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Interactive comment on "Distinguishing the impacts of ozone and ozone depleting substances on the recent increase in Antarctic surface mass balance" *by* Rei Chemke et al.

Anonymous Referee #2

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The submission has the potential to make a significant contribution to the literature, but it is not quite there yet. Before I would be able to recommend acceptance, there are a number of issues which need to be addressed.

Lines 12-35: Some of this literature is quite dated. In these various aspects here also make reference to the following more recent investigations – Golledge N. R. (2020) Long-term projections of sea-level rise from ice sheets. Wiley Interdisciplinary Reviews-Climate Change 11, e634, doi: 10.1002/wcc.634. Eric Rignot, Jeremie Mouginot, Bernd Scheuchl, Michiel van den Broeke, Melchior J. van Wessem and Mathieu Morlighem, 2019: Four decades of Antarctic Ice Sheet mass balance from 1979-

2017. Proceedings of the National Academy of Sciences of the United States of America, 116, 1095-1103, doi: 10.1073/pnas.1812883116. Cecile Agosta, Charles Amory, Christoph Kittel, Anais Orsi, Vincent Favier, Hubert Gallee, Michiel R. van den Broeke, Jan T. M. Lenaerts, Jan Melchior van Wessem, Willem Jan van de Berg and Xavier Fettweis, 2019: Estimation of the Antarctic surface mass balance using the regional climate model MAR (1979-2015) and identification of dominant processes. Cryosphere, 13, 281-296, doi: 10.5194/tc-13-281-2019.

Lines 24-31: Ozone depletion and associated changes in subantarctic synoptic activity have been linked with variations in the polar transport of a wide range of atmospheric constituents. To point to this broader context beneficial to cite the paper of Cataldo, M., H. Evangelista, et al., 2013: Mineral dust variability in central West Antarctica associated with ozone depletion. Atmos. Chem. Phys., 13, 2165-2175.

Lines 41-42: The paper should be clearer in describing the initial conditions (and specifically that temperature) for the different ensemble members, or this aspect should be deleted. The comments presented here are not particularly clear. Having said that, the relevant text from Jennifer Kay's 2015 CESM paper (cited here) is also somewhat unclear in saying '... spread in ensemble members 3–30 was generated by round-off level differences in their initial air temperature fields. Specifically, we applied random round-off level (order of 10**–14 K) differences to the air temperature field of ensemble member 1 to generate atmospheric initial conditions for ensemble members 3–30' (page 1337). More detail is required. E.g., was the perturbation applied independently to each grid point? Were the affected points spread over the global 3D model space, or restricted to certain models levels and/or geographical regions.

Lines 51-55: The analysis and results presented here are contingent on this linear assumption. This warrants some more words as to what the pitfalls and biases (e.g., introduced by self-dampening or self-amplifying processes) might be. Would be helpful here to reference some of the relevant comments in the LARMIP V2 paper of Anders Levermann, Ricarda Winkelmann, Torsten Albrecht, . . . and Roderik S. W. van de Wal, 2020: Projecting Antarctica's contribution to future sea level rise from basal ice shelf melt using linear response functions of 16 ice sheet models (LARMIP-2). Earth System Dynamics, 11, 35-76, doi: 10.5194/esd-11-35-2020.

Lines 62-63: Delete parenthetical comment. Acronym SMB has essential already been defined (at line 15).

Line 72 (Figure 2): Valuable and informative plots here for annual case. Might be useful to plot the 'zero line' on these. The plots show a great deal of white, but one is not sure what parts are showing increases versus decreases. Regarding the statistical significance of the changes, the panels for the three forcing experiments (b – d) show, e.g., significant changes centered on the 180E meridian from the RIS to the pole, while no such significance is displayed in the 'All' case. This may be OK (I haven't thought this thru) but at first sight looks strange, and is worth checking. It may be that the values are negative in the 'O3 strat' case (while they are unambiguously positive I panels b and d. Showing the zero line would help with this possible riddle.

Line 93: Replace 'mb' with 'hPa'. (Similar comment throughout the Ms, including lines 84, 96, 102, 139, 156, . . .)

