# Interactive comment on “Influence of Arctic Stratospheric Ozone on Surface Climate in CCMI models” by Ohad Harari et al.

**Anonymous Referee #1**

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The paper by Harari et al “Influence of Arctic Stratospheric Ozone on Surface Climate in CCMI models” investigates statistical links between zonal mean ozone averaged over the polar cap in the lower stratosphere and surface climate variables, such as surface temperature and sea level pressure, in seven coupled chemistry-climate models with interactive chemistry. The authors report statistically significant correlation between ozone and surface climate however they provide little discussion on mechanisms behind apparent links. Although the work may potentially lead to some interesting results I cannot recommend this current paper for publication for the reasons outlined below:

1. The motivation of the paper is not clear. The authors state that they “. . .revisit the connection between boreal spring Arctic stratospheric ozone variability on interannual timescales and surface climate. . .” but first of all I don’t think there is any doubt that ozone variability influences surface climate and I don’t see why this needs to be revisited. Ozone is radiatively active gas; it absorbs solar radiation in the stratosphere and thus heats the stratospheric air. This in turn affects stratospheric circulation and thereafter the troposphere and surface climate via stratosphere-troposphere dynamical coupling. Do the authors want to revisit this link? In any case I don’t think the statistical approach adopted by the authors can provide a progress here.

Of more interest is the question of whether anthropogenic emission of ozone-depleted substances affected surface climate via affecting Arctic ozone. But I don’t see that the authors address this question because they mix together periods when ozone was depleted (1970-2010), recovering (2011-2051) and fully recovered or possibly even “super-recovered” (2052-2092). Thus I think the authors need to reevaluate the moti-vation and objective of their study.

The reviewer appears to believe that the existence of a connection between Arctic ozone and surface climate is settled science. As discussed in the introduction (but evidently not clearly enough), three studies by three different research groups have reached the opposite conclusion. We are aware of only one modeling study that attempted to address this question that has reached the conclusion that there is a robust impact.

The 2018 WMO ozone assessment includes a review of this issue, and consistent with the introduction of our paper, the ozone assessment concludes that "interannual variability in springtime Antarctic and Arctic ozone may be important for surface climate, but work remains to better quantify this connection." and also that "future work is needed to evaluate whether differences in the ozone forcings, as well as other inter-model differences, among the various studies has contributed to the range of conclusions." The ozone assessment is the closest document the ozone community produces that is intended to reflect the current scientific consensus, and it clearly indicates that this issue is not settled and deserves revisiting. These statements are quoted from the fifth order draft from July 2018, i.e. after all scientific reviews had already been completed.

That being said, we plan to separately consider interannual variability in the depleted, recovering, and super-recovered states in our revised manuscript, but our preliminary analysis indicates that all results in this paper are valid if one considers just the historical depleted period, and differences among the three periods are minor (though see the figures in response to comment 3 below).

2. While reporting on statistical links the authors avoid discussing on possible mechanism. While it is true that inferring physical mechanisms from statistical relations is difficult, it would be valuable if the authors formulate clearer which mechanisms they keep in mind when they say "ozone influence on tropospheric climate". Do they mean (a) ozone induced dynamical changes in the stratosphere and following dynam-ical downward coupling or (b) downward radiative fluxes due to ozone variability, or something else? For example in the case of Antarctic ozone depletion, the likely mechanism through which ozone affects surface climate is through radiative cooling of the stratosphere and subsequent downward dynamical influence. Additionally, Grise et al (2009) studied possible radiative impacts of ozone depletion on the troposphere and found that most of the impacts is through cooling of the stratosphere leading to reduced downward flux of the infrared radiation. At the same time there is very little direct impacts of ozone on stratospheric transmissivity and emissivity. While this result appears in agreement with authors results according to which ozone connection to surface climate is "mediated by the dynamical variability" I don't think the authors provide new findings here.

