**Review of** “Ozone recovery effects on mesospheric dynamics in the southern hemisphere”

By A. Kuchar et al.

**Recommendation:** Reject

This study uses observations made over 2008-2019 at three meteor radar sites located at high latitudes in the Southern Hemisphere, together with reanalysis data and simulations from two high-top models to argue that (1) the transition of the zonal wind from the winter (westerly) regime to the summer (easterly) regime in the stratosphere has trended to earlier dates and (2) that the changes in the zonal wind are reflected in trends in momentum flux and gravity wave potential energy above 80 km obtained from the MR radars and SABER observations, respectively.

I found the conclusion on the earlier zonal wind transition convincing. This is essentially derived from the reanalysis data (MERRA-2 and JRA-25/JRA-55), since the two models used (WACCM-X and GAIA) are constrained by reanalysis in part or all of the stratosphere. The conclusions regarding momentum flux and wave energy density are much more tenuous, as discussed in the specific comments below. The discussion of those measurements glosses over many inconsistencies among the three MR sites to the point that I found the conclusions based on MR data unconvincing.

All in all, the paper is too long and the conclusions from the meteor radars are too weak to warrant publication.

**Specific Comments** (line number)

(5) “our results reveal a significant delay in the spring transition”: I think you have this backward. The spring transition actually occurs earlier (trends are negative in austral spring). See also comment at (176).

(16) “our planet’s climate”: I have not seen any evidence that the MLT plays a role in what is conventionally understood as “climate”, that is, the climate of the troposphere. There is also no such claim in the reference cited here, Smith (2012).

(26) “ZW”: I don’t see any need to introduce an acronym for zonal wind. It only leads to confusion when the reader forgets what “ZW” stands for. I would suggest eliminating ZW and just writing “zonal wind”.

(44) “reported a decrease in the ZW”: Quibbles about the acronym aside, this statement is ambiguous. What does “a decrease” mean? Is it a reduction of the magnitude of the zonal wind, regardless of direction? If so, “weakening” might be a better term to describe the change.

(62) “the translated effect”: I think it would be clearer to write “the impact”.

(95) “parameters … are given in hourly values”: Do you mean that the output frequency is hourly for the variables of interest in the present study? Or are you referring to the model time step?

(103) “an altitude-extended configuration of … CESM”: More precisely, WACCM-X is in extension into the thermosphere (to about 500 km) of WACCM (the Whole Atmosphere Community Climate Model), which has an upper boundary at about 140 km. Both models use tropospheric physics parameterizations from CAM, the low-top atmospheric component of CESM.

(105) “upper boundary in the thermosphere”: Please state explicitly at what altitude the upper boundary is located.

(108) “Further details …”: One detail that ought to have been included here is whether this version of WACCM-X is based upon WACCM4 or WACCM6 physics. There are large differences between the two.

(130) “resulting in large discrepancies in … ozone”: Would such deficiencies (through their role in radiative heating or cooling) impact the zonal wind reanalysis in the mesosphere in MERRA2? I think this deserves at least a comment.

(135) “SABER data are very sparse”: In what sense are SABER data very sparse? As shown by Salby (JAS, 1982), the SABER sampling pattern is equivalent to synoptic sampling with 0.5-day time resolution and horizontal resolution of about 4° in latitude and 6-7 zonal wavenumbers. However, the restriction on high-latitude coverage (poleward of ~53°) due to the 60-day yaw maneuvers is a much more severe problem for present purposes. Is that what you mean?

(175) “common negative trend in the stratosphere and mesosphere”: If you are referring to the strong (and statistically significant) negative trends in zonal wind seen in September through November in the upper panels of Fig. 2 (shaded in purple), these trends are reliably present only in the stratosphere. There is a *slight* extension of negative trends into the mesosphere, to about 60 km, but those trends are not statistically significant above about 40 km.

(176) “delay in the vortex transition”: The significant negative trends in zonal wind occur in austral spring, when the winds in the stratosphere and lower mesosphere are weakening but still westerly. That being the case, a negative trend at this time implies an *advance*, rather than a delay of the seasonal transition from westerlies to easterlies.

(198) “the same level of compatibility”: “the same level of consistency” might be clearer.

(200) “a weaker observational constraint”: I am not sure I understand this. Both components of the monthly-mean, large-scale flow ($u,v$) ought to be in thermal wind balance at these latitudes to a good approximation.

(210) “The trend estimate at Rio Grande”: The difference between Rio Grande and the other two stations, Rothera and Davis is striking, and not just as regards the trends. The zonal momentum

flux itself differs greatly among locations: it is predominantly negative at Rio Grande throughout the range of altitude shown, whereas it is predominantly positive at Rothera and Davis in SH winter (but not during the rest of the year at Davis). Do you have any idea why? Even if you do not, such a striking difference deserves a comment; it is difficult to have any confidence on explanations of the trends in momentum flux if one cannot even explain the differences in the *sign* of the momentum flux itself among these stations.

(211) "The weakening and strengthening of westward and eastward…": At Davis, I see negative (i.e., westward) momentum flux starting in September (Fig. 3a). The corresponding trend in flux is positive, so this may be interpreted as a weakening. However, at Rothera (Fig. 3c) the momentum flux is always positive (eastward), so the positive trend starting in September represents a strengthening.

