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Interactive comment on "Effects of Arctic stratospheric ozone changes on spring precipitation in the northwestern United States" *by* Xuan Ma et al.

Anonymous Referee #1

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This study examines the relationship between springtime Arctic lower stratospheric ozone concentrations and precipitation anomalies over the northwestern United States (Washington and Oregon). Using observations and WACCM model simulations (with various prescriptions of ozone and SSTs), the authors link Arctic lower stratospheric ozone depletion to precipitation increases over the northwestern United States. Their model simulations indicate that prescribing both the ozone and SSTs is necessary to recover the observed relationship in the model.

The premise of this study is very interesting . . . using Arctic lower stratospheric ozone anomalies to predict springtime precipitation. However, as written, I don't find the

manuscript to meet the standards of an ACP publication for the following reasons: 1) most of the observed correlations are based upon statistical significance at the 90% confidence level, 2) the authors fail to account for the role of stratospheric dynamic variability (sudden stratospheric warmings) in their analysis, 3) many of the figures (and associated text) simply repeat the same information, and most importantly 4) no physical mechanism is provided to explain why lower stratospheric ozone anomalies impact the North Pacific circulation (but not the North Atlantic circulation) and in particular how they can excite SST anomalies (which seem opposite to those that would be forced by the lower tropospheric wind anomalies). For these reasons, I am inclined to recommend that the paper be rejected at this point and encourage the authors to resubmit their interesting analyses after they have addressed some of these issues.

Major Revisions

1. Winters with sudden stratospheric warmings and strong stratospheric polar vortices are caused by natural wave-driven dynamic variability (lines 67-68), and thus chemical ozone depletion will only occur when the Arctic stratosphere is not dynamically active (strong stratospheric polar vortex years). So, Arctic stratospheric ozone (ASO) depletion is only relevant in years when the dynamics precondition the Arctic stratosphere for it to potentially occur. It's not immediately apparent to me what advantage looking at ozone (compared to polar stratospheric temperature anomalies) provides for tropospheric teleconnections. In other words, if instead of using ozone as a criteria for the years selected in Table 2, you used the strength of the stratospheric polar vortex, would you get the same patterns? Or, another way of stating this, are the years with positive ASO anomalies associated with sudden stratospheric warmings and/or early seasonal breakdowns of the stratospheric polar vortex? The paper is framed as if ozone is the predominant cause of NH stratospheric circulation anomalies. In reality, the ozone-induced stratosphere-troposphere connections should be secondary in importance to those driven by stratospheric dynamics in the NH. In the SH, where year-to-year dynamic variability is weaker, the ozone-induced stratosphere-troposphere connections

are more prominent.

2. The authors state that ASO recovery will cause the northwestern United States to become drier in the future (lines 19–20, lines 203–205). The analysis in this study is based entirely on detrended ozone anomalies (year-to-year variability). If the authors wish to make this argument, they will need to convincingly show that 1) springtime ASO has trended downward in recent years and 2) northwestern US precipitation has trended upward during April over the same time interval (independent of concurrent variability in ENSO and the PDO).

3. Prior studies have argued that stratospheric circulation anomalies can couple down into the troposphere with a spatial pattern similar to the Northern Annular Mode (NAM) or North Atlantic Oscillation (NAO) (lines 70-71). Yet, the authors' analysis shows a poleward circulation shift over the North Pacific, but not the North Atlantic. Some discussion needs to be provided about why the authors' results are different than those documented in previous studies. It would be nice to compare the patterns shown in Figs. 3–7 with those associated with the NAM/NAO. Additionally, given that the SST anomalies shown in Fig. 10 strongly resemble the Pacific Decadal Oscillation (PDO), the same patterns should be examined for the PDO and ENSO. With such a small sample size of years used in the analysis (Table 2), the authors could simply be sampling concurrent SST variability. Given that most previous studies on this subject see the strongest anomalies in the North Atlantic sector, the fact that all of the anomalies are in the Pacific in this study makes me concerned that Pacific SST variability is being aliased into the analysis.

4. As a related point, variations in March ASO should be linked closely to the timing of the seasonal breakdown of the NH stratospheric polar vortex. Black has examined this issue in detail in a series of papers (e.g., Black et al. 2006). Again, the authors need to better contextualize their results in the context of the past literature, which emphasizes the North Atlantic.

5. A consistent measure of statistical significance needs to be provided throughout the paper. Some figures show a 90% level, others a 95% level, and some show no significance at all (model results). 90% is a fairly weak threshold for statistical significance (1 in 10 chance that the point is significant by chance). I would recommend using the 95% level, or at least showing both the 90% and 95% levels (as is shown in Fig. 6).

