

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

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Author’s Response to Dr. L. Padman’s Review

We are grateful for the obvious time, interest and care Dr. Padman has contributed towards improving our manuscript. We note he is positive about the publication of such data stating “these very interesting data deserve to be seen” and that Ocean Science is an appropriate forum. He does however identify four areas for improvement: (a) relevance and clarity of graphics; (b) better description of “background” setting; (c) better description of data processing issues; and (d) isolating speculation from the more robust data presentation and conclusions.

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Whilst we respond specifically to his comments below, in general terms we have (a) re-developed the figures almost entirely, (b) re-worked the background setting material to highlight both the background sea ice conditions and the background oceanography, (c) expanded the data processing section and finally (d) we either eliminated or clarified a number of speculative points.

Here we address all his comments by first paraphrasing their substantive comments in normal font and our response follows in italics. A separate document highlights (as part of Rev 1 reply) the changes in the manuscript using track-changes.

General:

The authors are heavily invested in the idea of wake shedding off headlands; however, the text is very confused about whether the “ringing” they associate with wake might just be near-N waves that you might get from a released tide at an abrupt topographic boundary. This requires a much better description of “background” conditions including variability of stratification. *We have removed all reference to ringing and instead used the phrase “post-pulse oscillation” as a more general reference and collated related material into the Discussion where it is related to the improved description of background conditions.*

Technical/specific comments:

1440/26-1441/2: Mixing processes *are* part of the flow around a bluff body. *Amended to say Flow at the field site was likely a combination of tidal and Sound circulation flows that are affected by the islands, local bathymetry and the glacier tongue itself.*

1440/2-5: Of course the “resulting flows” *are* associated with oceanographic flows! This sentence is fairly waffly, and doesn’t progress your story. *Fair enough - half the sentence has been deleted.*

Fig. 1: This figure, especially the “insets”, is way too small. The setting is critical to your interpretation, so please expand this. *As per this and Reviewer 3’s comments we*

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have now split this into three figures.

1442/9-12: Description difficult to read: break into 2 sentences. *This has been done.*

General: Can you replace "around" with " " wherever possible? *We have made this change in numerous places.*

1442/20:21: Please add the site of this tidal flow measurement to Fig. 1. It may be there, but the figure is so small, I can't see it. *The tides were measured at Scott Base as described in the text and shown on the map. This has been emphasised in the text.*

1442/25-26: Reference to Fig. 1c seems wrong. It sounds like I'll see a time series, but instead I see a map. And the map doesn't show an ADCP: it shows the profiling site "MSC". *This has been corrected – the ADCP data were recorded at the MSC but we were unclear about this in the original text.*

1443/12: The "key property" of the profiler is not the derived quantity, but is the instrument suite that lets you calculate it. There are all sorts of tricks required to get dissipation from microstructure shear, and you need to spend a paragraph showing an awareness of how this is done. *We agree and are well aware of the pit-falls of such analysis and have published such a description previously (Stevens et al. 2009) so we have referenced this and included some additional material relevant to this particular deployment.*

Sections 2.2 and 2.3: You would be better off breaking 2.2 into different sections for each device, then including in that section all the relevant processing steps required to get what you want. Some of the information in Section 3 would be moved forward into these three subsections of 2.2. I would treat the profiler CTD separately from the shear microstructure, then have a third section for ADCP. *OK - we have made these substantial structural changes to this section and agree it has helped with the flow of the text.*

1443/28-29: You don't tell us what profile you are sorting, although it must be potential

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density from the SBE sensors. However, that being true, the problem is that getting potential density out of unpumped CTD sensors in the salinity-dominated weak stratification of polar seas is a really tricky business. . . *We have removed the material on Thorpe re-ordering as being beyond both the scope of the revised manuscript and the reliability of the data.*

1444/4-14: Start this section with a "background mean flow" description. As it stands, it seems plausible that the reason the tidal current seems stronger in one direction than the other is because it is a tide being added to a mean flow. Also, have the authors considered whether there is a "sea-breeze" contribution to diurnality? We have seen this elsewhere in coastal met station wind records. *There are a number of drivers for tidal rectification. We actually place flow separation around Tent Island above the suggested mechanisms because in the "mean flow" situation this would be a flow towards a boundary and in the "sea breeze" situation there would need to be quite a strong connection with the exposed ocean some 20 km to the north for this to happen. The text includes this material now.*

Fig. 2: The stick-plot view of currents is very difficult to understand. I think you should replace this with two orthogonal component plots, resolved into "cross-ice-front" and "along-ice-front" directions. *We have made these changes to what is now Figure 4 and agree that it substantially improves quantitative interpretation. Choosing the coordinate direction is a challenge as the glacier is highly lobed so we have gone with north-south east-west.*

