

Interactive comment on “Flow and mixing around a glacier tongue” by C. L. Stevens et al.

Anonymous Referee #3

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General comments

The paper presents a set of observations done near the Erebus Glacier Tongue, in McMurdo sound. The paper attempts to interpret vertical microstructure observations in terms of bluff body flow. The authors insistence to interpret the observations in terms of the theory of tidal flow around a bluff body detracts from the authors analysis of these data to characterise mixing regimes in polar oceans. The theory used to describe the flow is taken from flow around headlands and islands which, the authors acknowledge, is incomplete when applied to a floating ice tongue. The authors focus on one event during this time and the associated “ringing” that occurs afterwards. I am not convinced that the oscillations are not due to buoyancy oscillations or that the “pulse” is even caused by tidal currents. The manuscript would benefit by utilising other observations, such as meteorological observations and satellite imagery, that may aid

the analysis. Measurements of turbulence parameters on the Antarctic coastal regions represent a valuable contribution to the scientific community and, thus, I hope that a version of this paper is ultimately accepted. I think that the paper requires substantial rewriting, to improve both the style of the manuscript and analysis of these data, before publication in Ocean Science.

Specific comments

I think the paper should first give a thorough description of the data set and variability as it relates to mixing, which is interesting in its own right. The paper can then proceed with examining potential mechanisms that can drive the observed variability. As already discussed, I am not convinced that the the forcing of the initial "pulse" is related to bluff body generated vortices. Some specific questions/comments I had are:

1. What are the surface ice conditions in the region? How much sea ice, open water and fast ice is present nearby? A visible satellite image near the time of observations might help.
2. Can the enhanced mixing be induced by strong winds? Are there meteorological observations available?
3. How can the bathymetry influence the currents and mixing?
4. Why should the flow be guided around the glacier tongue, rather than underneath? Can the relationship between flow and stability of the water column be quantified in terms of say, the Froude number?
5. Your analysis is focused on a region of high flow and high vertical mixing. I would like to see more details of the "background" quantities, when the flow is slow.

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6. The text is difficult to read and a bit wordy. The difficulty is also partly due to the use of non-standard technical language, For example, such as saying “cryotopography” rather than just “ice tongue” or “glacier”. There are other instances of this throughout the text and the manuscript would benefit from a good rewrite to make the text clearer and easier to read.
7. What is the point of using observations of sea surface elevation from Scott Base?
8. The bluff body flow you describe assumes vertical coherence – its not clear in your argument why horizontal flow divergence should lead to the generation of vertical turbulence. In other words, why constrain the argument to only the horizontal bluff body flow scenario? Flow over seamounts or bottom topography can also generate internal lee waves and turbulence with periods at the buoyancy frequency.

Technical corrections

A non-exhaustive list of further comments/questions are detailed below:

1. Page 1441, lines 2-5: garbled sentence.
2. line 15: Jacobs reference should be at end of sentence.
3. line 20: frazil formation requires ice both turbulence and nucleation sites. It seem your observations show there is plenty of both. Why not just say something like: “The supercooling process leads to the formation of frazil.”.
4. line 26: what is “cyromechanics”?

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5. line 27: remove "and papers therein" and describe and reference this work in more detail.
6. Page 1442, lines 1:4: Can these subheadings be referred to as explicitly being contained in the discussion. It would also help if the same numbering format was used.
7. lines 7-8: move these to "Instrumentation". The paragraph should start with "The EGT divides the surface ..."
8. lines 9-12: confusing: You say "at the time of sampling" but then use earlier references.
9. lines 14-17: remove this sentence as I don't think its important.
10. lines 18-21: this paragraph does not belong here.
11. line 19: the reference to Fig 1c suggests we get to see the current data.
12. line 24: see last comment.
13. Page 1443, line 2: "... held 2.5m beneath the fast ice, with ..."
14. lines 4-6: this suggests that there was plenty of suspend material in the water column. Why not just say this instead?
15. Line 8: I am not sure of the point of using tidal elevation data from Scott Base when its clear the observed currents show a tidal signal.
16. lines 10-13: garbled sentence.
17. lines 23-25: I would like to see more on the times of slow flow too.

