Comments on: "**Bifurcation of potential vorticity gradients across the Southern Hemisphere stratospheric polar vortex.**"

written by J. Conway, G. Bodeker, and C. Cameron

for *Atmospheric Chemistry and Physics*

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In this paper, Conway et al. use potential vorticity and other data from the ERA-Interim and CFSR reanalyses to highlight a common feature of the southern hemisphere (SH) stratospheric polar vortex: a double peak in gradients of potential vorticity with respect to equivalent latitude (EqL). The authors describe a method to detect this bifurcation by first locating the maximum PV gradient between -80 and -40 EqL (the primary peak), and seeking the next local maximum having a sufficient peak ratio, peak separation, and in-between dip fraction from the primary peak (the secondary peak). They then quantify the bifurcation in the PV gradients by deriving a bifurcation index (BI) using thresholds of the peak ratio and dip fraction diagnostics. Conway et al. use these diagnostics to show occurrence frequencies in monthly and yearly composites that highlight when, where (in potential temperature and/or EqL), and to what magnitude these bifurcated structures arise. They also give a brief examination on how these bifurcations arise in the PV gradients with illustrations of EqL profiles of relative/absolute vorticity and vertical potential temperature gradients, and discuss the need to treat the SH vortex edge/barrier region more carefully.

The paper is generally well-written and clear, and I particularly appreciate its brevity. The subject matter of the SH PV gradient bifurcation alone is significant, and should be of interest to many who study stratospheric dynamics and transport. The detection algorithm and bifurcation diagnostics are also evidently useful. Thus, the paper is an important scientific contribution, and should be published in some form. However, I do think that the paper in its initial submission form suffers somewhat from a lack of references and discussion, as well as some minor organizational issues. Below I discuss my concerns and questions in more detail:

Comments and Questions

1. The introduction is rather short. While it is nice to have it be concise, the introduction in its present form does not discuss any previous examinations of the SH vortex boundary region. I do not know of any papers that show or mention the bifurcation of PV gradients, but there are many studies (some of which are already cited in the paper) that have discussed and shown that the SH vortex boundary region is often quite wide, especially relative to the NH vortex. Many of these studies have used more complicated diagnostics than PV gradients (e.g., effective diffusivity) to show this, so mentioning and briefly discussing such papers would reinforce the following statement in the intro *"Because of the minimal computational requirements, the PV gradient is an attractive method to define the vortex boundary region."* Some potential relevant references are already cited in the paper; for example, see figures and related discussion on: Figures 3 – 5 in Paparella (1997) show trajectories of balloons trapped inside the SH polar vortex core and polar vortex boundary; Figures 1 & 2 in Lee et al. (2001) show the width of the vortex boundary region in effective diffusivity during 1996; and plates 1 – 4 in Haynes and Shuckburgh (2000) show monthly means of effective diffusivity that can allow comparison of the widths of the Arctic vs Antarctic vortex boundaries. Other more recent studies that may or may not be relevant to look at include (but certainly are not limited to): de la Cámara et al., JAS, 2012; Abalos et al., QJRMS, 2016; and Curbelo et al., NPG, 2017.

2. Tied to 1, the results shown in the current paper could be made more impactful in both the introduction and conclusions by being more clear about the "magnitude" of what is shown. In my opinion, the paper sells itself a bit short. More specifically, I mean that many previous studies (including some of the ones discussed in comment #1) only show results relevant for a single dataset for one to a few winters; this paper shows results relevant for two reanalyses for years from 1979 to roughly the present. I think that is worthy of being highlighted as one of the strong points of the paper.

3. This is very minor, but in section 2.1, the years used for ERA-Interim and CFSR should be listed.

4. Figure 1 and Table 1 are helpful, but it would be nice if a new figure was included showing EqL profiles of PV gradients for different values of the BI. For example, a 2 x 3 figure with panels showing (along with dates, isentropic levels, peak ratios, and dip fractions listed) representative cases with BI = 0, 2, 4, 6, 8, and 10. I think this would give the reader a better sense of the connection between BI and the geometry of the PV gradient profiles, as well as enhance understanding and the significance of the other figures in the paper.

5. In Section 3.1 and Figure 2, is it really accurate to say that there are two regions of enhanced PV gradients? Since there is no clear dip, it looks as if there is only one broad region of enhanced gradients in all cases, even though the location of the climatological maximum gradients moves "equatorward" with height in August & September. This pattern seems to be clearly formed from averaging many cases with bifurcated PV gradients having peaks in different EqL locations, which would strengthen the idea that the PV gradient bifurcation needs to be examined on a year-to-year basis to really see and understand it (i.e., as is shown in Figure 3).

6. In Figure 3, I would be curious to see whether the bifurcated structure also shows up in “regular” zonal means of meridional PV gradients (i.e., with respect to “regular” latitude), since the SH flow and PV distributions are usually relatively close to zonally symmetric. If the bifurcated structure also exists in the zonal mean picture, this could be an interesting result with further implications (e.g., for wave guiding/propagation).

7. The comparison with CFSR in section 4.1 is so short that it almost seems unnecessary. First of all, why not show all the same panels for CFSR in Figures 7 and 8 as those in Figures 4 and 5? And since section 4.1 is so short, why not fold the CFSR results from Figures 7 and 8 into Figures 4 and 5 (and maybe even Figure 6) and discuss the comparisons in sections 3.3 and 3.4 directly? Figures 4 and 5 would obviously increase in size (and more labels would be necessary), but there would be an advantage to having the results from the two reanalyses side-by-side for easier comparison, and less figures overall. This would also reinforce one of the strengths of the paper (see my comment #2).

8. Figure 9 and its discussion in Section 4.2 would be a bit more useful/significant if Figure 9 showed composites of some form, so that readers would know the discussion is more broadly relevant than to a single day of data. I do realize, however, that this may not be easy or possible to do since the locations of peaks in PV gradients (and hence the geometry of the PV and relative/absolute vorticity profiles) will vary from day to day – but it is something that might be worth a try.

9. Also in Figure 9 – how were the fields of relative/absolute vorticity obtained? Were they derived from the potential vorticity and temperature fields, or the wind components, or were they downloaded?

10. The following is just an idea I had, but something the authors might consider: The current organization of the paper is fine, but since the paper is largely focused on presenting the methods and diagnostics, there is an alternative organization I see that might flow better for readers (and would only require minimal work). Arguably the bifurcation detection methods and diagnostics are part of the results of the paper. Thus, section 2 could be changed to a “Data and Background” section wherein section 2.1 would remain the same, section 2.2 would discuss how the EqL profiles of PV and PV gradients are obtained, but a new section 2.3 would be inserted (the background) that included Figures 2 and 3 (which would become Figures 1 and 2) and their discussion. The BI panel in Figure 3 would have to be changed to something like "average peak separation in degrees EqL" since BI would not be introduced yet. Then in section 3, the description of the bifurcation detection and diagnostics (and the current Figure 1) would be included in section 3.1 with a title like "PV gradient bifurcation detection and diagnostics." This organization would lead readers from the climatologies (which smear out the bifurcated gradients), to a single SH winter that illustrates the phenomenon, to the detection methods and bifurcation diagnostics, and finally to the results/figures that directly show the BI and other diagnostics (which, with this organization, would have just been defined). Again, this is just an idea; the authors should feel free to flatly reject it!

References

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