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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

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We greatly appreciate the reviewer's invaluable and constructive comments. We have revised our manuscript following your and the other reviewer's comments. Responses to each of the major and minor comments are written below.

Major comments

> 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.

It is true that the factor of a line element was missing in Eqs. 1, 3, and 4. Additionally, we also found another missing term which should be added in the right hand side of Eqs. 3 and 4. Correct forms of those equations are as follows. Their derivations are added to Appendix B of the revised manuscript and also at the bottom of this reply. We are sorry for the incorrect description. This correction does not affect our analysis results in this study, as the integral terms are not directly used.

$$\frac{d}{dt}\hat{A}_{\mathrm{tot}}(t) = \oint_{q=q_0}\left(-q\frac{\partial\dot{\theta}}{\partial\theta}+\dot{\theta}\frac{\partial q}{\partial\theta}\right)\frac{\sigma dS}{|\nabla_\theta q|} - \int_{q\le q_0}\frac{\partial}{\partial\theta}(\sigma\dot{\theta})dA\ , \quad + \ (\text{unresolved term}) \tag{1}$$

$$\frac{d}{dt}\hat{A}_{\mathrm{west}}(t) = -\hat{F}(\lambda_0) + \oint_{q=q_0,\lambda\le\lambda_0}\left(-q\frac{\partial\dot{\theta}}{\partial\theta}+\dot{\theta}\frac{\partial q}{\partial\theta}\right)\sigma dS - \int_{q\le q_0,\lambda\le\lambda_0}\frac{\partial}{\partial\theta}(\sigma\dot{\theta}), \quad + \ (\text{unresolved term}) \tag{2}$$

> 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.

About the causal relationship between convective forcing (particularly over Indian sector) and the dynamical variability in the upper troposphere, we do not very much agree with the argument of Wei et al. for two reasons. First, their result that the negative rainfall anomaly over northern India precedes the key day of the anticyclone east-west oscillation may be a consequence of the choice of the key day and the definition of the variability index, a difference between area-averaged GPH over Tibetan Plateau and Iranian Plateau. Second, they did not provide convincing mechanism of convection anomaly over northern India driving the dynamical field anomalies including midlatitudes. They referred to Karmakar et al. (2017), and Ding and Wang (2007), Karmakar et al. did not propose physical process responsible for it. Ding and Wang suggested a mutual positive feedback, in which midlatitude upper level circulation anomalies enhances the convection and in turn the convection triggers the midlatitude Rossby wave train. Thus they do not exclude the possibility that the convection anomaly is forced by the upper level circulation anomalies. The results of a composite analysis in Wei et al. (their Fig. 3) and ours (Fig. 14) do not necessarily agree for the feature of convection anomalies. The difference may come from the different reference index and the different pressure level. The discussion about this point has been added to the revised manuscript. We have added the reference to their work in line 350 in discussion section.

The formulation used in Ortega et al. has some similarity to ours, but it used the different definition of flux for the different purpose. They focus on PV value and its thickness-weighted flux in latitudinal direction, whereas our study focuses on the weighted area enclosed by a PV contour, and longitudinal flux of that area (mass). Their definition

cannot well quantify the movement of the air inside the anticyclone as the PV-based definition can do. We have added the reference to Ortega et al. (2018) and this explanation in line 156-158 of the revised 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.

We appreciate reminding the important difference between the conservation of the low PV area and the conservation of PV values. The description in the original manuscript which is based on PV conservation is not appropriate in this sense. Our discussion is based on the budget of the weighted low PV area. This view has better implication for the variability of chemical tracers, given that the threshold PV value properly reflects the mixing barrier. We have revised lines 158-160 in the revised manuscript to make this point clear.

Minor comments

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

These two questions can be treated separately, because the intensity and position/structure of the AMA can be quantified independently with each other.

> l. 63 “is often”

We have revised the sentence in this line as suggested.

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

We have added the derivation of Eqs. 3 and 4 in Appendix B in the revised manuscript.

> 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.

We have revised lines 148-149 in the revised manuscript as suggested.

> 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.

We used a low-pass filter to ensure that it represents the mean seasonal variation, although it does not change much of the result.

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

The statement in line 173 of the original manuscript ‘The analysis regarding the total area so far confirmed . . . ’ was not appropriate. We have revised the first paragraph of section 3.2.

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

We have added the reference of the equation.

> 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.

The notation ‘differential radiative heating’ was misleading, as other processes such as latent heating should be accounted as well. We have revised the line 193 of the revised manuscript.

> 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.

The value shown in Figure 4 has a unit of kg/K and factor of $10^{1}2$. It is the weighted area calculated over each grid area and summed up for latitudinal direction. We have revised the figure caption and the description in lines 201-202. We also have made blue contours in Fig. 4 thicker and dashed to make them more visible.

> 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.

