

Answer to the comments by Neil Ross:

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 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.

Comments by both reviewers underlined that the structure was somewhat unexpected. We restructured most of the paper in a more traditional outline, separating data, method, results and discussion. We hope that the revised MS is now easier to follow. We fully agree that our paper has an emphasis on some technical aspects but this is needed so that our measurements are well-understood by the readers. This emphasis is retained in the revised MS although we now try to improve the relevance for The Cryosphere with, for example, an improved glaciological context through Fig 1 and 2 and by discussing some further implications of our study for the understanding of subglacial water flow (e.g., the fact that most of the water release by Lake Cook<sub>E2</sub> probably stayed trapped under the ice and did not reach the southern ocean).

We decided to submit this paper for a special issue of The Cryosphere, [http://www.the-cryosphere-discuss.net/special\\_issue35.html](http://www.the-cryosphere-discuss.net/special_issue35.html), related to the "Earth observation and cryosphere science" symposium held in Frascati, 13-16 November 2012 ([www.eo4cryosphere.info](http://www.eo4cryosphere.info)). We hope the emphasis on technical content is better justified in this context. We also want to underline that a recent paper studying also the drainage of Lake Cook<sub>E2</sub> (Mc Millan et al., GRL, 2013, now cited in our revised MS) also had the same emphasis on technical aspect and was published in multi-disciplinary journal (Geophysical Research Letters).

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 Cook<sub>E2</sub>). 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 Cook<sub>E2</sub> 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 Cook<sub>E2</sub>, a 30 m lowering of a secondary lake, and a total discharge of  $5.16 \pm 0.47$  km<sup>3</sup>). 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 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.

We now give more information on the context and better underline the importance of the findings.

All the figures have been modified. We added a general location map (Figure 1) and there is a new Figure 2 that presents the glaciological context for the Wilkes Lands (velocity, known lakes, bed topography and hydropotential).

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.

and

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

and

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.

and

Introduction - I would add an additional line or two 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.).

We largely rewrote/shortened the introduction with the objective to give a better overview of the context.

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

We re-established the chronological order.

Page 843-line10: in rather than on?

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)?

We accepted those suggestions

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.

This section describes original work and gives the full methodology, in the objective that the work can be reproduced. We hope that the re-worked description makes it easier to read.

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)”

We rewrote part of this section, the sentence was changed.

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.

We added a new Figure 2 including the bed topography and its uncertainty. It is true that the density of bedrock measurements is uneven throughout our study area but there is no other dataset to work with. We note that our revised method for determining subglacial water flowpaths is now able to pass through Cook<sub>E1</sub> (this was not the case and we admit this was problematic with our initial choice of parameters). We also note that the cumulative volume of water stored downstream of Cook<sub>E2</sub> shows a weak sensitivity to the extent of the area (the envelop of subglacial water flowpaths) where active lakes are searched. This is now described in the revised MS (discussion).

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

We contacted a member of the BAS aerogeophysics team since we started working on the Wilkes Land but the data was not fully published yet and not available at this time (2011). We did not obtain them when we asked again for the WISE ISODYN earlier this year. It is unfortunate but not a huge problem for our study because BEDMAP2 data seems to be sufficient to obtain reasonable water flowpath and delineate the flood region.

The area described in Jordan et al. (2010) is slightly to the south-west of the Cook<sub>E2</sub> flowpath. Our flowpath is located underneath the upper right corner of their Figure 1a. (see figure R1).

We now mention the Jordan et al. (2010) paper in the discussion as an example of other major subglacial flood that happened millions of years ago in the area.

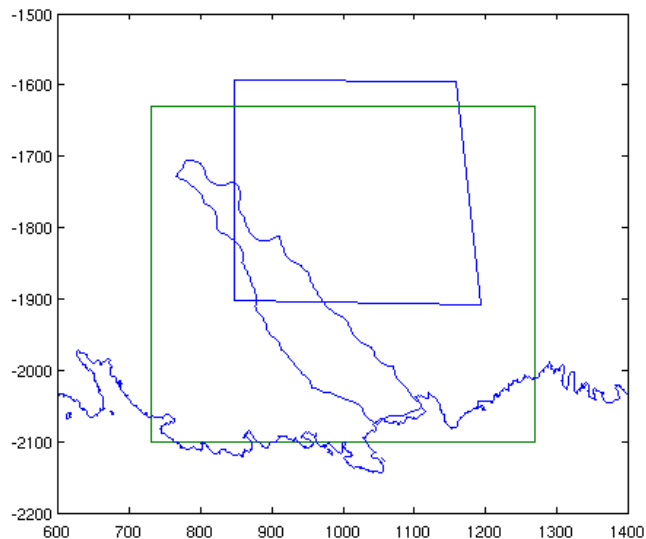


Figure R1: Location of our study area, green box and blue flowpath area, and that of Jordan et al. (2010), blue box. MOA grounding line (Scambos et al., 2007) is in blue.

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.

We moved large parts of section 4.2 in the “methods” section. Section 4.1 (drainage of lake Cook<sub>E2</sub>) indeed contains our uncertainty analysis. But this uncertainty analysis relies on some of the results. It was thus kept in the “Results” section.

