Review : Coupled estimation of inertia-gravity wave and turbulent balanced motions

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We would like to thank the Reviewer for agreeing to revise our manuscript. We have substantially revised the text, following the suggestions of the Reviewer and two other reviewers. In particular, the form of the manuscript (both in terms of language and organisation) has been reworked, leading to a significant improvement in its clarity. We have also made an effort to lighten the mathematical details. Below are our point-by-point responses to the reviewer's comments (our responses appear in dark blue).

1 Major points

Remark 1: Section 2.3 makes multiple statements without citing relevant references that can guide the readers for detailed calculations. The authors should carefully read through Dewar and Killworth 1995, Reznik et al. 2001, Thomas 2016, and Thomas 2023 and connect to these studies in section 2.3. As the authors say below line 115, one does not need to linearise the governing equations to get wave and balance equations. These calculations are discussed in detail in Dewar, W. K. and Killworth 1995, Reznik et al. 2001 and Thomas 2016 for O(1) amplitude waves. These calculations are generic in geophysical flow models, not specific to shallow water: see section 2 in Thomas 2023. Section 2.3 of the present manuscript can be much better written explaining these wave-balance splitting equations, thus informing a new reader important details and citing relevant references.

Answer: We have added the majority of the recommended references and linked them to our method. In particular, the complex amplitude of the wave can be seen as the dominant term of an asymptotic decomposition as presented in Reznik et al. 2001, Ward and Dewar 2010 for a RSW model. This has been stated in Sect.2.2. We should emphasize, however, that the main objective of the paper is to present data-driven methods for extracting and separating a mesoscale field and an incoherent wave field, and to estimate it from snapshots of observations. In this context, the wave/balance splitting equations provide some support to interpret these methods, but we do not aim at investigating the impact of the wave field on the mesoscale field (and we consider dynamical regimes in which the wave dynamics is essentially linear, see our answer to next question). Therefore, we do not provide an extensive list of references on these aspect. As the Reviewer mention, there is a vast corpus of papers on this subject which is much more relevant than our contribution. The suggested articles were mentioned at the end of Sect2.3.2.

Remark 2: In this study, the authors conducted five numerical simulations of the rotating shallow water equations by focusing on the interaction between a plane wave and a zonal jet. The key parameters examined here include the temporal frequency of the incoming wave, its direction of propagation, and the Rossby number of the turbulent jet. How do the energies of the incoming wave and the jet compare against each other? Can we treat the energy ratio between the wave and the jet as a parameter? Additionally, how would the results change if we alter the energy ratio of the incoming wave and the turbulent jet?

Answer: In this study, we deliberately placed ourselves in a low wave amplitude regime, which frees the study from the wave amplitude parameter by linearity, as remarked by the referee. Although this allows for a simpler dynamics, it is also associated with a more challenging retrieval of the wave field from SSH snapshots, as the mesoscale contribution dominates, and it is one of the main advantages of the propose decomposition method to be able to recover the wave field is such regime. Generalization to higher wave amplitude (whereby the wave/mesoscale amplitude becomes a parameter) is identified as a perspective for further work in the conclusion. Nevertheless, we propose below a discussion of the possible impact of higher wave amplitude on our results.

A first consequence of small wave amplitude is the absence of front sharpening, caused by wave-wave interactions, leading to the rise of harmonics. The second consequence is the absence of Reynolds stresses and then influence of the wave field on the jet. We agree with the referee that it may constitute an additional parameter, but we believe that the energy of the jet and the wave does not play a crucial role in the methodology, and in the fact that the primary interactions with the POD modes of the jet generate the most incoherent modes.
Concerning the rise of harmonics induced by front sharpening, they are filtered by the complex-demodulation step, which does not suppress energy transfers between scales, but still result in almost-sinusoidal waves.
Concerning the impact of wave amplitude on the system, Eq. (21), which shows that the EPOD modes are generated by interactions between the coherent part and the POD modes of the jet, can in all generality be deduced from an assumption of time scale separation. For example, for $O(1)$ waves, the only difference is the presence of Reynolds stresses in the evolution equation of the mean field (which is diagnosed from the non-linear simulation), as suggested in the references given by the reviewer. In the evolution equation for the wave, large wave amplitude may induce generalised Reynolds stresses which are related to energy transfer towards harmonics. This effect may affect the results, but we can remark that a part of these effects can be captured by the POD/EPOD procedure, since it is built using the outputs of the non-linear simulation. This constitutes a perspective for further investigations.
Preliminary tests in non-linear regime confirm that the results shown in the paper are not strongly affected. We have clarified these points in section 2.3.2. Discussion of the estimation results when the mesoscale flow is weak has been also added in the new section 4.4 "Discussion".