Fig. 3: This is your primary figure. it should be given a complete page. I would like to see the ADCP currents shown as cross- and along-ice front coordinates rather than speed and direction, as I think this will highlight the structure of the waves better, and the relationship to ice front orientation. *We have made these changes as well as moving one panel to a new figure on scalar properties.*

1444/18-21: This is waffly. You have not yet demonstrated why we should think of this

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as a wake recirculation, and that probably requires a figure that is specifically designed to make that point. Also, if the relationship to tidal state is so important, then some representation of the background tide should be on this figure. If Fig. 3 is given a full page, this will fit. If you use a separate figure to convince us of wakes, then put the tide there. *We have revised this text and moved the wake processes description into the Discussion. The three main plots now have tidal envelopes added.*

1444/22: "The pulse lasted 4 h". I think this argues for a brief description of time scales (not just a citation to Albrecht et al., 2006). Typical buoyancy period is ..., based on Figure 4 profile of N^2 : is this relevant? yet, later, we find that "background" N^2 can be much higher, so that the 70-min periodicity of the ringing might just be near-buoyancy-frequency waves rather than "wake". *We have improved the description of background conditions and removed all reference to ringing and instead used the phrase "post-pulse oscillation" as a more general reference and collated related material into the Discussion. The Reviewer is correct as the frequency bands do overlap – we have modified the text accordingly.*

1444/27: "but still a moderate proportion". This is way too vague! *We have simplified this comparison.*

1445/6-24: The backscatter *is* conceivably interesting. (a) Perhaps you need a blowup of backscatter through the pulse and including the following "ringing". (b) The discussion raises the question as to whether you expect much diel migration under >2 m of (presumably snow-covered) sea ice. How far to open water? What is the overall extent of "land fast" sea ice around the EGT, compared with open water and mobile pack? *We have now included a subsection on the state of the sea ice (3.1) and yes there is clearly much diel migration under > 2 m of sea ice and is the subject of ongoing analysis (e.g. Leonard et al 2010).. We chose not to generate a blow up of the magnitude panel as we expanded the scale of this figure by having fewer panels. Furthermore, if we can get the new Figures (was fig 3) printed sufficiently this should enable easy interpretation.*

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1445/25: (a) Should not use "S" for shear when S is salinity. (b) The equation for S is for "The magnitude of the shear", not shear itself, which is a vector quantity. (c) As things are written, it is not totally obvious that you are referring here to ADCP shear rather than microstructure shear used for epsilon. (d) better to put these descriptions of calculations and processing in the right section of 2.2. *The Reviewer is correct on all points and we have made these clarifying changes.*

1446/5: This should be in terms of *potential* density, yes? Especially important in weak stratification like this. *We have tightened up all our plot descriptions with regard to scalar quantities.*

1446/20-25: You need to be pedantic about whether you are referring to "surface" or "in situ" freezing point. The temperature looks to be about 0.01 C above surface freezing, and there is no line to show us what the in situ value is. (We get that on Fig. 6, but only to 40 m.). *As per above we have tightened up all our plot descriptions with regard to scalar quantities.*

Fig. 4: Is T or potential temperature the right thing to plot here? General: can you use $s-2$ instead of $(rads - 1)^2$ for N^2 and S^2 units? Definitely don't need "rad". *We were trying to avoid confusion here not generate more – we have amended the figures.*

1446/22: Supercooling when the water is above the surface freezing point should just not exist, not be "not apparent or indeed likely" Fig. 6: (a) Presently introduced before Fig. 5. (b) Should you plot T, or potential temp? (c) Nice figure, probably deserves more text rather than just an offhand description of how the new measurements might be anomalous. *OK we have amended to potential temperature and expanded the text to better contextualize the results as well as including a whole new figure (Fig. 7).*

1447/5: Reference to Fig. 5 (although, make sure figure order is right.) *Done as part of new figures.*

1447/3-9: This is a very confusing description of the Thorpe reordering results. I don't

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think this will survive once you investigate all the problems of getting accurate potential density from your unpumped SBE T and C cells. However, if it does, explain the processing, and Fig. 5, much more clearly. *We have removed the material on the Thorpe scale in order to maintain focus.*

1447/18-23: This is not at all convincing. Earlier you can drop the "One naive expectation" and replace it with the fact that the ice base is not visible in hydrography (and velocity?) *Done* You can make this case more clearly by putting marks on Figs. 3 and 4 indicating approximate local ice base. *Done*

1447/24-1448/4: You need to do a better job of explaining the time scales. *We have now explicitly laid out the various timescales.*

Fig. 4 gives the impression that the local buoyancy period is about 3-4 h. However, other text suggests much higher values outside the "pulse", so that what you call "wake" might be "near-N internal waves" instead, perhaps released as a highly sheared diurnal tide changes direction relative to the ice front. *The Reviewer is correct as the frequency bands do overlap – we have modified the text accordingly.*

