Responses to Comments by Referees on *The Moisture Mode-to-Gravity Wave Spectrum as a Framework to Define Tropical Weather Systems*

Muhamad Reyhan Respati Martin S. Singh Christian Jakob

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1 Referee 1

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.*

We thank the Referee for their nice and constructive comments. We are addressing the Referee's questions and comments below. The questions/comments are shown in blue and our answers in black.

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.

   We acknowledge that associating extreme rainfall with a particular convectively coupled equatorial wave (CCEW) type or to the Madden–Julian oscillation (MJO) might yield valuable insight on how their distinct physical mechanisms link to the generation of extreme rainfall. However, such study has commonly been done before (see review by Latos et al. (2026)) and is not our main focus here. Our motivation is to construct a defined set of tropical weather systems analogous to synoptic weather systems in the extratropics.

   Our conceptual idea is as follows. As in the extratropics, atmospheric waves are the "background" dynamics of the weather. For example, a typical extratropical Rossby wave can give rise to several pairs of low- and high-pressure regions in the midlatitudes. These lows and highs are the "weather systems" in the extratropics, not the Rossby wave itself. Applying this idea to the tropics, we posit that CCEWs are the "background" dynamics, and they can give rise to synoptic weather systems in the tropics, which are detected as "objects" here in our study. For instance, equatorial

Rossby (ER) waves are characterised by sets of gyres, and, among these, the cyclonic gyres may be detected as our moisture mode "objects".

An advantage of defining tropical weather systems using our current method is that we implicitly take into account the interactions among different waves in the same phase speed category, which are likely to be nonlinear, when we do the spectral filtering. This way, a detected synoptic "object" can be a more realistic manifestation of those interactions in the background.

In summary, here we want to fill the gap in between the planetary-scale CCEWs and the mesoscale convective systems, that is the synoptic-scale "weather systems" aside from tropical cyclones (TCs). We have added a discussion about this in L426–438.

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).

   Thank you for the important reminder that some moisture modes do not appear in the dry atmosphere models, such as the MJO, while others like ER wave can be considered a modification of its dry version. We have added the additional references into the text and made a clearer sentence about this in L80–85.

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?

   We acknowledge the use of zonal phase speed in decomposing tropical weather systems is a limitation of our method. For disturbances that propagate primarily meridionally, this results in an artificially low phase speed. We mention this caveat in L454–457.

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.

   We thank the Referee for their advice in taking a little more care when interpreting the results on dynamical characteristics of the three tropical weather systems investigated in this study. We are aware that the OLR anomaly in the observed convectively-coupled ER wave extends further westward into its cyclonic gyre, as depicted in the composites in e.g., Wheeler et al. (2000); Kiladis et al. (2009); Nakamura and Takayabu (2022). However, we argue that moisture modes, as defined in our study, are not purely ER waves or QBWO. As also mentioned above, our moisture mode category involves any OLR signal with zonal phase speed less than 9 m s^{-1}, including the westward-propagating ER wave, parts of the tropical depression (TD)-type wave, and even most of the eastward-propagating signals associated with the MJO. All these signals can co-exist at the same time and place, and may interact and manifest into the slow-moving synoptic-scale systems defined as moisture modes here. As such, we consider our composites of moisture modes, both equatorial and off-equatorial, as the general characteristics of the synoptic-scale systems associated with the slow-moving "background" dynamics, including, but not limited to, ER wave, QBWO, or the MJO.

   Additionally, while the global composites show that the OLR anomaly of off-equatorial moisture modes is located to the east of their vortex centre, there are some regional differences. As shown

in our Fig. 8, for example, northern hemisphere off-equatorial moisture modes over the Indian Ocean, especially during June–August season, have their OLR anomaly located to the west of their vortex centre, where the equatorward flow of the vortex advects moisture from the humid Indian subcontinent to the north. This agrees more with the moisture–vortex instability (MVI) theory (see e.g., Adames and Ming, 2018; Adames, 2021) and less with the structure of the ER wave or QBWO, because these systems do not represent ER wave or QBWO alone, but rather are the manifestation of various wave dynamics at play in the background.

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.

Regarding the evaluation of nonlinear balance, Eq. (4) is chosen to include not only the deviation from geostrophic balance—defined as the sum of the first three terms on the right hand side of Eq. (4), equivalent to that in Charney and Stern (1962) and Schubert et al. (2009)—but also that from other types of horizontal balance, such as gradient wind balance; hence we include the fourth term on the right hand side of Eq. (4) as in e.g., Adames (2022). In Fig. R1, we can see that slow-moving systems like moisture modes and TCs are quite far from geostrophic balance. However, Fig. R2 shows that the term $-2\vec{\mathrm{k}}\cdot\left(\vec{\nabla}_h u\times\vec{\nabla}_h v\right)$ partially cancels the deviation from geostrophic balance in moisture modes and TCs, making them actually closer to the "total" horizontal balance as shown in Fig. 7. On the other hand, for the faster-moving mixed systems and IG waves, especially the equatorial ones, the terms in Figs. R1 and R2 do not cancel each other, indicating that these two systems are indeed not under horizontal balance condition, with more contribution from the geostrophic balance terms in Fig. R1.

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?

   The main reason there is a wave-like pattern along the longitudinal axis in the extreme rainfall probability change plots is because we do spectral filtering along the zonal and temporal coordinates to retrieve OLR signals associated with each weather system category. This pattern is especially apparent in moisture modes with a zonal wavelength of about 25°, corresponding to a zonal wavenumber of about 14. In our Fig. 1, the "centre" of the spectral regions of moisture mode category (i.e., the "centre" of the brown triangle in either westward- or eastward-propagating component) is located between wavenumber 10 and 15, therefore the "average" wave-like signal that appears in the composites is also within that range. For IG wave category, the "centre" of its spectral regions is more towards small wavenumbers, meaning that the most-likely wave-like signal to appear in the composites has a rather very long zonal wavelength. This is why the green/blue shading in Fig. 8 gets more zonally "stretched" in IG waves, and the brown-shaded regions flanking it are way outside the plotted domain. We can also see this wave-like feature in moisture modes and zonally "stretched" appearance in IG waves in the OLR composites in Fig. 6.

