Review of

Conway et al.

"Bifurcation of potential vorticity gradients across the Southern Hemisphere stratospheric polar vortex",

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Recommendation: Should be suitable for publication in ACP with some modest revisions and clarifications.

General Comments:

This paper uses ERA-Interim reanalysis data to describe the climatological bifurcation of potential vorticity gradients in the Antarctic vortex edge region. Overall, the analysis is sound and the paper is well-written. To my knowledge, a detailed description/climatology of the bifurcation of PV gradients in the Antarctic vortex edge region has not been published, and the material will thus be of interest to ACP readers. In its current form, the paper is somewhat lacking in referencing previous work related to vortex edge definitions and mixing barriers, and in explaining the rationale behind the definitions of bifurcation index and other threshold choices used. The introduction, in particular, doesn't provide a clear picture of the relevant background and previous work. Once these issues, and some other specific (minor) points listed below, have been addressed, the paper will be appropriate for publication in ACP.

Note 1: Obviously, Zachary Lawrence and I work together. However, we were each notified about the paper separately and each read the paper and wrote up our comments that are posted here independently.

Note 2: I am not suggesting that you cite every one of the numerous Manney et al. papers mentioned in this review; it is inevitable that I am most familiar with those I wrote, and in some cases they may lead you to more appropriate citations; in a couple of cases they are just mentioned as "of interest".

Specific Comments (in order of appearance in the paper):

Introduction:

Overall, the authors should make clear that much of this discussion (and which parts of it) applies to the Antarctic polar vortex only; many of the statements made are either not applicable to the Arctic vortex, or would need to be qualified (e.g., the discussion of ozone loss and dispersal from the vortex) to describe the Arctic; this is fine -- the paper focuses on the Antarctic -- but should be said explicitly when there are statements that apply only to the Antarctic. In addition, some statements are rather vague, may not be quite accurate, and/or could use more appropriate citations. Specifically:

- Page 1, lines 20--21: "...cold air formed in the polar night…" -- "formed" doesn't seem quite accurate.
- Page 1, line 19: Schoeberl et al (1992) would be a good reference to add to those here; also please use "e.g.," ahead of the reference list in cases like this where there are many papers relevant to the statement.
- Page 1, line 22: Add "e.g.," before or "and references therein" after "(Solomon, 1999)"
- Page 1, lines 23--24: Please emphasize that this applies to the Antarctic vortex
- Page 2, lines 1--2: There are many, some earlier, references that could be used here for both statements (I'm sure that is also the case for the statement on line 3, though I am not well-read on such CCM studies). At the very least, "e.g.," or "and references therein" should be used. Some other useful papers to cite (including only ones specifically relevant to the SH) might be:
    - Schoeberl et al, 1995 (JGR)
    - Lary et al, 1996 (QJ)
    - Manney et al, 1999 (JGR), 2005 (JAS), 2005 (GRL)
    - Ajtić et al, 2004 (JGR)
- Page 2, lines 4--18: There are some vortex edge definition methods that are not mentioned here, as well as relevant papers It would be good to cite to briefly discuss other ways the vortex edge or boundary region can be defined besides those you already mention. Also, the statement that "Bodeker et al. (2002) formalised this method" is not accurate: Determining the vortex boundary using the maximum in windspeed x PV gradient is not the same as determining it by the maximum PV gradients but requiring that maximum to be in proximity to the windspeed maximum (see, e.g., the brief discussion in Manney et al., 2007, who implemented and compared the two methods, starting with code provided by Eric Nash for the method described in Nash et al). It would also be good to mention some other papers that discuss different ways of defining the vortex edge, especially some of the early papers that use PV gradients (eg, Trounday et al, 1995) and/or use other methods and compare with PV gradient methods (eg, Paparella et al, 1997, JGR, already cited here but the comparisons are not mentioned; Rummukainen et al, 1994, Ann. Geophys.) Smith and McDonald (2014, JGR) used the "function *M*" to look at the strength of the Antarctic vortex and the width (area) of the vortex boundary region. The authors may be interested (though certainly not obliged to cite a paper that will only be available online sometime in the next few days after I'm writing this review) in the vortex edge method in Lawrence and Manney (2017, JGR, in press), which also gives further discussion of various vortex edge definition methods and literature.
- Page 2, line 12: Add "e.g.," before "Manney and Lawrence, 2016" (I am sure others have done this as well; our two Manney et al (2015) papers are ones where I know this was done).
- Page 2, lines 11--18: May be worth noting that Manney and Lawrence (2016) compared their climatological selection of vortex edge PV values to daily PV gradients, effective diffusivity, the function *M*, and trace gas gradients.