The goal of this manuscript was not to shed light on mechanisms in these CCMI models. Rather the novelty of revisiting the Arctic Ozone-surface climate connection in CCMI models is that these models have an interactive ocean (which wasn't true of previous model generations), and also we have more than 1600 years of model output we can use to put the observed results in context (in contrast to previous modeling studies which had an order of magnitude less data available). As discussed in Garfinkel et al 2013 and Garfinkel and Waugh 2014, it is very difficult to tease out mechanisms as to how downward coupling occurs from long model runs like those contributed to the CCMI project. The best we can do is to form "emergent constraints", that is to use the connection between e.g. ozone and stratospheric conditions to better understand how ozone may affect surface climate. That being said, we plan to discuss this issue in more detail in the revised manuscript including a better description of our figures comparing correlations to correlations, and also to better explain the novel aspects of our analysis.

(As an aside, the conclusion of Grise et al 2009 is that the radiative mechanism the reviewer has in mind isn't important with regards to Arctic ozone.)

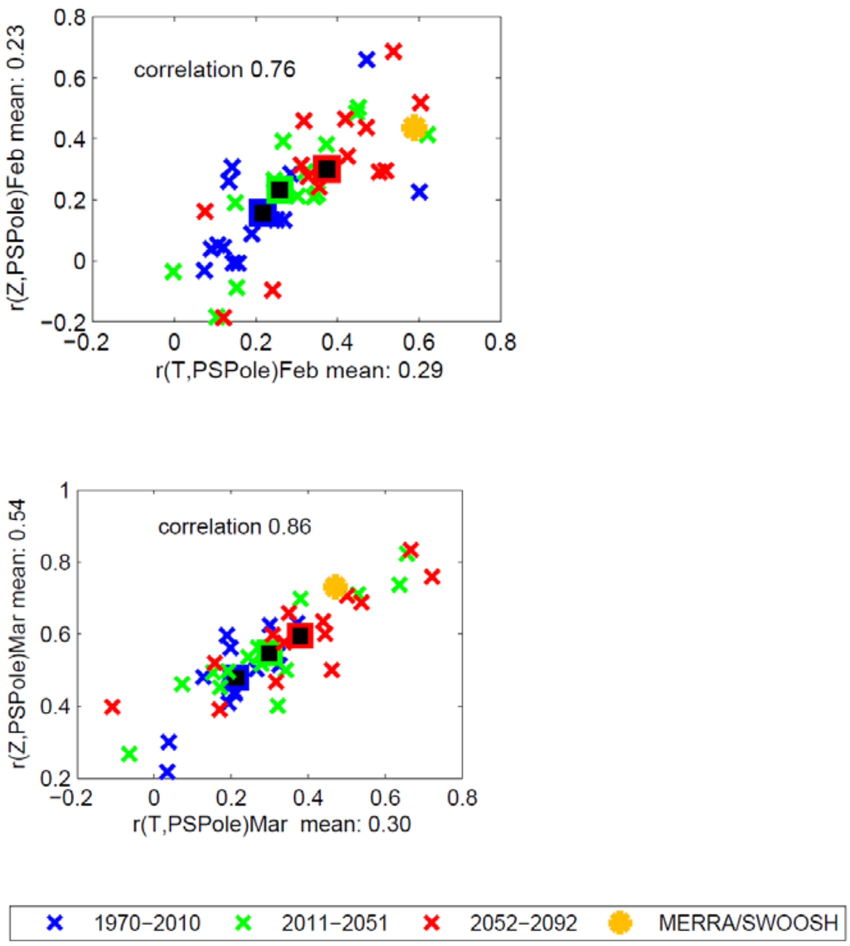
3.
On the basis of stronger statistical links between stratospheric dynamical indexes and surface climate, the authors conclude, "A connection between Arctic ozone vari-ability and polar cap sea-level pressure is also found, but additional analysis suggests that it is

mediated by the dynamical variability that typically drives the anomalous ozone concentrations.” It is true that stratospheric transport determines ozone distribution but ozone also affects stratospheric circulation through radiative heating. I don’t think author’s analysis can rule out the possibility that stratospheric circulation that affected the surface climate was modified by ozone variability.

We certainly agree that the stratospheric circulation anomalies that affected the surface could have modified by ozone variability, and we never meant to imply the contrary.

