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Interactive comment on "Boundary conditions of an active West Antarctic subglacial lake: implications for storage of water beneath the ice sheet" *by* M. J. Siegert et al.

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Reviewer comment on "Boundary conditions of an active West Antarctic subglacial lake" by Siegert et al., 2013.

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1 General Comments

The authors of this paper perform a characterization for a subglacial lake (Lake InstituteE2 or Lake IE2) in the Filchner-Ronne sector of the West Antarctic Ice Sheet, using primarily airborne Radar Echo Sounding (RES) from a recent field campaign in the greater region (Ross et al., 2012). The survey was one primarily of opportunity as the flat circular basin ice surface made an ideal field camp location for an airborne campaign (Robison, 1964). Indeed, until the 2011 - 2012 field season the only knowledge we had of this lake was the surface expression of its filling as inferred from ICESat repeat pass satellite laser altimeter measurements, making it an "active" lake. As a result Lake IE2 joins a growing number of subglacial lakes that were initially discovered from satellite altimetry and then subsequently surveyed with RES. The authors show that were it not for the surface expression this feature might not have been classified as a "lake" using traditional RES classification methods (e.g. Carter et al., 2007; Siegert et al., 2005; Oswald and Robin 1973). This has been found to be the case for a number of "active" lakes (e.g. Wright et al., 2012; Christianson et al., 2012; Horgan et al., 2012). consequently Lake IE2 joins a growing number of "active" lakes which would not have been identified from RES alone. Given the temporal and spatial coverage limits of the ICESat mission from which most “active” lakes have been identified (Smith et al., 2009; Wright and Siegert, 2012), the authors go on to state that it is likely that there remains a number of undiscovered areas of water storage in Antarctica.

They also demonstrate several applications of RES to understanding the subglacial hydrology associated with the Antarctic subglacial lake system in this area, that compliment the capabilities of satellite altimetry. For Lake IE2 they show that the spatial extent of bright basal reflections is far smaller than the spatial extent of the surface expression observed from ICESat. Where bright basal reflections are present they are highly variable and not specular, consistent with a water depth of less than 10 m (Gorman and Siegert 1999). Although Lake IE2 is associated with a depression in the bedrock topography on the order of a few 100 meters, the lake position relative to that depression is offset in the downstream direction and more closely correlated with a surface low. Through a comparison of the surface elevation from the RES survey against a Digital elevation model by Griggs and Bamber (2009), they show how the hydropotential of the lake’s surroundings is highly sensitive to small errors in the surface elevation, highlighting the need for precise measurements of ice geometry in order to determine water flow routing. These findings are all consistent with work on another lake of similar spatial extent, Subglacial Lake Whillans (Christianson et al., 2012; Horgan et al., 2012; Carter and Fricker 2012). Overall the paper makes an interesting read that contributes to our evolving understanding of the subglacial hydrologic system in Antarctica, and helps outline some of the critical data needed to inform improved models of water flow in the region.

There are however a number of areas in which the ideas presented in this work, do not appear completely flushed out to their logical end. In some areas the authors make claims that are not fully reinforced by the evidence presented. In other areas the authors do not give enough consideration of published explanations for some of the phenomena they observed. Indeed a closer read of come of the sources they cite, might greatly aide their efforts. The work might also benefit from some additional figures and/or reworking some of the existing ones. The open format of *The Cryosphere Discussions* provides an excellent forum for the maturation of these ideas and we are grateful to the authors for including the greater community in the understanding of their work. I have outlined the major points in my Technical comments. Some additional ideas pop up in the Specific comments.

2 Technical Comments

A. The authors make a good point regarding the sensitivity of inferred hydrologic flow-paths to small errors in the ice surface topography and the implications of this sensitivity for efforts to model the hydrology using Bedmap2 (Fretwell et al., 2013) and other gridded data products. Although a number of studies are making significant advances in

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this department (e.g Livingstone et al., 2013), it is important to be aware of the limitations. I feel this point could be better quantified by mentioning that when using the Shreve (1972) method of calculating the hydropotential, that a 20 meter error in the ice surface elevation is equivalent to ~ 220 m error in the ice thickness to give the readers perspective. Additionally it would be useful to see a more direct comparison between the uncertainties of the gridded data products against the potential errors in the Airborne geophysical data used.

B. I am a tad surprised that there's not a denser survey grid and / or more data shown in the bed topography figures. My impression from Ross et al., (2012) was that lines running parallel to 90 W were separated by no more than 10 km, and so we'd expect to see a few of them on your diagrams showing the data used.

