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Interactive comment on "Tidal forcing, energetics, and mixing near the Yermak Plateau" *by* I. Fer et al.

Anonymous Referee #1

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R E V I E W

of the manuscript (OS-2014-55) entitled "Tidal forcing, energetics, and mixing near the Yermak Plateau" by I. Fer et al.

The topic of the manuscript of great interest: quantifying sources of heat, which potentially affect the Arctic ice is of great importance. Materials presented in the manuscript are generally sufficient to justify conclusions made by the authors. Therefore I am in support for publishing the manuscript. There are, however, several issues resolving which may help the authors improve the manuscript: - The authors need to provide solid evidence that their estimates of tidal parameters and their derivatives are robust. A statement like that on page 2257 that the authors "expect unbiased estimates with error less than 50%..." is just not enough. There are multiple sources of errors, which can potentially affect the authors' conclusions. These sources should be explored, evalu-

ated and quantified. For example, one of the biggest issues with the analysis is lack of long enough time series. How robust are tidal parameters derived from daily time series? What is impact of nonlinear interactions, which leak tidal energy from tidal frequencies up and down the spectra forming essentially diurnal and semidiurnal bands? How about near-inertial motions contaminating the tidal signal, which are barely mentioned in the text? It is essential that estimates derived from observational records are complemented by error bars. One can use, for example, bootstrep techniques in order to explore potential effects associated with short records and impacts of various external forces. Throughout the text, all conclusions should be placed in the context of uncertainties, which may come from limitations of the authors' analyses. I would also recommend adding a section in "Conclusions" which may be called "Limitations and confidence boundaries of the study" where the authors would describe problems and limitations of their data, methods and conclusions. - Comparison of modeling results and observations is not thorough enough; after reading the manuscript it is difficult to evaluate the model performance based on presented comparisons with observations. This comparison should provide quantitative estimate of the simulation. Also, these two parts of the analysis should be glued together in a much better synergistic way; now they go almost separately. Based on these considerations, I recommend publication of the manuscript after major revision.

Comments: 1. Intro: Writing is rough; this section is not well structured. Some sentences stand out from the text flow. 2. Notations through the text should be unified (for example, now d and H are used for depth, notation for velocities is not consistent). 3. P. 2252. How were rho with overbar and u bold with overbar obtained? On page 2253 there is definition of u_bc, if this is the same quantity, I suggest to define it where if first appears. 4. Eq. 3 looks like a rather crude definition of vertical velocity. Is z (or, maybe, d) negative? How spatial derivative was taken from sparsely spaced observations? Is there any way to quantify errors associated with this estimate (I realize that it maybe tricky)? 5. P. 2253. Flat bottom approximation is used for derivation of vertical modal structure and just mentioned in the text that it may lead to problems. Please quantify (if possible) effect of this approximation on your estimates. 6. P. 2253. Last sentence of the first paragraph. How suitable 15hr long record for harmonic analysis? Please quantify your estimates. 7. P. 2253, very last sentence. I did not understand this explanation. 8. P. 2254, very last sentence. Is this difference due to short length of the available records? Please provide explanation of the difference between the two tidal bands. 9. P. 2257, line 5. How this estimate of 10% error was obtained? 10. P. 2257, lines 14-15. Expectation of error less than 50% is good but not enough; please provide evidence. 11. P. 2257, lines 15-20: How about S2 and O1 constituents for this area? 12. P. 2258, lines 16-18. This may be also associated with errors in both model and observations. 13. P. 2258, very last sentence. What conclusion can we derive from this sentence? 14. P/ 2259, lines 20-27: Does this difference suggest that the errors exceed 50% assumed by the authors? 15. P. 2260, line 6: What is it expected? 16. P. 2260, line 16: “Choice of error” does not sound right. 17. P. 2260, line 13, Since this flat-bottom approximation is questionable, I would suggest to provide at least a hint to what degree we can trust it in this particular circumstances. 18. P. 2260, lines 25-29: I do not follow this technique and I am not sure that I understood the method used to plot figure 4. Please elaborate. 19. P. 2262, line 21. Are S2 and O1 comparable to M2 and K1 or just between themselves? Also, I would add observational estimates from Table 2 to Table 4 for direct comparison. Quantitative comparison is needed here. 20. P. 2263, lines 3-4. This sentence reads like the authors question the importance of their conclusions derived from observations. It is honest but following this logic I have to ask them how valid are their observation-based conclusions presented in Abstract and Conclusions. 21. P. 2266, lines 9-10. Is there any way to quantify the effect of this contamination? 22. Fig 2: Why do panel a use 8 constituents and panel b use only 4? For panel c, please show position of instruments. Panel a: What is impact of the motion of instruments on this record? Difference between model and observations in panel b should be evaluated. 23. Figure 3. I guess u and v are baroclinic components, right? Please say so explicitly. What is the level of errors in these profiles? 24. Fig. 8: What does negative dissipation mean? 25. Fig 9 caption: semidiurnal ENERGY flux?

Interactive comment on Ocean Sci. Discuss., 11, 2245, 2014.