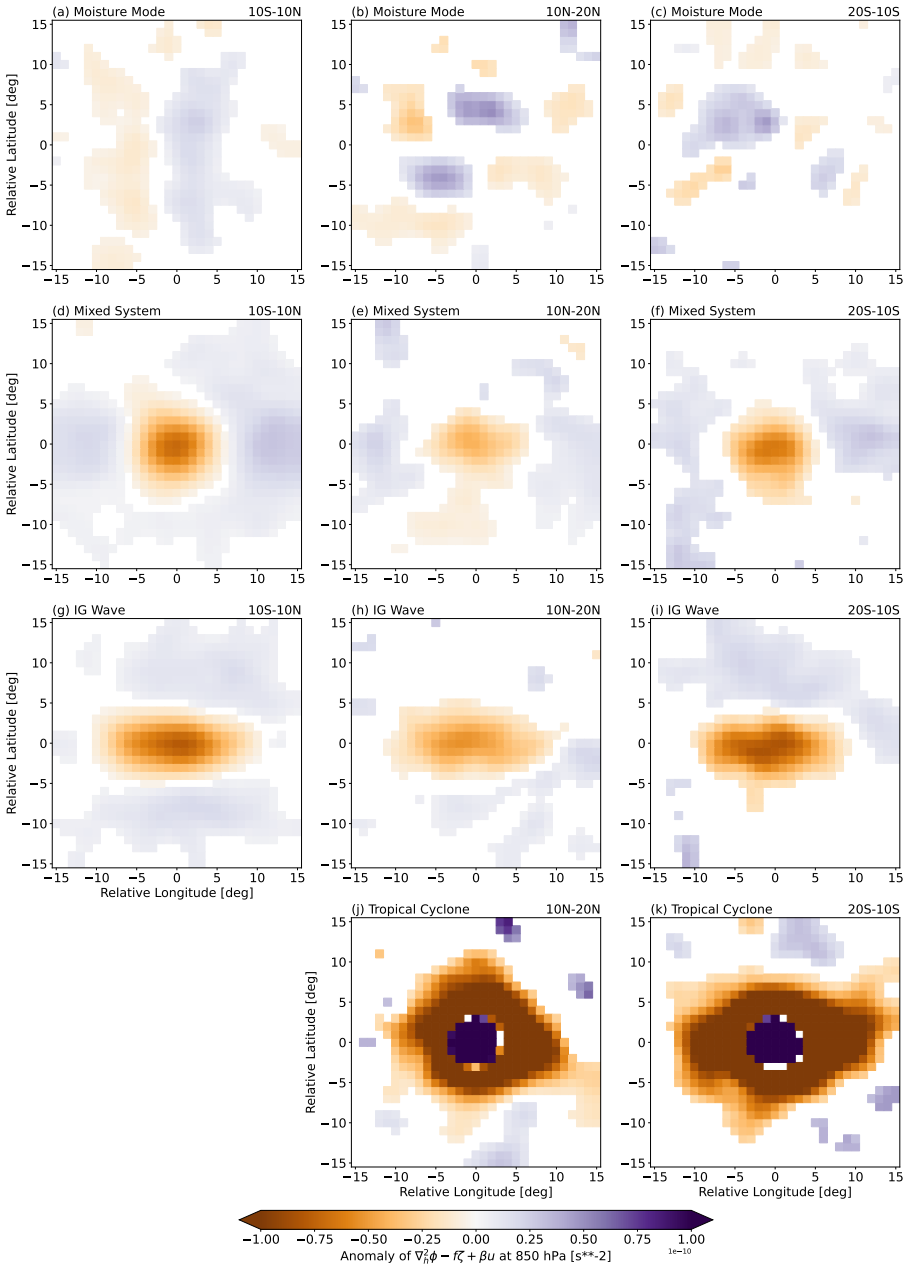


Figure R1: As in Fig. 7, but only for the sum of the first three terms on the right hand side of Eq. (4).