C. Also, although it not clear the Authors' respective roles in the survey design, it is my understanding that the survey post dates the publication of the 2009 (Smith et al.) inventory of active subglacial lakes and so it remains a mystery as to why there was not so much as a single flight conducted over an ICESat / GLAS ground track. Doing so would have greatly eased their efforts to estimate volume change following the cession of the ICESat mission in 2009. You might be able to work around this somewhat by using the intersections between RES profiles A-A', B-B' and C-C' with the ICESat track running along the north side of the lake. Indeed a quick study of Figure 6 in the paper seems to suggest that the largest discrepancy between the Griggs and Bamber (2009) surface elevations DEM and the surface elevations in this paper to occur along an

ICESat track coincides with the portion exhibiting the most surface motion (Figures 2, R1). Indeed given the above-mentioned evidence along with the parallels to Lake Whillans, I do wonder if lake IE2 is also at a lowstand presently?

D. Some of the authors' questions regarding the stability of the lake in its current locations might resolved through reviewing some additional literature on subglacial floods / subglacial lake drainage events. Evatt et al., 2007 provides a nice discussion on the surface deformation associated with the filling and draining of relatively small subglacial lakes. Fowler (1999) offers some insights on a possible triggering mechanism for subglacial lake drainage events relevant to your comments growth of Lake IE2 in relation to its stability. Evatt et al., (2006) and Fowler (2009) both address the question of whether or not a lake drains completely. Sergienko and Hulbe (2011) advances an interesting hypotheses about the formation of subglacial lakes in the lee of areas of high basal traction.

E. Work on flowpath determination in Carter et al., 2011 and Carter and Fricker, 2012 may provide some explanations as to why Lake IE2 surface signal appears where it does. Although the basal relief downstream of Lake IE2 is on the order of a few 100 m over 10's of km, upstream up the lake is a substantial bedrock trough over 1000 m below some of the surrounding topography (Ross et al., 2012). Indeed it appears Lake IE2 may lie at transition zone between a region where the hydropotential is dominated by surface topography downstream and bed topography upstream.

I took the liberty of running the model from Carter et al., 2011 using the Bedmap2 ice surface and bedrock geometry (Fretwell et al., 2013) along with some educated guesses for regional average melt rates of 1 mm a^{-1} (Evatt et al., 2006) and 0.6 mm a^{-1}. Lake IE2 is fed by a substantial catchment associated with the bedrock trough (Figure R2 and that with a melt rate of 0.6 mm a^{-1} we could reproduce the observed filling rate from the Smith et al., 2009 data set remarkably well (Figure R3). A further examination of the Smith et al., 2009 timeseries and associated paper would suggest that levels of volume change much less than those undergone by Lake IE2 would have been difficult to detect.

F. On a technical note regarding capitalization: “ICESat” is an acronym for “Ice Cloud (and land) Elevation Satellite.” Therefore it should not be written as “IceSat” but as “ICESat.” (Cham et al., 2008) provides a lovely discussion on the subject of scientific acronyms and capitalization.

3 Specific Comments

P2980 L10 and L 23: I am not sure if “appreciation” is the right word here.

P2981 L 01: It would be useful to provide some examples as to why this might be.

I can think of a few, note limited to but including crevassing (common along shear

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margins where lakes are often found.) The difficulties imaging through the relatively warm and heavily sheared ice, potential for accreted ice and sediment . . . to name a few.

P2981 L 11: You may also want to take a look at that Scambos et al., (2011) paper on a subglacial lake drainage in Crane Glacier.

P2981 L 22: I'd advise a close read of the methods section from Smith et al., 2009. Lakes from 1 line of ICESat were pretty rare. Also both Sergienko et al., 2008 and Pattyn (2009) addressed theoretical reasons for discrepancies between surface elevation change and lake volume change.

P2981 L 24: Thought experiment: If "lakes" are migrating packets of basal water, what would the ICESat signal from such a feature look like? I suggest the authors read some recent work by Flament et al., (2013), which could potentially shed some light on observing a travelling packet of water versus a lake drainage event.

P2981 L 24: I also advise reading Evatt et al., 2006 who suggests that lakes do NOT drain completely.

P 2982: L 1: Subglacial Lake Mercer from (Fricker et al., 2007) showed reasonably good agreement with Subglacial Lake Mercer from (Carter et al., 2007).