Lines 105-110: In equn (1) use extra nested parentheses to make clear that the vertical integral is taken over all terms in the integrand and not just the last one. Also, the RHS of the equation gives the (mathematical) convergence of moisture at latitude phi. This latitude dependence must be indicated on the LHS, as well as what infinitesimal area is being considered here. (The flux convergence relationship with P-E only make sense when a finite area is being considered.) Appropriate overbars are required on symbols in the legends in Figure 5, as well as in the caption.

Lines 112-124: Some of this material is poorly expressed and misleading. The net flux of moisture into the polar 'cap' (poleward of 65S), can actually be determined directly from the flux across 65S. The authors comment that changes in this net flux can be used to account for changes in SMB mostly over the Antarctic continent. This statement

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is very misleading. The seas around Antarctic are host to intense and frequent storms (make reference here to compilation of Keay et al., 2003: Synoptic activity in the seas around Antarctica. Mon. Wea. Rev., 131, 272-288). These are associated with large P-E in those subantarctic waters (and certainly south of 65S). Hence, a priori, the net precip south of that latitude cannot be regarded as a proxy for SMB over the content itself. In justifying this association, the authors cite the correlation of these two P-Es, but do not compare their MAGNITUDES. A quick calculation reveals these to be very different. The 65S polar cap covers about 25 million km**2. A flux of 0.1 mm/day (a typical value in Figure 4) gives a volume of water of 10**-4 m/day * 25 x 10**12 m**2 = 2.5 x 10**9 m**3/day. This is 2.5 Gt/day or 910 Gt/yr. This is about 3 times larger than the 300 Gt/yr (for 0.1 mm/day) indicated in Figure 4. This means that about two thirds of the vapor which crosses 65S precipitates into the ocean before it reaches the continent. While one MIGHT expect that if the total flux 65S changes by a certain fraction the P-E over the continent would change by a similar fraction. However this important part of the text must be expressed and justified more clearly. I appreciate that calculating eddy fluxes across latitudes in much easier than across irregular boundaries (like the Antarctic coast), and I have no great problem with what the authors have done. My main point is that they should be much more upfront with the caveats, and be clear on the synoptics in this complex part of the world.

Lines 118-124: Reinforce this message by refencing the study of Grieger, Leckebusch, et al., 2018: Subantarctic cyclones identified by 14 tracking methods, and their role for moisture transports into the continent. Tellus, 70A, 1454808, doi: 10.1080/16000870.2018.1454808 which demonstrates strong positive summer trends in subantarctic cyclone numbers (in association in increasing SAM), and their role in poleward moisture transport. Also valuable here to cite here the analysis of Papritz, Wernli et al. 2014: The role of extratropical cyclones and fronts for Southern Ocean freshwater fluxes. J. Clim., 27, 6205-6224 exploring the SH relationships between synoptic eddies and P-E.

Lines 153-166: Some interesting conclusions are reached here in connection with the relative importance of baroclinic and barotropic instability in driving the moisture-transporting transient eddies. I have a few issues with how this comparison was made. For the baroclinic part the authors calculate the delta vertical wind shear as the zonal wind difference between upper (300mb-500mb) and lower (600mb-800mb) levels. This will be very similar to the difference between 400 and 700 hPa, or an estimate of the baroclinicity at 550 hPa. The total moisture transport is dominated by the lower levels of the troposphere, and the 850 hPa level (which is frequently chosen for applications such as this) would be a much more appropriate level to take. For the barotropic instability case, the text states the this is determined from the 'vertically averaged delta uyy'. It is not clear whether this average was taken through the entire atmosphere and/or why were not some key atmospheric levels chosen for this. At the very least the authors should determine their baroclinicity at a more physically-consistent level, and justify why a similar level should not be used for the barotropic component. As it stands, the analysis has not convinced me that it '. . . suggests that increased barotropic instability is the primary mechanism via which ozone depletion enhances the eddy-moisture flux, resulting in a larger Antarctic SMB over the second half of the 20th century.

Lines 179: After '. . . gradient of the mean relative vorticity' insert 'of the zonal flow'. (This connection has only been shown for uyy.)

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