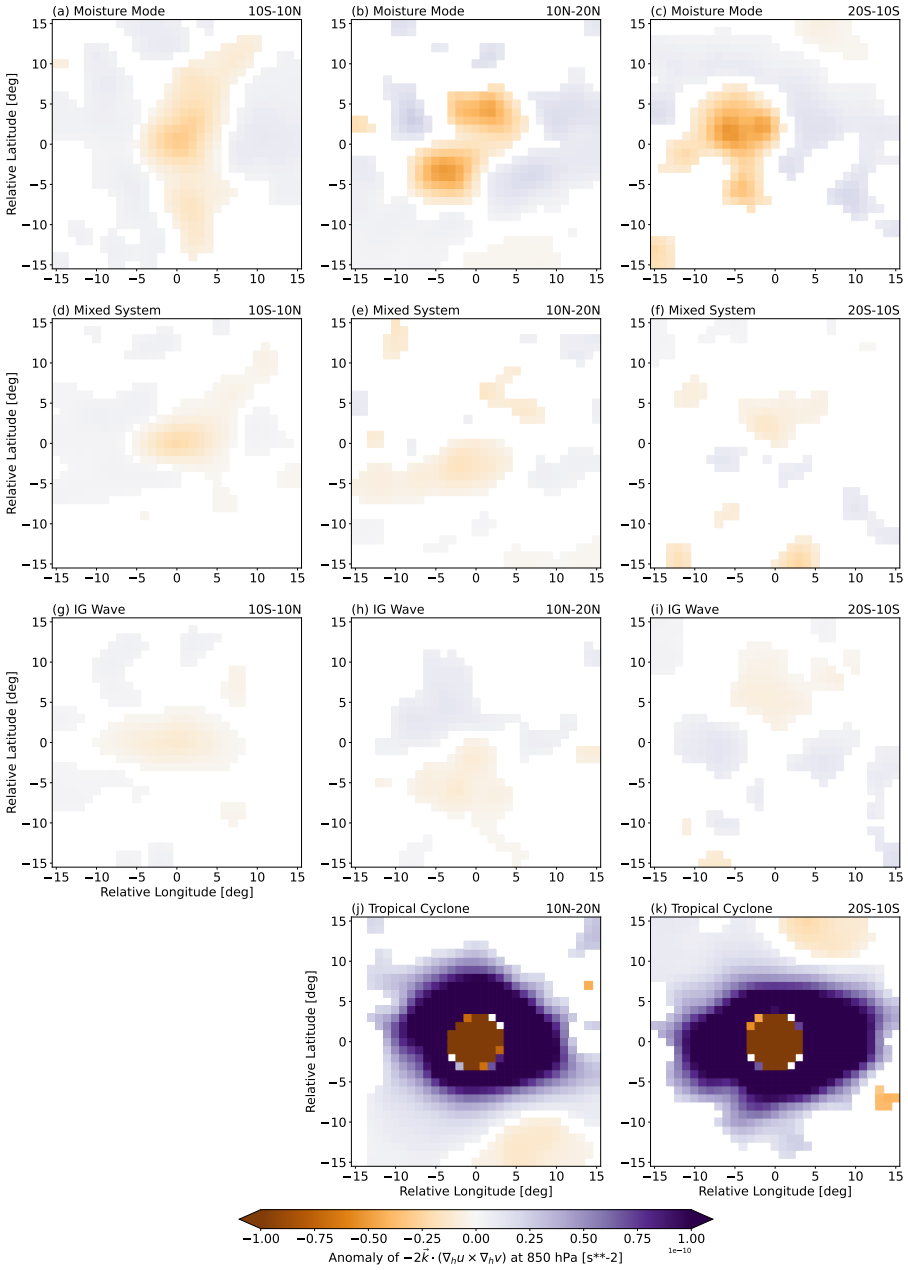


Figure R2: As in Fig. 7, but only for the fourth term on the right hand side of Eq. (4).

2 Referee 2

Summary and General Comments

This manuscript develops an object-based framework for defining tropical weather systems along a moisture-mode-to-gravity-wave spectrum. The authors combine phase-speed filtering of ISCCP OLR anomalies with Lagrangian object tracking, and then use ERA5 and GPM-IMERG data to examine the thermodynamic, dynamical, and rainfall characteristics of the resulting moisture-mode, mixed-system, and inertio-gravity-wave categories. The study argues that the observed ratio of DSE to moisture perturbations increases with phase speed, that slow and intermediate systems exhibit distinct equatorial and off-equatorial dynamical structures, and that these system types provide a useful way to relate large-scale tropical dynamics to heavy and extreme rainfall, with moisture modes especially important near moist margins and mixed systems important along the ITCZ.

I find the manuscript to be very valuable, clearly written, and scientifically interesting. In particular, it makes a worthwhile attempt to extend recent theoretical ideas on tropical motions into an observational, weather-system-oriented framework, and many of the presented results appear broadly convincing. However, I think the manuscript may need to address a few issues before publication, because the theoretical justification for using zonal phase speed alone as the basis for classification appears to be incomplete. The omission of the Burger-number contribution in the underlying scaling is potentially important for broad, off-equatorial, or subtropical disturbances, and it affects the interpretation of several central results. In addition, the composite analyses need clearer treatment of propagation direction, since combining eastward- and westward-propagating systems may obscure or bias the phase relationships on which some of the physical interpretation rests. These concerns do not undermine the potential value of the study, but they require some clarification, qualification, and possibly additional analysis before the conclusions can be fully supported. I therefore recommend **major revision**.

We thank the Referee for their positive appraisal and valuable feedback. The Referee's questions and comments are shown below in blue and our answers in black.

1. **Major Comments**

   Major Comment 1.

   The manuscript presents a compelling and well-written framework for classifying tropical convectively coupled systems by phase speed, but the theoretical basis for using phase speed alone as a proxy for N_{mode} needs further qualification, particularly for off-equatorial disturbances. As written, Eq. (2) and the subsequent classification in Section 3 implicitly assume that the Froude-number contribution controls N_{mode}. However, in the scaling of Adames (2022), the modal thermodynamic parameter is more generally controlled by the larger of the Froude-number and inverse-Burger-number contributions, i.e.,

   $$N_{\text{mode}} \equiv \max\left(\text{Fr}_\tau^2, \text{Bu}^{-1}\right) \frac{1}{\hat{\alpha}\left(1-\hat{\alpha}\right)}, \quad \text{Fr}_\tau \equiv \frac{c_p}{c}, \quad \text{Bu} \equiv \left(\frac{L_d}{L_y}\right)^2, \quad L_d = \frac{c}{f}.$$

   Thus, the reduction to Eq. (2),

   $$N_{\text{mode}} \approx \left(\frac{c_p}{c}\right)^2 \frac{1}{\hat{\alpha}\left(1-\hat{\alpha}\right)},$$

   is appropriate only when $\text{Fr}_\tau^2 \gtrsim \text{Bu}^{-1}$. When the Burger number is sufficiently small, as can occur at higher latitude, for larger meridional scales, or for disturbances with substantial off-equatorial structure, N_{mode} is no longer determined primarily by the zonal phase speed, as demonstrated below with concrete examples. This matters because if Bu^{-1} is nonnegligible, some slowly propagating off-equatorial disturbances may have thermodynamic scaling more consistent with mixed or moist-QG-like systems than with moisture modes.

