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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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General Comments

The authors present an in-depth study of a single subglacial lake in West Antarctica (Institute E2) using Radar-Echo-Sounding (RES) data. The lake is one of the "active lakes" originally identified using ICESat measurements of surface elevation changes (Smith et al., 2009). Antarctic subglacial lakes can be grouped into two broad categories based on how they have been discovered: radar lakes, located using RES measurements (Siegert et al., 2005), and active lakes, located using satellite altimetry measurements (Smith et al., 2009). This paper represents an attempt to bridge the two

categories of subglacial lakes.

What the authors discovered is that this particular active lake does not, in fact, look like a radar lake. Classic radar lakes are categorized by bright, specular, and flat radar reflections that occur within topographic minima. However, the authors show that radar reflections underneath the location of this active lake have variable reflectivity, are rough (non-specular and non-flat), and occur on the flank of a topographic slope, rather than at the minimum. If it were not for the fact that the active lake sits in a hydraulic (but not topographic) minimum, one would be tempted to conclude that there was no water present at all.

The results presented in this paper have wide-reaching implications for the use of radar to locate subglacial water. The results suggest that a large amount of subglacial water may be undetectable using classic RES techniques, and that much more subglacial water may exist in Antarctica than is presently known. In addition, the results demonstrate a pressing need for more advanced radar processing techniques that will be capable of locating a wider spectrum of subglacial water.

The authors mention these implications several times, however, I feel that the paper could be strengthened if they emphasized the broader impact of their work more. For example, an additional paragraph in the summary could be devoted to these implications, as well as to a discussion of the possible morphology the "unconventional water" might have (for example, linked cavities or a patchy distribution of saturated till). The results suggest that some of the active lakes may not actually be "lakes" (in the sense of large contiguous bodies of freestanding water) at all. The authors should not shy away from forcing the reader to confront this implication.

Specific Comments

A. Interpretation of active lakes. In addition to the broader implications described above, the authors should consider the possibility that this active lake might not have any water at all. Other processes, notably changes in basal drag, can lead to surface

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elevation changes (Sergienko et al., 2007). I think the authors are justified in concluding that water was actually present in this active lake based on two factors: the monopole nature of the surface elevation change (figure 2, it was also one of the selection criteria for active lakes in Smith et al., 2009) as opposed to the dipole nature of the change expected for changes in basal drag, and the coincidence between the hydropotential minimum and the outlines of the active lake (figure 4b). However, they should at least mention that they have considered all possibilities before dismissing them.

B. Radar attenuation. In section 3.2 and in figure 5 the authors discuss radar reflectivity data. In the caption to figure 5 they mention that an attenuation correction of 35 dB/km was applied to the bed-returned power to account for dielectric losses with the ice column. Where does this number come from? The authors should state how the attenuation correction was derived, or if it comes from a previously published source, they should state what that source is. In addition, the authors state in the caption to figure 5 "reducing the rate of dielectric absorption increases the relative basal powers in shallower ice, but does not adversely affect the general distribution of received RES strength." What does this mean? The most striking feature of figure 5 is that the highest basal reflectivity within the active lake corresponds to the hydraulic minimum. If that is what the authors mean, they should say so.

C. Hydrologic pathways. In section 2 (lines 20-23), section 3.3, and figure 4, the authors discuss hydrological pathways, and the difference between pathways calculated with their surface DEM and calculated with the Bamber DEM. However, they do not mention which routing algorithm was used to calculate the flow pathways. A large number of hydraulic routing algorithms exist (Tarboton, 1997) that can produce different pathways given the same input DEM. The authors should state which routing algorithm was used and discuss whether (and how) using a different routing algorithm would change their results.

D. Grids and input data. The flight lines shown are insufficient to generate the gridded

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elevation products presented in figure 1b and figure 4. Presumably, these grids were generated from the full survey data, not just the flight lines over the lake. The full set of flight lines should be shown somewhere.

Figures

Figure 1. Presumably (b) is a map of surface elevation? If so, this should be stated in the caption. The colorbars for both (a) and (b) should be placed in the white space to the right of the inset. In addition, the colorbars should have a larger font size, and the colorbar for (b) should have more than two values. The scale bars should have larger font size. (b) should have lat/lon lines and all survey lines, not only the survey lines used in this paper. The sun angle used to generate the hillshading for (a) should be stated. The lake outline in (b) is a black dashed line, not white as stated in the caption.

Figure 2. Presumably these are the same ICESat tracks shown in figure 1b? If so, that should be stated. In addition, an inset map with a simple arrow depicting the camera direction for this three-dimensional view would be helpful.

Figure 3. Why have all the dots not been connected?

Figure 4. The colorbars need to be larger, with larger font size and more numbers. The scale bars should have a larger font size. Consider moving both the colorbars and the scale bar below the figure panels. In addition, (a) appears to be hillshaded while (b) is not. (b) should be hillshaded as well, and the sun angle for both (a) and (b) should be stated in the caption. The purple lines representing the flow paths computed with the Bamber DEM are hard to see against the blue background in (b). Consider using a different color. Lat/lon lines should be added to the maps.

Figure 5. The font size should be increased for the reflectivity legend. The colorbar should have more numbers. Lat/lon lines should be added to the map. A scale bar should be part of the legend. Consider hillshading the hydraulic potential map to be consistent with figure 4. In addition, the description of how basal reflectivity was calcu-

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lated belongs in the methods section, not the figure caption.

Figure 7. Overlay the lake boundaries on the echograms. Make the display brighter (images always appear darker on a printed page or a projector than they do on a computer screen). Add a colorbar to this figure.

References:

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Smith, B., Fricker, H., Joughin, I. and Tulaczyk, S.: An Inventory of Active Subglacial Lakes in Antarctica Detected by ICESat (2003-2008), J. Glaciol., 55(192), 573–595, 2009.

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Interactive comment on The Cryosphere Discuss., 7, 2979, 2013.