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?)

We changed the reference

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.

The algorithm we present for the flowpath determination is fitting a plane to a disc of 5-km radius, which is similar to using a coarser resolution. We added a sentence to acknowledge the difference between the “true resolution” and grid spacing.

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.

Although our group has some experience in deriving ice surface velocities from satellite images in this sector of Antarctica (Berthier et al., 2003; Legresy et al., 2004), we stress that it is not a simple task especially for the central textureless part of the ice sheet. This is the reason why the first complete velocity mosaic of Antarctica was published only 2 years ago (Rignot et al., 2011) and repeat velocity measurements are generally only available for fast flowing outlet glaciers. Thus, producing a times series of ice surface velocity in our study region is a study in itself, especially to reach the high level of accuracy needed to measure temporal change in velocities.

We contacted Ralf Rosenau at the University of Dresden who computed flow velocity at the grounding line of the Cook Ice Shelf from Landsat image pairs using a method presented during an IGARSS meeting (Rosenau et al., 2012, Temporal flow variations of major outlet glaciers in Greenland using Landsat data, IGARSS 2012 Proceedings, 10.1109/IGARSS.2012.6351100). The ice velocity data in our area are unpublished and still analyzed but they do not show any obvious change during or after the flood (Figure R2, R. Rosenau, personal communication, 2012). One explanation could be due to the fact that most of the water release by the drainage of Lake Cook<sub>E2</sub> may have been trapped in the subglacial hydrological network. This is an important result of our study and it will help Rosenau et al. to understand the seemingly lack of temporal variations in surface ice velocity.

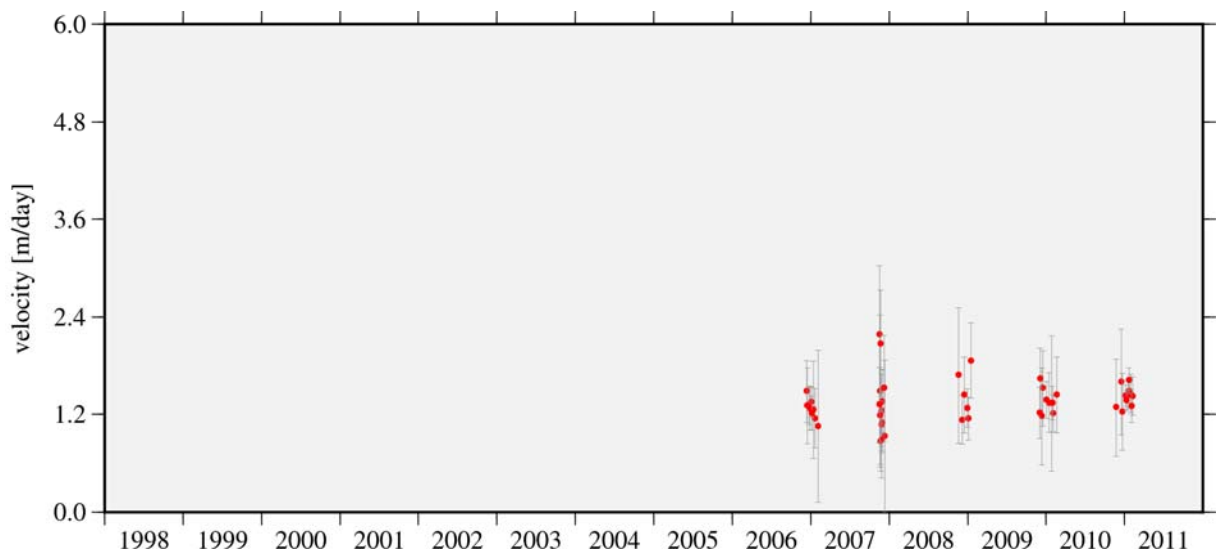


Figure R2: Evolution of the ice flow velocity estimated from Landsat image pairs at the point (68.53°S, 153°E), close to the grounding line of the Cook Ice Shelf. Unpublished results by R. Rosenau (University of Dresden).

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.

We worked on all figures and fixed many small issues.

We also added a new figure 1 for location, at the beginning of the paper and also Figure 2 to illustrate the glaciological context.

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

The reviewer is right, this is indeed the MOA masks. We modified this figure (now figure 6) and its legend, according to this comment and a similar comments by the other reviewer.

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).

We now included the lakes from the “Fourth inventory”.

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.

We changed to equally spaced contour lines of the hydrologic potential. Former figures 4 and 5 are merged in one single figure (Figure 7 of the revised MS).

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)?  
changed performed.

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

The MODIS mosaics are composite products and high-pass filtered; we do not aim to make a quantitative measurement of the optical properties of the surface. The 2008-2009 Mosaic is coded on 16 bits whereas the 2003-2004 mosaic is coded on 8. The brightness ranges are [78 144] (out of a maximum of 255) for the 2003-2004 mosaic and [15700 16350] (out of a maximum of 65535) for the 2008-2009 mosaic. The purpose of this figure is only to show that the drainage left a visible “scar” on the surface and that a similar pattern was visible at the surface of the ice-sheet before the drainage.