   (i) The theoretical reduction from the full scaling to Eq. (2) should be stated and justified explicitly. In Section 2, immediately before and after Eq. (2), the manuscript states that N_{mode} is a function of phase speed and that "systems with slow propagation will have a small value of N_{mode} and more dominant moisture perturbations, while fast-moving systems will have a large value of N_{mode}." This is a useful interpretation under the Froude-number-dominated limit, but it omits the Burger-number dependence that appears in the more general scaling. A short theoretical discussion may be beneficial to clarify the assumptions

under which Eq. (2) is valid, and to state whether or not those assumptions are expected to hold for the equatorial, off-equatorial, and subtropical parts of the analysis domain.

(ii) The off-equatorial composites may lie near a regime in which the Burger-number term is not negligible. The concern may not be merely formal. First, consider an example in which the Froude-number scaling works well. A similar computation is given in Section 5b of Adames (2022). Suppose that the latitude is approximately 12°, for which $f \equiv 2\Omega \sin\theta \simeq 3.0 \times 10^{-5}$ s^{-1}. We then compute the meridional wavelength as $\lambda_y = 2\pi L_y$. The factor of 2π is needed because L_y represents the inverse meridional wavenumber, so converting this scale to a wavelength requires multiplication by 2π; see the definitions in Eqs. (30a) and (30b) of Adames (2022). If the meridional wavelength of the convective disturbance is $\lambda_y = 1000$ km and $c = 50$ m/s, the Burger number becomes:

$$\mathrm{Bu} = \left(\frac{2\pi c}{\lambda_y f}\right)^2 \simeq 110 \tag{1.1}$$

Thus, $\mathrm{Bu}^{-1} \sim 0.01$. Therefore, over most of the range of c_p, we have $\mathrm{Fr}_\tau^2 > \mathrm{Bu}^{-1}$, and the Froude-number scaling is adequate.

However, the off-equatorial moisture-mode composite in Fig. 6c appears to have a substantially larger meridional scale, of order 16–17°, i.e. roughly $\lambda_y \simeq 1800$ km, at about 15° latitude. With $f \simeq 3.77 \times 10^{-5}$ s^{-1}, this gives $\mathrm{Bu} \simeq 21$ and $\mathrm{Bu}^{-1} \simeq 0.047$. By comparison, the upper phase-speed threshold for the manuscript's moisture-mode category is $c_p = 9$ m s^{-1}, for which $(c_p/c)^2 \simeq 0.032$. In such a case, the Burger-number contribution can exceed the Froude-number contribution, giving

$$N_{\mathrm{mode}} \simeq \mathrm{Bu}^{-1} \frac{1}{\hat{\alpha}\,(1-\hat{\alpha})} \simeq 0.52 \quad \text{for } \hat{\alpha} = 0.9.$$

This value is not obviously in the same regime as the low-N_{mode} moisture-mode criterion implied by the manuscript's phase-speed threshold. Consequently, some off-equatorial systems classified as moisture modes by zonal phase speed may instead be closer to the manuscript's mixed-system category in terms of the full thermodynamic scaling, or may correspond more closely to moist-QG-like disturbances.

(iii) The issue has direct implications for the interpretation of off-equatorial and subtropical features in several figures. In Fig. 2c, for example, the feature labelled as a moisture mode near Mexico appears as a meridionally slanted, synoptic-scale disturbance; dynamically, such a structure could plausibly reflect Rossby-wave or moist-QG influence rather than a pure moisture mode defined by Froude-number scaling alone. Similarly, Fig. 3 shows occurrences extending toward the poleward edges of the plotted domain, and the manuscript notes in Section 4 that some mixed-system and IG-wave occurrences along the northern and southern boundaries of the domain, near 30°N/S, are "likely to be the imprints of extratropical frontal bands and other systems moving with similar propagating speeds: the moist QG systems." The same point reappears in the Summary and Discussion:

> There are also some mixed system occurrences along the subtropical edges (i.e., 30°N/S), and these are likely to be the imprints of the moist QG system, which may be considered to be a mixed system with a small Rossby number and strong influences from the extratropics (Adames, 2022).

This is a useful caveat, but it may be too limited as currently phrased. The Burger-number estimate above suggests that the ambiguity is not confined to the 30°N/S edges of the displayed maps; it can already become relevant within the off-equatorial band used in the manuscript, especially between about 15° and 20° latitude and for broad meridional structures. The interpretation of Figs. 6b–c, 8b–c, 8e–f, 12b–c, and 13a–b as showing off-equatorial moisture modes should therefore be presented with more caution unless the authors can demonstrate that the sampled disturbances remain in a Froude-number-dominated regime.

(iv) The manuscript should either constrain the displayed domain and language or provide a quantitative justification for retaining the current classification. I do not think this concern necessarily requires the authors to redo the full analysis, since the results are scientifically interesting and the phase-speed-based decomposition is valuable. However,

the paper would be considerably stronger if the authors added an explicit Burger-number discussion and used it to justify the applicability and limitations of the classification, particularly off the equator. At minimum, I recommend that the authors:

i state the full scaling involving max $\left(\mathrm{Fr}_\tau^2, \mathrm{Bu}^{-1}\right)$ and explain why Eq. (2) is adopted;

ii assess whether or not typical meridional scales and latitudes of the tracked objects imply $\mathrm{Fr}_\tau^2 \gtrsim \mathrm{Bu}^{-1}$ in the equatorial and off-equatorial composites;

iii revise the discussion of off-equatorial "moisture modes" to acknowledge that some slowly propagating systems, especially near 15°–20° and beyond, may be better interpreted as mixed or moist-QG-like disturbances when the Burger-number term is considered.