6. Related to point #3 above, how can we be sure that the SST anomalies in Fig. 10 are in fact caused by the stratospheric anomalies? They seem inconsistent with the wind anomalies in Fig. 4 (enhanced air-sea fluxes and cooling should occur in regions of enhanced westerlies). Some physical mechanism linking ASO to the SST anomalies needs to be provided. Without prescribing ASO anomalies in a fully coupled model (with interactive SSTs), it's difficult to conclusively establish that the SST anomalies can in fact be forced by ASO.

Minor Revisions

7. The following sentence structure used in the abstract (and elsewhere in the paper) is very difficult to read:

"An increase (decrease) .... results in enhanced (weakened) ... but weakened (enhanced) ... facilitating (impeding) ..."

Please consider eliminating the words in parentheses, or using a difficult format to convey this information. It's confusing to discuss both polarities (both an increase and decrease in ozone) within the same sentence structure.

8. Line 37: The circulation changes mostly occurred in the late 20th century, not the early 21st century, as the ozone hole was increasing in size from the 1980s until around the year 2000. Since that time, the ozone hole has stabilized in size, and may in fact be starting to recover (see Solomon et al. 2017).

9. Lines 44–45. See Fig. 3 in Kang et al. (2011). The precipitation changes associated with Antarctic stratospheric ozone depletion are more accurately described as an

increase in the subtropics and high latitudes, and a decrease at mid-latitudes.

10. Lines 55–57: This explanation of the surface temperature anomalies associated with Antarctic ozone depletion is not consistent with previous literature. See discussion in Thompson et al. (2011) and the references therein. The surface temperature anomalies are linked to how the poleward circulation shift associated with the ozone hole affects localized wind patterns (and associated thermal advection) at each location.

11. Line 110: The vertical pressure range (100–50 hPa) contradicts that in footnote #2 of Table 1 (150–50 hPa). Please correct.

12. Line 142: If SSTs are specified, the term “coupled” here is misleading. Follow convention in the literature, I would recommend using the term “coupled” only if the atmosphere model is fully coupled to an interactive ocean model.

13. Line 148: This statement seems to contradict the statement on line 143. The model has middle atmospheric chemistry, yet the model does not include interactive chemistry. This needs to be clarified.

14. Lines 151, 154: The text refers to a reference period of 1980–2015, while Table 1 refers to a reference period of 1995–2005. This needs to be clarified and standardized throughout the paper.

15. Line 164: I would use “break down” rather than “rupture” here to be consistent with terminology in previous literature.

16. Line 167: This lead time is not unique to NH stratospheric ozone perturbations. It is consistent with the tropospheric anomalies associated with NH sudden stratospheric warmings (Baldwin and Dunkerton 2001) and SH stratospheric ozone depletion (Thompson and Solomon 2002).

17. Lines 236–240: This statement is not consistent with the figures. Figures 4–6 show a barotropic circulation response (same sign throughout the depth of the troposphere),

with an anomalous cyclone over western North America at all levels (Fig. 6).

18. Lines 248–250: How so? I don’t understand the dynamical basis for this statement.

19. Lines 254, 258, 365: Reanalyses cannot adequately resolve convective activity. The anomalous downwelling here is associated with synoptic-scale processes (see positive geopotential heights in northeast Pacific in Fig. 5). The pattern in Fig. 7a should closely correspond to sea-level pressure anomalies (a surface high in the northwestern United States and a surface low in the southwestern United States).

20. Line 282: It doesn’t look like opposite to me . . . just shifted a little further to the north in the model than in the observations (which, of course, would make a difference for regional impacts as the authors nicely state on the subsequent lines).

21. Line 301: The SST pattern looks a lot like the Pacific Decadal Oscillation (PDO) or the North Pacific Mode (Hartmann 2015). How well correlated is the time series of the “Victoria Mode” with these modes?

22. All figures: I think it’s unnecessary to show both the correlations and composite differences (Figs. 1 and 3, left and right columns of Fig. 4-5), as they basically convey identical information.

23. Figures 1 and 3: It’s difficult to interpret these patterns with so much of the map left blank. I would recommend showing the correlation coefficients for the entire map, and stippling those regions that are statistically significant.

24. Figure 2: What are the dashed black lines? A measure of statistical significance?

25. Figures 4–5: Is it necessary to show both geopotential heights and zonal wind? Both figures convey exactly the same information (via geostrophic balance).

26. Figure 7: It would be good to clarify that blue is upward motion and red is downward motion.

27. Figure 12: Because the model has prescribed SSTs, how do you know the model

SSTs associated with ASO anomalies? Are these some version of the observed SST anomalies as they don't look exactly like those in Fig. 10?

Typos

Line 96: central of China -> central China

Line 134: regarding -> regarded

Line 147: is at -> are at

Lines 173, 354: Washington and Oregon states -> Washington and Oregon

Line 176: the Fig. 1 -> Fig. 1

Line 259: enhances -> weakens

Table 1, R4–R7: a SST anomalies -> SST anomalies

References

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