Review of Amemiya and Sato, 2020

July 9, 2020

The study by Amemiya and Sato addresses the characterization of the variability of the Asian monsoon upper tropospheric anticyclone on sub-monthly timescales. The focus is on describing the east-west oscillations of the anticyclone on quasi-biweekly timescales, and assessing whether this movement is driven by variability in convective forcing, or whether the total area of the anticyclone (in terms of weighted low PV area) is conserved during this process. They show that both the analysis of variability of the low PV area, as well as analysis based on variability modes (EOFs) of geopotential height anomalies reveal the biweekly oscillation of the anticyclone, and argue that the oscillation is mostly associated with "passive advection" (i.e., not with variable forcing).

The paper addresses open research questions on the anticyclone variability, and used novel and promising techniques. However, I have a number of concerns regarding the derivation of the methodology, and regarding the interpretation of the results. After those issues are properly addressed, the paper will surely be an important contribution to the literature on the variability of the anticyclone.

1 Major comments

1. My first major concern is on the way, the methodology of the "weighted low PV area" is introduced and motivated. While the PV-area conservation equation (Equ. 1) is widely used, the low PV area weighted by the thickness (isentropic density sigma) is introduced in this study (at least to my knowledge). First of all, the weighted low PV area ($\hat{A}$) should be properly defined, e.g. as:

$$\hat{A} = \int_{\lambda} \int_{\phi} \sigma r^2 cos(\phi) d\phi d\lambda \tag{1}$$

   Most importantly, I would like to see the derivation of the equation for the weighted low PV area $\hat{A}$, i.e. Equ. (3) in the paper. I don't see that it is straight forward to obtain this equation (if so, please state the necessary steps), so a proper derivation (possibly in the Appendix) would be important. Moreover, the derivation of the equation for only the western part of the PV area (Equ. (4)) could also be explained better, e.g. to elucidate the emergence of the Flux term. Is the diabatic term (term 2 on right hand side of Equ. (4)) indeed integrated over the whole low PV area, or only over the western part, as I would assume? If sou, please modify the equation to make this clear.

   Next to the proper definition and derivation, I would appreciate a deeper physical reasoning for the weighting of the PV area. If I understand it correctly, the relevant effect here is that, given the conservation of PV for a given air mass, a vertical (i.e, in theta) compression of this air mass leads to a larger horizontal extend, i.e. an increase in area. Thus, by applying this scaling you essentially move to an equivalent 2-d representation of the air parcel, and $\hat{A}$ is the area of this equivalent 2-d air parcel. Maybe the addition of a simple sketch would help the reader to get a better understanding of the meaning of the scaling.

A related question on the scaled versus non-scaled PV area: according to your equations, the divergence term vanishes for the density-scaled area equation. If my above understanding of the weighted-PV area is correct, this would imply, that the role of the divergence term merely is to deform the air parcel. However, as you mention, in earlier work it was shown that the divergence term in the conventional PV area equation is closely related to the diabatic term, and indeed maximizes in the regions of low OLR (as indication for convection). Following this finding, the interpretation would be that the divergence (i.e., convective outflow) induces the low PV, and not merely deforms the air parcel. Is this effect completely incorporated in the diabatic term in the weighted low PV equation, or how can I understand the physical meaning of the terms?

2. The period of the "quasi-biweekly" oscillation is not clearly quantified until rather late in the paper, namely with the power spectrum in Fig. 6. Rather, the motivation for those time-scales is given by visual analysis of the timeseries of the weighted low PV area in Fig. 2. I'd encourage the authors to move the power spectrum to an earlier point in the paper, and compare it to the power spectrum of the total weighted low PV area (see also specific comment below). Further, the peak in the power spectrum of the fluxes (Fig. 6) is rather broad (with a plateau-like peak between 20 to 9 days), does this range of time-scales still correspond to the "quasi-biweekly" oscillation?

3. My third major comment is on the interpretation of the results, mainly the overarching question of the nature of the quasi-biweekly oscillation as "passive advection" versus reflecting variability in sources (i.e., convective forcing) or sinks (e.g., the actual shedding of air from the anticyclone). This comes down to the question, whether the total low PV area is conserved during the oscillations (as is shown for the life cycle analysis in Fig. 13), or, whether the total flux across the defined boundary at 60 E explains the in- and decrease of low PV area to the west and east of the boundary. The latter is indicated to be the case by the close match of timeseries of area change and fluxes in Fig. 5. However, I wonder whether this analysis couldn't be made more quantitative by repeating it for longer time series (multiple years), and actually quantifying to which degree the total western PV area is explained by the flux (for both increasing and decreasing areas, i.e., positive versus negative values of $d\hat{A}/dt$ and the flux F). Also, I wonder to which degree the eastern part of low PV area is explained by the flux term. As the diabatic source is located east of 60 E, it maybe not surprising that the western part is explained by the fluxes from the east. In general, also the location of the boundary at 60 E could be varied to test the sensitivity of the results on the choice of the boundary longitude.

   Moreover, the results mentioned above are all valid for the 370 K level (if I'm not mistaken, I found it hard to identify which level the analysis is performed on at many places, see specific comment below). According to your Fig.3, in which the fluxes of low PV area across the 60 E line are shown for both 370 K and 360 K, the fluxes behave rather different at the two levels: At 370 K, the flux is close to zero in the mean (indicating back- and forth advection), while at 360 K, the flux is clearly eastward, which is in accordance with the "source" of low PV (i.e. convection) being mostly located east of 60E. Therefore, I wonder in how far the result on the "passive advection" of low PV air is valid also for the 360 K level (which does not lie well above the main convective outflow and heating level, as does the 370 K level).