A conservative presentation choice would also be to truncate Figs. 2, 3, 9, and 13 to 20°S–20°N, since the main analyses are conducted within that latitude range and the phase-speed definition of N_{mode} becomes increasingly questionable poleward of it. The authors might also consider whether the off-equatorial composite region should be restricted to 10°–15° rather than 10°–20°, although I leave that choice to the authors; the essential requirement is that the theoretical limitation introduced by the Burger number be clearly articulated and reflected in the interpretation of the off-equatorial results.

We appreciate that the Referee gives detailed comments on our N_{mode} approximation in Eq. (2). We acknowledge that we omit some very important assumptions when introducing said approximation. We have revised this by elaborating in more details in L112–122 how we achieve Eq. (2) and why we think this approximation is justified at least for the scope of this study.

We have also added more discussions in L457–467 explaining that our choice to only include systems whose centroids are not beyond 20° latitude in the off-equatorial moisture mode composites is actually an attempt to avoid systems with small Bu due to large f. However, as further mentioned in L463–467, we acknowledge that this attempt may not be effective to filter out systems with small Bu due to large L_y. We conclude the discussions by explicitly stating that our off-equatorial moisture modes are likely to be "contaminated" by systems whose $N_{\mathrm{mode}} \gtrsim 10^{-0.5}$.

Major Comment 2.

The composite diagnostics appear to mix eastward- and westward-propagating disturbances within each phase-speed class, and this potentially affects the interpretation of the zonal–vertical and horizontal structures in Figs. 4–8 in which structures with opposite zonal phase tilts and opposite zonal phase relationships can be averaged together after centering on the OLR minimum. This issue may matter: several of the manuscript's physical interpretations rely on the phase relationships among convection, moisture, geopotential, winds, and vertical velocity. If eastward and westward disturbances are combined, the resulting composite may partly cancel physically meaningful asymmetries or may display the structure of whichever propagation direction dominates the sample, rather than the structure of a representative individual disturbance.

For example, Fig. 4c shows little apparent zonal tilt in the IG waves, even though convectively coupled IG waves are commonly associated with pronounced vertical/zonal phase tilts (e.g., Inoue et al., 2020, and references therein). A plausible explanation is that eastward- and westward-propagating IG-wave structures have opposing tilts that are cancelled by the composite procedure. Conversely, the apparent tilt in Fig. 4a may reflect dominance of the eastward-propagating MJO over westward-propagating ER-like disturbances, rather than a generic moisture-mode structure.

The same concern affects the interpretation of Fig. 6 and the related discussion in Section 4.2. The manuscript states:

> This low-level dynamical structure very much resembles the theoretical schematic of the equatorial moisture mode in Fig. 5 of Adames (2022), although the strong zonal pressure gradient is not as apparent here in Fig. 6a.

The weak zonal pressure gradient in Fig. 6a may not necessarily indicate a mismatch between the observed equatorial moisture-mode structure and the theoretical schematic; it may instead arise because eastward- and westward-propagating systems with opposite zonal phase relationships have been averaged together. This also bears on later statements about the location of convection and ascent relative to the vortex center, the poleward/equatorward flow, and moisture advection in the off-equatorial composites, including the interpretation of Figs. 6 and 8 in terms of moisture–vortex instability. Some of these horizontal structures may indeed be physically meaningful, but

with the present compositing strategy it is difficult to know whether a displayed phase relationship is representative of the individual disturbances, the residual after cancellation between opposite-propagating disturbances, or the structure of the more numerous or stronger propagation-direction subset.

Suggested clarification or additional analysis. I recommend that the authors either revise the analysis or more clearly state this limitation when interpreting composite structures. A cleaner approach would be to separate eastward- and westward-propagating signals before compositing. Alternatively, for one propagation direction the longitude axis could be reflected before averaging, so that positive relative longitude consistently denotes the downstream direction of propagation. If the authors choose not to redo the composites, the manuscript should at minimum add a clear caveat in Sections 3, 4.1, 4.2, and/or 6 explaining that the composites are centered on convective objects but are not oriented by propagation direction, so some zonal–vertical tilts, zonal pressure gradients, and horizontal phase relationships may be weakened, cancelled, or biased by the relative amplitude and sample size of eastward- versus westward-propagating disturbances.

We thank the Referee for providing a good point that combining both eastward- and westward-propagating modes might hide some physically important phase relationships, such as the tilted vertical profile in the westward-propagating IG waves. We have calculated the composites separately and added the figures to the Supplementary Materials. We now refer to these composites in L231–235, L269–271, and L468–473.

2. **Minor Comments**

Minor Comment 1.

Line 78: Please add Sobel and Maloney (2013) as a citation for the MJO moisture-mode theory.

We have added Sobel and Maloney (2013) in L81–82.

Minor Comment 2.

It might be better to make the definition and interpretation of N_{mode} more precise. Equation (1) defines

$$N_{\mathrm{mode}} \equiv \frac{s'}{Lq'},$$

and the text then interprets this as the ratio of DSE to latent energy perturbations. However, in the scaling arguments of Adames et al. (2019) and Adames (2022), the relevant numerator is more accurately the enthalpy perturbation scale, i.e. C_pT', with the approximation $C_pT' \simeq s'$ requiring an implicit assumption that the perturbation in DSE is dominated by the enthalpy rather than by the geopotential (as proven below). Thus, a more precise statement is that N_{mode} represents the ratio of the enthalpy perturbation scale to the latent-energy perturbation scale.