Remark 3: Equation 7 onwards: the authors have to explain whether there is any overlap between the components of the field after the decomposition. Additionally, time- filtering does not guarantee that energy of the different components separate. More discussion on the decomposition implemented numerically is needed for readers to follow.

Answer: In section 2, we have placed ourselves in an idealised situation where the frequency overlap between the jet and the wave is negligible, justifying the timescale separation assumption. This is verified in the spectrum of the solution Fig.1, showing a clear frequency separation between the jet and the wave. This allows a clean separation between the components by a simple time filtering. We agree with the reviewer that if these conditions are not met, such as in the presence of submesoscale activity, the separation between components is more challenging. We therefore mention throughout the manuscript that the jet is a mesoscale flow.

Remark 4: Lines 140 - 145: Citations needed to justify the statement "We now make the assumption that the wave amplitudes evolve on the slow timescale compared to the wave time-period $2\pi/\omega$ (which can be formalized more rigorously by a WKB approach)". Sutherland 2014 and Fabrikant et al 1998 are relevant books that could be referenced here.

Answer: We have added the proposed reference Sutherland (2014). However, we couldn't manage to have access to Fabrikant *et al.* (1998).

Remark 5: Line 151: Authors separate the wave solution into a coherent and incoherent part. However, the definitions of coherent and incoherent wave fields are not clearly described in the manuscript. Is the mean flow categorized as the coherent part and the fluctuating component regarded as the incoherent wave field? Please include an elaborate discussion.

Answer: The definition of coherent and incoherent has been improved in section 2.2 "Complex amplitudes ansatz". In this context, the term *mean field* refers to the average of the low-frequency solution associated with the mesoscale flow, and the term *fluctuations* also refers to the fluctuations of the mesoscale flow. The coherent/incoherent parts of the wave fields corresopnd to the mean and fluctuations of the complex amplitude (which is extracted from the times series via complex demodulation).

Remark 6: Line 299: It is not clear why an eastward wind forcing is incorporated in the simulations. Is it possible that this wind forcing generates inertial oscillations? If so, why does the sea surface height (SSH) spectrum presented in Fig. 1 does not display a peak at the inertial frequency f_0 ?

Answer: Wind forcing is added to the simulation to maintain the Eastward jet throughout the simulation (L297-299). Otherwise, the jet energy would decrease due to the dissipation terms of the numerical simulation. The wind forcing is constant in time and and the Rossby number is moderate, therefore it does not generate inertial oscillations, as can be seen in the SSH spectrum in Figure 1.

Remark 7: Throughout section 3, the authors describe the details of the BBPOD and EPOD methods. However, the algorithm detailed in subsection 3.3 is extremely difficult to follow. The motivation or the outcome of each step in the algorithm is unclear. Specifically, it is not clear why a training stage is required as all equations are definitive and require no algorithmic learning. There is also no clarity on what is being trained. Additionally, I don't understand how a relative error is calculated as there is no mention of a ground truth in the algorithm.

