Review 3 : Coupled estimation of inertia-gravity wave and turbulent balanced motions

Igor Maingonnat, Gilles Tissot, Noé Lahaye

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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: My primary concern with the manuscript is its readability; the dense mathematical details required multiple readings to fully grasp. The text is mathematically quite involved, and I think, it can be simplified. For instance, the authors initially permit the complex amplitude of the inertia wave to vary over time, but later disregard this time dependency. I wonder whether it would be possible to reach the final resolvent equation by imposing classical Reynolds decomposition and linearization about the mean. I have listed specific areas that require further clarification; however, beyond addressing these, I recommend that the authors re-evaluate the entire text to enhance overall clarity.

We have rewritten significant parts of the manuscript to improve its clarity and fluidity, and have clarified the mathematical developments in section 2, as suggested by the reviewer. In particular, the time dependence of the complex amplitude is discussed more explicitly in the revised version of the manuscript, and we have added the equation describing the evolution of the incoherent part in which its time derivative is not neglected (Eq. 12). The fact that the complex amplitude is time-dependent is a definition of the incoherent complex amplitude field (Eq. 4), which distinguishes it from the coherent amplitude – which is constant in time. This variation with time is due to scattering from the unsteady background flow and corresponds in spectral space to the spectral broadening mentioned in section 3. Although we neglect the time derivative in the subsequent developments, in order to build a predictive – and diagnostic – model of the incoherent wave, and to provide physical and mathematical interpretation of the results of the statistical decomposition methods presented in the manuscript, it must be kept in mind that the variability of the wave field across snapshots essentially arises from this time dependence. We have clarified to make this statement in the revised manuscript.
Concerning the simplification of the mathematical developments, classical resolvent analysis is indeed usually obtained by writing on the left-hand side the linearised system over the time-averaged mean flow and on the right-hand side a forcing term aiming at modelling the effect of the unresolved non-linearities. In our context, we identify the dominant nonlinearity, which is the interaction between the slow base-flow variations and the coherent wave. In this context, a careful writing of the interactions of the two components is necessary. This allows us to identify clearly the assumptions necessary to obtain the final resolvent equation (13). We have moreover clarified this last

step in the revised manuscript, highlighting that neglecting the time variations of the wave amplitude is related to considering a jet frozen with respect to the propagation time of the waves to travel the domain. This cannot be easily seen by performing approximations earlier in the derivation.

Furthermore, the inconsistent use of terminology—particularly terms that carry different meanings across disciplines—complicates the understanding of the methodology. For instance, using an expectation operator, they decompose the jet into mean and fluctuating parts. But, the same expectation operator separates the wave into its coherent and incoherent parts.

We have tried to standardise as much as possible the terminology of the article as suggested by the reviewer, and to make explicit the precise meaning of the terms used when there is a potential ambiguity across disciplines. An example is the change from internal waves and inertia gravity waves to internal tide, as this is the main subject of the study. The term coherent and incoherent part is standard in the context of internal tide dynamics, and indeed corresponds to a Reynolds decomposition of its complex amplitude, which have been clarified in section 2.2. The term fluctuation now refers exclusively to the low-frequency turbulent flow.

2 Minor points

Remark 1: I don't see why the authors define $\tilde{q}_\omega$ as a function of time, which is assumed to be constant later on anyway.

Answer: see our response to the main remark 1 above.

Remark 2: lines 148-149: The authors do not make any assumption about the frequency band they choose. How do they conclude about the fact that the resolvent operator is approximately constant? I would rephrase this sentence as '... can also be interpreted as assuming that the resolvent operator is approximately constant'.

Answer: The filter frequency cutoff corresponds to a typical width of the mesoscale frequency band, i.e. the most energetic low frequencies $< f_0$ (*c.f.* Fig.1). For simplicity, and because the spectral broadening of the wave results from the interaction with the broadband spectrum of the flow, the frequency band chosen is the same for the wave. A higher cut-off frequency has little effect on the results. In fact, a higher cut-off frequency will only introduce low-energy components into the jet or wave. A twice higher frequency cut-off was also tested, and POD/EPOD modes were the same.
Equation (13) of the revised manuscript, states that the incoherent wave component can be obtained applying the resolvent operator to the bilinear term $\boldsymbol{B}(\boldsymbol{q}'_{jet}, \mathbb{E}[\tilde{\boldsymbol{q}}_\omega])$ representing the interaction between the jet fluctuation and the coherent wave – here we focus on the single scattering term for simplicity. At first sight, it may appear to be obvious following a classical resolvent analysis point of view, since the resolvent operator maps the non-linear term and the response. However, it has to be remarked that the bilinar term is broad-band, and the resolvent operator is the one at the frequency ω. Equation (13) can be interpreted by the fact that in the frequency band described above, the resolvent operator is almost constant, and can be used to predict the incoherent wave.

Remark 3: line 151: Why do the authors define this decomposition as coherent and incoherent while it is a Reynolds decomposition?

Answer: The terms "coherent" and "incoherent" are standard in the physical oceanography community. We do not use the term "mean" and "fluctuation" because it corresponds to a Reynolds decomposition of the complex demodulated wave amplitude (not directly the wave field, which obviously gives 0 upon averaging). We have tried to remove this ambiguity by clarifying their precise definition in section 2.2.

Remark 4: Eq. (11): If I understand it correctly, the first term in this equation is not fluctuating since the expectation operator (which amounts time or ensemble averaging, I guess) applies to the bilinear operator. So $\tilde{q}'_\omega$ is actually fluctuating about this term. If that is the case, I find it confusing that a prime term has a nonzero mean. Regarding the first use of the prime on line 140, I would relate it to quantities with zero mean.