Section 2.1:

It would probably be useful to cite Fujiwara et al. (2017), the overview paper for the SPARC Reanalysis Intercomparison Project, here -- they provide a detailed description of all the commonly used reanalyses.

While not critical to this paper, it would be interesting to see how the bifurcation changes and where it is present at levels above 850K. I realize that ERA-Interim isentropic level PV data are not provided above 850K, but a few isentropic levels in the upper stratosphere are provided in the CFSR dataset. Could you use that to make a brief comment as to what we might expect at higher altitudes?

Section 2.2:

Page 3, lines 9--13: The description of this procedure is not clear to me. As I read this, you are using the PV data as distributions from which to find, at each timestep and vertical level, what PV value each of 181 linearly spaced percentile values corresponds to, and then calculating the EL of each of the resulting 181 PV values? Since PV as a function of EL is calculated (by whatever method) before the gradients with respect to EL are taken, how could this lead to spurious zero values of PV gradient? Since the fact that there are regions (which vary from day to day) with much weaker and stronger PV gradients means that the resultant PV values at which you find EL will not be uniformly spaced, it appears to me that using the percentile steps might lead to a very non-uniform coordinate mapping (from lat/lon to EL) and one that changes from day to day and level to level -- could this then have a significant effect on the subsequent numerical derivatives to get the PV gradients? I apologize if I am off the mark with these questions -- if I am, I suspect that also argues for clarifying the description.

Page 3, line 16: Is 40deg EL always low enough for the SH polar vortex? This may not be a problem at the middle and lower stratospheric levels you are looking at here, but from the vortex edge diagnostics I've looked at, in the upper stratosphere, especially in the SH, the vortex is often very large, and I frequently found peaks in PV gradients or windspeed x PV gradient that were slightly equatorward of 40deg.

Page 3, lines 16--19: Is anything done to eliminate spurious peaks in this primary and secondary peak identification? One of the advantages of using windspeed x PV gradients or constraining PV gradient maxima to be near windspeed maxima is reducing the occurrence of such peaks. Can you say anything about how much of an issue this might be, especially, eg, where you find widely separated peaks and one is near 80 EL where spurious peaks are more common? Also, are any other criteria used (e.g., a minimum windspeed, such as in Nash et al, 1996 and Manney et al, 2007; and/or a minimum PV gradient such as in Manney et al, 2007) to determine when the vortex is well-defined enough to analyze (you do mention a minimum PV gradient criterion in a figure caption later; if this is applied in general, it might be good to move it to this methods description and explain the reasoning behind the choice of value)?

Page 3, lines 20 to 28 and Table 1: The rationale behind the choices of combinations of peak ratio and dip fraction for the bifurcation index isn't clear, could you please explain this further? Why does it make sense for the dip fraction to go up as the peak ratio goes up? What did you look at to determine these values (e.g., extensive visual inspection and of what if so, or some other measures)? When you say on lines 24--25 that the dip ratio has more influence on BI, why is that desireable (presumably it is or you wouldn't have made the choices you did)?

How are the various bifurcation diagnostics (BI, as well as peak ratio and dip fraction) affected by changes in EL resolution (e.g., using 91 or 361 percentile steps)? Would such a change significantly affect your results (including the thresholds used to determine BI)?

Figure 1: You might consider using thicker lines and/or bolder colors; while it is OK on a good high-resolution monitor, on many printers or lower resolution monitors, the red line in the lower panel will be barely visible. (A similar comment applies to Figure 9.)

Section 3.1:

Page 5, line 9: What is meant by "the inside and outside edges of the region of maximum wind speed"? Since the maximum wind speed is a point (on each isentropic level), the "region" of maximum wind speed is not defined unless you define it, which must be done before you can define the inside and outside of it and whether something is near that. (This occurs again later in the paper as well.)

