is needed for the significant expenditure of compute resources for such studies.

Line 124: Nice to see the noMPA\_noRadCFC simulation which is very useful for attribution.

Figure 2: If this is showing the TCO difference between MPA-ssp245 and noMPA, as in MPA-ssp245 minus noMPA, surely these values should all be positive? It is also stated in the figure caption that "The colored areas mark statistically significant results at the better than 95% level" but \*everywhere\* is colored. In that case wouldn't it be simpler to say "All differences are statistically significantly different from zero at the 95% level"?

Line 154: I disagree with this statement "...learn more and verify our knowledge about...". Running models does not add to our knowledge for the simple reason that models contain only what we already know and nothing that we don't know. Running a model can add to our understanding of how some system works and could provide new insights, but no new knowledge.

Figure 3: The figure caption provides no indication as to whether these results are withMPA minus withoutMPA or vice versa. I can certainly guess and I am 99% sure that I would guess correctly, but readers should not have to guess. There appear to be dots in some regions of some panels of Figure 3 but no explanation is given as to what these represent. Finally, it is stated that "colored areas mark statistically significant results at the better than 95% level" but as far as I can see, \*everywhere\* in every panel of Figure 3 is colored.

Line 157: I don't understand this. What is the "earlier periods" that you refer to? and when you say "while the magnitude of the signal is larger", larger than what?

Line 173: I would have thought that it is due more to the Clausius-Clapeyron relationship rather than enhanced evaporation. For example, climate change could, conceivably, reduce surface wind speeds and evaporation but the warmer troposphere would still have higher water vapour loading because of the Clausius-Clapeyron relationship. If you are certain that it is the result of enhanced evaporation rather, then you need to provide evidential support for that assertion.

Line 176: But, presumably, a much clearer troposphere with far less pollution as a result of an increase in the self-cleaning capacity of the troposphere. Given the number of people that die from pollution-induced respiratory diseases every year, this would have a big health benefit right? OK so you die from skin cancer rather, but nonetheless...

Line 203: I am surprised you have not cited Velders, G.J.M.; Andersen, S.O.; Daniel, J.S.; Fahey, D.W. and McFarland, M., The importance of the Montreal Protocol in protecting climate, Proc. Natl. Acad. Sci., 4814-4819, 104, 2007 here.

Figure 4: I find the caption very confusing. It says that Young et al. is light blue. I don't see any light blue line in the figure. The legend says that Young et al. is green and I do see a green line. But then it says that Goyal et al. is red but the legend says it is orange. The caption however says that orange is noMPA which the caption says is red. Anyway, as a result, I could make no sense of Figure 4.

Line 209: With regard to "This disagreement is not easy to understand", how do you know that these difference are statistically significant given the uncertainties and inherent unforced variability in the model simulations?

Lines 237-238: I don't see any indication in the rightmost panel of Figure 5 this "except marginally significant cooling in equatorial Africa" that you refer to.

Line 239: You need to cite some references here to "Arctic amplification" no matter how well known.

Line 245: Can you are also not providing statistical significance on your results right?

Figure 6, rightmost panel: The figure caption does not make clear whether this is noMPA minus MPA\_ssp245 or vice versa. Likewise for Figure 7.

Line 271: The rate of "1-4% per 1 K warming" that you quote is considerably lower than I have see in other publications, e.g. Sun, Q., Zhang, X., Zwiers, F., Westra, S. & Alexander, L. V. A global, continental, and regional analysis of changes in extreme precipitation. *Journal of Climate* 34 (1), 243–258 (2021), find values of 6.6% (5.1% to 8.2%; 5%–95% confidence interval) while Westra, S., Alexander, L. V. & Zwiers, F. W. Global increasing trends in annual maximum daily precipitation. *Journal of Climate* 26 (11), 3904–3918 (2013), found values of 5.9% to 7.7%. You need to provide strong justification for your quoted 1-4% sensitivity.

Line 302: Again, I would suggest not referring to SOCOLv4.0 as the state-of-the-art ESM.

#### GRAMMAR AND TYPOGRAPHICAL ISSUES

I understand that the author's first language is not English. The paper would benefit substantially from proof-reading by someone whose first language is English. I apologise for not taking the time to correct the many grammar and typographical errors in the paper. I just don't have the energy today (insufficient chocolate). Some suggested corrections are included below.

Line 47: By 'efficiency' do you mean 'efficacy'? I suspect you do.

Line 60: No need to defined hODS again here. You have already defined the acronym twice above.

Line 132: You defined the MPA acronym above but now seem to have decided to stop using it.