Answer: The section "Algorithm" 3.3 has been clarified. In particular, it is now mentioned that the POD/EPOD modes are learned and that the POD coefficients are estimated by a least square algorithm. In the context of separating wave and currents from data, the knowledge of a model is a precious information, but cannot be employed directly. It can be employed for instance in a context of data assimilation by coupling model and data, through *e.g.* adjoint-based optimisation. In the present paper, the strategy is data driven and considers an a posteriori reduced-order modelling technique, which does not require a numerical implementation of the model, but at the price of a training procedure. We have clarified in the text the motivation and outcomes of the estimation procedure. The definition of the ground truth has been clarified as well in section 4.3 L387-388.

Remark 8: Lines 295 - 300: The authors use periodic boundary conditions in both x and y direction. However, in the β plane approximation, periodicity in the y direction cannot be maintained. This is a contradiction. Are the authors setting $\beta = 0$? This is not explained in the manuscript.

Answer: The treatment of inhomogeneity in the y direction is performed with sponge layers, which is a standard technique in spectral methods. The linear dependence βy in the Coriolis parameter is rendered periodic using a smooth recovery function acting only in the sponge layer. Furthermore, the wave field is generated (and re-absorbed at the North) in these sponge layers by nudging toward the incoming plane wave solution. These regions are not physical and are not included in the POD and BBPOD computation. These details have been added in the manuscript in section 4.1.

Remark 9: Line 301: The authors used a radiative damping term to dissipate energy input from the wind and to obtain stationarity. Can the authors relate this damping term to any physical process or is it just a numerical technique which is implemented to facilitate numerical stability?

Answer: The radiative damping term is mainly added in our simulation in order to ensure an energy balance and to obtain statistical stationarity of the solution. Indeed, some dissipation at large scale is required due to the inverse energy cascade in order to avoid energy accumulation. Such a term is often introduced in the literature to model radiative damping, but in our study it is rather employed for numerical reasons than physical modelling. We have clarified this in the text.

Remark 10: Line 304: What is a nudging layer? Line 325: What are the sponge regions? Why are these regions removed while calculating the POD and BBPOD modes? These details have to be incorporated in the revision.

Answer: The term nudging layer has been changed to sponge layer, which is mentioned earlier in the manuscript to designate the same concept. As mentioned previously, the sponge regions are a numerical procedure to treat inhomogeneity with spectral methods, and clarifications have been provided in the revision.

Remark 11: Line 370: Authors point out that in W5 run even though the patterns are similar to W1 and W2 runs, the meanders formed in W5 run have twice larger zonal wavelength and weaker energetic contribution. However, from the colorbar provided it is not clear how the energetic contributions are weaker in W5 run than W1 and W2 runs. I would like to see a difference plot between the runs that show that the energetic contribution are weaker in W5 than W1 and W2 runs?

Answer: Indeed, the amplitude difference is only qualitatively visible by the difference of color saturation between figure 3 and C1 of the revised manuscript. (Formerly Figure 7 has been moved to the appendix (C1) for the sake of conciseness, following another Reviewer's suggestion.) We have added the values of eigenvalues, which corresponds to the corresponding energy captured by the mode, in the graphs figures 3, 5 and C1, in order to add the quantitative information.

Remark 12: In Fig. 5, the authors show the modal energy distribution of the incoherent wave field. It is not clear how exactly the incoherent energy is calculated. Line 353: Why is it expected that for the stronger jet the energy will be higher?

Answer: The definition of incoherent energy from BBPOD eigenvalues has been specified in the text in section 4.2.1 and in the legend. Figures 4 and 5 have been merged, and the normalised cumulative energy has been removed because it was redundant with the non-normalised version.
The authors have clarified the meaning of sentence L353, in L349-351 of the new version. Since the Rossby number is higher in the W5 simulation, the contribution of the non-linear terms between the wave and the jet, responsible for the generation of its incoherent part, is a priori more important as suggested by Eq.13.

Remark 13: In Fig. 10 (caption is missing), the error almost saturate or decay very slowly in the range $20-30$ modes. However, the authors claim that using a large number of modes render better estimates. Can the authors comment on the computing cost required for using large number of modes in performing the estimates? Do we expect better estimates if we use more than 30 modes? As far as I understand from Fig. 10, increasing the number of modes further will not provide better estimates as the error seems to have already saturated by N=30 modes.