Answer: Eq. (11) is now Eq. (13). According to the definition Eq. (4), $\tilde{q}'_\omega$ has zero mean, justifying the use of the prime notation. In addition, taking the expectation operator of the right hand side of Eq. (13) leads to 0, and this equation is therefore consistent with the definition (Eq. 4). Under ergodicity hypothesis, Eq.12 is also consistent with the definition.

Remark 5: lines 167-170: This spectral broadening effect applies to any term in eq. (11). But I think the authors particularly think of the term $B(q'_{jet}, \tilde{q}'_\omega)$. If that's the case, I think it would help if they explicitly stated that such as 'Taking $B(q'_{jet}, \tilde{q}'_\omega)$ for instance, ...'. It took me a while to figure out which incoherent component they were mentioning on line 170.

Answer: This part of the text has been removed in order to shorten the paper. The idea of convolution is now introduced in the introduction section, at a less detailed level. We hope that the idea of spectral broadening is more clearly presented in the revised version.

Remark 6: Eq. (13): How do the authors come up with this norm? Is it common in oceanography?

Answer: This norm corresponds to the total energy, *i.e.* the sum of kinetic and potential energy, for a small perturbation. This has been clarified in the revised version of the manuscript.

Remark 7: Algorithm 1 – Training stage, last equation: This assumes a strong correlation with h_ω and q_{jet}, as stated later in the text. Is not it a very limiting assumption? How realistic is it?

Answer: We have detailed the meaning of this sentence in the new section 4.4 "Discussion" where we discuss the limit of the method. We have connected it with sentence L270-271 of the preprint "However, the part of the wave that is completely decorrelated from the jet is not estimated in this algorithm, as it is a quantity that is more difficult to quantify". We point out in section 4.4 that this is not such a limiting hypothesis since in a large part of the ocean regions the incoherent wave is generated by the (bilinear) interaction of the wave with the currents, and is therefore correlated with these currents.

Remark 8: Eq. (22): How is this minimization achieved? The coefficients at the training stage are computed using the full vector q_{jet}, while in the minimization problem, only h is used. Does not this potentially cause a uniqueness issue? How do we know that the result of the minimization is unique?

Answer: The minimisation is solved by a least square problem, with POD coefficients of the jet as control parameters (clarified in the text). In all our tests, there are more observations than modes sought. The least square problem is then unique. However as there are no regularisation term, the system may be subject to overfitting, as it is discussed in section 4.3.1.

Remark 9: Section 4.1 - A schematic showing the domain, discretization, the forcing and how the wave is introduced would be very helpful to visualize the test case.

Answer: We have added the sponge layers to this figure, delimited by dotted lines. We have also added grey areas corresponding to "relaxation" zones where the variables are damped to 0 during simulations, to ensure the periodicity of the solution calculated by the spectral method. In these sponge zones, we have added the region where the wave is generated.

Remark 10: lines 359-360: How are the EPOD modes calculated, applying POD to $[q_{jet}, q_\omega]$ or first applying to POD to q_{jet} and then using the coefficients $a_n(t)$ to reorganize q_ω? How much error would be introduced by the former?

Answer: In practice, the EPOD modes are calculated by performing a POD with a semi-norm (*i.e.* with a POD on $(q_{jet}, \tilde{q}_\omega)$, with no weight on $\tilde{q}_\omega$). This has been more precisely stated in Sect.3.2. This is equivalent as calculating POD on q_{jet} and using the coefficients to calculate the EPOD modes, as presented in the text formally. This has also been verified numerically. We also checked that putting weights on the wave did not change the estimate, due to the small amplitude of the wave. The paragraph mentioning this comparison has been removed from the manuscript to lighten the text.

Remark 11: line 366: 'jet POD mode wave BBPOD mode' → 'jet POD mode and the wave BBPOD mode'

Answer: This sentence has been modified.

Remark 12: lines 371-375: Isn't it actually possible to calculate the single scattering term in eq. (19) and compare it against the EPOD modes?

Answer: This can be verified numerically. These results are available in the PhD thesis of Maingonnat (2024), chapter 4, with a homogeneous direction. The reference has been added in the text. We do not show the results here because the methodology is very different from that used in the article.

Remark 13: Figure 10. The orange line is not mentioned in the caption.

Answer: It is now mentioned in the revised manuscript.

Remark 14: lines 443-444: Why does the method fail for the W2 case?

Answer: The loss of coherence is more intense for W2 and the resulting incoherent wave field is more complex. This was detailed in Ward and Dewar (2010) when the frequency becomes high. For example the contribution of multiple scattering term is greater for small scale waves. This also can be seen in figure 5, where the drop of amplitude of the coherent part in the north of the domain is very clear. Furthemore there is a larger scale separation between the domain size and the wavelength. This may increase the need for localisation, and the difficulty of a reduced basis to represent a more complicated solution. These effects combined may cause the loss of estimation performances.
These explanations are conjectures, and deeper analyses are required to clarify this point. We have noticed in section 4.3.1 the fact that it is still an open question.

Remark 15: lines 450-451: Is there a way to predict where the cut off the modes a priori?

Answer: Usually, the number of modes is selected *a posteriori*. Some *a priori* standard criteria exist such as Baysian information criterion (BIC) or Akaike information criterion (AIC). They account for the number of samples and the number of degree of freedom of a model to predict if we are likely in an overfitting regime. These were not tested in our configuration, since the method is computationally fast and allow easily an *a posteriori* choice.