Review 1 : Coupled estimation of inertia-gravity wave and turbulent balanced motions

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We are grateful to the reviewer for carefully reading our manuscript and providing useful comments. We have substantially revised the manuscript following the Reviewer's suggestions, as well as 2 other Reviewers. In particular, the form of the manuscript (both regarding the language and organisation) has been reworked, which we believe have lead to substantial improvement of its clarity. Furthermore, we have make an effort in better discussing the potential realistic applications of the proposed methods. Below are our point-by-point responses to the Reviewer's comments (our answers appears in dark blue).

Major points

Remark 1: The form of the manuscript is insufficient at the moment and I fear it will adversely affect the impact this work may have. The manuscript is first of all lengthy and authors should strive to reduce its length. Here are some leads as to how to do that:

there are numerous repeats in transitions about what is about to be performed which are superfluous. L327-329 for instance is one example. Another example is L331-L334 where we have to wait three sentences before entering into the "meat" of the results, this is too long. Some optimization in the number of choice of figures could also help: Figure 5a and 5b are fairly redundant and their overall relevance may be questioned for the sake of conciseness; Figure 9 is barely discussed which should be taken as a sign it does not bring much and may be discarded.

Second, the level of language is low and substantially complicates the prompt understanding of the paper. I suggest resorting to AI (deepL, chatGPT), a professional editing service, or any other solution that may improve the flow of the manuscript.

Answer: We have reworded almost every sentences to improve the readability of the manuscript and remove language errors. We have also shortened some portions of the text, and removed redundancies. Following Referee's remarks, a particular attention has been give to headers of sections where redundancies were present, such as the beginning of Sect.3 "Methods".

In order to further reduce the length of the manuscript, as suggested, figure 9 has been removed from the manuscript with the corresponding lines L411-415. As suggested, Figure 5b is given in the appendix. We have also moved Figure 7 to the appendix, since it is similar to Figure 6. Figures 4(a), 4(b), 5(a) have also been reduced in size and merged into a single figure.

Remark 2: A second major concern is about the insufficient discussion regarding the applicability of the proposed method to more realistic configurations and the steps required to go there. The last paragraph of the conclusion is too light for that purpose. Potential questions I'd have liked to see addressed:

Is the level of mesoscale activity the sole parameter relevant to identify potential geographical areas of applications?

Answer: The mesoscale activity is the main parameter, but not the only one. Indeed, the EPOD technique considers instantaneous correlation between the jet and the wave. Therefore, it is well suited for configurations where 1) the signature of mesoscale dynamics is dominant over the internal tides, and 2) the domain is of limited extent – to minimize the impact of interactions at distance, which are associated with lagged (instead of instantaneous) correlation due to the finite wave propagation speed. Furthermore, the statistical convergence of POD modes (as a function of the number of samples) can become critical over large domains, which is identified as a limitation of our technique which deserves further work. Finally, a wave source must be well identified (either inside the domain or at the boundaries), such that the scattered field results from interactions between this predictable part and the mesoscale flow. The western boundary currents (*e.g.* Gulf Stream, Kuroshio, North Brazilian current) are example of configurations that match, at least partially, these criteria. We have enriched and clarified the discussion in the manuscript, also giving perspectives for how to deal with configurations that deviate from these restrictions.

Can we anticipate increased/decreased performances in realistic configuration given identified limitations? What observations and datasources could be used to evaluate the applicability of the method in realistic/operational configurations ? Or is more work required on the idealized side? And if yes to answer what questions? Do we have a grasp as to whether the methods will have to be adapted ? "localisation technique" is mentioned albeit with little detail and no references.

Answer: This method is based on statistical learning of the variability of waves and currents from a simulation. Consequently, variability that is not represented in realistic models can't be estimated from observations. In addition, a large number of samples is required to achieve statistical convergence of the POD/EPOD modes. Methods are needed to improve the convergence of statistical estimates – and localization techniques is a good candidate (the corresponding discussion has been enriched, with references added, in the conclusion). Another methodological constraint is due to the multiple constituents for the internal tide. As the different frequencies are very close with each others, it is necessary to be able to model the variability resulting from their coupling and from interactions with the mesoscale flow. These limitations are now identified as future work in the conclusion.

