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Interactive comment on "Modes of Vertical Thermodynamic and Wind Variability over the Maritime Continent" *by* Jennie Bukowski et al.

Anonymous Referee #2

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Overall this is a well written, intelligent paper that appears technically competent (to someone not well versed in PCA). And I think that through the RC's, the author's have identified something intrinsic in the temporal variability of T, RH, U, and V profiles.

However, I am not really sure what new it brings to the table in addition to the usual procedure of generating T/RH composites about high rain events, and I think the paper oversells how useful this technique is likely to be in the future. For example, from work by Kiladis and others, we already know a lot about the temperature and wind anomalies associated with different types of convectively coupled waves in the tropics. This type of procedure, in which we project anomalies on to the types of physically known propagating 3D convectively coupled waves seems more insightful than the 1D PCA done here, in which there is no attempt to physically separate any of the myriad of influences on a particular profile. Below are some specific comments.

(1) Section 4.1. The justification for the physical interpretations to which the various RC's are assigned is often unclear. For example, "shallow convective heating is represented in RC4". But do we really know what kinds of temperature anomalies are likely to be associated with shallow convective heating? For example, if shallow convective clouds occur more frequently (e.g. are triggered) by the moistening and cooling associated with low level upward motion (likely, especially in the vicinity of deep convection), then perhaps shallow clouds are correlated with low level cold anomalies, and any positive correlation between shallow convection with positive RH should not be interpreted as a consequence of detrainment moistening, but some external dynamically imposed influence. For example, even the net effect of precipitating shallow convection on the RH of a particular level is unclear. It is a residual of the drying associated with induced descent, moistening from detrainment and evaporative moistening, and then a slower dynamical response driven by the geopotential anomalies associated with the convective heating. More generally, causality between T, RH, u, v anomalies in the background atmosphere and convective clouds always goes both ways. There can't be a simple one to one relationships between certain types of T/RH anomalies and certain cloud types or heating profiles, as implied here. (Otherwise it seems to me that convectively coupled waves in the tropics could not exist.)

(2) Similarly, sometimes the RC's for U and V are assigned physical interpretations and again the justification is unclear. E.g. "The overwhelmingly dominant signal in the V-component of wind is the seasonal monsoon. The MC monsoon is characterized by a complete reversal ...". I guess it is not clear to me here what exactly is meant by "monsoon" in a region of such complicated topography, or why it must have these impacts on U and V. For example, the three radiosonde locations are at quite different locations in the Marine Continent, so the dynamical signature of the monsoon must vary between locations, but the RC's of the three locations are the same almost (except for ordering). This is noted in point 4 of the conclusion. Should the "monsoon" have the same dynamical signature in all three locations? Perhaps give some explanation of what is really meant by "monsoon". It seems that the authors have simply defined

a particular RC as a monsoon signature, and then remarked that this RC is the same at all three locations, and then say the monsoonal signature is the same at all three stations. Everything proceeds from the initial categorization. But is this really more than a semantic game? Do you really know for certain what types of large scale dynamical motions are associated with a particular RC? How would you prove this? I realize there is some discussion of this in lines 13-14 of Section 4.1, but this wasn't fully convincing to me.

(3) Figure 9. I found this hard to interpret. Especially there was so much variability in the top 4 panels, that the features discussed in the text were not clear to me.

Overall, the authors have done some interesting calculations. It just isn't clear to me what new physical insights are generated, or how these might be used as diagnostics tests of climate models. It would be useful if the authors made more definitive attempts to establish the basis of their physical interpretations, or if not possible, avoid what I see as over-interpretation.

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