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Atmospheric Chemistry and Physics
Discussions

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 motivation and objective of their study.

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 dynamical 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.

3. On the basis of stronger statistical links between stratospheric dynamical indexes and surface climate, the authors conclude, "A connection between Arctic ozone variability 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.

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.

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.

Reference: Grise, K. M., D. W. J. Thompson, and P. M. Forster (2009), On the role of radiative processes in stratosphereâĂŘtroposphere 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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