We have revised Figs. 4 and 5 to show the results at three different levels 360, 370, and 380 K. As expected, the contribution of the longitudinal advection to the low PV area distrubution is less clear at 360 K at 380K, as there are more significant source and sinks. The whole picture of the budget of low PV airmass is more complicated than what is seen at a single level, we consider the longitudinal ocsillatory behavior with a time scale of quasi-biweekly is one important feature, and the low PV air at 370 K is the best proxy for that feature. We have added these discussions to section 3.2 of the revised manuscript.

> 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).

As the other reviewer also pointed out, the PV conservation needs not be assumed here for the use of the longitudinal flux of the low PV area. We have removed that phrase from the original manuscript.

> 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.

We agree that the peak in Fig. 6 does not show clear characteristic time scale of 'quasi-biweekly'. There is broad range of possible characteristic time scale centered around quasi-biweekly. We have revised lines 226-228 in the revised manuscript.

> 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.

The use of Montgomery potential may be consistent in our analysis. However, we rather chose geopotential at the 100 hPa pressure surface for the EOF analysis in order to make the comparison with earlier studies easier, as most studies on the bimodality of the AMA center location used 100 hPa geopotential height. The result would be similar if we perform the analysis using Montgomery streamfunction on 380 or 390 K level. The extended EOF analysis is mathematically equivalent to MSSA (Plaut and Vautard, 1994) and more often used in geophysical studies in which the number of spatial grid points are much larger than the number of time steps in a cycle. For example, Wang and Duan (2015) used extended EOF for 10-20 day filtered diabatic heating in the Asian monsoon region. We did extended EOF and complex EOF analyses and got the similar results to the case of EOF analysis, as mentioned in line 225 in the original manuscript. We only show the result of EOF analysis for simplicity.

> l. 220 "dividing by their"

We have revised the sentence in line 236 in the revised manuscript as suggested.

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

When the data is weighted by the area, the variance or correlation should be multiplied by the area. This corresponds to multiplying the anomaly by the square root of the area.

> 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.

The caption of Fig.9 was incorrectly input. We are very sorry for the mistake. We have replaced it with the correct caption.

> 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.

About the role of the convection variability, we does not conclude but suggest the possibility that the dynamical variability drives the convection variabilty, considering the spatial location of OLR anomaly and low PV area in phase 6. This has been written in lines 324-330 in the original manuscript but the explanation may not be clear. We have revised lines 276-278 in Section 4 and a paragraph starting from line 336 in Section 5 in the revised manuscript.

Derivation of Eqs. 1 and 2 (Eqs. 3 and 4 in the manuscript) :

Equation (1) can be derived in a way similar to the derivation of Eq. (13) of Butchart et al. (1986). For consistency, their notation is used in the following. They begin with the small change in the area $\Delta A(t)_{\chi\geq\chi_0}$ enclosed by an isopleth $\chi = \chi_0$, denoted by Γ, with respect to the change in the contour position $\Delta\vec{x}$. Note that they assumed that the value of χ increases inward of the area.

$$-\oint_\Gamma \Delta\vec{x}\cdot\frac{\nabla_\theta\chi}{|\nabla_\theta\chi|}ds = \Delta A(t)_{\chi\geq\chi_0} \tag{3}$$

To extend this formulation to the thickness-weighted area $\hat{A}(t)_{\chi\geq\chi_0}$, there needs an additional term for thickness change $\Delta\sigma$ in the left hand side,

$$\int_{\chi\geq\chi_0}\Delta\sigma dA - \oint_\Gamma \Delta\vec{x}\cdot\frac{\nabla_\theta\chi}{|\nabla_\theta\chi|}\sigma ds = \Delta A(t)_{\chi\geq\chi_0} \tag{4}$$

Using the equation for thickness

$$\frac{\partial\sigma}{\partial t} + \nabla_\theta\cdot(\sigma\vec{v}) \quad = \quad -\frac{\partial}{\partial\theta}(\sigma\dot{\theta}), \tag{5}$$

the first term on the left hand side of Eq. (4) can be rewritten as follows.

$$\begin{aligned}\int_{\chi\geq\chi_0}\Delta\sigma dA \quad &= \quad \Delta t\left[-\int_{\chi\geq\chi_0}\nabla_\theta\cdot(\sigma\vec{v})dA - \int_{\chi\geq\chi_0}\frac{\partial}{\partial\theta}(\sigma\dot{\theta})dA\right] &(6)\\ &= \quad \Delta t\left[\oint_\Gamma \vec{v}\cdot\frac{\vec{\nabla}_\theta\chi}{|\vec{\nabla}_\theta\chi|}\sigma ds - \int_{\chi\geq\chi_0}\frac{\partial}{\partial\theta}(\sigma\dot{\theta})dA\right] &(7)\end{aligned}$$

where $\vec{v}$ is two-dimensional velocity, and $\vec{\nabla}_\theta$ is two-dimensional gradient on an isentropic surface.

