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## ***Interactive comment on “Cascading water underneath Wilkes Land, East Antarctic Ice Sheet, observed using altimetry and digital elevation models” by T. Flament et al.***

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This paper describes analysis of a major subglacial lake drainage event in East Antarctica, using an array of remotely sensed datasets.

This is not an easy paper to read. For a TC paper it contains a lot of technical information. That is not necessarily a problem if the paper is well written, but I’m afraid that to make this paper acceptable for publication in TC, it will probably need the text to be edited significantly to improved readability, and may also require some restructuring. At the moment, the technical nature of a large part of the paper may make it more suitable for a specific remote sensing journal. This would be a shame, however, as there is an

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[Interactive Discussion](#)

[Discussion Paper](#)



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Comment](#)

important glaciological paper in here somewhere. At the moment, however, the glaciological message is very much buried in some rather complex and poorly structured, technical descriptions and information. I do appreciate that the author's first language may not be English, but there is no excuse, for example, for including descriptions of your methods in what appears to be the results section.

The importance of this paper, and its methodology, lies in the high spatial and temporal resolution of their data (from multiple instruments), which enables an unprecedented, catchment-long analysis of a major subglacial hydrological event (the drainage of CookE2). It is becoming increasingly recognised that to characterise basal subglacial hydrological networks, such a methodology may need to become the 'standard' approach in future.

Furthermore, the CookE2 event is the largest subglacial drainage event yet recorded beneath the Antarctic Ice Sheet(s). The authors demonstrate a 500 km, catchment-long, theoretical flow path for the drainage event (and a 70 m drawdown of the ice sheet surface within 18 months for CookE2, a 30 m lowering of a secondary lake, and a total discharge of  $5.16 \pm 0.47 \text{ km}^3$ ). This is an important finding and needs to be emphasised far more in the paper. This is an incredibly dynamic system both in time and space.

As well as the difficulties of following the complex narrative, and the lack of a clear statement justifying why the study of this event is important, the paper is somewhat undermined by some irritating problems with the figures (some, though not all, of which are detailed in the specific comments below). I recommend that the authors undertake a thorough review of all their figures.

In addition, what is very much lacking is a figure of the bed elevation around Lake CookE2. This should show both the bed elevation data used for the hydrological calculations, and the coverage of the measurements that went into making that bed elevation digital elevation model (see specific comments below for page 849). The authors

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should overlay the Antarctic lake inventory (not just the active lakes) over this bed elevation map.

Nowhere does the paper explain the implications of the authors' findings for other Antarctic subglacial hydrological systems. The discussion needs to compare this event with others in Antarctica (e.g. the Adventure Subglacial Trench event).

There is a really important paper, based on some impressive analytical work, hidden in this manuscript somewhere, it will just require some work to bring it to the fore.

Specific comments:

Abstract – The existing content of the abstract can be condensed significantly. The authors should then add justification for their investigations. Stating that “These observations contribute to a better understanding of the water transport beneath the East Antarctic ice sheet” is rather generic; the authors should make a more specific statement as to what their study contributes.

Page 842- line 15: I'm not convinced the word 'wave' is really the most appropriate. How about 'pulse'?

Introduction – The introduction is a slightly basic appreciation of our current status of knowledge and why understanding subglacial hydrology is necessary. I recommend that it is significantly sharpened and condensed. It currently contains a lot of superfluous text.

Introduction - I would add an additional line or two that that states why the work the authors have undertaken is important (the justifications are manifold: e.g. potential influence on ice dynamics/a phenomena not currently captured in ice sheet models/sediment transfer etc. etc.).

Page 843-line6: Gray reference has precedent, please move to before Wingham et al.

Page 843-line10: in rather than on?

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

Page 843-lines 12-13: “explicitly taken” rather than “taken explicitly”?

Page 843 – line 21: You might want to consider dropping the word ‘exceptional’ as it may only be exceptional because of our current lack of data (spatially and temporally)?

I am not an expert in the processing of remotely sensing data, but the content of the data and methods section appears robust. As a non-expert, however, I did find it to be a rather dense and complex methods section to work my way through (particularly the section on ‘DEMs from stereo-imagery’). Whilst this may perhaps reflect my own deficiencies, rather than anything that the authors have done wrong, the authors may wish to consider re-working the grammar to improve readability.

