Re-review of: *Seasonal resonance of diurnal coastal trapped waves in the southern Weddell Sea, Antarctica*, by S. Semper and E. Darelius.

This paper describes evidence that observed modulation of diurnal tidal currents along the outer continental shelf and slope of the southern Weddell Sea can be explained by sensitivity of the dispersion curve for coastal trapped waves (CTWs) to changes in ocean stratification and the strength of the Antarctic Slope Current (ASC). The revised paper is well written and tells a valuable story. I have only relatively minor additional comments for the authors and editor to consider before publication.

As an aside, however, the authors seem to gloss over the fact that everything here was known, from old studies by Foldvik, Middleton and others. The key features of the new work are (a) more current meters, (b) a fairly convincing demonstration that we should worry more about stratification and less about the ASC, and (c) improved knowledge of upper-ocean stratification in winter thanks to seals. While I wouldn't require revision of the paper to clarify this, the authors might keep this in mind as they make any final tweaks to the text.

Major comments:

24-25 and elsewhere: the authors never define how WDW differs from MWDW. Some people use 0 deg. C as the threshold. By that definition, a lot of deep water in Figures 7 and 13 is WDW, not MWDW.

43-44, 73-75, 90-95: and maybe elsewhere in Section 1: I recommend removing all the sentences that talk about what this paper will do, and put them together at the end of Section 1. This will also make it clearer how your work improves on earlier work.

Minor comments:

28: Change to "Some climate models"; Hellmer et al. 2012 actually evaluated several climate models and chose just one or two for forcing their high-resolution ocean/ice model.

49-50: 'to exist and decay' => "to exist, and their energy decays'

50: 'While CTWs propagate with shallow waters to the left …'. Depends how you define propagation. If energy is going to the *right*, that's the important propagation direction from the point of view of regional energetics. Maybe be specific about it being the direction of phase propagation.

63-65: sentence beginning "In our study …". "strong tidal currents" don't "break" down"; they weaken (or perhaps disappear). You might do better with "… tidal currents, and they attributed a weakening of currents in winter to …"

71-72: => "Our study is based on a more extensive data set, with observations of current velocities from 29 moorings being used to quantify the strength of diurnal tidal currents and to describe …"

73-75: This is where scattering “we will do” text throughout the Introduction gets messy. As stated here, you are only looking at stratification. But in fact you also look at currents, so the text in line 82 is actually more useful (“change in the oceanographic background {state}”). Move all “roadmap to the paper” text to the end of Introduction.

77-77: You already told us what we could learn from Foldvik et al. (1990 in lines 67-69.

97-99: You don’t really need this. If you want something to end with, summarize for us how this advances what was already known from the earlier studies.

103: “are of 1-2 years” => “are each of 1-2 years”

110, 113: beta is an unfortunate symbol to use for this, as oceanographers frequently associate it with Coriolis (‘beta-plane’).

170: “distances” => “separations”

175-179: We should know at this point what the practical differences between WDW and MWDW are.

306: I don’t think you need quotation marks around ‘generated’

315: I would NOT describe “by 30% to 180%” as being “CONSISTENTLY enhanced” !

320-321: “The observed FRACTIONAL difference”

321: “although on the high end when compared with” is too vague.

324-327: Clearer alternative text? “It is possible that other factors contribute to the observed semi-annual variability. For example, there may be a semi-annual cycle of mixing (and hence stratification) caused by the K1–P1 interference. Alternatively, the observed variability of diurnal KE may be the sum of the effect of an annual cycle in stratification that is phase-shifted relative to the effect of changing background current (L. Padman, personal communication, 9 August 2016).”

340-341: change to ‘… region occur above the pycnocline, similar to …’, then delete “above the pycnocline’ on the next line.

352: It seems strange to call the SSM “the manifestation of the transition from …”; it really IS the transition, right?

360: number format for range of L: just quote in km (300-500 km).

361: I feel like I’ve been told about the Foldvik et al. (1990) result too often!

368-369: “The stronger current observed during austral autumn and winter will however add to the effect of the low winter time stratification and move the RF to higher frequencies.” This is quite important, and you didn’t really explore it in your model tests. That’s okay. But here you make it sound like it is a minor factor, whereas the whole concept of resonance is that things are very sensitive to getting RF close to tidal forcing. Maybe make it sound more possible that the SUM of current and stratification may be critical, even if current by itself is not.

371-372: “While tidal energy cannot be dissipated when ice is drifting freely, high ice concentration provides the friction which is needed to reduce tides (Padman et al., 2002).” This sounds like it is plausible that the large reduction is diurnal currents is plausibly a frictional effect. Really, all Padman et al. (2002) said, in the context of their barotropic model, was that you might want a larger drag coefficient to account for non-free-drift ice (including ice shelves, land-fast ice, and high-concentration drifting ice). Here’s an alternative, although you might want to rephrase it: “When sea ice is in free drift (Padman and Kottmeier, 2000) no tidal energy is dissipated at the ocean-ice interface. However, as ice concentration increases and internal ice stresses prevent the ice from responding to local tidal currents, the stress at the ocean-ice interface may be significant compared with friction at the seabed, thus removing tidal energy and reducing tidal currents (Padman et al., 1992).”

382-383: “rendering a considerable effect of sea ice questionable.” => “inconsistent with the response if damping by ocean-ice interactions was a significant factor.”

406 and 416: structure with ‘e.g.’ is awkward.

452: Change to “Studies with realistic 3D ocean models are needed to …”