Review : Coupled estimation of incoherent internal tide and turbulent motions via statistical modal decompositions

Igor Maingonnat, Gilles Tissot, Noé Lahaye

February 6, 2025

I have one minor question that I kindly ask the authors to respond to either in the final version of the paper or in a short note sent to the editor.

Question They investigate the effect of partial measurements by taking strips of the data with increasing thickness. Why did the authors choose this form of partial measurement over, for instance, pointwise measurements distributed over the domain? Is there a practical reason behind this? And, how would the predictions change in the case of pointwise measurements with increasing density instead of strips with increasing thickness?

Answer: The authors indeed considered SSH bands to model partial observations instead of a cloud of points. These SSH bands are supposed to represent observations from altimetry data. The authors chose these type of data as it is widely discussed and used in the literature for estimating turbulence and tides, and the launch of the SWOT satellite reinforces interest in this type of observation. Nonetheless, it could indeed have been possible to consider an ensemble of points in order to model observations from tracers, collected by argo data for example. We believe that a possible advantage of a cloud of points is that it provides more homogeneous coverage of the domain, compared with SSH strips where some areas are completely masked. This may attenuate errors in the case of very low domain coverage (e.g. 10% coverage in Fig.10). For realistic applications, knowledge of both types of observation is essential.

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1. Below equations 1: The β term can be retained in the equations only if the domain is unbounded in the y-direction with solutions decaying far off in y. This is essentially achieved with sponge layers in the present work. Given this, it is incorrect to say the domain is periodic in both directions. Periodicity implies that it is some what a finite domain with solutions repeating over and over again, while solutions have to decay in y over large distances.

   Answer: We have deleted the two sentences below equation 1 L99-100 indicating that the domain is periodic to avoid any confusion on this point. Instead, we explain more clearly in section 4.1 how we treat numerically the upper/lower boundary conditions. We agree that using a periodic domain with a beta plane Coriolis parameter is indeed contradictory, since it would be associated with a discontinuous sawtooth profile for the latter. Using a truly bounded domain in the y-direction is, however, absolutely fine. Here, we employ a widely used strategy (especially for wave problems, *e.g.* [1, 2]) that consists of using absorbing sponge layers at both boundaries combined with a periodic domain. Following this approach, two subdomains are defined: the inner subdomain has an unaltered physics (with no additional damping and linear Coriolis parameter) while the outer domain has a modified physics with a nudging toward a desired state. In the sponge layer, the Coriolis parameter does not follow the beta-plane approximation, but instead decreases to match the value at the other endpoints (thus leading to a smoothing of the discontinuity in the original profile, and resolving the initial contradiction between using a beta-plane Coriolis parameter and periodic domain). This allows us to use periodic boundary conditions for the numerical simulations, although the dynamics across the sponge layers (that is, over repetitions of the domain) are not connected with each other because the nudging terms damp any dynamical features that comes from the inner subdomain. Thus, the numerically resolved domain is indeed doubly periodic, thanks to the addition of sponge layers in the y-direction which delimit a physical domain which is bounded in that direction.

2. Above equation 3: Even in this revision the writing is sophisticated with the unnecessary expectation operator. In line 116 the authors say that the expectation operator is equivalent to a time average. Much later, in line 237 the authors say that "the expectation operator is a time average in this study". there is no reason to introduce an ambiguous expectation operator if it is identically a time-averaging operator. The former can confuse a lot of readers and deter them away from the main message of the manuscript.

   Answer: As suggested by the reviewer, we have replaced the expectation notation $\mathbb{E}$ with a time-averaged notation $\langle\cdot\rangle$. The filtering operation has been replaced by $\bar{\cdot}$.

3. Line 161: how do you subtract an equation TO another equation? Line 466: now should appear instead of know.

   There are a lot of typos and English errors throughout the manuscript. I suggest getting an online editor to fix all the mistakes in writing.

Answer: We thank the reviewer for pointing out these errors. We have corrected them. We have tried to correct these language errors as far as possible.

4. Line 300: why is hyperdissipation in this fractional form?

   Answer: Hyperviscosity is in a fractional form following case 7 in Table 1 of Ochoa et al (2011), which is referenced in the manuscript. This form ensures conservation of angular momentum and energy dissipation for a RSW model in the f-plane. It has been scaled according to our scaling.

5. Several figures (1, 7, 10) have no captions, which makes it difficult for a reader to processes the information in these figures when looking at them.

   Answer: The captions are correct on our submitted version.

6. The paragraph bellow line 506 can be better written with references. As mentioned in my previous review, surface waves, acoustic waves, and near-inertial waves are scattered by eddies and topography in the ocean and the method developed in the manuscript could be useful for those problems as well. The authors don't seem to appreciate how far reaching their method is with regards to all these different waves, as seen from their response to the comment I wrote. The authors should read the references I mentioned; see for example figure 3 in Daniuox and Vanneste i 2016. Similar scattering of acoustic and surface waves can be seen in the references I mentioned. Including those references and making connections will lead to a better summary paragraph regarding the broader outreach of the method used in this manuscript. Readers will appreciate getting an idea that the data-driven method developed in this manuscript can be used for other wave scattering problems mentioned in the referenced papers.

   Answer: The paragraph has been modified to include citations recommended by the reviewer as well as certain links with our study. The estimation procedure employed in our study relies on the knowledge of a coherent field, in order to identify the relevant non-linearities through complex demodulation. Such a feature is not always available in the mentioned configurations, rendering impossible the exact application of the estimation strategy of our paper. However, we do believe that the concept of EPOD on complex demodulated fields may find nice applications in these configurations, such as near inertial waves, provided that a separation between the flow and wave fields is possible. We modified the conclusion in this sense, and we thank the reviewer for his enthusiasm regarding the method. Besides, the case of scattering by a topography does not seem to fall within the scope of application, since it is necessary for the scatterer to vary over time in order to compute its statistics.

7. After rewriting the above mentioned paragraph with references, it is better to modify the last line of the paragraph into a new paragraph. Three-dimensional scattering requires a bigger discussion and the last part where the authors say vertical mode decomposition can be used is too short and unclear. Similar to the comment above, I suggest that the authors write a bit more in detail keeping in mind readers who might be unfamiliar with what they are trying to say.

   Answer: We have created a new paragraph, where explain more in detail how our method can be extended to 3D scattering from a base of vertical modes. We have also provided examples of 3D models projected onto these bases for the study of internal tides.

8. I suggest shortening the manuscript title to make a more apt and catchy one.

   Answer: As suggested by the reviewer we have shortened the title ”Coupled estimation of incoherent internal tide and turbulent motions via statistical modal decompositions” in Coupled estimation of internal tides and turbulent motions via statistical modal decompositions". Note that as it has already been rebuilt at the suggestion of another reviewer, we prefer not to modify it further.

References

[1] Brès GA, Jordan P, Jaunet V, et al. Importance of the nozzle-exit boundary-layer state in subsonic turbulent jets. Journal of Fluid Mechanics. 2018;851:83-124. doi:10.1017/jfm.2018.476

[2] Sasaki, K., Tissot, G., Cavalieri, A.V.G. et al. Closed-loop control of a free shear flow: a framework using the parabolized stability equations. Theor. Comput. Fluid Dyn. 32, 765–788 (2018). https://doi.org/10.1007/s00162-018-0477-x