P2982 L 19: Did you use the Smith et al., (2009) timeseries from the paper?

P2983 L 22: It is my understanding that the hydropotential does not “deliver hydrological pathways,” but the authors can define hydrological pathways using the hydropotential surface.

P2984: L 22: It would be incredibly useful to see a plot of the bed elevation and hydropotential along the flow line in order to back up this point. Carter et al., (2011) contains such plots. I have also provided an example in Figure R4 using the Bedmap2 topography (Fretwell et al., 2013). I suspect the one you’d make will be better. This may also be a good time to compare the area of ICESat detected ice surface uplift against the bed topography and hydropotential surface. In particular I’d be curious to see how the hydropotential of the basin looks at the time of maximum uplift. This would be helpful in determining the validity of the argument you make regarding the ephemeral nature of Lake IE2.

P2984 L24-26: This is an excellent point. Did the Livingstone et al., (2012) model detect Lake IE2?

P2985 L6: This would be a good time to consider the effects of flexure on the apparent hydropotential (e.g. Ridley et al., 1993; Evatt et al., 2007). Indeed I believe Christianson et al., (2012) had similar findings for Subglacial Lake Whillans.

P2985: L 6: I noticed you reference (Carter et al., 2009) here, but in references you have (Carter et al., 2007).

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P2985: L 25: It might be useful to look more closely at the “basal roughness” calculations for the area (Ross et al., 2012) to determine whether the lake is surrounded by dry bed or wet sediments.

P2986 L 13 - 18: In general this paragraph feels a bit detached from the rest of the paper, except perhaps for reiterating the sensitivity of flow routing to the surface elevation. I'd suggest adding some more context from the Ross et al., (2012) paper here.

P 2986 L20: This is a very interesting discussion on the discrepancy between lakes identified with RES and lakes identified with satellite observations of surface elevation change. It has popped up in meetings and is definitely ripe for discussion now. Indeed it may be useful to try and move some of the scattered

P2988 L 6-27: This paragraph is generally quite well written section of the paper.

P2988 L22 - 23: This sentence however needs some clarification. Do you mean that it can only fill to certain height? If so what would be the level for floatation of the seal?

Figures

General figure comments:

The figures could use a substantial degree of improvement, as many of them are hard to read and fail to make optimal use of the space they occupy. I suspect the

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originals were substantially larger and that they inevitably shrunk during the TCD formatting and that the authors did not account for this during the creation of the figures. As it stands I find myself squinting to read them at 200% magnification with 20 / 15 vision.

The colorscales should all be in a consistent format, longer and contain more values (see Figure R2 for an example).

The scalebar should be more substantial. Also consider one of the following: lines of longitude latitude, or coordinates in PS71. A distance scale should be present on every figure.

More effort should be placed into making the shoreline for Lake IE2 visible in all figures. In some the color for the lake outline does not sufficiently contrast with the colormap.

There are a few figures that are missing from this draft that I consider to be necessary for you arguments:

B. A figure with contours of hydropotential, one colorscale for RES reflectivity, and other for ICESat inferred surface elevation change, the lake outline and your flowpaths (Figure R1)

A. A figure displaying the hydropotential along the flowpath (See Carter et al., 2011 or Figure R4)

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Specific figure comments:

Figure 1.

It would be better to have the color scale for figure 1a in the same format as other figures in the paper.

I am assuming figure 1b is surface elevation, please include this information in the caption.

Figure 2.

I'd suggest putting the outline of the lake on this figure.

This is also the only figure with a satisfactory colorscale bar.

I am assuming the coordinate system is Polar Stereo 71. Please include this information in the legend.

Figure 5.

This figure needs a scalebar, and also perhaps labels for A - A' , B - B' C - C'.

The dots for radar reflectivity values require a microscope to see, please enlarge (See Figure R1)

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The contours need labels (see Figure R2)

Figure 7.

It would help if 7a and 7b were reversed so that A' and B' were on the left as they are for Figure 1. As it stands the reader trying to match features in these against features in Figure 1 is going to get a bit confused.

It would be nice to see a plot of hydropotential from each of the RES sampled that coincided with radargram.

Axis labels need to be larger

X-axis would be more readable if it were in km as opposed to m.

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Figure R1: Overlay of ICESat elevation change, basal reflectivity, and hydropotential.

Figure R2: Distribution of water flux in region surrounding Lake IE2.

Figure R3: comparison of modeled versus observed volume change for Lake IE2

Figure R4: Example of flowpath geometry across Lake IE2