There are probably several possible paths, but we think that the most straightforward development is to test our method using outputs from realistic tide-resolving high-resolution numerical simulations (such as the mitGCM-base ECCO LLC4320 run, or HYCOM run). Preliminary tests using the NEMO-based eNATL60 simulation over the North-Atlantic have already shown some technical challenges (see our answer above) that need to be tackled. In addition, more work based on idealised experiments is also required to address the issues described above.

A more detailed comparison with similar approaches (Egbert and Erofeeva 2021 for instance) is also missing.

Answer: A more thorough comparison with Egbert & Erofeeva (2021) has been added in the new section 4.4.

Minor points

Remark 1: I suggest adjusting the title according: "Coupled estimation of incoherent internal tide and balanced turbulent motions via statistical modal decomposition". Modal decomposition is ambiguous with vertical modes used for internal tide descriptions, the method is targeting internal tide and not internal gravity waves in general.

Answer: The title has been changed as suggested by the reviewer. We also have removed the word "balanced" since it is another level of detail.

Remark 2: Terminology: the use of "internal tide" instead of "internal wave" seems more appropriate and I would recommend sticking to this choice throughout the manuscript.

Answer: This term has been modified in the entire manuscript.

Remark 3: Abstract: mention of the fact that this work is carried in an idealized configuration should come very early; sentences are too long; the first sentence is not necessary.

Answer: The abstract has been extensively modified for greater clarity. The first sentence has been removed as suggested by the reviewer, and the mention that the study is being carried out in an idealised configuration now appears earlier.

Remark 4: L28: "realistic" $\rightarrow$ idealized

Answer: The term realistic has been changed into idealized as suggested by the reviewer.

Remark 5: L29: "contribute significantly to" $\rightarrow$ enhances

Answer: "contribute significantly" has been changed to "enhances" as suggested by the reviewer.

Remark 6: L73: "the model used to investigate the dynamics" $\rightarrow$ the dynamical model used to investigate interactions between...

Answer: We changed the corresponding sentence, which has been shorten for conciseness and fluidity. The fact that the model is used to investigate the interactions is mentioned later (L.80 in the introduction of Sect.2.)

Remark 7: L111: "the potentially broaden spectrum" $\rightarrow$ typo?

Answer: This has been changed in broadband spectrum.

Remark 8: L112: "the fluctuations" $\rightarrow$ anomalies

Answer: For the sake of clarity, we have introduced the incoherent amplitudes as the residual of the coherent part. "Fluctuations" is used only for the low-frequency turbulent flow.

Remark 9: L117: "can be performed" $\rightarrow$ could have been performed

Answer: This part of the text has been removed during the shortening of the paper.

Remark 10: L145: can we expect that an inverse of the operator is always available ? If yes, this should be mentioned

Answer: We specified that the resolvent operator is well defined only if $-i\omega$ is not an eigenvalue of $L + B(\mathbb{E}[q_{jet}, \cdot)$. This is indeed not always the case in general. However, it is shown numerically that this operator is invertible in the thesis Maingonnat 2024. In addition, this operator has a countable number of eigenvalues, and due to the eddy diffusion term, the eigenvalues have a slight negative real part, while $-i\omega$ is pure imaginary. We do not consider $\omega = 0$ (eigenvalue associated with solutions close to geostrophic balance), where a standard POD is performed for the jet. This renders very unlikely to fall on an eigenvalue.

Remark 11: L145-150: physical implications for this are missing. If the domain was large enough, the impact of jet fluctuations should be delayed at a distance. Can this approach represent such situation? Such element are important to gauge the applicability of the method in other configuration (e.g. more realistic ones)

Answer: As suggested by the reviewer, we have added the physical interpretation of this hypothesis in section 2.3.2. The Reviewer is entirely correct in his remark on the impact of the size of the domain. Indeed, this hypothesis corresponds to a local scattering hypothesis and is relevant on small domains, which is now explicitly stated in the text. This is a limitation of our derivation: one main consequence is that interactions at distance are filtered out in the EPOD modes (since only instantaneous correlation are retained), and consequently, the estimation will not be able to represent the corresponding fraction (see also our response to the Reviewer's main remark 2). This is

identified as a perspective for further development of our method.
Additionally, we have identified that this physical assumption is associated with neglecting the time derivative of the slowly-varying amplitude term between equation (12) and (13) of the revised manuscript. Detailed comments have been provided L.176, end of Sect. 2.

