

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

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Author’s Response to Anonymous Referee 3

*We thank the Reviewer for the obvious time and interest they have taken in our work and note they hoped the work would be published in Ocean Science in some form. Furthermore they emphasised the value of turbulence observations for Antarctic as well as general ocean science. They did identify some shortcomings in the manuscript and were especially helpful in their suggestions for revision of the figures. Here we address all their comments. This response paraphrases their substantive comments in normal font and our response follows in italics. A separate document included as a supplementary document in the response to Reviewer 1 highlights the changes in the*

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*manuscript using track-changes. We note that we have produced several new figures in direct response to this Reviewer’s comments.*

General comments 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. *As with Dr. Padman’s comments it is clear that we shouldn’t have presented this as an a priori understanding but rather, come to it in the Discussion. To this end we have deleted or relocated much of this material. New material has been added to subsection 4.1 on the nature of the flow.*

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. *Agreed, for starters a glacier tongue is floating and generates buoyancy flux boundary conditions. However, we believe (as does Reviewer 1) we have presented the first such results and so this must be regarded as an attempt to remedy this gap in our knowledge.*

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. *The jury seems to be split here – we and Reviewer One seem happy with it being tidally-forced (Reviewer One more so that us even), Dr Padman counsels at least questioning this and Reviewer Three appears more strongly to suggest other processes at work. It is not at all clear to us where satellite imagery will get us with regard to determination of the drivers of such flows. We have however included two satellite images as part of other queries. Furthermore, as in the response to Dr. Padman on this point, the ice edge was around 50 km to the north so the manifestation of meteorological forcing at tidal frequencies will likely not be direct or unambiguous.*

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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. *Our modifications in light of Dr Padman's request would seem to satisfy this criticism in that we now spend much more time highlighting aspects of the measurements themselves, leaving the speculative interpretation until the Discussion.*

As already discussed, I am not convinced that the forcing of the initial "pulse" is related to bluff body generated vortices. *This is an issue identified by all the Reviewers and so we have followed their guidance and removed direct reference to this driving mechanism from everywhere except a section in the Discussion where we speculate on explanations for the flow.*

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. *In our modifications to figure 1 (now figures 1-3) we have included two satellite images – one from very near the time of sampling. Furthermore we have included more text on sea ice conditions in Section 3.1.*

2. Can the enhanced mixing be induced by strong winds? Are there meteorological observations available? *There are meteorological records available but with open water being some 20 km to the north and conditions largely unchanging during the short sampling period we did not think it warranted inclusion and this is stated in Section 3.1.*

3. How can the bathymetry influence the currents and mixing? *It was unclear if this was a rhetorical question? The flow can not pass through the solid boundary provided by the seabed and so is guided by bathymetry. Velocity shear results from both this steering and due to the boundary-layers generated by the no-slip condition. Velocity shear then induces instability 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

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in terms of say, the Froude number? *The Reviewer almost answers the question themselves – buoyancy will retard vertical flows up to a point. We noted this in the manuscript. The Froude number is the root of the inverted bulk Richardson number and so whilst it finds application in hydraulic stability theory here the Reviewer is asking in regard to slow and stability – exactly the Richardson number usage so we have left this material as it was.*

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. *We focus on the more energetic events because they have the most influence on averaged transport rates and energy conversion.*

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". *Agreed and we have made changes where possible – however we also note that we are working on a new topic that doesn't simply fit the terminology in common use. A key example here is the oceanographic flow is a response to ice in various forms (glacier, shelf, sea ice – both first year and multi year, and possibly involves even suspended frazil/platelet ice) hence the use of the term "cryo".*

7. What is the point of using observations of sea surface elevation from Scott Base? *Primarily this gives an indication of the spring-neap cycle at the time as well as an indication of the phase of the tide at least in terms of elevation. This explanation is now included in the text.*

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. *This is an issue*

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identified by all the Reviewers and so we have followed their guidance and removed direct reference to this driving mechanism from everywhere except a section in the Discussion where we speculate on explanations for the flow and now include the role of lee waves.

Technical corrections

1. Page 1441, lines 2-5: garbled sentence. *Also identified by Dr. Padman and changed.*
2. line 15: Jacobs reference should be at end of sentence. *Ok changed.*
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.". *Thanks yes this has now been simplified.*
4. line 26: what is "cryomechanics"? *the mechanics of ice! It has been used in the literature. It is used here rather than "glacier mechanics" because the influence of frazil/platelets, and sea ice (first year and multiyear) is important too.*
5. line 27: remove "and papers therein" and describe and reference this work in more detail. *This paper and the topics referenced are all about the glacier mechanics and do little to advance the present paper. We have kept the text as it is in order to maintain focus at the same time as making it clear there has been a good deal of work on the glacier itself.*
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. *Good idea and amended.*
7. lines 7-8: move these to "Instrumentation". The paragraph should start with "The EGT divides the surface ..." *We have moved the text to the start of the overall section.*

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8. lines 9-12: confusing: You say "at the time of sampling" but then use earlier references. *OK – we have split this sentence to clarify the point.*
9. lines 14-17: remove this sentence as I don't think its important. *We understand what the Reviewer is getting at but this is a key background point readers always ask about and, in keeping with Dr Padman's request for better background information, we have retained the sentence.*
10. lines 18-21: this paragraph does not belong here. *OK we have moved this to the "background mean flow" section requested by Dr. Padman – this forms a new section 3.1.*
11. line 19: the reference to Fig 1c suggests we get to see the current data. *Corrected.*
12. line 24: see last comment. *Again corrected.*
13. Page 1443, line 2: "... held 2.5m beneath the fast ice, with ..." *No, not quite! The sea ice has a finite thickness so we wanted to make it clear we had the instrument just beneath the base of the ice and what that depth was. We have amended slightly to clarify this.*
14. lines 4-6: this suggests that there was plenty of suspend material in the water column. Why not just say this instead? *Because we wish to make it clear that in our experience this is not always the case in these waters as per the references included.*
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. *Scott Base to the south of the sampling location is the nearest site with tidal data so it is excellent background data to show as requested by all the Reviewers.*
16. lines 10-13: garbled sentence. *This has been modified as part of the response to Dr. Padman's request to expand this aspect.*
17. lines 23-25: I would like to see more on the times of slow flow too. *When looking at*

