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The Cryosphere
Discussions

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: Through analysis of high-resolution satellite/airborne
>observations using a Lagrangian method, the authors quantify patterns of oceanic
>melt at the base of Pine Island Glacier ice shelf. Previously observed transverse and
>longitudinal basal channels are found to play a significant role in controlling the
>spatial distribution of melting, indicating that these small-scale (1 km) features must
>be understood and either resolved or parameterized if the melting is to be modeled

>accurately. Theories about the formation and evolution of basal channels that are
>consistent with observations and known physical processes are presented. The
>analysis is technically sound, and the authors do an excellent job of summarizing
>earlier work on PIG and integrating their study into a coherent picture of this ice
>shelf.

Thank you.

>Specific comments: Overall, this is a strong contribution, so my specific comments
>consist only of requests for clarification.

>P1596, L12: How is the Lagrangian elevation change assigned a position relative to
>the Eulerian grid used for the other terms?

The Lagrangian elevation change position is assigned mid-way between the initial (2008) and the final (2011) positions of each ice parcels.

>Section 2.4: It might be easier for the reader (especially the first time through) if the
>more usual method were discussed before your new method, rather than after. Also,
>a brief comment on the advantages of the Lagrangian method might be a useful
>preview of the analysis presented later.

We agree and have modified the text accordingly.

>Section 2.5: I have several questions on this section. How was the scale for
>smoothing chosen? Why does this smoothing window imply a length scale of > 10
>km for the medium scale? Why are features smaller than 2 km eliminated from the
>small-scale anomaly field? Also, just in terms of word choice, it seems rather odd to

TCD

7, C851–C853, 2013

>have a "small" scale with a lower bound and a "medium" scale with no upper bound.

The scale for smoothing was chosen by looking at the spectral characteristics of the surface elevation field (in space), and trying to define a cutting frequency that would separate the channel signal from larger scale signals. In essence, the 4 x 4 km (16 km2) boxes window smoother removes channels with 4 km wavelength. The first 're-solved' wavelength of the smoothed field is closer to 12 km than 8 km, but we settled for 10 km in the text. The resolution of our deduced Lagrangian elevation change depends on the area we use for cross-correlation. Here, we use 2 x 2 km boxes, and hence should not expect to resolve elevation changes at scale much smaller than 2 km. This why we initially limited the 'small-scale' to >2 km. It was unnecessary, and not very well defined, however, so we removed this scale boundary. We also changed our medium-scale denomination to large-scale, for clarity.

>Section 3.3: Is the "channel" scale the same as the "small" scale, or is it smaller? If

>they are different scales, how were they separated?

Channel and small scales are identical in this analysis. This is now clarified in the text.

>Technical comments: P1594, L4: The length scales "short (1 km)" and "medium (

>10 km)" are different from the definitions given in section 2.5 later.

Text modified for coherency. Thank you.

>Caption of Figure 3: This should state more clearly that parts (a) and (b) are the

>small scale anomalies from the smoothed fields.

Done.

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