Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-424-RC2, 2020

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Characterizing quasi-biweekly variability of the Asian monsoon anticyclone using potential vorticity and large-scale geopotential field" *by* Arata Amemiya and Kaoru Sato

Anonymous Referee #2

Received and published: 13 July 2020

The manuscript is an interesting study of the variability of the Asian monsoon anti-cyclone (AMA) that brings new results on the quasi-biweekly variability and should eventually be published. There are, however a number of major and minor points that deserve further work from the authors

Major points

It is unclear whether it is a major or minor point but the basic equation (1) which is taken from Garny and Randel (2013) is technically wrong as it is presented. The integrand

dS in the first member of the r.h.s. is not a line element but a line element divided by the modulus of the horizontal gradient of the PV. This is stated by Garny and Randel and, otherwise, the equation is not even dimensionally homogeneous. I hope that this detail has not been missed by the authors and that the error was only introduced during writing but it is quite worrying.

The main claim of the manuscript is that the oscillations are mostly of dynamical origin and reversible, and that forcing by convection and dissipation are not involved. This is quite opposite to conclusions of previous works and also to Wei et al. (2019, doi:10.1029/2019GL086180) and references herein which is another stream of research that should be quoted and discussed. Another relevant work that studies PV fluxes on isentropic surfaces is Ortega et al. (2018, doi: 10.1002/qj.3261) which is also missing in the reference and should be used to compare the results of the manuscript.

The manuscript focuses on the zonal mass flux of air with low PV and finds that the mean flux at 370K oscillates around zero over the range of latitudes of the AMA. This is basically the main result. However, this does not mean that there is no zonal flux of PV. It is clear from fig.11 that during the phase of eastward flux at 60E, the air carries less negative PV that during the phase of westward flux. Therefore the zero mean mass flux does not rule out a non zero mean PV flux, where negative PV is created on the east by convection and dispersed and lost to the background on the west by vortex shedding within a biweekly cycle. It is useful to notice that the circulation time around the anticyclone and its erosion rate are also of about two weeks (Legras and Bucci, 2019, doi: 10.5194/acp-2019-1075). PV is clearly not well conserved during the bi-weekly cycle.

Minor points

l. 51-53: Is it so clear that the two questions are well separated?

l. 63 "is often"

l. 141: I do not see why the divergence term disappears in this equation.

l. 145: The integral is at fixed longitude and the integrand is latitude over the range where the PV is below the threshold and F is the mass flux (rather than the movement) of low PV air across a given latitude. This is badly described and the scheme in fig.1c adds to the confusion.

l. 164: I do not see the need for a 31-day filter when the average is done over 38 years. This should be enough to scramble the phases of the AMA oscillations.

l. 173: At this stage, the evidence is only based on the visual appearance of a single year record.

l. 176: This line should refer to eq. (5) if this is what is shown.

l. 181: This line makes me worrying whether the total heating, including latent heating, is accounted as it should in this work or whether only radiative heating is used. At 370K, it is however correct to assume that radiative heating dominates.

l. 190: I asume that the results are shown on the 370 K surface but this should be stated. It is very difficult to distinguish the blue and red contours in fig. 4. The text mention that fig.4 shows the mass weighted length that should have dimension kg/(K x m) and the caption says that it is a weighted area with dimension kg/K. Please clarify. Provide a definition for this weighted area that depends on the longitude and discard L if is not used.

In the sequel, no PV diagnostic is shown on other surfaces than 370K. The choice of 370K is justified in the appendix on the basis of the best definition of AMA in terms of PV but is would nevertheless be interesting to look at over surfaces. 380 K was privileged in Ploeger et al. (2015) and 360 K is closer to the level where convective detrainement is the strongest. It is also where the mean eastward and westward branches of the AMA are maximum and where the isentropic divergence is maximum.

l. 197: I do not see why PV conservation is invoked here. It is clear that PV is not well

conserved here (see fig.11).

l. 209 and Fig. 6: How should we interpret the significance curves on Fig. 6? The peak is not that strong and shows there is a plateau in the spectrum intensity between 9 days and 25 days. 9 days is more a cutoff period than a dominating period.

Sect. 4 It is a bit surprising that the study switches here to the geopotential on the 100 hPa surface. Having done all the work to interpolate basic variables on isentropic surfaces would have made easy to calculate the Montgomery potential on such surfaces. Basically, the results would not have been very different but this would have been more consistent, especially because isentropic and isobaric surfaces may differ quite significantly in the Asian monsoon region.

As the authors are looking for a cycle, they should have considered the MSSA method which is particularly well suited (Ghil et al, 2012, doi:10.1029/2001RG000092) and would have saved time and space.

l. 220 “dividing by their”

l.220 Why the square root of grid area and not the area in the weight?

l. 240: It is quite difficult to understand fig. 9 which is introduced in a section where PV plays no role. Please improve the caption such that it makes sense when the reader is at line 240 in the text.

Sect. 4.2 The first paragraph concludes that variability is determined by internal inviscid and adiabatic dynamics but the second paragraph shows there is a pattern of convection associated with the oscillation which somewhat contradicts the first paragraph if we admit that convection does not only react passively but generates a forcing. The authors do not attempt to provide a balanced view and just discard the convective influence in this section and in the conclusions l.323-330.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-424,

2020.

C5