In *The Moisture Mode-to-Gravity Wave Spectrum as a Framework to Define Tropical Weather Systems*, Respati et al classify tropical systems into moisture modes, inertio-gravity waves, and mixed systems, examine their thermodynamic and dynamical properties in light of the attendant theory and attempt to link them to extreme rainfall. The authors first decompose tropical systems into these three categories based on their phase speed; this is discussed in Section 2 and uses the fact that moisture modes, whose dynamics is largely controlled by moisture anomalies (at relevant timescales), are slow while the other end of the spectrum are fast inertia-gravity waves where temperature/buoyancy variations play an important role. The methods and data used for this decomposition, as well as the tracking of the so-called tropical objects is described in Section 3. Following this, Section 4 presents the composite thermodynamic and dynamic fields associated with the three classes of systems. As expected, differences in Lq' are clearly notable across these categories while the DSE does not vary much. The dynamical fields, especially the relation between OLR anomalies and the rotational flow as interesting (see comment below) and so is the nature of geostrophic and nonlinear balance (again, see comment below). Regional differences in these fields are also discussed in this section. Finally, Section 5 looks at the relation between these three types of systems and extreme tropical rainfall, and suggests that a large fraction of extreme events are linked to moisture modes. *As a whole, the paper is clearly written and in my opinion the results will be of interest to a section of the WCD community. I recommend its publication only after the questions listed below are addressed.*

1. Section 1: Can the authors better motivate the need to associate extreme rainfall with a particular type of "object" from these three categories? I would think that this association would be more fruitful if one links extreme rainfall to a particular CCEW, or the MJO. Say, for example, if a moist Rossby wave (or more generally, the QBWO), which has a distinct physical mechanism, is linked to extreme rainfall, this might yield insight into the spawning of a synoptic system (that causes the extreme event) in the environment set by the Rossby gyre. In fact, most of the moisture modes identified here and elsewhere have an intraseasonal character, and extreme rainfall that results from them is usually via the favorable environment they create for smaller scale synoptic systems.

2. Section 2: While this summary of moisture modes and IG waves is nice, it might be useful to be a bit careful in that certain moisture modes rely on moisture, i.e., they do not appear in dry models. For example, the MJO, which seems to be absent when moisture is not dynamically active — some useful references apart from Adames & Maloney are Suhas et al, QJRMS, doi:10.1002/qj.4191; Ahmed, JAS, doi:10.1175/JAS-D-21-0071.1; Monteiro & Sukhatme, QJRMS, 10.1002/qj.2644. Whereas, others, such as the moist ER wave is a modification of a traditional ER wave due to convective coupling (eg. Matthews, QJRMS, doi:10.1002/qj.4917).

3. Section 3: A prominent missing moisture mode in the analysis here is the BSISO or the Boreal Summer Intraseasonal Oscillation. Indeed, with eastward movement, its most prominent feature is poleward propagation over the Indian landmass during the boreal summer. In fact, this basin supports poleward movement of the QBWO during the winter in the southern hemisphere too. Both of these are very important in terms of rainfall in modulating active-break cycles in the Indian monsoon and the generation of tropical cyclones in the south-west Indian Ocean, respectively. Given that the wavenumber-frequency diagram used for partitioning is based on zonal propagation, these are missed in the present framework. Do the authors have any plans to remedy this or consider them in future work?

4. Section 4: I feel the dynamical characteristics of the three categories needs a little more care. In the context of moisture modes, while the position of the OLR anomaly on the eastern edge of the gyre

supports MVI in certain geographical locations, this is not, as far as I am aware, a universal feature of this category. For example, in a moist ER or the QBWO, the moisture/OLR anomaly can be in almost in quadrature or lie within the vorticity gyre depending on the geographical location (see for example, the recent work by Biswas et al, https://arxiv.org/abs/2512.10409). In fact, a composite of ERs, as seen in Nakamura & Takayabu, JAS, doi:10.1175/JAS-D-21-0080.1 shows a much larger intrusion of the moisture/OLR or brightness temperature anomaly into the flow vortex.

A second point that the authors may wish to consider is the evaluation of balance. Specifically, which of the terms in Σ causes the most error, is it the nonlinear piece or is it the notion linear balance in the first term on the RHS of Equation 4? Indeed, why not try the global ”QG” formulation, which is not really geostrophic but rotational in nature, i.e., follow Schubert et al, JAMES, doi:10.3894/JAMES.2009.1.2; or the original Charney & Stern, JAS, 19, 159, 1962? This may prove to yield a more insightful description of the three categories in terms of a rotational-divergent dominance for advective purposes.

5. Section 5: I appreciate this is a first investigation of the association of the three categories with extreme rainfall. I am curious though about Figure 12. Why does the increase in probability associated with moisture modes have a wave-like character in longitude (especially in the 10-20N band) which is not seen in the other categories?