I emphasize this point not because I try to be strict, but because the analyses in this manuscript are very valuable, and I hope that future studies will adopt similar approaches. In that context, an accurate definition of this quantity would be especially beneficial for future studies.

(i) Clarify the definition in Eq. (1). As written, it is more accurate to write Eq. (1) as

$$N_{\mathrm{mode}} \equiv \frac{C_pT'}{Lq'} \simeq \frac{s'}{Lq'}.$$

This is proven below.

(ii) Show how the simplified diagnostic follows from the published scaling theory. The relation between the intuitive diagnostic $C_pT' / \left(Lq'\right)$ (or $s' / \left(Lq'\right)$) and the expression for N_{mode} in Adames et al. (2019) is not completely transparent to readers who have not worked through the algebra in that paper. I therefore recommend adding a short appendix derivation showing how the diagnostic used here follows from the theoretical scaling. For example, starting from hydrostatic balance,

$$\frac{\partial \phi}{\partial p} = -\frac{R_dT}{p},$$

and assuming separable vertical structures,

$$T = \widetilde{T}\,(x,y,t)\,a\,(p)\,, \quad \phi = \widetilde{\phi}\,(x,y,t)\,\Lambda\,(p)\,,$$

one obtains

$$C_p\widetilde{T}a = -\frac{C_p}{R_d}\widetilde{\phi}p\frac{\partial\Lambda}{\partial p}.$$

With $a = p\partial\Lambda/\partial p$, this gives

$$C_pT = -\frac{C_p}{R_d}a\widetilde{\phi}.$$

Using the first-baroclinic gravity-wave speed written in terms of the vertical structure function,

$$c = \left(\frac{R_d\overline{M_s}}{C_p\langle a\rangle}\right)^{1/2},$$

where $\langle\cdot\rangle$ denotes vertical integration and $\overline{M_s}$ is the dry stratification, vertical integration and rearrangement give the enthalpy scale in terms of the geopotential scale,

$$C_pT \sim \frac{\overline{M_s}}{c^2}\widetilde{\phi}.$$

Combining this with the scale estimates in Adames et al. (2019),

$$\widetilde{\phi} \sim Uc_p, \quad \widetilde{q} \sim \frac{N_c\overline{M_q}U}{Lc_p},$$

then yields

$$N_{\text{mode}} \equiv \frac{C_pT'}{Lq'} \simeq \left(\frac{c_p}{c}\right)^2\frac{1}{N_c\left(1-\widetilde{M}\right)},$$

which is the same quantity given by the theoretical scaling.

Note that although Adames (2022) begins with a DSE equation, that study scales the DSE perturbation by the enthalpy perturbation in its Eq. (30c), where $s' = C_pT\hat{s}$. Thus, although this point is not stated explicitly, the formulation effectively assumes that $s' \simeq C_pT'$.

Presenting this kind of derivation in Appendix would make clear why the ratio used in this manuscript is not merely an ad hoc empirical diagnostic but a simplified form of the theoretical N_{mode}.

We have revised Eq. (1) to now include the original definition of N_{mode} as in Adames et al. (2019) and Adames (2022). As suggested, we have also added a short appendix showing how the thermodynamic ratio in Eq. (1) can actually be related to the theoretical N_{mode} as in Adames et al. (2019).

Minor Comment 3.

The manuscript appears to overgeneralize the role of the background moisture gradient in moisture-mode dynamics, especially by treating the meridional moisture-gradient mechanism associated with moisture–vortex instability as if it were a necessary condition for moisture modes more generally.

In Section 4, where the manuscript interprets Fig. 3, the authors write:

> Figure 3 shows that moisture modes mainly occur along the edges of the deep tropics, around 15°N/S. These regions coincide with the climatological location of the moist margin (see e.g., Mapes et al., 2018; Robinson et al., 2024), where the background horizontal moisture gradient is large. This is thought to be a necessary condition for moisture modes to occur (Mayta and Adames Corraliza, 2024).

I find this phrasing too broad. A background moisture gradient is central to the moisture–vortex instability mechanism, but it is not a necessary condition for all moisture-mode theories. Moisture–vortex instability is better understood as one member of the broader class of moisture-mode dynamics, not as synonymous with moisture-mode dynamics as a whole. For example, moisture-mode theories of the MJO can involve different maintenance mechanisms and can be formulated in settings where the meridional moisture distribution is approximately parabolic about the equator, rather than requiring a large local meridional moisture gradient (e.g., Adames and Kim, 2016; Wang and Sobel, 2022).

A similar concern arises where the manuscript states later in Section 4.2:

> While the MVI mechanism described so far is useful in elucidating the dynamical structure of the off-equatorial moisture mode, it cannot, however, explain why the convectively active region of the equatorial moisture mode occurs where the background meridional moisture gradient is virtually zero (Figs. 8a,d).

We have revised the sentence in L213–214 to avoid the overgeneralisation of the role of background moisture gradient in the dynamics of moisture modes. We have also added a sentence in L325–327 stating that the growth of equatorial moisture modes may be maintained by different mechanisms which do not require a large local meridional moisture gradient, such as those in Adames and Kim (2016) and Wang and Sobel (2022).

Minor Comment 4.

In lines 211–214, the manuscript states:

> we do not see any significant differences in DSE anomaly magnitudes among the three systems. Unlike the Lq perturbation, the DSE anomaly varies only weakly with the system's propagating speed. Hence, although the ratio of DSE to latent energy anomalies (i.e., N_{mode}) increases with phase speed as hypothesised in Eq. 2, it is mainly due to the change in $Lq^{'}$ alone, rather than the changes in both $s^{'}$ and $Lq^{'}$.