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18. lines 28-29: I presume you use potential density. There are other instances of this throughout the manuscript. Please also refer to either "potential temperature" or "in-situ temperature", not just "temperature".
19. lines 28- page 1444, line 2: more explanation is needed here. Why are you calculating a lengthscale? Why use the Thorpe lengthscale instead of, say, the Ozmidov lengthscale?
20. Page 1444, line 2: Roget et al 2006 is an inappropriate reference here. Why not use Thorpe (1977)?
21. lines 4-8: I am still not sure why the tidal elevations are of relevance here. Your "flow magnitudes" is not purely representative of the tidal currents and so cannot be compared against those estimates that are only tidal currents. You need to estimate the background currents here (see my comments related to lines 8-10).
22. lines 8-10: have you attempted a harmonic analysis of the data? The residuals would help give you some quantitative idea of the direction and magnitude of the mean flow. Progressive vector diagrams are another method of gauging the relative effects of tidal v background currents.
23. line 17: to what part of Fig 2 do you refer?
24. line 22: please give the start and end times of the pulse.
25. line 22-24: I would prefer to see "decaying oscillations" used instead of "ringing". Please report the period. How about rephrasing this to read: "Oscillations with a 70 minute period decayed over about x hours to background levels."
26. Page 1445, line 3: Fig.3c ?

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27. lines 6-24: I think this paragraph needs rewording to consider all sources of suspended material. Suspended frazil, for example, could also indicate the presence of ice shelf water.
28. Page 1446, line 4: "...frequency squared, N^2 , is calculated ...". Do you calculate the frequency or angular frequency squared? Your figures report the buoyancy frequency with units of the square of the frequency squared (this is the conventional use) but in the text you use the angular frequency squared, which is unconventional. If you have assumed units of $\text{rad}^2 \text{s}^{-2}$ which are actually s^{-2} then your oscillation period, Ri_{gr} and subsequent analysis can potentially be be out by 2π . This needs to be clarified.
29. line 7: Fig.3g ?
30. line 20+ : I would like to see time v depth plots of difference of in-situ temperature from the freezing temperature (calculated from observed salinity and pressure) shown.
31. line 22: I would not expect to see high levels of supercooling anyway, as most of the supercooling would be removed by frazil ice production. However, in-situ temperatures below the surface freezing point can be indicative of glacial ice/ocean interaction.
32. line 21: "... the in-situ temperature ..." ?
33. Page 1449, lines 11-21: I find this paragraph confusing.
34. Page 1450, lines 5-7: $Ri_{gr} = 10^{-5}/3 \times 10^{-8} = 333.\overline{33}$, not 100 ?
35. line 9: where is equation 1?
36. lines 11-16: please explicitly state the modifications you make to the P&P 1981 formulations.

37. Page 1451, lines 1-2. I get ≈ 7.4 minutes, instead of 8 minutes, from $2\pi/(2 \times 10^{-4})^{0.5}$?
38. Section 4.3: the inclusion of local supercooling has not, to my thinking, been properly discussed in the “Observations”. For example, the “Observations” section would benefit from including sections of in-situ temperature and salinity as has been done for the quantities in figure 3. A plot of showing the difference between the local freezing point (calculated using the observed pressure and salinity) and the in-situ temperature that is discussed in light of the other observations would add to the discussion presented here.
39. line 21: I prefer to use the term “frazil” instead of “platelet”. If you mean something different when discussing platelets then please say so in the introduction.
40. line 23: I think you have confused dimensions here when discussing “cylinders of platelets”. This entire paragraph is confusing. Its not clear what you are trying to say.
41. page 1452, section 4.4. I think this section needs to be renamed “Conclusions” and a clear and concise summary of the observations and the analysis, as I and other reviewers have suggested, should be presented.
42. Figure 1: I suggest combining (a) and (b) into one sub figure, and combing (c) and (d) into another sub figure. Please mark the location of the observations on (e). Your acknowledgment of your map source should be moved to the acknowledgments.
43. Figure 2 caption "(a) 5 day record of sea surface elevation at Scott base (b) ...". I think this figure is unnecessary and can be removed.
44. Figure 3 needs to be larger. I suggest presenting the observations, CTD, velocities as one figure and the derived quantities as another. The blob of dense water

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- in (f) at 324.2 days between 40–80 m is worth discussing in-text– how did it get there? The heavier water overlying lighter water is what produces the large L_T values observed at this time. Is this a problem with your sampling regime as has been pointed out by reviewer 2?
45. Figure 4. as mentioned previously, why not show the time series of in-situ temperature, salinity as you have done in figure 3? I also think that representative profiles of slow flow should also be included for completeness.
 46. Figure 5. the large lengthscales observed at time = 324.2 needs more discussion. It does not seem to fit with your theory very well.
 47. Figure 8. I think this should be shown together with L_T as a derived quantity.
 48. Figure 11. this figure is confusing. I suggest that it can be removed.

Interactive comment on Ocean Sci. Discuss., 7, 1439, 2010.

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