The second term on the right hand side is transformed using $\Delta\vec{x} \cdot \vec{\nabla}_\theta \chi \simeq -\Delta t \cdot \partial\chi/\partial t$ as shown in **?**. Then by applying $\Delta t \to dt$ we obtain the following,

$$\frac{d}{dt}\hat{A}(t)_{\chi\geq\chi_0} = \oint_\Gamma \left(\frac{\partial\chi}{\partial t} + \vec{v}\cdot\nabla_\theta\chi\right)\frac{\sigma ds}{|\nabla_\theta\chi|} - \int_{\chi\geq\chi_0}\frac{\partial}{\partial\theta}(\sigma\dot{\theta})dA \tag{8}$$

As the first integral contains the advection term, the bracket can be replaced with a nonconservation term F.

$$\frac{\partial\chi}{\partial t} + \vec{v}\cdot\nabla_\theta\chi = F \tag{9}$$

When χ is potential vorticity and the area A is defined to have potential vorticity below the reference value, Eq. (1) is obtained. The subgrid scale mixing term in Butchart et al. (1986) comes from the difference between the true divergence term and the divergence term calculated from resolved variables. Although our equation does not have divergence term, we consider it is still better to include unresolved effect such as subgrid scale mixing, which is included in F. Then, when χ is potential vorticity and the area A is defined to have potential vorticity below the reference value, using

$$F = -q\frac{\partial\dot{\theta}}{\partial\theta} + \dot{\theta}\frac{\partial q}{\partial\theta} + (\text{unresolved term}) \tag{10}$$

thus we obtain Eq. (1).

Equation (1) can also be derived from the general mass conservation expression in a PV-θ coordinate introduced in Nakamura (1995) ;

$$\left(\frac{\partial m}{\partial t}\right)_{q,\theta} + \left(\frac{\partial\mathcal{M}(\dot{q})}{\partial q}\right)_{\theta,t} + \left(\frac{\partial\mathcal{M}(\dot{\theta})}{\partial\theta}\right)_{q,t} = 0 \tag{11}$$

where $m = \mathcal{M}(1)$ and thickness-weighted area integration operator $\mathcal{M}$ is defined as

$$\mathcal{M}(*) = \int\int_{q\leq q_0}(*)\sigma dA = \int_{q^*\leq q} dq^* \oint_{q*}(*)\frac{\sigma ds}{|\nabla_\theta q^*|} \tag{12}$$

Substituting $\dot{q} = F$ and expand $\mathcal{M}(\dot{q})$ and $\mathcal{M}(\dot{\theta})$, we obtain Eq. (1).

Next, let us derive Eq. (2). Now suppose the small change of the area $\Delta\hat{A}(t)_{\chi\geq\chi_0,\lambda\leq\lambda_0}$ enclosed by the isopleth $\chi = \chi_0$ and the circle of longitude λ_0. Let Γ_q and Γ_l respectively be the isopleth and the circle of longitude consisting the border of the area as shown in Fig. 1. Equation (3) is modified as follows,

The integral of the first term on the left hand side is performed over the area $\Delta\hat{A}(t)_{\chi\geq\chi_0,\lambda\leq\lambda_0}$, and the line integral of the second term is performed only over Γ_q, as Γ_l is constant with time. The first term can be rewritten as follows, using the Gauss'theorem for Γ_q and Γ_l,

$$\int_{\chi\geq\chi_0,\lambda\leq\lambda_0}\Delta\sigma dA = \Delta t\left[-\int_{\chi\geq\chi_0}\nabla_\theta\cdot(\sigma\vec{v})dA - \int_{\chi\geq\chi_0}\frac{\partial}{\partial\theta}(\sigma\dot{\theta})dA\right] \tag{13}$$

$$= \Delta t\left[\int_{\Gamma_q}\vec{v}\cdot\frac{\vec{\nabla}_\theta\chi}{|\vec{\nabla}_\theta\chi|}\sigma ds + \int_{\Gamma_l}u\sigma ds - \int_{\chi\geq\chi_0}\frac{\partial}{\partial\theta}(\sigma\dot{\theta})dA\right] \tag{14}$$

Then we obtain Eq. (2) with the additional term $\hat{F}(\lambda)$ defined as follows. The integral is performed over the longitude circle λ_0 consisting the border of the area.

$$\hat{F}(\lambda) = \int_{q\leq q_0,\lambda_0}u\sigma ds = \int_{q\leq q_0,\lambda_0}u\sigma R d\phi \tag{15}$$

Reference: Nakamura, N. (1995). Modified Lagrangian-mean diagnostics of the stratospheric polar vortices. Part I. Formulation and analysis of GFDL SKYHI GCM. Journal of the Atmospheric Sciences, 52(11), 2096–2108.

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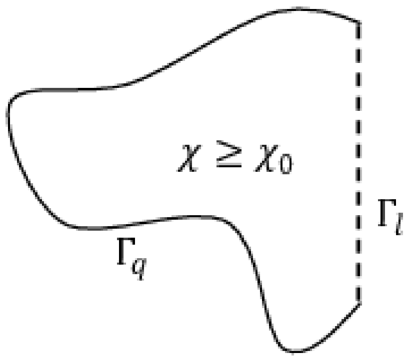

Fig. 1.