This result appears to be consistent with the theory of Adames (2022). In Eq. (45), the enthalpy perturbation scale is set by the static stability, scale height, and dry-gravity-wave phase speed, all of which are independent of the relevant time scale. Thus, the enthalpy perturbation scale is also independent of that time scale. By contrast, the moisture perturbation is scaled by the Chikira scale and the wave time scale, as shown in Eq. (58) of Adames (2022). Therefore, as the time scale becomes longer, the moisture perturbation magnitude increases, consistent with the manuscript's finding that the increase in N_{mode} is primarily attributable to changes in $Lq^{'}$ rather than to changes in $s^{'}$.

We have added a sentence in L397–399 to express how these results are, in fact, consistent with those in Adames (2022).

Minor Comment 5.

The manuscript states in lines 268–270:

> It clearly shows that the faster moving systems are more horizontally unbalanced than the slower moving ones, even for those in the equatorial region.

This result is also consistent with the theory of Adames (2022). In that theory, the degree of nonlinear balance is controlled by ϵ in Eq. (47). When ϵ is small, the deviation from WTG becomes less divergent, which corresponds to a state closer to nonlinear balance. Moreover, ϵ is determined by the Froude number in a manner analogous to N_{mode}. Thus, a reduction in N_{mode} is expected to correspond to improved nonlinear balance.

We have revised the sentence in L422–424 to state that our results regarding dynamical balance spectrum are in agreement with the theory of Adames (2022).

3 Referee 3

The manuscript entitled "*The Moisture Mode-to-Gravity Wave Spectrum as a Framework to Define Tropical Weather Systems*" by Respati et al. proposes a novel framework for describing tropical weather systems using both spectral analysis and object tracking. The study classifies convectively coupled tropical motions into moisture modes, mixed systems, and inertio-gravity (IG) waves based on their phase speed, and investigates their thermodynamic, dynamical, and rainfall characteristics.

Overall, I found the manuscript conceptually interesting and potentially impactful. In particular, I appreciate the attempt to bridge large-scale tropical wave dynamics with object-based tropical weather systems to define and understand tropical weather phenomena and precipitation extremes. The paper also makes an important effort to connect theoretical ideas regarding moisture modes and balanced/unbalanced tropical dynamics with observational analyses.

The statistical analyses, both for weather system classification and for composites comparisons, are carefully conducted, the figures are clearly presented, and the manuscript is generally well written. Overall, this is a solid and well-executed study. I have a few specific comments below where the interpretation and presentation could be further strengthened. I recommend publication after **minor revisions.**

We thank the Referee for their appreciation for our study here. We are addressing the Referee's comments and questions below, where their comments/questions are shown in blue and our answers in black.

Main comments:

1. While it is elegant to apply wave dynamics to defining and classifying tropical weather systems, the manuscript would benefit from deeper discussion of how the filtered and tracked OLR objects relate to more tangible raw object-tracked systems such as MCSs and TCs. In particular, are the identified systems best interpreted as manifestations of idealized dynamical modes, theoretically grouped dynamical entities, or physical weather "objects" analogous to synoptic systems? I think adding more text discussing this would be helpful.

   We thank the Referee for this comment. To more clearly describe our interpretation of the identified systems and their relationships to more traditional object-based definitions (such as MCSs and TCs), we have included new text in L426–438.

   > ...we argue that our study here is an attempt to define a set of synoptic-scale weather systems in the tropics analogous to those in the extratropics. As in the extratropics, continuous atmospheric waves are the "background" dynamics of the weather. For example, a typical Rossby wave train in the midlatitudes can give rise to several pairs of high- and low-pressure systems. These highs and lows are the extratropical "weather systems", not the Rossby wave itself. These synoptic-scale systems are the bridge between the planetary-scale background dynamics and the smaller-scale tangible weather occurrences, such as thunderstorms and wind gusts. Applying this idea to the tropics, we posit that equatorial waves, including the MJO, are the dynamics occurring in the "background", and they can spawn synoptic-scale weather systems investigated in this study. For instance, ER waves are characterised by sets of gyres, and, among these, the cyclonic gyres may be identified as moisture mode "objects", or may amplify and become TCs (e.g., Frank and Roundy, 2006; Feng et al., 2023). Furthermore, as different types of waves commonly co-occur, our methods here implicitly take into account the interactions among different waves in the same phase speed category, which are argued to have similar thermodynamic characteristics, when we do the spectral filtering. In this way, a detected synoptic "object" can be a more realistic manifestation of those interactions in the background, rather than representing only a single type of wave.

   A related question: the current study filtered out TCs before applying the spectral analysis; if not filtered out, would TCs themselves be decomposed into the other three spectral weather types (moisture modes, mixed systems, and IG waves)?

   Regarding our method of filtering out TCs prior to the spectral filtering, it is because TCs are agreed upon as a special type of tropical synoptic weather systems, separate from the rest in the moisture mode-to-gravity wave spectrum (Adames, 2022). If we do not filter out the TC signals, due to their slow propagation (~ 5 m s^{-1}), they will "contaminate" the moisture mode category, even though their dynamical structures are different (see Fig. 6).

2. There are a few places in the manuscript (e.g., L212–215) stating that "the ratio of DSE to latent energy anomalies increases with phase speed" or that "the relative role between moisture and buoyancy decreases with phase speed". What is the underlying physical reason for moisture anomaly amplitude decreasing with phase speed? A brief explanation of the mechanisms would strengthen the interpretation and help with understanding the three types of weather systems along the moisture mode-to-IG-wave spectrum.