Answer: We agree with the Reviewer that further increasing the number of modes leads to slight performance improvement, or even some increase of the error in situations of over-fitting (with partial observations). Even if the projection error is guaranteed to be reduced, this is not the case for the estimation step. It is often preferable to consider a more robust estimation with a reasonable number of modes kept than trying to estimate high-order modes leading at the end only to a slight improvement. The computational cost is very small in our case whatever the number of modes kept, since the minimisation problem is a simple least square of small dimension. The discussion of these aspects have been improved in the revised manuscript section 4.3.2.

Remark 14: The manuscript focuses exclusively on gravity waves, although this data driven method applies broadly to other similar wave scattering problems in the ocean. Scattering of small amplitude waves by a vortical flow or topography makes the wave field incoherent, leading to the excitation of new wave-modes with similar frequency as the parent wave. This has been seen for near-inertial waves (Danioux, and Vanneste 2016), acoustic waves (Thomas 2017) and surface waves (Thomas and Yamada 2018). The authors should broaden their summary section explaining the broader potential of the method they demonstrate for tides. Scattering of different small amplitude oceanic waves have been studied by Danioux, and Vanneste 2016, Thomas 2017 and Thomas and Yamada 2018 using asymptotic models. Since the present data driven method could be applied for these waves as well, the authors should discuss the broader applicability of their technique in the concluding section

Answer: We thank the reviewer for this remark. Indeed, the method is not specific to IT, and can be extended to other wave-flow interaction configurations. However, our strategy is primarly to configurations where a coherent wave contribution is identified. This may be the case for instance for internal tides, barotropic tides, of in the context of aeroacoustics with a tonal incident wave interacting with a turbulent flow. Near inertial waves and surface waves should require either to develop other strategies or to identify clearly a coherent wave generated of incoming in the domain. We have enriched the conclusion is that sense.

Remark 15: Following up on above comment, the present study addresses two dimensional scattering while oceanic waves can be scattered in three dimensions: see recent work by Kafiabad et al. 2019. How would the method the authors use in the present manuscript extend to three dimensions?

Answer: This is indeed a promising perspective and extension of the present work. We mentioned in the conclusion that extended modes can be defined in 3D, but we recommend combining this with other decomposition techniques, such as vertical mode decomposition as in Kelly (2016). This allows to obtain a coupled shallow water system, where each vertical mode component is driven by a RSW-like equation, which are coupled by additional terms. This may constitute a direct application of EPOD-based estimations.

Remark 16: In the conclusion section the authors should also discuss in detail how the method would be challenged if O(1) waves were scattered by balanced flows, while being still in the small Rossby number limit. Some discussions on submesoscale interactions will also be useful.

Answer: We mention in the conclusion that a large amplitude incoherent wave remains correlated to the jet and therefore the extended modes remain a priori well defined. As stated in remark 2, the problem may remain very similar, but Reynolds stresses and generalised Reynolds stresses should be taken into account. A part of them are potentially accounted for implicitly through the BBPOD and correlation procedure, but this needs to be tested, and is a nice perspective of the present study.
The submesoscales does not respect the time-scale separation with the coherent wave. This would require further developments. It mainly challenges the ability to separate jet and wave components, where filtering is not possible anymore and models are likely to be necessary.

2 Minor points

Remark 1: Line 46: The abbreviation for spectral proper orthogonal decomposition is not specified here, but the authors use SPOD in Line 53.

Answer: The abbreviation SPOD has been added to the introduction.

Remark 2: Line 94: “A wave forcing term ...

Answer: This mistake has been corrected in the manuscript.

Remark 3: Lines 95-100: The domain of the physical space is given as $\Omega \in \mathbb{R}^2$. Later, however, the physical domain of the simulation is periodic (lines 295-300). This creates a confusion. Writing the equation in a conventional form (eg. the ones given in Vallis 2006) might be better.

