Review of *Coupled estimation of incoherent internal tide and turbulent balanced motions via statistical modal decomposition*

The revised manuscript is much improved than the first submission, making connections with several related references and tighter writing. However, some of the important issues pointed out in the first review remain in the present submission as well. I recommend another revision taking into account the comments below.

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.

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.

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.

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

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.

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

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.

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.

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