

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

L. Padman (Referee)

padman@esr.org

Received and published: 29 September 2010

General:

This paper describes microstructure turbulence, CTD and ADCP velocity data collected near the floating Erebus ice tongue (EGT) in McMurdo Sound.

Any new turbulence data, especially at odd locations at high latitudes, deserves to be aired, and Ocean Science is an appropriate forum for that. However, the paper requires revision in several areas, notably: (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.

The latter is very important. The authors are heavily invested in the idea of wake shedding off headlands; however, the text is very confused about whether the "ringing"

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



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.

In summary: Follow the review guidelines (I agree with everything Anon. rev. 1 said as well); focus on presenting the data better, then relax your commitment to headland dynamics and see if other processes deserve equal speculation. And ... keep what you know quite separate from what you speculate. As I said, these very interesting data deserve to be seen.

Technical/specific comments: 1440/26-1441/2: Mixing processes *are* part of the flow around a bluff body.

1440/2-5: Of course the "resulting flows" *are* associated with oceanographic flows! This sentence is fairly waffly, and doesn't progress your story.

Fig. 1: This figure, especially the "insets", is way too small. The setting is critical to your interpretation, so please expand this.

1442/9-12: Description difficult to read: break into 2 sentences.

General: Can you replace "around" with "~" wherever possible?

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.

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".

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.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Sections 2.2 and 2.3: The separation of this material leads to some confusion and dropped information. You would be better off (I think), 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, including the equations for ADCP shear (magnitude) and CTD buoyancy frequency. I would treat the profiler CTD separately from the shear microstructure, then have a third section for ADCP. The latter would cover the sampling characteristics and the calculation of shear.

1443/28-29: You don't tell us what profile you are sorting, although it must be potential 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. You have to get the T-to-C lag just right, and the thermal mass adjusted exactly. Just using nominal values from Seabird is not enough. Until you describe this process, I can't trust the Thorpe reordering or the profiles of S and density in Fig. 4.

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.

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. This will make aperiodicity much clearer, and also show subtidal "mean" flows. Each plot could show currents are 2 or 3 depths; near-surface, near ice base; and "deep".

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

speed and direction, as I think this will highlight the structure of the waves better, and the relationship to ice front orientation.

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

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 ~ 4 k, 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". They'd still be interesting, but your interpretation might be totally different.

1444/27: "but still a moderate proportion". This is way too vague!

1445/6-24: The backscatter *is* conceivably interesting. (a) Perhaps you need a blow-up 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?

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.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



1446/5: This should be in terms of *potential* density, yes? Especially important in weak stratification like this.

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.)

Fig. 4: Is T or potential temperature the right thing to plot here? General: can you use s^{-2} instead of $(\text{rad s}^{-1})^2$ for N^2 and S^2 units? Definitely don't need "rad".

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.

1447/5: Reference to Fig. 5 (although, make sure figure order is right.)

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

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?) You can make this case more clearly by putting marks on Figs. 3 and 4 indicating approximate local ice base.

1447/24-1448/4: You need to do a better job of explaining the time scales. 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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

changes direction relative to the ice front.

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.

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).

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 choose the range of 50-500 m for it.

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.

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.

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 R_i 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 R_i , I don't think you should show R_i .

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".

1450/5: With this large value of N^2 , buoyancy period is 0.6 h, and so the ringing waves

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



might just be near-N, not wake.

1450/9: I don't see equation numbers on my copy.

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 $1e-5$.

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.

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

1451/1-3: So, now you tell us that a time scale is 8 minutes, and you quote a value of N^2 that has not been seen or talked about before. Where do these values come from, and why are they relevant now?

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.

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.

Misc. typo/grammar: 1440/18: "that"=>"than" 1442/2: change "largely" to "primarily" 1445/3: Call-out should be to Fig. 3c, not 3b. 1449/14: "Present" => "Recent" 1450/21: Give a citation to "Osborn", and spell his name correctly.