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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 #2

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Summary This paper evaluates relationships of Arctic stratospheric ozone (ASO) and surface climate, and ASO and El Niño-Southern Oscillation (ENSO), using model output from the Chemistry-Climate Model Initiative (CCMI). They find that the connection between ASO and surface climate may arise due to dynamic variability in the lower stratosphere, which has a strong correlation with both ASO and surface climate. They also find a weaker-than-observed (though "still significant") relationship of ASO and ENSO, with ASO leading ENSO by one to two years.

General Comments In general, I think the writing is clear and the analysis is well-explained. I think there is definitely utility in looking at these relationships in state-of-the-art chemistry-climate models, particularly since it's apparent that some of the observed relationships might be an artifact of sampling. My one major comment relates

to the relationship of ASO to ENSO. While the authors do due diligence and attempt to make sure this relationship isn't a factor of auto-correlation, I'm confused about their method. For example, I don't understand why the "minimal useful correlation" is basically zero (so basically anything greater than zero is useful?). See specific comments below too. I think this technique at a minimum needs to be better explained, but I would also recommend addressing the issue using Granger causality techniques, as in McGraw and Barnes (2018). Additionally, I think in parts the authors are attempting to use their results to support observational results in Xie et al. but I think if anything their results more clearly indicate the weakness of this relationship between ASO and ENSO, and that it's likely a sampling artifact in the observations. I would encourage them to remove statements such as in the abstract that state that ASO "may also influence the surface in both polar and tropical latitudes"- my impression from the results was actually the opposite, that ASO has very little influence on either polar or tropical surface climate, and that significant correlations can occur randomly for 40-year subsets, which is probably what we are seeing in the observations. In general I think understanding these possible relationships between Arctic ozone and surface climate is important, and the CCMI dataset provides a new tool to do so (particularly since it's hard to find models that are both coupled and have interactive chemistry and decent stratospheric processes). But I would like to see the authors address these concerns. I recommend a major revision.

Specific Comments Page 1, Line 16- maybe mention why ASO has been "spared from the worst ozone destruction", e.g., the relatively warmer polar temperatures due to stronger wave forcing.

Page 3, Line 12 – Can you explain what Ref-C2 is, i.e., what radiative forcings, specifications do these runs use.

Page 3, comment about Data- you need to include more information about SWOOSH- how long is the record, which version are you using, which data goes into SWOOSH, are you using the "anomaly filled" version, which latitude resolution, etc. One thing

I'd be interested to know- how much data does SWOOSH have polewards of 60N to calculate the ASO? Is there any data from 80-90N? If not, do you need to take that into account when comparing the model ozone?

Page 4, lines 5-7- is the multiple linear regression performed before or after subdivision into 40 year chunks? (does it matter?). Is this removal of the GHG/ODS effect done for both dynamic (T,Z) and ozone time series? Is this also performed for MERRA2/SWOOSH data? Page 6, line 5-7- change to "ASO and polar cap SLP than is observed in March" (the connection is stronger than observed in April). Also, is the statistical significance true for the r=0.09 value? Or just the April r=0.17 value? Even if it's significant. . . is a value like r=0.09 very useful? It's implying that only 0.8% of the variance in polar cap SLP is explained by ASO. Stating only that it's significant statistically may be misleading (and, I think, not strong support for statements in the abstract or conclusions that suggest such a relationship provides useful information about predictability).

Page 6, lines 24-27- might acknowledge here that there could be non-linear feedbacks at play that linear regression would not remove

Page 7, line 7- in general, I find the authors to be trying too hard in this section to reinforce Xie et al (2016) results; this statement is an example- "This relationship is nearly statistically significant at the 95% level". This should be changed to either the specific significance level that it meets, or it should say "this relationship is not significant at the 95% level". Particularly since, if anything is striking about Figure 6, it's that almost no correlations shown (not even the observed ones) meet significance levels. These results to me more strongly argue that the relationships suggested by Xie et al. are artificial. Page 8, line 29-30 more effectively state what I think the results of this study conclude- that there is no strong evidence of this mechanism/relationship in the CCMI models. But this message isn't clearly reflected in either the abstract or the conclusions (such as point (3) on page 11, line 1)

Page 7, Line 22- could this have to do with the power spectra of ENSO in models having higher amplitude at periods of 12 months instead of 24 months? (e.g., AchutaRao and Sperber 2002). Again, this would suggest these lead-lag relationships are more a reflection of ENSO auto-correlation than physically-based relationships.

Page 9, Line 1-10- I commend the authors for trying to deal with the auto-correlation issue. However, I'm not sure I understand their method. Why is ASO at a lag of 3 months chosen? Why not at the lag where the relationship peaks, in either the observations or the model? Then the "minimally useful correlation" is shown in equation (2) and plotted in red in Figure 6/7, but it's not clear to me what the right side of that equation has to do with the correlation values in plotted in black (yet it's then stated that "for both observations and the CCMI models the actual correlation between ASO and ENSO far-exceeds the minimally useful one". . . but the actual correlation r(ENSO,ASO) is not part of the criteria in equation 2). Also confusing is that the minimally useful correlation appears to be nearly zero at all lags, so how is the criteria in eqn(2) satisfied? This should be clarified to better explain what this analysis tells us, but I also would recommend that instead of this method, additionally consider applying the Granger causality techniques as detailed in McGraw and Barnes (2018).

Page 9, line 13- this relationship at zero lag is true only in the multi-model mean CCMI, right? It seems to be the opposite sign in the observations.

Page 10, lines 1-17- this part of the conclusions was nicely written and well-phrased.

Technical Corrections Page 3, Line 6- Capitalize the appropriate letters for MERRA

Page 3, Line 18- remove repeated "the"

Page 5, Line 8- remove comma after heights and put it instead after the first word "stratosphere".

Page 5, Line 17-18- should be "with a single x, and the. . .". Also it should be yellow asterisk, not green?

Page 5, Line 27- remove “polar cap” and change to “sea level pressure anomalies” (I assume climatology is removed?). Could also add “anomalies” in line 31 (and elsewhere throughout paper). Figure 5a shows the correlation of sea level pressure anomalies at each grid point with the ASO in March, right?

Page 5, Line 30- semi-colon instead of comma after Ivy et al. 2017.

Page 9, line 5- should be “3 months”

Page 10, line 10- change “an” to “a”

Page 11, line 5- capitalize “acknowledgement”

Page 14, caption- misspelled “stratosphere” on line 3. Should these be stated as anomalies in T and Z or are these full fields? (also true in other captions) Also the y-axis on panel (h) seem mislabeled (should be ZApr?). Note also in y-axis of (a) and (b) that the correlation value is 0.36 on one and 0.35 in the other; I believe this should be the same number.

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