(214) "Similar to Davis … Rothera": In what way are Rothera and Davis similar? At Rothera, the momentum flux is always positive (eastward), per Fig. 3c, so a positive trend beginning in September strengthens the flux. On the other hand, at Davis (Fig. 3a) the momentum flux is mostly negative (westward) after September, so a positive trend at that time weakens the flux.

(215) "a reduction of the westward momentum fluxes in the overall spectrum": It is difficult to understand what this means. Are you saying that, with reduced filtering by the winds at lower altitudes, the spectrum-integrated momentum flux has a smaller net westward magnitude?

(216) "Above 90 km we observe a strengthening of the westward momentum flux": What Figs. 3a and 3c show is a negative (westward) *trend* in the momentum flux above 90 km. The momentum flux itself is *not* westward (negative) anywhere above 90 km at Davis, and it is westward only above ~95 km at Rothera.

(217) "These features indicate the coupling via GW": The results from the three MR stations shown in Fig. 3 are so disparate that it is difficult to interpret them. Specifically, the momentum flux itself varies substantially with both altitude and season among the three stations—no two stations show a consistent pattern. The momentum flux trends are roughly similar (but of different magnitude) at Davis and Rothera, but they are either negligible or of the opposite sign at Rio Grande. Given the lack of consistency in both the momentum flux distribution and its trend, I find it impossible to draw useful conclusions.

(219) "the ZW trends in January and February …": The zonal wind trends (Fig. 2 a-c) in January-February are much weaker than in September-November and are not always significant. That aside, the trends are positive (green shading), which would indicate an *advanced*, not delayed, transition from the summer (easterly) to the winter (westerly) regime.

(228) "violin plots": What does the ordinate represent in the "violin plots"? Is it a PDF of slope values? If it is, why do we need a violin? (i.e., does the mirroring about the thin line that shows the median and quartiles add any useful information?)

(228) "allows us to assess" → allows us to conclude

(231) “long enough … to emerge a significant trend” → long enough to detect … a trend

(232) “time to emerge”: I believe “time to emergence” is the usual phrase

(243) “Fig. 7 … GW potential energy … at all three locations: Add “derived from SABER observations” to remind the reader what data was used to estimate the GW potential energy.

(244) “with limited data coverage”: I am not sure what you mean here. The large data gaps are due to the yaw maneuvers, when no observations poleward of about 53°S are made. That is, there is *no* data during this period rather than “limited data coverage”.

(245) “similarly slight positive trends”: Where do you see that in Fig. 7? Austral fall and early winter may reasonably be taken to mean the months of March through June. I do not see “similarly slightly positive trends during these months” at Rio Grande compared to Davis and Rothera of the locations shown. That aside, the trends are for the most part not statistically significant.

(247) “The negative trend starting in November above 80 km … corresponds with the weakening of the zonal momentum flux indicated in Figs. 3, 4, 5 and 6”: I have no idea what you are trying to state here. In Fig. 7, I can see a clear negative trend in GW potential energy (PE) in November at all three locations. In what way does this correspond to “a weakening of the zonal momentum flux indicated in Figs. 3-6”? Momentum flux is a signed quantity, whereas PE is not, so “weakening” is ambiguous, not to mention that the momentum flux trend in Fig. 3 during November is not uniform across locations or altitude. Adding to the confusion, Figs. 4-5 show a *positive* trend in momentum flux at Davis (in December, at 82 km), and Fig. 6 has nothing to do with trends but is instead a “time to emergence” plot!

(264) “probably since”: “possibly because” would be better. (There are many reasons why simulations might differ: the upper level of the reanalysis constraint is one; the reanalysis used to constrain the model is another; the response of ozone in the polar cap to the phasing out of ODS is yet another.)

(264) “the simulation constraint goes higher”: This would have been clearer as “the simulation is constrained through a higher altitude (~50 km) in WACCM-X than in GAIA (30 km)”.

(284) “trend slopes in SD-WACCM-X indicate a stronger change”: What does this refer to? In Fig. 9d-e for the meridional wind, the WACCM-X trends are everywhere smaller than those in GAIA or the Davis MR.

(302) “and by thermal tides”: Why do you include a reference to thermal tides among coupling mechanisms? You have not investigated the behavior of the tides in this paper.

(303) “diagnostics strengthen the evidence for vertical coupling”: While vertical coupling is a reasonable hypothesis, I find the MR data is insufficient to support it. See previous comments on the momentum flux and GW potential energy results. The response of the momentum flux in the mesosphere, shown in Fig. 3, is inconsistent among the stations. In my view, what this study

shows clearly is that there is an earlier transition from the winter (westerly) regime of zonal wind to the (summer) easterly regime in the lower stratosphere, and that this transition is probably linked to the recovery of Antarctic ozone. Furthermore, this conclusion is ultimately derived mainly from the MERRA-2 and JRA-25/55 reanalysis data because the models (WACCM-X and GAIA) are reanalysis-driven in the stratosphere.

(314) "positive trend in zonal momentum flux around 80 km … in September is robust": How so? If this statement refers to Fig. 3, Davis and Rothera do show positive (eastward) momentum flux trends between 80 and 90 km but Rio Grande does not. Now, it may be possible to argue that the location of Rio Grande, in the lee of the Andes, somehow makes this station different from the two Antarctic locations, but you have not presented such an argument.