4. Another major comment I have is on the interpretation of the OLR anomalies during the oscillation life cycle (as presented in Fig. 14). I can identify from the figure a clear signal in the OLR anomalies, with westward propagating negative OLR anomalies during phase 5 to 8, and positive OLR anomalies during phase 2 to 4. Thus, this might suggest that variability in the forcing of the anticyclone does play a role on those time-scales after all. This possibility is indeed phrased in the summary (lines 329-330), but this is a bit controversial to what is stated earlier (e.g. lines 302 to 303). On the other hand, the consistent OLR anomalies could also indicate that the quasi-periodic circulation anomalies of the anticyclone influence the occurrence of convection. This result might be consistent with the troposphere-deep circulation anomalies at 35-45 N, as shown in Fig. 15. This possible implication is discussed in the summary (lines 335 onwards), but I'd suggest that you could add here, that the OLR anomalies in the "life cycle" also show indications in this direction.

2 Specific / minor comments

- title: Change to "geopotential height fields". In my opinion, the latter half of the title (".. using PV and geopotential...) could also be skipped, but this is a matter of taste, so I leave it to the authors to decide.
- line 24: "to be dominant": consider rephrasing to "to be the dominant transport process"
- line 48-50: I wouldn't agree in that the paper by Nuetzel et al showed that the bimodality is a robust feature. Indeed, they showed that the bi-modality is very prominent only in older (NCEP) reanalysis data sets.
- line 113: This sentence makes it sound as if the low PV area in the anticyclone is usually conserved, but just not strictly, because of the "forcing processes such as deep convection". Diabatic heating and associated outflow from deep convection (divergent motion) is THE forcing process of the anticyclone, if it wasn't for that, there would be no low PV area to start off with. Therefore, I find this formulation a little weird. Please rephrase it to make the role of deep convection on forcing low PV more clear.
- line 130 / Equ. 2: On a similar note as major comment 1, a definition for the longitude-dependent quantity $L(\lambda, t)$ along the lines suggested for $\hat{A}$ could be given (i.e. as integral over ϕ).
- line 171: here, the authors state that the analysis of the timeseries in the preceding subsection "confirmed" that the dominant timescale of variability is the "quasi-biweekly" timescale. However, in the daily timeseries presented in Fig. 2, a monthly period is predominant, and the quasi-biweekly timescale is, if at all, only to be guessed "by eye". So either you have to weaken the statement here (e.g. indicates that quasi-biweekly

variability can be identified from the timeseries) or make the analysis more quantitative (see also major comment 2).

- line 186: For 360 K, the flux is negative around its minimum, even within the range given by the standard deviation. So this argument holds only for Fig 3a (370 K), right?
- line 193/194: "fixed latitude range in the southern part of the AMA" - in Garny et al, the total low PV area within 15-45 N was shown (see their Fig. 6), so this is not really a fixed latitude range, and neither only the southern part? The main difference is, apart from the slightly larger latitude range here, rather the weighted versus non-weighted low PV area, and the addition of the fluxes.
- line 197: why only "as far as PV conservation holds"? Is the flux not valid if the the diabatic term is not equal zero? This would be worrisome for the whole analysis of the paper.
- line 209: "dominant period of the variability in total low PV area" - actually show the power spectrum of the low PV area (see also comment on line 171, and major comment 2)?
- line 219: Do you mean to say that the timeseries filtered with a band pass filter within 5-20 days periods? Please specify.
- line 232: "zonally averaged total perturbation variance": Do you mean the variance in terms of anomalies at each longitude, and then this variance is zonally averaged?
- line 233: the studies mentioned here rather analyzed tele-connections to the mid-latitudes than variability of the anticyclone itself, correct? So maybe it is not surprising that they find different pattern? (Also, correlation to the time-series "at a point at a midlatitude" is a bit vague - please clarify).
- I find the EOF analysis, and the PC lag analysis and life cycle a great approach to characterize the variability. Possibly, adding actual data points to Figure 9 to see the progression of the phases would be beneficial?
- Fig. 11: agree that there is clear westward extension from phase 5 to 8, but does the low PV area "move back" to the east from phase 1 to 4, or is it shed? From the extend and strength of the low PV occurrence, it seems like the total PV area decrease over those phases. Related, is the total integral over the westward flux (in Fig. 12) equal to the sum of the eastward Flux over all phases? This would prove this point, and I guess it has to be the case, given that the total area appears to remain rather constant according to Fig. 13.
- Fig. 15: Not sure what the difference of black contours and color shading is - deviations from zonal mean versus anomalies from this deviation?
- lines 281-282: I'm not sure I understand the statement on the role of the subtropical jet on the deep gph anomalies. Please either remove, or add explanation/ citation for this statement.
- line 307: would you consider 30% of variability from both the 1st and 2nd EOF together the "dominant variability"?

- line 305/306: here you state that the west/east-ward flux of low PV is consistent with "eddy shedding", which could be true, but this notation implies that the eddies are at least partly "shed" from the Anticyclone, while your analysis seems to suggests that the total area is conserved during the quasi-biweekly variability, i.e. no actual "shedding" occurs. In Fig.3 b), where you show that the total eastward flux at 360 K is negative, does that imply actual westward shedding?
- general: State in all Figure caption, and possibly more often in the text, at which level the analysis is performed on! (I found this information to be rather hidden).

3 Typos / technical

- line 165: "pointed out by..." (add "out")
- line 220: dividing "by" their standard deviation (insert "by")
- line 225: "longer months": change to "longer period" ?
- line 275: "rest phases": change to "for the rest of the phases" ?
- Fig. 15: Title and legend should say 15-25N and 35-45N rather then E.
- line 409: "noize": change to "noise"
- line 420: "persentage": change to "percentage"
- Fig. 4: which level?
- Fig. 13: I would suggest to change/remove the heading "A_west", as not only "A_west" is shown