Page 844-lines 19-20: Change the comment in the brackets to “. . .because this date is our best estimate for the start of the drainage of Cooke2 (see section 5.2)”

Page 849 – lines 10-12: How well constrained is the BEDMAP2 dataset in the area of the Cooke2 flow path? I would like to see a diagram of the BEDMAP2 product AND the coverage of data (both available from the BEDMAP2 data website) presented, at the very least to demonstrate the geographic context of Cooke2. Plotting these data myself, it appears that there is a considerable bed elevation ‘data gap’ to the north of Cooke1.

Also, is there a higher resolution dataset that might be available, upon request, from those that acquired ice thickness measurements in the area of interest? Lake Cooke2 appears to be within the area of the Wilkes Land survey (WISE ISODYN, the UK-Italian collaboration). If so, perhaps the authors may want to ask for a copy of these data for their analysis. It is important that the authors consider the impact of the more detailed bed topography, and should at the very least reference the paper by Jordan et al., Terra Nova, 2010 (DOI: 10.1111/j.1365-3121.2010.00944.x) “Hypothesis for mega-outburst flooding from a palaeo-subglacial lake beneath the East Antarctic Ice Sheet” that details the detailed geomorphology of this area, inferring a potential palaeo subglacial hydrological routing. There are other numerous papers (mainly potential field

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data) on this survey. The papers that I am aware of were led by members of the BAS aerogeophysics team (Ferraccioli/Jordan/Corr).

Page 849-850: Section 4.1 comprises methods, and should therefore be moved to section 2. It could also be argued that large parts of section 4.2 should also be moved to section 2. Sections 4.1 and 4.2 do not describe the actual results sufficiently. It is not enough to simply say “The result of this selection is shown in Figure 4/5”. For a TC readership you should be drawing their attention to the pertinent areas and results of interest.

Page 849 – line 20: I don’t think that Carter et al. came up with this method. I suggest that you find, and use, the appropriate reference (probably Shreve 1972?)

Page 850 – line 3: The authors should consider carefully whether their 1 km grid spacing of the hypopotential map is appropriate. In their area of interest the bedmap2 ice thickness dataset would have been gridded at a resolution of 5 km (see section 2.1 ‘Note on grid resolution’ of Fretwell et al., Cryosphere, 2013, doi:10.5194/tc-7-375-2013). The ice thickness grid is only really 1 km resolution within 10 km from rock outcrops.

Page 857 – line 19-20: Personally, I would have liked the authors to have actually assessed the impact of the drainage event on ice dynamics in this paper, rather than concentrating overly on the technical aspects of the data and leaving it to others to consider the ice dynamics. I think assessing the impact of the drainage on ice dynamics would have made for a paper far more appropriate for a Cryosphere audience.

Figures: There are numerous problems with the figures that need addressing. For example, the location map is hidden away in figure 3 and is quite unclear.

Figure 3 – I don’t believe that this is the MODIS Mosaic of Antarctica. I suspect instead that it is the MODIS MOA masks for the coastline and the grounding line. Even if it was the MODIS MOA it would be of little use reproduced at this scale with elevation trend

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data overlain. The labels in this figure (e.g. CookE2) are also very unclear. Label the Cook Ice Shelf.

Figure 4 – Labels are unclear again. What about the SPRI lakes (SPRI 56 and 57?) that are in this area? Might be worth checking/including all the lake inventory data (Wright and Siegert, A fourth inventory of Antarctic subglacial lakes, Antarctic Science, 2012).

Figure 5 – personally, I'm not terribly taken with the shift from a 400 m contour below 2000 m asl, to a 50 m contour above this line. I understand why the authors have done this, but I'm not sure it helps.

Figure 6 – x-axis label should simply be 'year'.

Figure 7 – sorry, why are the x-axes simply not just 2002-2012 (a) and 2002-2008 (b)?

Figure 8 – I assume that the two sub-figures both use the same range of values for the MODIS brightness?

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Interactive comment on The Cryosphere Discuss., 7, 841, 2013.

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