Remark 12: L155: I find the presence of the nonlinear correction intriguing, maybe puzzling. Can we expect this correction to be substantial, e.g. shouldn't the correlation drop?

Answer: We interpret this term as a multiple scattering term, since it is the non-linear interaction between the incoherent wave component and the flow. This term may be small in the context of weak interactions and/or in localised regions where the scattered wave does not have the time to interact again with the flow. It may not be neglected in the general case.

Remark 13: L173: "showing that. . . as expected" : this statement does not follow scientific writing standards.

Answer: We removed this sentence. This whole discussion has been moved in the introduction.

Remark 14: L184: the SPOD acronym should be introduced as it is not described in the appendix

Answer: We introduced this acronym in the introduction.

Remark 15: L190: "The algorithm. . . " serious language issues here

Answer: This sentence has been fixed, thank you for pointing at it.

Remark 16: Eq 17 and elsewhere: it may be useful to retain spatial coordinates dependance at least sometimes. One may loose track about what depends on space and not at times.

Answer: The dependence of the field on (x, y, t) has been added in Sect.3.1 and Sect.3.2.

Remark 17: L272 : "alternative method to using the geostrophic balance for BM. . . " this statement probes the question as to how far are velocity estimates from geostrophy and polarization relationships. You are not answering this in the manuscript which you may want to specify in order for the reader not remain in expectation.

Answer: Since this issue is not addressed in the manuscript, we have withdrawn this remark.

Remark 18: L282: " To our knowledge,. . . " I would strongly disagree, you need to be more specific.

Answer: We have modified this sentence: in the new version, we say that this is a difficult configuration for wave estimation and we cite Zaron (2017) L.282.

Remark 19: L331-332: need to report on values of alpha, hyperviscosity and all other parameters employed in the numerical simulations.

Answer: We have added the values of all the parameters, including hyperviscosity and α, L.301.

Remark 20: L310: " a sufficient sampling " I do no understand what you mean – reformulate

Answer: We meant that the time series must be well resolved in time, so that the wave can be extracted from data by time-filtering (*i.e.* in order to prevent aliasing effect). This sentence has been reworked and merged with L.306 (of the preprint), which specifies the saving frequency of the ouputs.

Remark 21: L314: "less than 3 days" you need to specify at what latitude

Answer: We have the typical latitude at which this duration corresponds.

Remark 22: L314: "magnitude spectrum" don't you mean "power spectrum" instead?

Answer: Indeed, we have corrected "magnitude spectrum" to "power spectrum". We also have updated the figure by displaying the magnitude square, and considering a Welch method to estimate the power spectrum.

Remark 23: L314: the method employed for spectral estimation needs to be specified

Answer: The details of the Welch method have been provided L.316.

Remark 24: L320: "scale separation in amplitude" this is an awkward formulation

Answer: We have deleted this sentence and the paragraph, to avoid repetition, as the comparison with the Gulf Stream has already been made in Sect.3.

Remark 25: L324-325: This paragraph seems out of place

Answer: This was a typo and has been corrected in the manuscript.

Remark 26: L327-335: see major comment 1

Answer: The authors have tried to make these two paragraphs more precise and concise.

Remark 27: L335: "nudging" it would be more straightforward to talk about "wave forcing"

Answer: The term "nudging" has been replaced by wave forcing as suggested by the reviewer. In addition, we have replaced "nudging layer" by "sponge layer", which is a more proper formulation.

Remark 28: L339: "almost zero mean" this statement does not follow scientific writing standards.

Answer: We have fixed this formulation, and the whole sentence has been modified.

Remark 29: L346: "definition" $\rightarrow$ "construction"

Answer: We have reworded all the sentences and this passage has been deleted. We now mention that these modes are decorrelated from the coherent mode.

Remark 30: L346: "essentially non-zero" awkward formulation

Answer: This sentence has been removed to make the presentation of the modes more concise. In the revised version, we simply state that the modes consist of nearly-plane waves deflected by the jet in the upper part of the domain.

Remark 31: L349: "slight deviations to a single-mode structure" This needs to be more clearly reformulated.

Answer: This formulation has been clarified, thanks.

Remark 32: L351-L357: see major comment

Answer: This part of the manuscript has been reworked and shortened following the major comment.

Remark 33: L372-L373: could you use (19) to compute EPOD modes? If yes, has the correspondence been verified?