1448/15: First mention of "fast" ice. This might be confusing to general readers, and also raises the question of how much there is, and what the broader environment looks like. *We now introduce fast ice and the general cryography of the region in a coherent fashion as well as the expanded Fig. 1-3 (section 3.1).*

Fig. 7: Since you are making a case, based on this figure, of polarization near the 20 cpd "ringing" frequency, perhaps you need a second view of the spectrum that is done in "area-preserving" format; e.g., linear-linear, or linear (freq*PSD) vs log(freq). *We have reworked the spectral analysis now using the vertical velocities and used an area-preserving method – new Fig. 10.*

1448/19: (a) You know f exactly, so rounding it off to $2e-4$ doesn't seem right. (b) You need to tell us that L is a "vertical" length scale (assuming it is); then why you

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choose the range of 50-500 m for it. *We have clarified this text although for scaling we don't really know the shedding frequency particularly well and have retained it's one significant digit.*

1448/22-24: Revise this sentence. I think, if this is where you plan to discuss a conceptual model, you should already have introduced Fig. 11. However, in that case, you'd find it easier to discuss if your earlier plots were in terms of cross- and along-ice-front, rather than speed and direction. *We have removed the original Fig. 11 and so consequently re-worked this text.*

1449/17-21: (a) The Muench et al (2009) study is "way" far north of yours, and deals with dense plumes rather than upper-ocean flow near ice shelves and tongues. (b) These sentences are extremely waffly! Try to clean up your thoughts here. *The text has either been deleted or moved to the Conclusions for future work section 4.4.*

1449/26: (a) I don't think you mean "the flow here" but rather "the Richardson number here". (b) Then, that raises the issue of what the noise levels are, and how much the equivalent noise in Ri will be. When stratification is weak, noise in N^2 is significant, and shears are low so that noise in ADCP current estimates can dominate real shear. Until you check the contribution of noise to a calculation of Ri , I don't think you should show Ri . *Agreed and we have now passed a threshold test through our Ri calculation (requiring $N^2 > 10^{-8} s^{-2}$) which resulted in the rejection of 8% of data. The text now states this.*

1450/4: "and this was borne out". Well check through this. What you mean by "this" is not totally clear, and it is really "consistent with", not "borne out". *Amended.*

1450/5: With this large value of N^2 , buoyancy period is 0.6 h, and so the ringing waves might just be near-N, not wake. *As with this point above the Reviewer is correct as the frequency bands do overlap – we have modified the text accordingly.*

1450/9: I don't see equation numbers on my copy. *Corrected with apologies.*

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1450/11-12: I disagree that Arctic fjord equations for viscosity and diffusivity are "likely" relevant here. The background values in Fer (2006) are extremely large, much greater than, say, mid-latitude canonical values of $1e^{-5}$. *Correct but we are again at high latitudes and more importantly near topography – this we believe is key to making these values relevant here.*

1450/15-24: This is very ugly. Start by telling us what you do use (Shih et al., 2005) and why. Only then, relate that to Fer (2006). Or, show the difference between the functions and state why you use the one you do. *Agreed – we have re-structured this paragraph substantially.*

Fig. 10: Not cited in text, as far as I saw. probably should be at 1451/2. *Amended.*

1451/1-3: So, now you tell us that a time scale is 8 minutes, and you quote a value of $N\epsilon^2$ that has not been seen or talked about before. Where do these values come from, and why are they relevant now? *We apologize for the lack of clarity. We have now amended the text and timescale to be consistent.*

1452/Generalization: I am concerned that, once you are careful with explaining time scales and the true range of N, that you will find that other mechanisms make more sense than wake effects. At the moment your interpretation is tied almost entirely to wake shedding at headlands (the Edwards et al., 2004 paper). it seems quite plausible, but maybe doesn't deserve quite so much bias here. *To some degree these scenarios merge in that one of the loss terms explored in the Edwards work was the loss to baroclinic energy so that the two interpretations are not competing arguments. We have clarified this text.*

On the same topic, I didn't understand the focus on aspect ratios and size of various tongues. Perhaps a more useful discussion would be in terms of expected observations relative to "offshore" distance. There must be laboratory studies of eddies shed off headlands that could help here. *This material was included in an effort to generalize the results from this particular ice tongue. The salient study here is probably the*

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numerical work of Signell and Geyer (1991) which is now referenced in the revised manuscript. This is where the emphasis on headland scales comes from as it is important to understand just how thin the headland is in terms of flow separation and how wide it is relative to tidal excursions. We have included an overarching statement to this end.

Misc. typo/grammar:

1440/18: "that"=>"than" – *fixed.*

1442/2: change "largely" to "primarily" *changed.*

1445/3: Call-out should be to Fig. 3c, not 3b. *corrected with thanks.*

1449/14: "Present" => "Recent" -*changed.*

1450/21: Give a citation to "Osborn", and spell his name correctly. *Both changes made.*

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