Answer: We have improved the definition by stating "...where $\Omega \in \mathbb{R}^2$ is the bounded spatial domain". This includes periodic domains.

Remark 4: In Eq 2, it is redundant to write $\Re$, instead using $\cos(\omega t)$ makes the it clearer.

Answer: As $\tilde{q}_\omega(t, x, y) \in \mathbb{C}^3$, which follows from the complex demodulation operation (Eq.5 in the revised version) Eq.2 is not equivalent to taking the $\cos(\omega t)$ instead of $\Re$.

Remark 5: Lines 110-115: The details of the expectation operator are absent. It is unclear whether it is an expectation over multiple realisations, time, or physical space.

Answer: The expectation operator is a time average. This has been specified in the new version in section 2.2 and in the methods in section 3.3.

Remark 6: Line 105: A time scale separation is assumed between wave and balanced flow. This means that the authors are operating at mesoscales and not submesoscales, the latter scales having no wave-balance time scale separation. This needs to be acknowledged in the writing.

Answer: As suggested by the reviewer, we have specified throughout the manuscript that the turbulent flow is a low-frequency mesoscale flow.

Remark 7: Lines 115-120: Adding "linearisation of Eq (1) about a balanced jet," instead of only "linearisation about Eq (1)," clears, at the onset, the steady state about which the linearisation is being performed.

Answer: The term linearisation has been removed and the decomposition of the solution $q_{jet} + \epsilon q_\omega$ has been given directly, to be more concise.

Remark 10: Lines 145 - 150: The operator $\boldsymbol{R}$ seems to be the Greens function of the linearized equation. Mentioning that is useful.

Answer: As suggested by the reviewer, the operator $\boldsymbol{R}$ is related to the Green's function. This has been mentioned in the revised version, and a references has been given.

Remark 11: Line 156: " Finally, substracting ...", did the authors mean to write "substituting" in place of "substracting" ? Or is it "subtracting" ? The sentence is not clear and I would suggest rewriting this line.

Answer: The authors did mean subtracting. This error has been corrected in the text.

Remark 12: Line 276: "...it could be subtracted... "

Answer: This paragraph has been removed from the manuscript in order to reduce its size, which was considered too long by the other reviewers.

Remark 13: Line 322: It seems like this line is misplaced. Also no description on Fig. 2 is provided.

Answer: This paragraph has been amended accordingly, with a fuller description of the figure as suggested by the reviewer.

Remark 14: Line 341: Did the authors mean "..., which is pronounced for W3.."? In table 1, it is mentioned that $\omega = 3f_0$ for the W2 run. Also from Fig. 3 it is clear that it is in the W2 run, the north part of the domain exhibited a drop in the amplitude.

Answer: Indeed, the authors wanted to refer to W2 and not W3. This has been corrected in the article.

Remark 15: The labels on all the figures are very small, especially Figs. 4, 5, and 7. In Fig. 5 what is the x axis? Is it the number of modes N? Also, y axis label is not written correctly. Do the legends correspond to the different runs W1, W2, and so on? If yes, I suggest adding this as legends instead of showing the parameter values.

Answer: We have improved the figures.

Remark 16: Figure 10 doesn't have a caption and the legends are missing. Thus the description of the figure given in lines 425-463 is very hard to follow. A revised figure with proper labels and legends should be provided.

Answer: This has been corrected.

Remark 17: q in Eq 2 is used to denote the flow degrees of freedom whereas $q_{frc,\omega}$ is used to denote the wave forcing. This calls for confusion in the reader's mind. The flow is later decomposed into several components adding further subscripts to the flow degrees of freedom q, thereby increasing the confusion in reader's mind.

Answer: The notation was indeed confusing, we have changed $h_{frc,\omega} \mapsto f_{h,\omega}$, $\vec{v}_{frc,\omega} \mapsto \vec{f}_{\vec{v},\omega}$.