Figure 2: Please specify in the caption that these are monthly means (I believe that's true).

Section 3.2:

Figure 4: It might be helpful to put horizontal lines at the boundaries of what you call "mid-levels", since those are mentioned several times in the text. Also, what is the reasoning behind the choice of the PV gradient threshold?

Section 3.3:

Page 7, line 5 and Figure 5: It isn't at all clear to me that Figure 5 shows the peaks to be "more dispersed in August (which I would take to be "winter" rather than "Spring") than September, if that is what you mean by this statement. If by "earlier in the season" you mean before August, why not show July as well to demonstrate this?

Section 3.4:

Page 8, line 2: Can you be more specific than "in some years"? How many of the years do you see it in during a significant part of the month?

Figure 6 shows a sharp change between 430 and 475K in September, which can also be seen in Figure 5 in August and September. Do you have any thoughts on why this might be?

It would be nice to see August and October in Figure 6 as well as the three months shown, especially since the months that are shown look very different -- what does the transition between these regimes look like? Or at least describe this in the text.

In Figure 6, it is very hard to distinguish the lowest 2-3 values from the zero values that represent times without a sufficiently well-developed vortex -- perhaps you could adjust the color palette and/or find another way to clearly indicate “missing data”.

Section 4.1:

It would be helpful to have the CFSR plots together with the ERA-Interim ones, to facilitate comparison; this comparison also seems like something that it would be good to show earlier in the paper.

Section 4.2:

Figure 9 and discussion: It would be very helpful to show the gradients of relative vorticity, absolute vorticity, and d(theta)/dp -- it is difficult to judge a change in the gradients from the fields themselves. Also, it would be much more convincing (perhaps the same term is not the dominant factor in all cases? Or not as dominant for all BI values?) if this analysis was done for some sort of climatology or composite of cases with bifurcation rather than a single day in a year that may highlight a particular regime of bifurcation.

Page 11, line 3, I’d suggest saying something like “...provide context for recent *reports of* trends in ozone…”, since the results of Solomon et al (2016) are controversial.

Section 4.3:

Page 11, lines 6--9: Not necessarily something to change in the paper, but I just note that such large variations from day to day are fairly common even in cases where there is no bifurcation (e.g., in the NH -- you can see examples of this in pretty much any winter season on some isentropic levels, eg, in the vortex edge PV and area routinely posted on https://ozonewatch.gsfc.nasa.gov/meteorology/NH.html
-- these are calculated using the method of Nash et al, 1996, but other PV gradient-based methods can suffer from the same problem). This is one reason why a constant climatological value is sometimes preferred over an automated day-to-day calculation for applications where a single value for the vortex edge is needed (e.g., Manney et al., 2007; Manney and Lawrence, 2016; Lawrence and Manney, 2017).

Page 12, lines 6--7 and Figure 10: It would be interesting (and possibly a better demonstration) to compare this with what the inner and outer edges of the vortex boundary region look like when the method of Nash et al. (1996) is used without taking the bifurcation into account.

Typographical errors, minor wording suggestions:

Page 1, line 9: suggest changing “reduces” to “decreases” (“reduces” is not commonly used as a verb).

Page 3, line 9: “across the polar vortex” sounds a bit odd, since you are calculating the PV gradients over an EL range that includes both vortex and extra-vortex regions. Perhaps reword this.

Page 3, line 23: When you say “profiles” (this also occurs elsewhere in the paper), do you mean the PV gradient vs EL curves for each time? This may seem a trivial point, but can be confusing to those of us who automatically think “vertical” when they see the word “profile”! Perhaps you could orient those of us who are thinking in a different direction by, the first time you use the terminology, saying something like “profiles of PV gradient as a function of EL”.

Page 6, line 9, “display” should be “displays” (or “wind speed” should be “wind speeds”).

Page 6, line 12, suggest “decreases” instead of “reduces”

Figure 6 caption, should be “Data are”, not “Data is”

Page 9, line 4, “pause” (implies a time variable) doesn’t seem like the right word here.

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