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Interactive comment on "Pine Island Glacier ice shelf melt distributed at kilometre scales" *by* P. Dutrieux et al.

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We thank the referee for his/her time, positive criticism, and suggestions.

>General comments

>The paper provides detailed calculations of basal melt beneath the Pine Island
>Glacier ice shelf. Understanding the ice/ocean interaction in such regions as this is
>very important for understanding how the system might respond to changes in the
>ocean, and this paper provides a step forward in our understanding of the scales at
>which interactions take place. The paper also introduces an alternative method for

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>considering ice shelf change which has not been used before. As a result the paper >represents a valuable addition to the literature.

Thank you.

>However, because the paper is introducing a new method to this area, the paper >could be more friendly to non-specialists. I have a limited amount of experience with >Langrangian methods, and found the methods difficult to follow, especially as it >combines both Langrangian and Eularian methods. As a result, the importance of >using the approach does not perhaps come across as strongly as it could. Given that >the paper is under consideration for a journal with a generalist audience, rather than >an audience familiar with the methods, I suggest more background is given to the >method, and more help/narrative given to the reader as to why each calculation is >being carried out. I have also highlighted the parts I found difficult to follow below, >and have given suggestions for how it could be made clearer, assuming I am >interpreting it correctly!

We have used most of your advices and modified the text to introduce the Eulerian and Lagrangian framework in a more pedagogical way. The Eulerian framework is now presented first. A sentence explaining the benefit of the Lagrangian framework has also been added.

>My other concern relates to the fairly major assumption that the ice is floating in >hydrostatic equilibrium, which the authors go on to demonstrate is not actually a >valid assumption in the region. The authors dismiss this fairly easily at the end of the

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>discussion section, without making any attempt to quantify the impact of the
>assumption violation on the results, just that it smooths the result a bit. I strongly
>recommend that a quantification of the impact of this assumption is carried out to
>provide an error estimate due to this assumption.

Most of the discussion section is actually investigating the validity of the assumptions, including that of hydrostatic equilibrium. There is unfortunately no definite way to conclude, as at km scales bridging stresses and horizontal variations in ice density are both likely playing a role, but we have shown that in some places (for the longitudinal channels), the hydrostatic equilibrium assumption might lead to an underestimate of melting, while in other places (for the transverse channels), this assumption might lead to an overestimate. We therefore expect that our results are a smoothed version of reality at the channel (km) scales, but do not see how we could investigate this issue further.

>Specific comments
>Section 2.2: Add a paragraph to the start of this section which introduces the
>Lagrangian approach (and eularian too, readers may not be familiar with this term
>either) more thoroughly and how it compares with a eularian approach. You have
>briefly introduced it in the intro and earlier in the methods section, but it needs
>further description, for example introducing the notation (see next point). Perhaps
>the section from p1597, section 2.4 might be more appropriate in here to set up why
>you are doing a Langrangian approach, and what extra information you can gain in
>comparison with previous studies, though this may not work as you need to have
>introduced the terms.

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Hopefully, the changes made clarify the need for a Lagrangian approach.

>P1595, line 20: this would be clearer written as ": : :from melt or accumulation, >either surface or basal"

Indeed. Change made.

>P1595, line 18: specify somewhere in the section that DH/Dt (capital D) is >Langrangian notation, and put brackets around the divergence operator and .U in eq >1 to make it easier to see that the divergence is associated with the velocity, and not >the ice thickness.

Changes made.

>P1595, line 24, as above, add the brackets.

Change made.

>P1597, eq 4 and generally. It would be useful to have both the words and the >mathematical notation for each of the terms, to help keep track of which is which, >this could replace the "second term in equation : : :" bit in parentheses.

>P1597, eq 4, is this (u.div)h? brackets again would help with understanding this.

>P1597, suggestion for modification of paragraph (changes in <>): (. . .)

Agreed to all.

>Section 2.5, some more narrative distinguishing why/what is Lagrangian and Eularian >would be helpful.

This section concentrates in decomposing the budget in small- and medium-scales. Getting back to defining Eulerian vs Lagrangian here would probably be confusing.

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>Section 2.5, eq 7, again, some brackets around the divergence operators would help

>to understand this equation.

Done.

>P1598, line 4, “so that substituting Eq. (5) and Eq (4) into Eq. 2”

Change made.

>P1598, line 8: “medium scale surface height advection”

Change made.

>P1598, line 22: clarify where “these” areas are.

’These’ is contextualized within the same sentence.

>P1599, section 3.2: more narrative is required here as to why you are doing these

>calculations and why you are doing it in the Eularian framework.

Although it might suffer from aliasing, the Eulerian framework simply allows a decomposition of the time rate of change and height advection contributions. It therefore might give a ’more detailed’ view, or perhaps a more customary view, of the physics at play. We feel that our introductory sentence, along with the changes made earlier in the text, are now clear.

>P1599, line 22: you talk about extending the basal melting extent over the entire

>trunk, but don’t show a figure for this?

We are not exactly talking about extending the area of the basal melting estimate, but merely stating that if we assume a balance between height advection and melting, then we can use the larger area coverage of the height advection estimate to infer melting over this larger area.

>Figures: There I some inconsistency is the size and layout of the figures, figures 34

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>could be laid out in a 2x2 format rather than a 1x3 format which would allow them to

>be larger.

The figure format is likely to change when (if) the paper is accepted and in production. All figures should then appear with a consistent size.

Interactive comment on The Cryosphere Discuss., 7, 1591, 2013.