Answer: We have tested numerically the correspondence between EPOD and resolvent modes in this configuration. The results are in the PhD thesis of Igor Maingonnat entitled "Compréhension et modélisation de mécanismes non-linéaires dans l'océan : les interactions entre ondes internes et écoulement" (Chapter 4; PhD recently defended, the manuscript will be soon available via the French "thèse en ligne" repository). Moreover, it can be noted that a similar correspondence between resolvent analysis and EPOD has been established in Karban *et al.* (2022, 2023; cited in the paper) in the contect of turbulent channel and jet flows. Giving the corresponding details is out of the scope of the paper, but we have added the remark and the reference in the paper.

Remark 34: L381: "time evolution" of. . . ?

Answer: This sentence has been clarified.

Remark 35: L389: "stationary wave" Is "stationary" the most adequate term here, I fail to understand its precise meaning here.

Answer: Indeed, "stationary" is not the most adequate term. We replaced it by "standing wave", which describes the nodes and lobes formed by the superposition of two oppositely propagating plane waves. Thank you for this comment.

Remark 36: L390: "one third" → 30%

Answer: This has been modified.

Remark 37: L394-395: the second part of the sentence needs clarification

Answer: As this sentence was unclear and a matter of detail, and given the reviewer's major concerns about the length of the manuscript, we decided to delete this paragraph.

Remark 38: L405: "an accurate estimate" → "a visually accurate estimate"

Answer: This has been modified by "Qualitatively, the estimation is in good agreement with the reference".

Remark 39: L406: "a well identified structure" → vague statement

Answer: This description has been clarified.

Remark 39: L407-410: the description of the "naive" method needs to be improved. It may be also relevant to push this alternative approach towards the end of the section. Figure 9 is not particularly useful and may be skipped, color map is not adequate in any case.

Answer: Following the Reviewer's suggestion, Figure 9 has been removed. We have moved the description of the BBPOD-based method of the section. For the sake of conciseness, only key aspects are given in the main text, but a more detailed description is now given in Appendix.

Remark 40: L412: here or elsewhere you may want to specify that you could also have estimated wave energy fluxes

Answer: This is indeed a potential outcome of the method. We have added this remark in the conclusion and perspectives. Thank you for this suggestion.

Remark 41: L412: "more than 50% of the energy recovered at approximately each point. . . " this is not an adequate report of performance, you may want to report on an averaged or percentile value instead.

Answer: We have deleted this sentence as it was referring to Figure 9, which has been removed from the text at suggested by the Reviewer.

Remark 42: L416-417: specify that this is as a function of the number of modes

Answer: This detail has been added in the text.

Remark 43: L422 "The figure. . . " is this expected?

Answer: Indeed, since the jet is close to geostrophic balance, the SSH contains enough information to perform an accurate estimation of the velocity. We have added a remark in the text L.415.

Remark 44: L423: "For the wave…" I fail to see how you compute incoherent energy…make sure you explain this somewhere

Answer: We specified the calculation in section 4.3.1.

Remark 45: Figure 10: x label and legend are missing on my computer.

Answer: This has been corrected.

Remark 46: Table A1: I would bring this table in the core of the manuscript and discuss it properly. The sensitivity to wave and jet properties is interesting. The table needs a bit of work (first lign is useless, make it clearer this is for wave part and/or add jet corresponding metrics)

Answer: The table has been improved in accordance to the suggestions.

Remark 47: L443: "Yet, this frequency…" please double check and specify latitude

We have double-checked and we specified the latitude at which this value corresponds.

Remark 48: L463: "of regularisation" you may want to specify "that penalizes higher mode amplitudes"

Answer: The formulation proposed by the Reviewer has been added in the text.

Remark 49: L479 "standing wave" is it the same as the earlier "stationary" wave? If yes you may want to align terminology and make sure it makes sense

Answer: Yes, this term has been used to designate the same thing. As the reviewer suggests, we have kept the term "standing wave" only.

Remark 50: L495-L501: see major comment 2

Answer: This discussion has been moved to the conclusion, and these points have been more detailed as suggested by the Reviewer in his major point 2.

Remark 51: L501: "localisation" reference missing

Answer: References have been added, thank you for pointing at this.

Remark 52: L504: "SPOD" acronym not specified I believe

Answer: We have added the information.

Remark 53: L540: this seems like a different subject from here on, so I would create another appendix section

Answer: We have created another appendix section.