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*average transport processes we believe it is the larger events that dominate. However, our amended Figure 4 now has these data.*

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". *We have clarified this in all the figures now.*

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? *We have removed the material on the Thorpe scale in order to maintain focus.*

20. Page 1444, line 2: Roget et al 2006 is an inappropriate reference here. Why not use Thorpe (1977)? *We have removed the material on the Thorpe scale in order to maintain focus.*

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). *We have clarified likely background residual currents. We believe, as does Reviewer 1, that flow variation over the timescales we observed is only possible due to tidal forcing albeit in some complex fashion. With the revised structure we believe we address this issue more clearly. However, we strongly maintain that the tides are a vital part of the picture.*

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. *We believe such an analysis of only four days data would be open to criticism. We have instead included progressive vector diagram (Fig 5).*

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23. line 17: to what part of Fig 2 do you refer? *We have reworded this point – we wanted to point out that the 24 hours chosen for the microstructure contained the fastest flows of the ADCP data.*

24. line 22: please give the start and end times of the pulse. *Done*

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." *Yes agreed and this is in keeping with other Reviewers comments.*

26. Page 1445, line 3: Fig.3c ? *yes corrected thanks*

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. *Indeed and this was discussed in Stevens et al. 2006. We have included a slight amendment to this end here.*

28. Page 1446, line 4: "...frequency squared,  $N^2$ , is calculated ...". Do you calculate the frequency or angular frequency squared? *As noted in response to Dr. Padman we've clarified the units.*

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. *We have produced such plots but they were not particularly informative as it is clear that our 2009 observations were well above freezing. We stated this in the original manuscript. An important point is that the dynamics will still be similar in the presence of supercooled water so the present data are relevant to that issue. The figure the Reviewer wishes to see is now a panel in the scalar data plot Fig. 7.*

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

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below the surface freezing point can be indicative of glacial ice/ocean interaction. *Yes correct but also there is a hypothesis that local levels of nucleating materials are so low that it is possible for supercooling to not be immediately relieved by ice production.*

32. line 21: "... the in-situ temperature ..." ? *amended*

33. Page 1449, lines 11-21: I find this paragraph confusing. *This paragraph has been partially re-written in order to improve clarity.*

34. Page 1450, lines 5-7:  $R_{gr} = 10^5 = 3 \cdot 10^8 = 333:33$ , not 100 ? *Yes, we were seeking to maintain the spirit of the scaling however we have corrected the approximation.*

35. line 9: where is equation 1? *Apologies this was lost in the typesetting.*

36. lines 11-16: please explicitly state the modifications you make to the PP 1981 formulations. *We didn't make them – Fer (2006) did and we have clarified the changes.*

37. Page 1451, lines 1-2. I get 7.4 minutes, instead of 8 minutes ? *Amended.*

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. *We view the Observations section as a "Results" section and so we try not to discuss anything there instead we simply present results. Having said that we now have new a new figure panel showing degree of supercooling as requested – Fig. 7b.*

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. *Yes this is a point of contention and is somewhat peculiar to Antarctic shelf-influenced waters – but seeing as "Antarctic waters" is a rather large volume it's important to take the distinction*

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*seriously. Strictly we are talking about very large frazil. However, these crystals have come to be termed platelets once they get bigger than a few cm. This is now clarified 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. *This has been revised as it's an important point only really fully appreciated through exposure to ice shelf field conditions.*

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. *Agreed – although we have maintained "Generalization" in there as we believe some readers might say "so what? It's just a local anomaly".*

42. Figure 1: I suggest combining (a) and (b) into one sub figure, and combining (c) and (d) into another sub figure. Please mark the location of the observations on (e). *We have combined the present Reviewer's requests with the suggestions of Dr. Padman and have now split this figure into three.*

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. *We are at a loss to understand the Reviewer's ambivalence towards a key piece of background data and we will maintain the figure as none of the other Reviewer's queried it in so pointed a way.*

44. Figure 3 needs to be larger. I suggest presenting the observations, CTD, velocities as one figure and the derived quantities as another. *Yes Dr. Padman highlights this issue as well. We have taken great effort to improve the readability of the figures by generating (i) a scalar results figure, (ii) an ADCP results figure and (iii) a calculated results figure.*

The blob of dense water in (f) at 324.2 days between 40-80 m is worth discussing in-

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text– how did it get there? The heavier water overlying lighter water is what produces the large LT values observed at this time. Is this a problem with your sampling regime as has been pointed out by reviewer 2? *Yes of course the heavier water at mid depth is the source of the high LT. We have now removed the material on the Thorpe Scale.*

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. *We now include this new figure as Figure 7 and this also includes the slow flow information.*

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. *We have now removed the material on the Thorpe Scale.*

47. Figure 8. I think this should be shown together with LT as a derived quantity. *We have now removed the material on the Thorpe Scale.*

48. Figure 11. this figure is confusing. I suggest that it can be removed. *Reviewer 1 was ambivalent about its worth and Dr. Padman thought it should at the very least be moved and so it has been removed.*

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