Below are two figures we have produced in which the x-axis shows the correlation between polar cap temperature at 100hPa and polar cap SLP for each 40 year subsample of each model, and the y-axis shows the correlation between polar cap geopotential height at 100hPa and polar cap SLP for each 40 year subsample of each model. Subsamples during the "super-recovery" period are in red, during the recovery period in green, and during the depleted period in blue. The top panel is for February and the bottom panel is for March. The mean of each period is indicated with a square. The connection between conditions in the stratosphere and surface climate is stronger during the super-recovery period by up to a factor of two. We plan to include the figures below in the revised manuscript.

We realize that the sentence highlighted by the reviewer was poorly phrased, and we plan to clarify it for the revised manuscript. However our results do indicate that the dynamical pathway is dominant, as if we statistically remove the dynamical pathway then the connection between ozone and subpolar surface climate goes away.




4. The authors report that they found “connection” between Arctic ozone variability and polar cap sea-level pressure and El Nino but looking at their results one can see correlation coefficients in the order of 0.1..0.2 in their multi-model mean results. While these

may be statistically significant I really wonder about their usefulness especially since there is lack of physical understanding. It would be desirable if the authors proposed ways how these links can be utilized; however this has not been done.

We never meant to imply that a correlation on the order of 0.1 is useful in an operational sense. Correlations in observational data are almost four times higher, however, and hence indicate that such a relationship could be useful. Our goal was to assess whether this observational relationship is simulated by the CCMI models (the first and only multimodel ensemble that could be reasonably expected to capture this relationship), and more crucially to evaluate the spread in response in the models in order to get a sense of whether the observed response is "forced" or just reflects internal variability in a short sample. The fact that the multi-model mean signal is ~4 times weaker than the observed signal, but that individual 40-year subsamples show relationships very similar to that observed, indicate that the borderline useful observational result is potentially inflated by internal variability. We plan to clarify this issue in the revised manuscript.

In summary I am not sure if the authors can address the above issues within the current manuscript and therefore I recommend a rejection. Perhaps a good way forward is to adopt the approach by Calvo et al. and analyze periods of ozone depletion and ozone recovery separately trying to isolate the role of ozone rather than making an obvious (in my opinion) point that the ozone impact is mediated by the stratospheric dynamics.

As noted above, with careful rewriting and some new analysis that has already been completed we believe we can address those comments of the reviewer that are reasonable (i.e. the second, third, and fourth comments). More specifically, we plan to include figures analogous to what is shown in the response to comment 3 above in the revised manuscript. With one notable exception (that shown above), there is generally little difference between the super-recovery period and the depleted period. We also plan to address the minor comments below in the revised manuscript.

Reference: Grise, K. M., D. W. J. Thompson, and P. M. Forster (2009), On the role of radiative processes in stratosphereâA Rtroposphere coupling, J. Clim., pp. 4154–4161, doi:10.1175/2009JCLI2756.1

Minor points:

P2L5: “This sensitivity suggests that the radiative perturbation due to ozone requires tropospheric feedback” Can it also be interpreted that ozone forcing in isolation is too weak to modify stratospheric circulation and that additional forcing from SST –driven wave activity is needed?



P3L12: “but utilizes up-to-date CCMs”. Perhaps also including tropospheric chemistry?

P3L13: remove double “the”

P3L20: “in all cases there is a peak between 2 and 5 years (not shown).” In agreement with observations?

P3L28: Please explain how do you get 42 model samples?

Table 1: Add reanalysis to the table caption

P4L10: What does it mean: “limited data was either missing, corrupted, or non-physical”?

P7L7: When testing statistical hypotheses there could be only two outcomes, either the test is passed or not. There is no “nearly statistically significant” results. Based on your Figure 6 I conclude that the null hypothesis that the observed correlation between ASO and ENSO is accidental cannot be ruled out at the 5% rejection level.

P7L29: What do you mean by “the same model” here?

Figure 4: This figure makes me crazy. Correlation of correlation coefficients?!! What am I suppose to learn, for example, from the fact that there is a correlation of 0.54 between r(PS_March, ASO) and r(ZMArch_ASO)? I don’t think the discussion in the text makes my task any easier. My specific concern is Figure 4d where the spread of the crosses is visually inconsistent with the reported correlation of 0.6 (for example the correlation of just 0.54 in the nearby Fig. 4c visually appears tighter). Can the authors double-check this number?

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-1031, 2018.