   We have expanded the sentence in L395–397 to include the explanation of why the ratio of DSE to latent energy perturbations changes with phase speed. Systems with slow propagating speed have a larger temporal scale ($\tau \propto c_p^{-1}$), which allows them to have their temperature anomalies due to latent heat release in convection smoothed out by gravity waves, leading to the WTG condition reached. In faster moving systems, this condition is more difficult to achieve, as the temporal scale of the systems is smaller, making the ratio between temperature and moisture perturbations larger. More in-depth explanation can be found in e.g., Adames et al. (2019) and Adames (2022), and the new Appendix A also shortly describes how the ratio changes with phase speed.

   A minor note on the use of "buoyancy" throughout the manuscript (e.g., L215): moisture perturbations themselves affect, and can strongly contribute to, buoyancy anomalies. It may therefore be clearer in some places to refer specifically to DSE or temperature perturbations, rather than using "buoyancy" in a general sense.

   Regarding the word "buoyancy", we have replaced it with "temperature" in some instances to avoid misunderstanding. We have also added a sentence in L73–75 to clarify this.

   > While buoyancy itself is also affected by moisture anomalies, moisture modes are distinct in that they are present even under weak temperature gradient (WTG; e.g., Sobel and Bretherton, 2000) conditions, with no large-scale buoyancy anomalies.

Minor comments:

The use of "perturbations" throughout the manuscript (e.g., L99, L201): it would help to clarify, at least for the early usages, the reference state relative to which the perturbations are defined (e.g., time mean, zonal mean, or anomalies relative to the surrounding environment outside the identified weather systems).

Perturbation or anomaly of a field is calculated by removing its seasonal and diurnal cycles. We have clarified this in L162–164.

Section 3: Several important methodological steps are described in this section. Adding subsection subtitles/headings would improve the presentation and make the analysis flow easier to follow.

We have divided the Methods section into smaller subsections to improve readability.

Figure 11: The violin plots show probability density (normalized by bin size and total counts). It would be clearer to explicitly note this in the caption. It may also be helpful to include the total sample counts for each system category.

We have revised the figure to show the total counts for each category and we have also added a sentence in the caption to clarify that the half-width of a violin represents probability density, which is frequency of occurrence normalised by bin size.

Section 5 and Figure 12: Throughout this section, the authors refer to the probability of extreme precipitation occurrence associated with a given system category, normalized by the total probability of extreme precipitation occurrence (0.01), which is defined in Eq. 3, as "changes in the probability". This phrasing can be somewhat confusing. It may be clearer to describe this quantity instead as the probability ratio (or probability enhancement) of extreme precipitation associated with system type X, or similar wording along these lines.

We have revised the caption of Figure 12 and the sentence in L353–355 in Section 5.

Relatedly, Eq. 3 and the associated description (L178–181) may benefit from minor adjustment. The text discussion appears conceptually close to a conditional probability (i.e., extreme precipitation conditioned

on the occurrence of system X), whereas the numerator in Eq. 3 is currently written using "AND". A conditional probability notation "|" may be instead intended.

We agree that the notation "|" better suits the intended expression in the numerator in Eq. 3, therefore we have revised it.

L299–301: The discussion of "compound" influences could be clearer. Since the three non-TC categories are defined using non-overlapping phase-speed filtering ranges, I would have expected them to be mutually exclusive by construction. Does the "compound" contribution therefore mainly reflect TCs co-occurring with the other three system types? If so, clarifying this would be helpful.

Since the separation by speed is done in the spectral space rather than in the physical space, the resulting categories may be overlapping. As discussed in L454–456, if a system propagates in the physical space with speed close to a dividing threshold between two categories, its signal may get "split" in the spectral space, and the results are two overlapping systems from two categories, as can be seen in Fig. 9. We believe our definition of "compound" in L335–338 is adequately clear.

L295–305 and Figure 10: The extreme rainfall attribution results are interesting. There could potentially be additional discussion comparing these results with attribution results based on purely object-based classifications of weather systems; for example, whether common object-based weather systems account for a similar fraction of total and extreme rainfall. This could also provide an opportunity to discuss how the weather systems defined through spectral analysis relate physically to more conventional object-tracked systems.

We have added a discussion in L449–453 regarding how our rainfall attribution results relate to previous studies that used pure object-based weather system classifications (e.g., Dowdy and Catto, 2017; Utsumi et al., 2017; Zhao, 2022; Prein et al., 2023; Tsai et al., 2025).

L360–362: What are the reasons for the differing N_{mode} results for IG waves and mixed systems across the studies discussed here? A brief explanation would help clarify the interpretation.

The main reason for this is because studies like Adames et al. (2019) and Adames (2022) are purely theoretical and rely on scale analysis to obtain the values of N_{mode}. They used some "typical" values of the perturbation magnitudes and applied other fundamental assumptions, such as the vertical structure of the perturbations. Meanwhile, Mapes et al. (2006) calculated the ratio based on real observed values, therefore theirs are more similar to what we get based on the composited observed systems. We have revised the sentence in L399–400 to clarify this.

A minor note, the sentence "In other words, it is only the relative importance of moisture and temperature that changes with phase speed..." does not seem to most accurately fit the logic flow here. Since the preceding text already discusses that differences in moisture perturbation amplitude are the dominant contributor to the variations in N_{mode} and are closely associated with the corresponding phase-speed differences, it may be sufficient to simply emphasize in L362 that moisture perturbations remain non-negligible even in the faster-moving systems.

We have revised the sentence to better express our intended message that, even though temperature never "overtakes" moisture in terms of perturbation magnitude, the *relative* importance between moisture and temperature does change with phase speed as the moisture perturbation decreases in the faster-moving systems.

L396–397, also L365: I suggest being a little more careful in addressing the phase speed, or N_{mode}, as proxies for, or reflections of, thermodynamics. They are tightly connected to the dynamics.

We have added a discussion in L457–467 regarding the validity of our assumption that phase speed can be a proxy for thermodynamics through N_{mode}.

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