

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

## **Anonymous Referee #2**

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The manuscript presents a well-designed study of the effects of variability in spring-time Arctic stratospheric ozone (ASO) on the tropospheric circulation over the Pacific basin, extending into the north-west United States. The authors present statistical relationships between a variety of physical climate variables and ASO in observations, finding an inverse correlation between ASO anomalies and March precipitation over the north-west United States, and then explore the causality with a number of WACCM model simulations using anomalies applied to the prescribed ozone and sea-surface temperatures used in the model. The model simulations provide convincing evidence that the combined effect of the ASO anomalies and correlated changes in sea-surface temperatures over the Pacific can reproduce the observed pattern of changes in winds

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and precipitation. The study is well thought out and presented and I have no serious concerns about the methodology. The one significant missing aspect to the manuscript is the way the authors discuss ozone variability and the effects of ozone variability on dynamics as a completely independent forcing. The model simulations are convincing in that the specified ozone can be modified and the impact on the dynamics can be estimated in a one-way cause-and-effect manner. But in the real atmosphere there is a very tight coupling between dynamical modes of variability and Arctic ozone. Variability in the amount of planetary wave forcing from the troposphere has a direct connection to the strength of the Brewer-Dobson circulation and the amount of poleward ozone transport each year. And the occurrence of Sudden Stratospheric Warmings in the late winter or early spring can determine whether polar stratospheric temperatures cold enough for heterogenous chemistry on polar stratospheric clouds will occur and produce significant chemical ozone destruction in the Arctic. I think there are two important implications for the manuscript under consideration here. One is that the observation-based analysis must discuss the strong coupling between dynamical variability and ozone variability and must recognize that the correlations of certain physical variables with ozone also reflect correlations with other aspects of dynamical variability. And second, I believe the authors cannot state that the Victoria Mode anomalies in Pacific sea-surface temperatures are caused by, as opposed to being associated with, the ASO anomalies.

As given below in the minor comments, in a few places through the manuscript the differences in the circulation between different WACCM experiments are described in very direct ways. It would be much more illustrative for the reader if these changes could be associated with changes in the position of significant climatological features, in a similar way that the Antarctic wind changes can be summarized as a pole-ward shift of the jet.

Minor comments:

Lines 15 – 18: Following my concerns about correlation and causality, the sentence ‘In

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addition, the ASO changes cause sea surface temperature anomalies over the North Pacific that would cooperate with the ASO changes to modify the circulation anomalies over the northwestern US.' should be softened.

Lines 109 - 111: As stated here, the ASO is calculated as an anomaly after removing the annual cycle and trend. I would imagine the long-term trend is predominately due to the rise in ozone depleting substances. Why was the trend removed from the calculation of ASO, as I would think the March ASO anomaly related to ozone depletion would be part of the signal you are looking for? And is the trend calculated as a single linear trend across the entire period or some measure that is related to halogen loading in the stratosphere such as Equivalent Effective Stratospheric Chlorine (EESC)? As the period analysed is 1984 – 2015, or so, this would include both the rapid increase in EESC up to ~2000 and the plateau or slow decline since then and a single linear trend across the entire period would be a less than ideal estimate of the forced response.

Line 117: 'Another set of ozone dataset is...' sounds a bit redundant. Could I suggest 'Another set of ozone data is...'

Lines 149 – 151: The statement 'The model's radiation scheme uses these conditions: fixed greenhouse gas (GHG) values, averages of emissions scenario A2 of the Intergovernmental Panel on Climate Change (IPCC) (WMO, 2003) for 1980–2015.' is difficult to interpret. Is it that the fixed GHG values that were used are the 1980-2015 average from the A2 scenario? It seems a bit clearer in the text in Table 1, but there the average is said to be over 1995-2005.

Lines 212 – 214: The correlation of zonal wind anomalies with the ASO is described as: 'This implies that the increase (decrease) in ASO can result in enhanced (weakened) westerlies in the high and low latitudes of the North Pacific but weakened (enhanced) westerlies in the mid-latitudes.' The changes in southern hemisphere winds associated with ozone depletion are often described in terms of a shift of the jet that produces a dipole pattern of changes in wind. Here the authors argue that the ASO is associated

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with a tripole of changes in zonal wind. Do the authors have an explanation for the pattern of changes that can be related to shifts or changes in magnitude of climatological features like the Aleutian Low? And can other explanations for the changes at low latitudes, such as ENSO, be ruled out?

Lines 236 – 240: ‘This kind of circulation anomaly corresponds to an anomalous cyclone (anticyclone) in the western US in the middle and upper troposphere, which is likely associated with a strong low (high) pressure system in the middle and upper troposphere and a relatively weak high (low) pressure system in the lower troposphere.’ I can see how this description fits with the pattern of wind changes shown in Figure 6, but that the pattern of changes shown in panel (A), for example, showing a cyclonic pattern centered over the south-western US does not necessarily mean that this is caused by the appearance of a well-defined, anomalous cyclone. While the pattern of the differences is cyclonic, it could be due to the weakening of an anticyclone? The description would have a stronger physical basis if the changes were related to changes in the strength of position of well-recognized climatological features.

Lines 248 -250: ‘In addition, a strong low-pressure system in the middle and upper troposphere over the western US during positive ASO anomaly events (Fig. 6) suggests downwelling flow in the region.’ Similar to the concerns about the interpretation of Lines 236 – 240, there is a direct link made between a pattern of changes and the appearance of a particular meteorological feature.

Lines 251 – 262: While I can understand how changes in vertical velocity ( $w$ ) are coherent with the large-scale changes in circulation, the text in this paragraph makes a direct link between changes in  $w$  from the NCEP2 reanalysis and changes in convective precipitation. For example, at lines 253 – 255: ‘When the March ASO increases, tropospheric convective activity in the northwestern US ( $115^{\circ}$ – $130^{\circ}$  W) weakens, corresponding to anomalous downwelling.’ Can a direct link between convective precipitation and changes in monthly-average vertical velocity be made? I think the authors would need to support this statement with citations to previous work. I am also some-

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what sceptical about the general direction of the argument, which appears to be trying to link the circulation changes to precipitation changes. Is convective precipitation an important fraction of precipitation in the north-west US in March-April? I would have thought the precipitation changes shown in Figure 1 are a much more straight-forward reflection of changes in orographic precipitation related to the decrease in wind and (presumably) moisture transport?

Lines 267 – 268: The WACCM experiments detailed in Table 1 show that the perturbed ASO simulations vary ozone by +/- 15% between 30N and 90N. How realistic is this perturbation compared with the estimates from SWOOSH and GOZCARDS datasets? Perhaps a figure of the zonal-average difference could be included for the composite positive and negative ASO years? At high latitudes a +/-15% variability does not sound too large, perhaps even a bit small, but a +/- 15% change at 30N seems quite large.

Line 275: Beginning here, the results from the WACCM simulations are presented. Figures 9, 11 and 13, which show the differences between the WACCM experiments do not have any indication of the statistical significance. All of the other difference plots did have some manner of denoting statistical significance at the 90% level and these three plots should as well.

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