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Discussions

Interactive comment on "Propagation and dissipation of internal tides in the Oslofjord" *by* A. Staalstrøm et al.

Anonymous Referee #2

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This paper documents measurements meant to constrain the energy balance in Oslofjord, a well-studied fjord in Norway. The authors estimate the energy in internal motions, infer dissipation of those motions from drops in energy fluxes, and from that also infer the mixing rate in the fjord. I think this is a good start to this paper, but I have a large number of concerns, some of which may be presentation, and also of the conclusions drawn from the data. Hopefully clarifying these will improve the paper.

To start, the paper would benefit from an improved introduction. As noted, Oslofjord is well-studied, so what is not known, and what new will be learned from this study? The introduction is written as a literature review (explicitly so on Page 318, lines 1-9) and does not make a strong case to me for reading the rest of the paper. I also think a clearer discussion of energy sources and sinks in the fjord would have improved this

discussion, and much of the rest of the paper. I think all the info is in there, but it is scattered, and could use being made cohesive and clear. (i.e. energy comes in from the barotropic tide, which loses x% of energy to bottom friction, and y% to internal wave generation. Of the internal waves, a fraction radiates from the fjord, the rest breaks internally.) I think laying this out clearly would have helped one thing that irked me - not mentioning the barotropic loss until section 5.1. Wouldn't it make more sense to start with this number?

I found quite a few of the analyses far too cursory to follow, while others went into too much detail.

Sec 2.2: I could not determine from any of the information where the many CTDs and temperature sensors were deployed.

Sec 3.1: I don't think finding the strongest correlation between two stations at near the mode-1 phase speed at all implies there is no mode-2. Also, you need to be careful what depth you are considering - the mode-1 maximum in displacement is at a null in the mode-2 displacement for most stratifications. Given your stratification I'd expect to the mode-1 crossing is near 20 m, where you did this analysis. (BTW, it would help a lot if you plotted the mode shapes for the first 2 or 3 modes). I wondered why you didn't simply make a modal fit of the velocity perturbations, and estimate the energy density in each to argue the importance of mode-1 over mode-2. The circuitous method you used isn't very satisfying.

Pg 324, line 24 "This indicates that the internal tide propagates as a first-mode progressive wave"; this seems a key argument, since you want to argue later that there are no mode-1 reflections, but you haven't done a good job of making it. A fit to Stigebrandts model is pretty suspect without a lot more details of how you applied that model. Unfortunately none are given, just a sketchy description of the process. If this is important to your argument, you need to provide more detail. How does the forcing vary with time? How important is the stratification assumptions in getting the agreement? As you'll see below, I'm pretty dubious the wave is purely progressive, so I think the extra effort is warranted. You also use this model in Sect 3.2, where more details are revealed, so why not carefully describe the model?

Sec 3.2. I don't understand the merit in comparing local surface elevations to internal elevations. I guess you need to motivate it better, perhaps re-summarizing Stigebrandt's theory. Given that the surface tide is likely almost completely a standing wave, and you are arguing the internal tide is progressive, I don't see that the internal displacements and the surface will have any simple relationship.

Sec 3.3: not sure why you included this section. I guess its nice to see the harmonics decay at S5 relative to S2, but...

Sec 4: My biggest problem with this is the F=c_g*E method of calculating energy fluxes. This is well-known to have huge problems: a) if there is any energy in the inlet not associated with the wave moving at c_g, then this number will be too high. b) if there is any reflections, even of the mode-1 wave, this number will be too high. To fix a) you should bandpass near the tidal frequency and fit mode-1 to your data. To fix b), you should check that Ep = Ek, which you did, except Ep was not equal to Ek, it was larger. And at S5 it was smaller. Thats a pretty classic sign of a partially standing wave, isn't it? Whats more, F calculated from mode-fits to u' and p' gets a far lower number than c_g*E. Its hard to say without you doing frequency and mode-filtering, but my guess would be your wave is not entirely progressive, and that the u'p' estimates of energy flux are closer to being the correct ones. Besides, you say in Sec 5.1 that there is 250 kW of barotropic energy available, so how could the S2 energy flux be anything near 480 kW?

I'm also not clear on the physics of measuring things at S5. Its not in the main channel. Is that not possibly a problem?

Section 5: This suffers from a complete lack of detail into the mixing calculation. The integral of d\rho/dt has to be horribly noisy, and I'm not sure I'd believe it anyway

because you aren't constraining the advective fluxes into the fjord. You are trying to detect changes caused by mixing rates of 10ˆ-3 mˆ2/s. This is a tiny number. I think it'd be wonderful if you could believably integrate Eq 21, but you've shown absolutely no detail on what is a complicated calculation, so I don't have any confidence you have done this correctly.

To summarize, all the right sort of calculations are being attempted in this paper, and could make a nice contribution, but they have all been half completed, and not terribly believable as presented.

Interactive comment on Ocean Sci. Discuss., 9, 315, 2012.