Review of *Coupled estimation of incoherent inertia gravity wave field and turbulent balanced motions via modal decomposition*

The manuscript proposes a data driven method which can be used to extract the structures of a inertia-gravity wave field scattered by balanced eddies in the ocean. The authors extend the idea of proper orthogonal decomposition (POD) to broadband POD (BBPOD) and extended POD (EPOD). Using these two decompositions, the authors split the net flow into a geostrophically balanced coherent jet, fluctuations of the balanced jet and a wave field with a particular frequency ω. They find that the two decompositions give the same qualitative results, and for low wave amplitudes and Rossby numbers, the decompositions do well. They also try to estimate the sea surface height (SSH) contributions of each mode for different wave forcings.

The study is essentially trying to make progress on identifying and separating fast inertia-gravity wave signals from sea surface height. This is an important and useful area of research, given the satellite data sets oceanographers work with and the state of the ongoing SWOT mission. However, I found the present manuscript to be lacking in multiple areas, in addition to mistakes in the writing and many relevant references missing. I recommend a major revision of the manuscript taking on board the comments detailed below.

Major Comments

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 & 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. & 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.

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?

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.

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.

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.

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?

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.

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.

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?

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 BPOD modes? These details have to be incorporated in the revision.

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?

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?

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.

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, & Vanneste 2016), acoustic waves (Thomas 2017) and surface waves (Thomas & 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, & Vanneste 2016, Thomas 2017 and Thomas & 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.

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?

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.

Minor comments

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

2. Line 94: "A wave forcing term ...

3. Lines 95-100: The domain of the physical space is given as $\Omega \subset \mathcal{R}^2$. 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.

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

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.

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.

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.

8. Lines 145 - 150: The operator R seems to be the Greens function of the linearized equation. Mentioning that is useful.

9. 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.

10. Line 276: "...it could be subtracted... "

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

12. 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.

13. 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.

14. 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.

15. q in Eq 2 is used to denote the flow degrees of freedom whereas $q_{frc,w}$ 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.

References

1. Sutherland, B. R. 2014, Internal Gravity Waves, Cambridge University Press.

2. Thomas, J. (2016): Resonant fast-slow interactions and breakdown of quasi-geostrophy in rotating shallow water, J. Fluid. Mech., 788: 492-520.

3. Dewar, W. K. & Killworth, P. D. 1995 Do fast gravity waves interact with geostrophic motions? Deep-Sea Res. I 42 (7), 1063–1081.

4. Thomas, J. (2023) Turbulent wave-balance exchanges in the ocean. Proc. R. Soc. A 479, 20220565.

5. Reznik, G. M., Zeitlin, V. & Ben Jelloul, M. 2001 Nonlinear theory of geostrophic adjustment. Part 1. Rotating shallow-water model. J. Fluid Mech. 445, 93–120.

6. Thomas, J. and Yamada, R. An amplitude equation for surface gravity wave-topography inter- actions, Phys. Rev. Fluids 3, 124802 (2018).

7. Danioux, E. & Vanneste, J. 2016 Near-inertial-wave scattering by random flows. Phys. Rev. Fluids 1, 033701.

8. Fabrikant, A. L., Stepanyants, Y. A. & Stepanyants, Y. A. 1998 Propagation of Waves in Shear Flows, World Scientific Series on Nonlinear Science Series A, vol. 18. World Scientific.

9. Thomas, J. New model for acoustic waves propagating through a vortical flow, J. Fluid Mech. 823, 658 (2017).

10. Kafiabad, H. A., Savva, M. A. & Vanneste, J. 2019 Diffusion of inertia-gravity waves by geostrophic turbulence. J. Fluid Mech. 869, R7.